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Estimating slim-majority effects in US state legislatures with a regression discontinuity design under local randomization assumptions

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Regression discontinuity design could be a valuable tool for identifying causal effects of a given party holding a legislative majority. However, the variable 'number of seats' takes a finite number of values rather than a continuum and, hence, it is not suited as a running variable. Recent econometric advances suggest the necessary assumptions and empirical tests that allow us to interpret small intervals around the cut-off as local randomized experiments. These permit us to bypass the assumption that the running variable must be continuous. Herein, we implement these tests for US state legislatures and propose another: whether a slim-majority of one seat had at least one state-level district result that was itself a close race won by the majority party.

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Regression discontinuity design (RDD) has provided political science with an important tool to identify causal relationships between wining an election and a series of outcomes. For example, a vast literature has looked at the electoral effects of incumbency (e.g., Lee 2008 and De Magalhães 2015). The conventional analysis of RDDs in these applications uses vote share as the running variable and relies on the assumption that the running variable is continuous, i.e., takes a continuum instead of a finite number of values. This assumption allows us to get arbitrarily close to the cut-off in order to estimate the local average treatment effect. This assumption does not hold, however, if the running variable is the number of seats in a legislature, as this variable has clear mass points at the integers. Thus, the results from the "continuity-based" canonical RDD setup are not applicable, nor do the standard smoothing methods apply without additional assumptions (Lee and Card 2008 and Kolesár and Rothe 2018).

The use of the number of seats as a running variable is further complicated as there is no clear mapping between the underlying vote share – the preferred running variable for an RDD – and the number of seats allocated to each party. Folke 2014 is the first to discuss this issue in the context of proportional elections and their seat allocation rules. In our case, the underlying vote share is determined by district-level first-past-the-post races where the cut-off is known, but also where these district-level races can be aggregated in various ways and result in the same seat share (Feigenbaum, Fouirnaies, and Hall 2017).

The contribution of this paper is to draw on recent theoretical work regarding the implementation and interpretation of RDD as a local randomized experiment (Cattaneo, Frandsen, and Titiunik 2015 and Sekhon and Titiunik 2017) and use that work to study whether the number (or percentage) of seats can be directly employed as a running variable for RDDs. We apply the local randomization RDD framework to determine a specific window for our discrete running variables (the number and the percentage of seats). Furthermore, we use the underlying vote share to establish whether there were enough district-level races that were themselves unpredictable/unanticipated, so that we may think of the slim-majority versus slim-minority treatment status also as unpredictable/unanticipated.

Note that the method presented here can be directly applied to proportional elections. The discrete running variable remains the number of seats and the same window selection method applies. The equivalent to the test proposed here would be to check how close the vote share of each party was to the threshold that determines the allocation of an extra seat.

There are two necessary assumptions in order for the number (or percentage) of seats to be an appropriate running variable in the local randomization RDD framework. First, for a specific window around the cut-off, the value taken by the running variable (i.e., the number (or percentage) of seats) is considered to be randomly allocated. Second, we must impose an exclusion restriction: the size of the majority within this window (e.g., whether a majority of one or two seats) has no direct effect on the outcome of interest.[1] All that matters for the outcome is assignment of majority status, i.e., treatment. We see this is as a justifiable assumption as the posts of House Speaker and Senate President are assigned as soon as there is a one-seat majority.[2] A few more seats do not increase formal power over procedures. The exclusion restriction also requires that differences in the winning margins for each seat have no direct effect on the outcome of interest. We see this as justifiable as we find that almost all slim-majorities have some seats that were themselves the result of close district-level races.

Implementing the method suggested by Cattaneo, Frandsen, and Titiunik 2015, we find

[1]See Cattaneo, Titiunik, and Vazquez-Bare 2017 for a method to relax this assumption.
[2]These posts come with clear powers: agenda, committee appointments, and resources (Clucas 2001).

that windows around the cut-off of two seats or one percent of seats in state Houses have enough observations and achieve balance of predetermined covariates. This indicates that RDD estimates within these windows can be interpreted as local randomized experiments. However, slim-majorities in state Senates cannot be interpreted as local randomized experiments, as the balance of predetermined variables is not achieved for any window.

We go beyond Cattaneo, Frandsen, and Titiunik 2015 and propose a further test. We verify whether slim-majorities had enough state-district races that were themselves close and won by the majority party. For example, we determine that within our sample all state Houses with slim-majorities of one seat resulted from elections with at least one close state-district race won by the majority party. In other words, there is no evidence to suggest that one-seat slim-majorities could have been predetermined or manipulated.

The closest paper to ours is Feigenbaum, Fouirnaies, and Hall 2017. They propose a continuity-based multidimensional RDD using district-level voting in order to address the lack of continuity in the number of seats in the US state Houses (they do not apply their method to the Senates). Their method requires the choice of a metric to build a one-dimensional running variable from a series of state-district race results. With infinite metrics to validate, their method would – at the limit – be a matching estimator and no longer a design-based approach (Feigenbaum, Fouirnaies, and Hall 2017, p.18). Here, instead, we employ the number of seats directly as the running variable. Thus, the method presented here – if valid – is more straightforward to apply. Moreover, our approach does not require the availability of lower-level electoral data. If lower-level data is available or partially available, as in our case, the test proposed here can be implemented.

Another stance would be to define an aggregate measure of the results in district-level races as an alternative running variable that is measured with error. In this alternative setting, an election could yield a slim-majority without this running variable being close to the cut-off or vice versa.[3] Our approach is different. We assume that the running variable is the number (or percentage) of seats and that it is measured without error. We identify our effect by comparing slim-majorities within a precise window. Any observation that does not fall within the determined widow is excluded from the analysis, whether or not it could have been classified as 'close to the cut-off' with an alternative running variable based on the aggregation of district-level races.

As an illustration, we use the method proposed here to estimate the effects of partisan control of the state Houses on electoral outcomes. Formally, our RDD parameter of interest is $E[Y_i(1)|X_i = 1] - E[Y_i(0)|X_i = -1]$, where $Y_i(1)$ is the outcome in $t + 1$ given that party i won a slim-majority in t of one seat, $X_i = 1$; and $Y_i(0)$ is the outcome in $t + 1$ given that party i won a slim minority in t of one seat, $X_i = -1$. This parameter is identifiable under the assumptions described above (see also Cattaneo, Idrobo, and Titiunik 2019). In practice, we determine in the following whether we can compare slim-majorities and minorities of only one seat or whether we must increase the interval to more seats.

Our estimates suggest there is no causal effect of winning a slim-majority in the state House on: i) winning another majority in the subsequent election, ii) influencing the election for Governor, or iii) influencing the majority in the state Senate.[4]

Validity of RDD with slim-majorities

The available data is comprised of that from the 48 continental American states from 1960 to 2006 and is the same as Besley and Case 2003. We have updated their sample from

[3]One could produce valid estimates in such a setup with methods described in Pei and Shen 2017.

[4]In the on-line Appendix we also show null effects on the state tax level, and unemployment.

1960 to 1998 with data from 1999 to 2006.[5] To keep the sample comparable, we focus the analysis on the states with the most common set of institutions: a partisan two-chamber legislature and a governor with line-item veto power. The working sample has 41 states from 1960 to 2006. Each observation is a state-election-year.

We also have data on state electoral returns at the district-level spanning 1967 to 2003 (Carsey et al. 2008). There are an approximate 18% missing values for the variable that we are interested in: the margin of victory, defined as the difference between the percentage of votes that the winner received and the percentage of votes that the second-place candidate received in each state district.

For clarity, we reserve the word 'election' for the state-wide poll that determines how many seats each party will hold in the state legislature. We reserve the word 'race' for a state-district-specific poll that determines the partisan identity of each state-district's representative.

Slim-majorities and close state-district races

Each US state is divided into state-districts. During an election, each state-district chooses a representative to the state legislature by a first-past-the-post system.[6] If each of these state-district races were the result of large and predictable winning margins, then there would be no uncertainty regarding which party would win the majority of seats in each election. A slim-majority of one seat would be as predictable and predetermined as a landslide majority of 20 seats. Therefore, the first test we propose in order to establish that

[5]Sources: Census Bureau, state legislature websites, the website for the National Association of State Budget Offices (NASBO), and the website for the National Conference of State Legislatures (NCSL)

[6]A small minority has free-for-all multi-member districts. These are excluded.

slim-majorities can be interpreted as local randomized experiments is to establish that enough state-district seats were themselves the result of close races that were won by the same party that won the majority of seats.

In Table 1, we present the data in the form of different windows around the cut-off of our running variable, *Democratic majority*. The variable takes a value of 1 if the Democrats hold a majority of one seat, 2 if there is a two-seat majority, -1 if the Republicans hold a majority of one seat, -2 if the Republican majority is of two seats, and so on. The cut-off is 0. *Democratic majority* also takes the value 0 if there is a tie (some legislatures have an even number of seats). In row 1, column 2, we can see that there are 10 elections for state Houses that have ties (i.e., both Republicans and Democrats hold the same number of seats) or slim-majorities of one seat. In row 1, column 3, we can see that all (i.e, 100%) of these 10 elections had at least one state-district race won by the majority party with a winning margin of less than 5% of the district vote, our chosen threshold for a close election.[7] This suggests that all majorities of one seat were neither predetermined nor predictable. Had that one seat gone to the other party, the partisan identity of the majority in the House would have changed.

As we descend through Table 1, we increase the size of the window around the cut-off. Row 2 shows that out of 20 observations (i.e., state-years) with slim-majorities of two seats or less, 19 (i.e., 95%) had at least two state-district races won by the majority party that were close (the remaining observation only had one district race won by the majority party that was close). Ill-health, scandal, or the weather, for example, could also change the result of a particular seat in unpredictable ways and still deliver a winning margin

[7]One potential interpretation of this threshold is that 5% is the margin of errors of surveys of 1,000 respondents. This suggests that even if there were district-specific polls, it would be difficult to predict the result of each race.

TABLE 1 *Percentage of slim-majorities that could not be predicted*

Democratic majority	Number of observations (state-years)	Percentage with enough close district races (less than 5% of district vote)
State House		
Number of seats		
-1,1	10	100
-2,2	20	95
-3,3	34	79
-4,4	23	78
-5,5	60	78
Percentage of seats		
-1,1	18	83
-2,2	40	62
-3,3	61	54
-4,4	84	49
-5,5	96	43
State Senate		
Number of seats		
-1,1	41	53
-2,2	69	46
-3,3	98	43
-4,4	83	45
-5,5	164	38
Percentage of seats		
-1,1	18	83
-2,2	43	84
-3,3	67	57
-4,4	83	37
-5,5	115	31

Note: The first column indicates the size of a window around the cut-off $Democratic\ majority = 0$. The second column reports the number of observations in each widow for which we have state-district electoral data. The third column reports the percentage of state-district races in each window that were close wins for the majority party. The data on election results by state district has been provided by Carsey et al. 2008.

above 5% of the vote-share. For that reason, we see the rate of 95% in row 2, column 3, as indicating that the -2,2 window should not be rejected as a potential window within which we can interpret the results as a local randomized experiment. As the window size increases to the -5,5 window, the percentage of close state-district races won by the majority party approaches 80%.

We now pursue the same exercise, but instead look at the percentage of seats as the running variable (rows 6 to 10 in Table 1). There are 18 slim-majorities of 1% of seats or less; 83% of them have at least 1% of close state-district races that were won by the majority party. As mentioned in the previous paragraph, the result in individual seats can be unpredictable and still deliver winning margins above 5%. For this reason, we see a rate above 80% as an indication that these slim-majorities were neither predetermined nor predictable. As the window size increases in rows 7 to 10, the percentage of slim-majorities that had enough state-district close races won by the majority party decreases to 62% for the -2%,2% window and to approximately 50% for larger windows. Overall this indicates that windows other than the -1%,1% interval are not valid as a local randomized experiment.

From rows 11 on-wards in Table 1, we follow the same procedure as before for the state Senates. In all five windows in which the number of seats is the running variable, approximately half of slim-majorities in the state Senates are the result of elections in which none of the state-district races won by the majority party were close. This suggests that even the smallest window of -1,1 seats cannot be interpreted as a local randomized experiment. Utilizing the percentage of seats, both the -1%,1% and the -2%,2% interval have more than 80% of slim-majorities with enough close state-district races that were won by the majority party. Therefore, we do not reject that these two windows could be interpreted as local randomized experiments.

Determining the windows for local randomization

In this section, we follow the practical steps suggested by Cattaneo, Frandsen, and Titiunik 2015 to establish whether local randomization is plausible in small windows around the cut-off and determine the size of such a window. The procedure involves a simple difference-in-means test for the predetermined covariates comparing their values on each side of the cut-off. This test is carried out for each candidate window. If the p-value regarding the null that a covariate has the same value for both sides of the cut-off is below 0.15 (as suggested by Cattaneo, Frandsen, and Titiunik 2015), then that window is rejected and we attempt the procedure with a smaller window. A window is selected if one cannot reject the null for any of the predetermined covariates using a threshold p-value of 0.15. As suggested by Cattaneo, Frandsen, and Titiunik 2015, we only consider windows with at least 10 observations on each side of the cut-off.

The implementation of this method for the US legislatures can be seen in Table 2. In column 1, we list different windows around the cut-off, $Democratic\ Majority = 0$.[8] In column 2, we list the number of observations (i.e., of state-years) on either side of the cut-off. In column 3, we list the lowest p-value from the balance test for a series of predetermined covariates. In column 4, we list the covariate with the lowest p-value.[9]

The electoral covariates that we test for balance are the following: *Dem Majority Senate*, an indicator variable that takes a value of 1 if the Democrats gain control of the

[8] We exclude observations in which the state House had a tie between Republicans and Democrats (seven observations). Treatment cannot be defined in these cases. We only exclude ties in the Senate from states that did not have the post of Lieutenant Governor (four exclusions). Lieutenant Governors supply the deciding vote in case of a tie.

[9] We use large sample inference to calculate the p-values in the paper. In the on-line Appendix we show that finite sample inference would not alter our results.

TABLE 2 *Minimum p-value of predetermined covariates for different intervals*

Democratic majority	n. obs	Min. p-value	Covariate
State House			
Number of seats			
-2,2	14/12	0.27	Dem Majority Senate
-3,3	25/18	0.20	State Taxes (%GDP)
-4,4	34/26	0.25	Dem Governor
-5,5	43/37	0.13	Dem Majority Senate
Percentage of seats			
-1,1	12/11	0.23	Dem Majority Senate $t-1$
-2,2	25/27	0.15	State taxes (%GDP)
-3,3	37/42	0.13	Midterm Election
-4,4	63/55	0.02	Dem Majority House $t-1$
-5,5	72/65	0.00	Dem Majority House $t-1$
State Senate			
Number of seats			
-1,1	27/23	0.07	Dem Governor
-2,2	53/36	0.00	Dem Majority Senate $t-1$
-3,3	69/57	0.00	Dem Majority Senate $t-1$
-4,4	90/75	0.00	Dem Majority Senate $t-1$
-5,5	108/101	0.00	Dem Majority Senate $t-1$
Percentage of seats			
-1,1	6/14	0.01	Dem Majority Senate $t-1$
-2,2	21/27	0.05	Dem Governor
-3,3	46/39	0.00	Dem Majority Senate $t-1$
-4,4	60/44	0.00	Dem Majority Senate $t-1$
-5,5	80/65	0.00	Dem Majority Senate $t-1$

Note: The first column indicates the size of the window around the cut-off *Democratic majority*$= 0$. The second column reports the number of observations on each side of the cut-off within a widow. The third column reports the lowest p-value of a balance test for a series of covariates using large-sample inference. Column four reports the covariate with the lowest p-value.

state Senate in the current election; *Dem Majority Senate in* $t-1$; *Dem Majority House*, an indicator variable that takes a value of 1 if the Democrats gain control of the state House at the current election; *Dem Majority House in* $t-1$; *Dem Governor*, an indicator variable that takes a value of 1 if the governor is a Democrat during the current election or wins the current election; *Dem Governor in* $t-1$, i.e., the election result two or four years previous depending on the electoral cycle. The economic covariates that we test for balance are: State *real income per capita* in 1982 US$; *unemployment rate*; *local property taxes* (%GDP); and *state tax level (%GDP)*. We also treat the year of the election as a covariate.

The -2,2 window does lend itself to be interpreted as a local randomized experiment (Table 2, row 1). There are more than 10 observations on each side of the cut-off and the predetermined covariate with the highest p-value is *Dem Majority Senate* with a p-value of 0.27, above the 0.15 rejection threshold. Note that the number of observations is similar on either side of the cut-off, also a condition for the validity of RDD. Finally, from Table 1, we know that more than 90% of the observations in this interval are slim-majorities with at least two close state-district races won by the majority. Windows -3,3 and -4,4 seem valid as local randomized experiments (Table 2, rows 2 to 4) as all covariates are balanced. The -5,5 window, however, is rejected as a local randomized experiment. The reason we do not have a row entry for the -1,1 window is because the number of observations on either side of the cut-off, 4 and 7, is too low for inference.

The same exercise is repeated for the percentage of seats in the House as the running variables (rows 5 to 9 in Table 2). Only the -1%,1% window cannot be rejected as a local randomized experiment.

Slim-majorities in the state Senate cannot be interpreted as local randomized experiments. This can be seen in the remaining rows of Table 2. For every window, there is at least one covariate for which the balance test is rejected with a p-value below 0.15.[10]

Continuity-based RDD

The number of seats is a discrete running variable with mass points at the integers as can be seen in Figure 1a and 1c. If the running variable is defined as the percentage of seats, however, it is not obvious *a priori* that such a variable would have mass points.

In Figure 1b we plot the number of observations at each value of the variable *Democratic majority* in the Senate measured as the percentage of seats. There is a clear mass point in the proximity of -1% (five observations) and of 1% (four observations); an empty interval in the proximity of 0 and a mass point at 0 (16 observations). Hence, the assumption of continuity of the running variable does not hold and the "continuity-based" RDD setup should not be implemented for the state Senates.

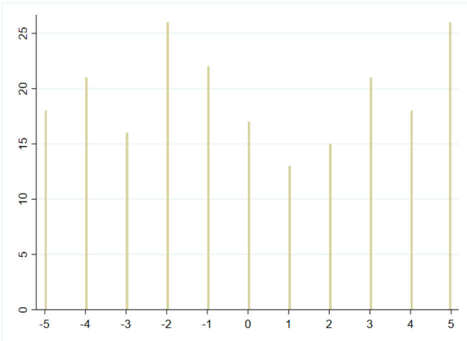
In Figure 1d, it is less clear whether we should reject the assumption of continuity for the variable, *Democratic Majority*, measured as the percentage of seats in the House - as there are fewer mass points. In particular, owning to some of the Houses having high numbers of representatives, the variable reaches the value -0.2% from the left and 0.2% from the right.

Suppose we were to assume continuity for the running variables in the Senate and House, number of seats, and percentages. Usual practice would dictate we decide on the validity of the regression discontinuity design by analyzing Table 3. In this table, we test the balance of covariates via a local linear regression with a triangular kernel, the optimal

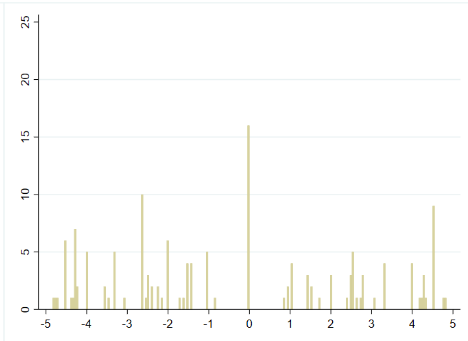
[10]In the on-line Appendix we apply the Kolmogorov-Smirnov(KS) test for equality of distributions instead of the t-test used in Table 2. Qualitatively the results are similar. The main difference is that the KS test suggests that the -1,1 window for the Senate is also valid for local randomization.

Figure 1. Number observations at each value of Democratic majority

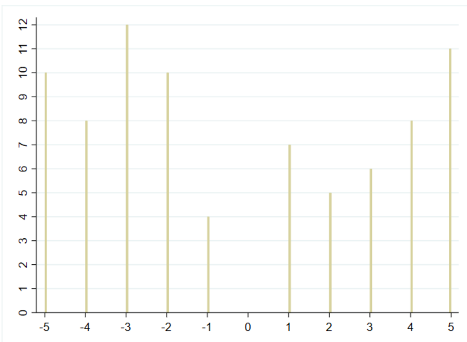
(a) Senate - number of seats



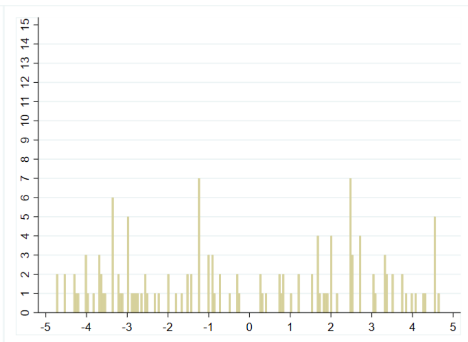
(b) Senate - percentage of seats



(c) House - number of seats



(d) House - percentage of seats



Note: *Democratic majority* is the running variable presented on the horizontal axis. Positive values imply a Democratic majority and negative values a Republican majority. In each figure, the caption indicates an alternative definition of the running variable: Senate, House, number of seats, and percentage of seats. The number of observations for each value of the running variable is shown on the horizontal axis.

bandwidth suggested in Imbens and Kalyanaraman 2012, and p-values as suggested in Calonico, Cattaneo, and Titiunik 2014. The lack of any statistically significant jump at the cut-off would suggest we maintain the assumption of continuity of the potential outcome functions at the cut-off. These results, in addition to the lack of a jump in the density as measured by the McCrary 2008 test in the last two rows, would suggest we maintain the assumption of continuity of all variants of the running variable.

TABLE 3 *Balance Test for Covariates and* Democratic Majority - *CCT Robust*

Variable	Test Diff=0 p-value			
	House		*Senate*	
	Numbers	Percentage	Numbers	Percentage
Dem Majority Senate	0.60	0.85	-	-
Dem Majority Senate $t-1$	0.68	0.62	0.35	0.12
Dem Majority House	-	-	0.62	0.94
Dem Majority House $t-1$	0.51	0.76	0.61	0.60
Dem Governor	0.64	0.92	0.38	0.16
Dem Governor $t-1$	0.44	0.67	0.92	0.85
Midterm Election	0.33	0.42	0.90	0.87
Income per capita	0.97	0.36	0.37	0.73
Local Property Taxes	0.95	0.36	0.20	0.09
State Tax Level (%GDP)	0.48	0.49	0.80	0.86
Unemployment Rate	0.70	0.70	0.57	0.61
Year	0.69	0.19	0.56	0.91
McCrary density test estimate	-0.19	-0.22	-0.21	-0.20
standard errors	(0.13)	(0.23)	(0.16)	(0.21)

Note: The first column reports the predetermined covariates. Columns 2 to 5 present the p-value for the balance test for different running variables. Local linear estimates and bandwidth tests are implemented as suggested by Imbens and Kalyanaraman 2012 and p-values as suggested in Calonico, Cattaneo, and Titiunik 2014. Density test as suggested in McCrary 2008.

We would then, go on naively to implement the canonical "continuity-based" RDD for running variables that we have shown to be discrete and with clear mass points. Therefore, any result obtained would be hard to interpret or wrong. The potential exception is the results for the *Democratic Majority* in the House measured as a percentage of seats - as it is not clear from Figure 1d that we should reject the assumption of continuity regarding this running variable.

Electoral outcomes in the state Houses under local randomization

We present results for the smallest window deemed valid as a local randomized experiment for both the number of seats and percentage of seats, respectively -2,2 and -1%,1%. Sample sizes are low, but we show in the on-line Appendix that the results are robust to larger windows with larger sample sizes. We also estimate the canonical continuity-based RDD with the running variable *Democratic Majority* in the House measured as the percentage of seats.

First, we check whether winning a slim-majority today increases the chance that the party wins a majority in the next election (Table 4, rows 1, 2, and 3). The outcome variable *Dem House in t+1* takes a value of 1 if Democrats gain control of the state House in the next election. None of the estimated jumps is statistically different from zero. The point estimates are themselves near zero. This result is in line with what Feigenbaum, Fouirnaies, and Hall 2017 have estimated using a multidimensional RDD.

We depart from Feigenbaum, Fouirnaies, and Hall 2017 as the null effect we estimate is persistent over time (see the on-line Appendix). One potential reason for our results differing may be that their method makes use of more information, i.e., the race result of each individual district. Another potential reason may be that their downstream result is dependent on the particular metric chosen to construct their aggregating running variable.

Another individual electoral outcome that has been studied through RDD and close elections is coat-tail effects. Broockman 2009 finds that congressional incumbency has no impact on presidential electoral results. The parallel slim-majority effect would be for a slim democratic majority in the House to affect the odds of the Democrats becoming the

TABLE 4 *Randomization-based estimation of slim-majority effects in state Houses*

Running variable	Outcome variable			
Democratic majority	*Democratic House in t + 1*			
	Jump at cut-off	Test Diff=0	Sample size	
Window	Point estimate	p-value	Dem.	Rep.
-2,2 (number of seats)	-0.11	0.58	12	13
-1,1 (% of seats)	0.14	0.53	10	11
CCT-robust (% of seats)	-0.06	0.53	132	145
Democratic majority	*Democratic Senate in t + 1*			
	Jump at cut-off	Test Diff=0	Sample size	
Window	Point estimate	p-value	Dem.	Rep.
-2,2 (number of seats)	0.04	0.85	12	13
-1,1 (% of seats)	0.02	0.92	10	11
CCT-robust (% of seats)	0.02	0.99	199	157
Democratic majority	*Democratic Governor in t + 1*			
	Jump at cut-off	Test Diff=0	Sample size	
Window	Point estimate	p-value	Dem.	Rep.
-2,2 (number of seats)	-0.06	0.76	12	13
-1,1 (% of seats)	0.22	0.30	10	11
CCT-robust (% of seats)	0.08	0.55	129	120

Note: Local linear estimates and bandwidth tests are implemented as suggested by Imbens and Kalyanaraman 2012 and p-values as suggested in Calonico, Cattaneo, and Titiunik 2014. Local randomization windows chosen according to Cattaneo, Frandsen, and Titiunik 2015 and p-values calculated using large-sample inference.

majority in the Senate or winning the governorship in the following election. We also find a null effect in both cases (Table 4, rows 4 to 9).

Further, in the on-line Appendix we explore the effect of partisan control on the tax level and unemployment and find null effects as well.

Concluding remarks

Our result indicating that slim-majorities in the state Senates are not valid for RDD may have been expected. The average Senate has 39 seats and one-third of the seats are contested in each election. The probability of at least one of them being a close race is small compared to the average House that has 100 seats contested in each election. We contribute by establishing that indeed there are not enough close races in Senate elections to warrant an RDD. Even if we were to ignore the lack of close races (they could have been unpredictable even if not close), we show that slim-majorities in the state Senates do not satisfy the conditions to be interpreted as local-randomized experiments. These results are important because under the erroneous assumption that the running variable is continuous, the usual balance tests would fail to reject the validity of Senate slim-majorities as RDDs.

Slim-majorities in the state Houses, on the other hand, seem to be valid under the local randomization assumption. If the running variable is the number of seats, the clear presence of mass points implies that the we should not use RDD methods that are based on the continuity of the running variable. However, if the running variable is the percentage of seats, it is not clear that we should reject the assumption of a continuous running variable. In practice, we show that results using the “continuity-based” canonical RD setup with percentage of seats as the running variable yields similar results to the local-randomization methods. Therefore, following Cattaneo, Titiunik, and Vazquez-Bare 2017 we suggest that results under both assumptions be shown in order to check for robustness. Further, if available or partially available, information on the underlying vote share should be used as a falsification analysis in the form of the test depicted in Table 1. The application of these methods contributes to the literature by allowing a series of questions on slim-majority effects to be addressed with an easily-implementable design-based method.

A remaining concern is that such a design may be unable to capture partisan effects even if there was one. This could be based on power concerns, for example. In the on-line Appendix, we show that the RDD method described in this paper does capture a statistically significant effect: a unified government - defined as the state House and Governor belonging to the same party - has a higher tax level than a divided government. This result is a re-estimation of the effect described in De Magalhães and Ferrero 2015 using the methods discussed herein.

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