Political Parties and the Tax Level in the American states: A Regression Discontinuity Design

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Abstract

With a regression discontinuity design I show that the partisan identity of the majority in the state House of Representatives has no causal effect on the tax level. This result goes against recent findings in the political economy literature. In the state Senate I find a significant discontinuity in the tax level, but I also find a discontinuity in the density of the forcing variable - which implies that we can not interpret the discontinuity in the Senate as a causal relation. Another contribution of the paper is to investigate under which conditions slim majorities in the American states (as opposed to close election) are appropriate for a regression discontinuity design. (*JEL* D72, H1, H2)

If parties play a role in policy making, one would expect their influence over policy to be related to the number of seats they hold in the state House and Senate. In particular, voting rules in the Legislatures make it such that a party's influence should change discontinuously once it has the required majority to pass or block bills. In most Legislatures, a party with 50%+1 of the seats in either chamber has the power to both propose, modify, and block the budget, and also to propose and block changes to the tax level. This discontinuous change in party influence allows us to use a regression discontinuity design to try to identify whether there is a causal link between the majority in the Legislature and the state tax level.

The general idea of a regression discontinuity design in a political setting is that *close elections* can be regarded as random (see Lee (2008)). We propose that *slim majorities* in the state lower House can be regarded as random.[1] Since we are focusing on slim majorities instead of vote count, our design must pass an important test. For a slim majority of one seat to be considered as random, at least one seat out of all the seats won by the majority must have been won in a close election. If this is the case, the party identity of the majority itself can be considered as random as a close election. On the other hand, if every seat was won by a landslide majority, even an election result that delivers a majority of 50%+1 could not be considered as random. We have electoral return data at the state-district level and show in Section 2.2 that slim majorities of one or two seats do satisfy the condition of having at least one or two close district-level elections. Section 2.2 is a contribution on its own because it allows future research to use slim majorities in the states' Houses to identify other causal relations of interest.

Under the identifying assumption that slim majorities can be considered as random, we can therefore check whether there is a discontinuous increase in the tax level at the cutoff *Democratic control* = 50%. We define *Democratic control* as the percentage of seats held by the Democratic party in the state lower House. Above

[1] Every state's Legislature (except for that of Nebraska) has two legislative chambers: a state Senate and a state House. The Senate also plays an important role in writing and approving the budget, but, as we discuss below in Section 2.2 and 2.3, only the state House lends itself to a regression discontinuity design.

the cutoff, the Democrats have the majority in the state House.[2] If we observe a jump in the tax level at the 50% cutoff, we can assign the higher tax level to the Democrats holding the majority, and therefore interpret the jump as a causal relationship. The identifying assumption implies that all confounding factors, observable and unobservable, should on average be the same on both sides of the 50% cutoff, so that the difference in the outcome variable can only be attributed to the treatment effect. An interesting feature of the design is that we can test for discontinuities in observable covariates as a way to check whether our "randomization" has worked well. The limitation of such a design is that we are only able to identify a causal relation locally, at the 50% cutoff. The result is not generalizable to all the support.

My main result is that I find no discontinuity in the state tax level at the cutoff *Democratic control* = 50%. I describe this result in Section 3. The tests I perform indicate that the result is robust and valid as a quasi-experiment. We may therefore interpret this result as evidence against a causal relationship between the partisan identity of the majority in the state House and the tax level.

Even though I find no discontinuity in the tax level at the point at which the Democrats gain a majority in the state House, the estimates indicate a positive relationship between the percentage of seats the Democrats hold in the state House and the tax level. Moreover, in Section 3.4, I show that there is a positive jump in the tax level when the Democrats gain the majority in the state Senate. But I show in Section 2.2 and 2.3 that the percentage of Democratic seats in the Senate is not a valid forcing variable in a regression discontinuity design. Therefore, this positive discontinuity estimated in the Senate and the positive relationship between the number of Democratic seats and the tax level in the House can not be interpreted as a causal relationship. They can only be interpreted as a positive correlation between the number of Democrats in the Legislature and the tax level.

The result in this paper contrasts with recent results in the political economy

[2]Some state years have independent representatives seating in the Legislature. Our data does not allow us to identify them. This implies that if *Democratic control* is less than 50%, either the Republicans have the majority (which is the most common case) or neither party has a clear majority. All our results are robust to the alternative forcing variable: *Republican control*. These results are available on request.

literature such as Reed (2006), who finds that Democratic control over the Legislature, measured as the fraction of the five-year period in which Democrats controlled both state chambers, has a positive impact on the tax level. Others who have found a significant partisan effects on the size of government in the U.S. states are Alt and Lowry (2000), Caplan (2001), Besley and Case (2003), and Warren (2009). There has also been evidence that parties have an effect on government finances in other settings. Pettersson-Lidbom (2008) also uses regression discontinuity design and finds that left-leaning Swedish local governments do spend more. Krehbiel (1993) finds some evidence for party effects in the US House, and Blais et al. (1993) find some evidence of party effects across countries.

The results in this paper support a literature that has found no partisan effects over the public finances of the American states. Some examples are Dye (1966), Winters (1976), Garand (1988), and Poterba (1994).[3] Our paper is closely related to Ferreira and Gyourko (2009) who also use a regression discontinuity design and find no evidence that the partisan identity of the Mayor has an effect on government size. This is so even though their OLS results suggested a positive correlation.

In Section 1 I present the data. In Section 2 I discuss the design and our estimation methods. In Section 3 I present the main result. I discuss the results in Section 4.

1 Data

My full sample comprises 50 American states from 1960 to 2006. Most of the political, fiscal, and population variables are the same as those used by Besley and Case (2003). I have updated their sample from 1960 to 1998 with data from 1999 to 2006. The source of the new data was the Census Bureau, Legislature websites, the website for the National Association of State Budget Offices (NASBO), and the website for the National Conference of State Legislatures (NCSL). To keep the sample comparable,

[3]The literature has also found little evidence that the Governor's partisan identity has an effect on the tax level.Besley and Case (2003), Reed (2006), and Leigh (2008) find no evidence that the party identity of the Governor affects the tax level.

I focus the analysis on the states with the most common budgetary institutions: a two-chamber Legislature, the requirement of a simple majority to pass the budget, and a governor with line-item veto power. Nebraska is excluded from the sample for being the only unicameral state and for having a non-partisan Legislature. I exclude Arkansas, California, and Rhode Island, because they all require a two-third majority in order to pass the budget, which implies a different cutoff point at $66.\overline{6}\%$ of the seats in each chamber in the Legislature, and there is not enough data to reliably reproduce our estimation procedure at this cutoff. The states with the block veto power are a minority and are also excluded. Their inclusion would not change the results qualitatively.[4] There is not enough data to include Alaska, Hawaii, or Minnesota.[5] My working sample has 38 states from 1960 to 2006: 1712 observations.

In my sample, the average tax level in the American states is around 5% of GDP. The tax level is defined as the sum of state income, sales, and corporate taxes divided by state GDP. I also have data on total state expenditure, which averages at 10% of GDP. The expenditure is also funded by locally-determined property taxes and by Federal transfers, both of which are not under the control of the state Legislature. I therefore choose to focus the analysis on the revenue side of the state budget. The result in Section 3.1 is robust to using state expenditure as the dependent variable.[6]

I also show results with an alternative measure for the tax level: state taxes per capita. However, taxes per capita seem to be more time dependent than tax

[4] In the Appendix Section A.1, Table 8, we can see that the main result is robust to the inclusion of the states with block veto. I also show in Figure 4 in the Appendix, Section A.1, that in the set of states with the block veto the density of the forcing variable is discontinuous at *Democratic control*=50%. Democratic controlled state Houses are less frequent than Republican controlled. Including these states may affect the validity of the design. I therefore keep the states with the block veto out of the main analysis.

[5] Until 1972 Minnesota had a non-partisan Legislature. Moreover, it has an unique political system which is not defined on bipartisan lines. Its Governors were either officially independent from 1982 to 2002 or are classified as such by Brandl (2000). For a detailed account of contemporary Minnesota political history, see Brandl (2000).

[6] We will abstain from discussing the role that the Federal and local governments have in state fiscal policy. In particular, we are assuming away how tax rates are set in federal units that take central government tax policy into account. For a discussion, see Klor (2005). We are also assuming away how the partisan alignment between states and the federal government may affect federal transfer. For an empirical discussion on Spanish data, see Solé-Olléa and Sorribas-Navarro (2008).

revenues over GDP. This can be seen in Table 1. The average taxes per capita across states in 1982-dollars during the 1960s is \$330. This jumps to \$574 in the 1970s and continues to increase thereafter. Taxes over GDP are much more stable across the same period. We choose taxes over GDP as our preferred dependent variable because it is potentially less vulnerable to outliers from the 1960s and to comparisons of estimates from observations of different decades. Even when using taxes over GDP, however, the 1960s is an outlier decade. Because of this, one of the robustness checks is to exclude this period.

Table 1: Different measures of the states' tax level

Measure	1960s	1970s	1980s	1990s	2000s
state taxes per capita (1982-dollars)	330	574	664	813	904
state taxes over state GDP (%)	4.4	5.7	5.7	5.9	5.6

Note: This sample comprises 1712 observations from 1960 to 2006. Each observation represents a state within a year. The tax level is measured as the total sum of a state's income, sales, and corporate taxes. Each entry is the average of all observations within a decade.

Table 2: Political parties and the adoption of income and/or corporate taxes

State and year	Majority in the House	Majority in the Senate	Governor
Connecticut (1970)	Democrat	Democrat	Democrat
Florida (1972)	Democrat	Democrat	Democrat
Illinois (1970)	Republican	Republican	Republican
Indiana (1964)	Republican	Republican	Democrat
Michigan (1968)	Republican	Republican	Republican
New Hampshire (1971)	Republican	Republican	Republican
New Jersey (1962)	Democrat	Republican	Democrat
Maine (1970)	Republican	Republican	Democrat
Ohio (1972)	Republican	Republican	Democrat
Pennsylvania (1971)	Democrat	Democrat	Democrat
Rhode Island (1970)	Democrat	Democrat	Democrat

Note: Our sample comprises data on corporate and income tax revenue from 1960 to 2006.

An alternative measure to tax revenues over GDP or per capita would be to look at the tax rates themselves. I do not have data on the changes in the tax rates, so I cannot follow such a strategy. I do have data on tax revenues, however. So if the revenue of a certain tax goes from zero to a positive number from one year to the next, this means a new tax has been adopted. As can be seen in Table 2, out of the eleven states that adopted either income alone or both income and corporate taxes in the period 1960 to 2006, five had a Democratic majority in the state House and six had a Republican majority in the state House.

I also have data on the following variables: state population and income; the average state property tax, which is not decided by the Legislature; the political identity of the Governor; the partisan identity of the majority in the state Senate; whether or not the election was a midterm election; and election turnout. I also have data on whether the state has other institutional features that may affect the tax level: supermajority requirements for a tax increase, and tax and expenditure limitations. I follow standard practice and check these covariates for significant discontinuities around *Democratic control* = 50%.

Finally, I have data on state legislative election returns at the state district level from 1967 to 2003. These were provided by the ICPSR (Inter-University Consortium for Political and Social Research) and collected and organized by Carsey et al. (2008). I was unable to find state-district level data for the remaining years of my working sample. Also, as Carsey et al. (2008) point out, due to various reasons, there is about 18% of missing values for the variable that we are interested in: the margin of victory, defined as the difference between the percentage of the votes that the winner received and the percentage of the vote that the second-place candidate received in each state district. I end up with state-district level data for 714 state-years.[7]

[7] Our working sample has 1712 observations. An observation is a state in a year. Elections, however, only take place every two years, so we only have election results for 856 observations. This number is the basis for a comparison with the number of observations for which we have state-district election returns data: 714.

2 Regression discontinuity design

2.1 Design

Regression discontinuity is a quasi-experimental design. Its defining characteristic is that the probability of receiving treatment changes discontinuously as a function of one or more underlying variables.[8] The treatment, call it T, is known to depend in a deterministic way on an observable variable, d, known as the forcing variable, $T = f(d)$, where d takes on a continuum of values. But there exists a known point, d_0, where the function, $f(d)$, is discontinuous.[9] The main identifying assumption of the design is that the relation between any confounding factor and d must be continuous at the cutoff d_0. If that is the case, the only variable that is different near both sides of the cutoff is the treatment status. As a result, the discontinuity in the outcome variable is identified as being caused only by the variation in treatment status. One main caveat of the design is that it can only claim to identify a causal relation locally, i.e. at the cutoff.

In this paper, the forcing variable is *Democratic control*, and the outcome variable is the state tax level. If the forcing variable is above 50%, the observation receives treatment. The treatment is a Democratic controlled state House. At each period, a state is either assigned the treatment or not. For the observations in which the election for the state House delivered a slim majority, we argue that the assignment of treatment was as good as random. If this is the case, differences in the average tax level between the treated group and the control group are an estimation of the treatment effect.

[8]For a detailed review of the regression discontinuity in economics, see Lee and Lemieux (2009).

[9]More formally, the limits $T^+ \equiv \lim_{d \to d_0^+} \mathbf{E}[T|d]$ and $T^- \equiv \lim_{d \to d_0^-} \mathbf{E}[T|d]$ *exist* and $T^+ \neq T^-$. It is also assumed that the density of d is positive in the neighborhood of d_0. There are two types of discontinuity design: fuzzy and sharp. In sharp design, treatment is known to depend in a deterministic way on some observed variables. In fuzzy design, there are also unmeasured factors that affect selection into treatment. Our case fits the sharp design.

2.2 Slim Majorities and Close Elections

The regression discontinuity design in this paper is based on the idea that slim majorities in state Legislatures can be interpreted as randomly assigned. The party identity of the majority in a Legislature, however, is not chosen in a single state-wide district in which the party with 50% +1 of the votes wins the majority. Instead, each state is divided into state-districts that choose a representative to the state Legislature by a first-past-the-post system.[10] Therefore, an important condition for a slim majority to be considered random is that at least a few state-districts must have had close elections themselves.

The benchmark case is a legislative election in which each party has the same number of secure seats, only one seat is competitive. Whichever party wins that seat, wins the majority in the legislative chamber. If that seat was decided in a close election, then the assignment of which party holds the majority in the legislative chamber is as random as the election for the competitive seat itself. Lee (2008) discusses why close elections can be considered as random and are therefore appropriate for regression discontinuity designs. The rule-of-thumb definition of a close election by Lee (2008) is an election in which the margin of victory was less than 5% of the votes in a particular district.

In Table 3, row 1 we can see the legislative elections that delivered majorities - for either Democrats or Republicans - of 1% of the seats (the average state House has 110 seats). In this interval, I have state-district level data for 33 election years in different states. In each of these legislative elections, I counted the number of seats that were won with a margin of victory of less than 5% of the votes. In all of these 33 legislative elections at least 1% of the seats were decided by close elections. The results in row 1 indicate that slim majorities of one or two seats can be regarded as random insofar as the district level election in the competitive seats can be regarded as random. This implies that the exercise of using slim majorities for a regression discontinuity

[10] Some states have multi-member districts. I include in the data used in Table 3 and 4 the multi-member districts that have different candidates for each post. I exclude from our data the free-for-all multi-member districts, in which all candidates run together and those with the most votes win a seat.

design is a valid one. For majorities of one or two seats, there seems always to be enough close election to make the result of which party gains the majority random itself.

Table 3: Randomness condition for the state House

Democratic control (%)	Percentage of seats that must be close elections	Number of observations	Randomness condition (% of obs. in the interval)
49-51	1	33	100
48-52	2	59	93
47-53	3	86	83
46-54	4	117	73
45-55	5	132	67
44-56	6	161	59
43-57	7	203	53

Note: The data on election results by state district has been provided by Carsey et al. (2008). We have election results by state district for 726 state-years. Election returns at the state-district level are only available from 1967 to 2003, and within this periods there is about 18% of missing values. *Democratic control* is defined as the percentage of seats in the state House of Representatives that belong to the Democrats. We define the randomness condition for a majority in the state House to be that at least the percentage of seats above the 50% cutoff were close elections themselves at the state-district level. We define a close election to be an election won with a margin of victory of less than 5% of the votes. Column 4 indicates the percentage of the observations in that interval that satisfy the condition. Let's use row 2 as an example. In 93% of the 59 observations with a majority of up to 52% of the seats (Democratic or Republican) at least 2% of the district-level elections for state House representative had a margin of victory below 5% of the votes.

The other rows in Table 3 show what happens if we look at majorities of more seats. As an example, let's look at a majority of, say, 53% of the seats. If at least 3% of all seats were the result of close district-level elections, then we can say that the identity of the majority in that election satisfies the randomness condition. Out of the 86 observations in that interval, 83% satisfy the condition. Note also that the condition implies that the winner of the majority was uncertain. The condition does not imply that the probability of gaining the majority is the same for both parties. If, for example, the Democrats are sure to win half of the seats by a landslide, they only need one of the close-election seats to go their way, whereas the Republicans would need all close-election seats to go their way.

Table 4: Randomness condition for the state Senate

Democratic control in the Senate(%)	Percentage of seats that must be close elections	Number of observations	Randomness condition (% of obs. in the interval)
49-51	1	19	89
48-52	2	46	86
47-53	3	73	79
46-54	4	94	71
45-55	5	130	61
44-56	6	149	56
43-57	7	176	51

Note: The data on election results by state district has been provided by Carsey et al. (2008). We have election results by state senate districts for 679 state-years. Election returns at the state-district level are only available from 1967 to 2003, and within this periods there is about 18% of missing values. *Democratic control in the Senate* is defined as the percentage of seats in the state Senate that belong to the Democrats. We define the randomness condition for a majority in the state House to be that at least the percentage of seats above the 50% cutoff were close elections themselves at the state-district level. We define a close election to be an election won with a margin of victory of less than 5% of the votes. Column 4 indicates the percentage of the observations in that interval that satisfy the condition. Let's use row 2 as an example. In 86% of the 46 observations with a majority of up to 52% of the seats (Democratic or Republican) at least 2% of the state Senate elections had a margin of victory below 5% of the votes.

The randomness condition is defined according to an arbitrary threshold that defines as close an election won by a margin of victory below 5% of the votes. Different thresholds would imply different values for column 4 in Table 3. The broad picture would remain the same, however. Slim majorities of a few seats can more easily be considered as the result of a random process than majorities of many seats.

For any given "randomness condition", one could estimate the discontinuity with the methods described in Section 2.4 by restricting the sample to observations that satisfy the condition. I have experimented with this alternative. The results are qualitatively the same. I have therefore omitted them here. They are available on request.

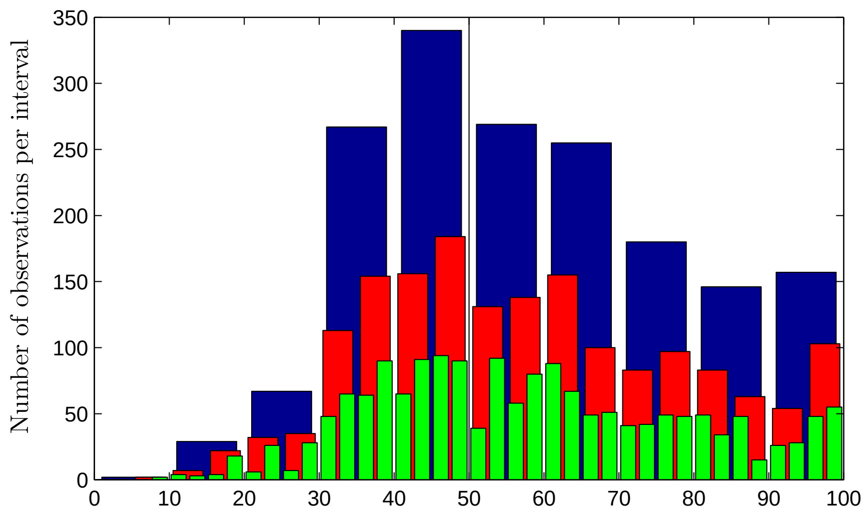
In Table 4 we can see the randomness condition for the state Senates. Even in row 1, we observe elections that do not satisfy the randomness condition. A possible explanation for this difference between the state Senates and Houses is the sheer number of seats. The state Houses have many more seats up for election in any given electoral year than the state Senates. The state Houses have an average of 110 seats and all seats are contested every two years. The state Senates have an average of 40 seats and staggered elections, and only half of the seats are contested at each biennial election. It is easier for all of the seats in a particular election for the state Senate to have predictable results.

2.3 Forcing Variables

As we have seen in the previous section, some slim majorities do not satisfy the randomness condition. This is particularly a problem for the state Senates. If the party identity of the majority is not random at the cutoff, this implies that voters can manipulate the forcing variable even at the cutoff. To test for this, I check the density of our potential forcing variables to see how they behave around the 50% cutoff.

Figure 1 suggests that voters are able to manipulate the composition of the Senate around the cutoff. In Figure 1 we can see that there are more than double the number of observations immediately to the left of the cutoff than to the right (focus on the bins of size 2.5). This seems to suggest that voters are able to prevent slim Democratic majorities from controlling the state Senate. And if the voters are able to manipulate the composition of the state Senate at the 50% cutoff, we cannot interpret slim majorities in the Senate as random.

Figure 1 :Histogram of forcing variable - *Democratic control in the Senate*

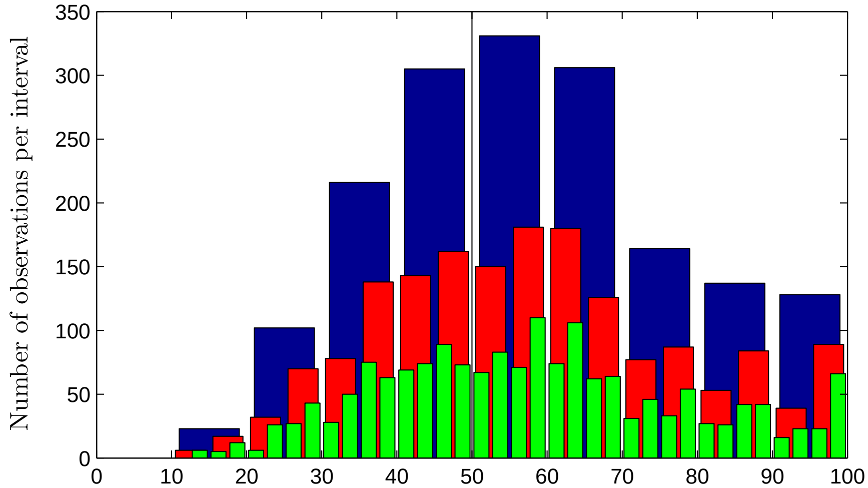


Democratic control - Seats held in the Senate by the Democratic party (%)
(bins of size 2.5, 5, and 10)

In Figure 2 we can see that there is almost no difference between the number of observations in a Democratic controlled House and a Republican controlled House at the 50% cutoff. Figures 2 gives us no reason to believe in the manipulation of the composition of the lower House around the cutoff.

Given what we observe in Figures 1 and 2, the only adequate forcing variable for a regression discontinuity design is the percentage of Democratic seats in the state House. Since, however, the House and the Senate have similar powers over the budget, I must look into whether the political control of the state Senate may be influencing my result. Specifically, I show that the likelihood of the state Senate being controlled by the Democrats is continuous around the cutoff at *Democratic control* = 50%. Therefore, my main result in Sections 3.1 can not be driven by differences in

Figure 2: Histogram of forcing variable - *Democratic control in the House*



Democratic control - Seats held in the House by the Democratic party (%)
(bins of size 2.5, 5, and 10)

the political control of the Senate in either side of the cutoff. The interpretation of the result in Sections 3.1 is the effect of a change in the political control of the state House keeping everything else constant, including the political control of the state Senate and the partisan identity of the Governor.

2.4 Estimation Methods

I implement the regression discontinuity design methods following Lee and Lemieux (2009) and Imbems and Lemieux (2008). In this section, I discuss the estimation methods used, and I present the main result in Section 3.1.

The discontinuity at the cutoff can in practice be estimated in a number of ways. The simplest approach is just to compare the average outcomes in a small neighborhood on either side of the treatment cutoff. The problem with this approach is that it may generate imprecise estimates since the regression discontinuity method is subject to a large degree of sampling variability. To rely solely on this approach would require a very large sample size. I present in Figure 3, Section 3.1, local averages of the dependent variable in intervals of width 0.5. These intervals are constructed so that the interval immediately to the left of the 50% cutoff is $(49.5, 50]$. The interval immediately to the right is $(50, 50.5]$. The local average estimates are a crude estimate of the discontinuity, but they are a good indicator of the variability of the data.

An equivalent but more efficient method is to estimate two functions: one with observations to the left of the cutoff and one with observations to the right. The precision of the estimate depends on how much flexibility we allow the functional form to have. One option is to impose a parametric structure; I use a third-degree polynomial for each side of the cutoff.[11] The advantage of this method is that both estimating the discontinuity and calculating the standard errors are straightforward. One of my main concerns is that some of the results may be sensitive to the polynomial degree and that this method, as opposed to a nonparametric estimate, uses data points too far from the 50% cutoff point. In Figure 3, Section 3.1, the solid line indicates the parametrically estimated functions.

Another equivalent alternative is a nonparametric approach.[12] This method does not impose any constraints on the functional form. I follow the standard nonparametric approach and use local linear regressions with a triangular kernel.[13] The local linear regression method, as argued in Hahn et al. (2001), fairs relatively better at the boundaries than other methods and therefore is the most appropriate to use

[11] I have experimented with other polynomial degrees and found similar results to our main specification when allowing for a quartic polynomial or higher. These results are available on request.

[12] By "equivalent" I mean that conditional on the sample being large enough, all three methods should estimate the same discontinuity.

[13] The method is described in detail in Pagan and Ullah (1999), p.93. It consists in minimizing for m and γ:

$$\sum_{i=1}^{n} \{y_i - m - \gamma(x_i - x)\beta\}^2 K\Big(\frac{x_i - x}{h}\Big),$$

where $K(.)$ is the kernel function and h the bandwidth. Let $s = \frac{x_i - x}{h}$, the triangular Kernel is defined as:

$$K = (1 - |s|),\ for\ s \leq 1\ and\ 0\ otherwise.$$

with regression discontinuity design. A local linear regression estimates a regression function at a particular point by using only data within a bandwidth surrounding this point. The kernel function gives more weight to the data that are closest to the point being estimated.

Nonparametric results are sensitive to bandwidth choice. Imbens and Kalyararaman (2009) propose a method to calculate an optimal bandwidth specifically for regression discontinuity design. According to their method, the optimal bandwidth in my sample is $h = 13$.[14] In Figure 3, Section 3.1, at each estimation point, the predicted value by the local linear regression is denoted by $\times$.

For the parametric estimates of the discontinuities at the cutoff, I present Huber-White standard errors robust to clustering by state. To estimate cluster robust standard errors for the nonparametric estimate, I use the wild cluster bootstrap. This does not require the residuals to be i.i.d.; nor does it require each cluster to have the same size.[15] Cameron et al. (2008) use Monte Carlo simulations to show that the wild cluster bootstrap works well, particularly when the number of clusters is small. As we can see in all results, the theoretical cluster robust standard errors in the parametric estimates are similar to those estimated by the wild bootstrap procedure with a local linear regression.

[14] A bandwidth of 13 implies that the point immediately to the left of the cutoff is estimated with data in the interval (37, 50], and that the point immediately to the right is estimated with data in the interval (50, 63]. Within these two intervals there are 815 observations, making up 48% of the sample. In the Appendix, Section A.2, I have experimented with other bandwidths and the results are robust.

[15] Each new sample of residuals in the wild cluster bootstrap are the original residuals multiplied either by $\frac{(1-\sqrt{5})}{2} \simeq -0.618$ with probability $\frac{(1+\sqrt{5})}{2\sqrt{5}} \simeq 0.7236$, or by $1 - \frac{(1-\sqrt{5})}{2}$ with probability $1 - \frac{(1+\sqrt{5})}{2\sqrt{5}}$. We resample the residuals 1,000 times for each regression. For more on the wild bootstrap, see Horowitz (2001).

3 *Democratic Control* and the Tax Level

3.1 Main Result

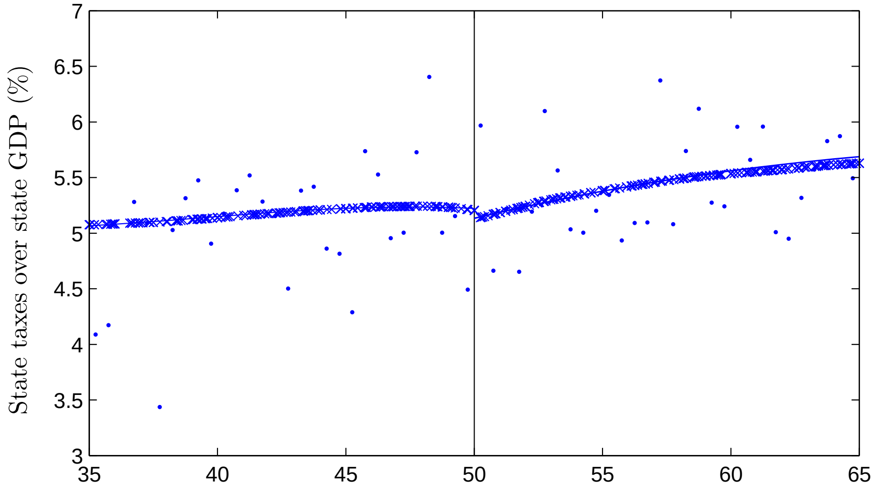
The forcing variable is the percentage of seats controlled by the Democratic party in the state House, which I call *Democratic control*. We can see the estimates graphically in Figure 3 and numerically in Table 5. On the y-axis, we have the state tax level. As the percentage of seats held by the Democrats moves from the left to the right of the 50% cutoff point, the Democrats gain a majority in the state lower House. The estimates shows no significant discontinuity in the tax level even though we have estimated two independent functions, each using data on only one side of the 50% cutoff. This is the main result of this paper. The regression discontinuity design indicates no causal relationship between the partisan identity of the party controlling the state lower House and the tax level. As I mentioned in the Introduction, this result goes against the recent literature that has looked at the question of whether partisan identity has a causal effect on the tax level in the American states (Reed (2006)). On the other hand, this result is similar to the Ferreira and Gyourko (2009) findings regarding American Mayors in U.S. cities.

Table 5: State tax level and *Democratic control*

Method	Jump at 50%	Bootstp mean	SE
Polynomials	-0.08	-	(0.30)
LLR(bandwidth 13)	-0.07	-0.07	(0.25)

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 2006. Each observation represents a state within a year. The dependent variable is the total sum of a state's income, sales, and corporate taxes divided by state GDP and is shown as a percentage. The forcing variable is *Democratic control* - the percentage of seats in the state House of Representatives that belongs to the Democrats. The discontinuity is estimated at *Democratic control* = 50%. Row 1 shows the results for a 3-degree polynomial on each side of the cutoff. Row 2 shows the result for a local linear regression specification with a triangular kernel and a bandwidth of 13. Theoretical cluster robust standard errors are provided for the polynomial regression together with bootstrapped cluster-robust standard errors by state for the nonparametric regression (wild bootstrap with 1,000 draws each).

Figure 3: State tax level and *Democratic control*



Democratic control - Seats held in the House by the Democratic party (%)
· local average, × local linear, − 3-degree polynomial

3.2 Robustness checks

In Section A.2 in the Appendix, we can see that the result is robust to estimating the local linear regression with different bandwidths.

As I mentioned in Section 2 (see Table 1), the 1960s had a considerably lower average tax level than the other decades. As a robustness check I exclude all of the observations from the 1960s. I then continue and exclude one decade at a time. As we can see in the Appendix, Section A3, the result is robust to each exclusion.

The result could also have been driven by a particular state. To accommodate this, I also perform a robustness check excluding one state at a time. The exclusion of no particular state changes the result. We can see this in the Appendix, Section A.4.

I also check to see if the results in Table 5 hold with alternative measures for the tax level. First, I use state tax revenues per capita in 1982-dollars. As in the case with taxes over GDP in Table 5, I find no significant discontinuity. This result can be seen in the Appendix, Section A5. Second, I use expenditures over GDP as an alternative dependent variable, I find not significant discontinuity either. This result can be seen in the Appendix, Section A.6.

3.3 Checking the Validity of the Design

As we can see in Section 2.3, Figure 2, the number of observations on either side of the cutoff is very similar. This suggests that our forcing variable is not being manipulated at the cutoff. Another check for the validity of the design is to see whether any other covariate is discontinuous at the 50% cutoff. If this were the case, it could indicate that the "randomization" did not work, that is, that observations on both sides of the cutoff are not similar and therefore we cannot read our results as the lack of a causal link between *Democratic control* and the tax level. As we can see in Table 6 I find no significant discontinuity in any of the other covariates.[16]

Row 1 in Table 6 shows that observations on both sides of the cutoff are as likely to have the Senate controlled by the Democratic party. This is an important result. Even though the Senate role in setting the budget is as important as that of the House, around the cutoff at least, the discontinuous change in political control comes from the House only. Row 2 shows a similar result for a variable indicating the partisan identity of the Governor.

Finding no discontinuities in variables such as turnout and on the indicator variable for midterm elections reassures us that the forcing variable is not being manipulated around the cutoff by voters. As Table 6 demonstrates, elections on both sides of the cutoff are equally likely to be midterm or simultaneous, and have the same average turnout.

Discontinuities in variables such as population, income per capita, and average local property taxes could indicate that observations on both sides of the cutoff are not comparable. But because we do not find any discontinuity in these variables, as can be seen in Table 6, we are confident that the design has worked well.

[16] In Table 6 we only show the results for the parametric specification. The nonparametric specification give the same result. These are available on request.

Table 6: Covariates and *Democratic Control* - States with the line-item veto

Variable	Jump at 50%	SE
Democratic control Senate	0.02	(0.12)
Democratic Governor	-0.01	(0.15)
Turnout	-0.02	(0.03)
Midterm election	0.09	(0.09)
Population	0.76	(1.25)
Income per capita	1.21	(1.13)
Local property taxes	0.21	(0.25)
Tax and expenditure limitations	0.09	(0.11)
Supermajority requirements	0.02	(0.07)
State tax level lagged twice	0.04	(0.30)

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 2006. Each observation represents a state within a year. *Democratic control Senate* takes value 1 if the state Senate is controlled by the Democratic party, and value 0 otherwise. *Democratic Governor* takes value 1 if the Governor is a Democrat, and value 0 otherwise. *Turnout* is defined as the fraction of the population that turned out to vote in the last election. *Midterm election* takes value 1 if the election for that observation was a midterm election, and 0 if the Governor was also chosen in that election. *Population* is the state population in millions for a given year. *Income per capita* is the state income per capita in thousands of 1982-dollars. *Local property taxes* is the percentage of a state average property tax in a year divided by state GDP. *Tax and expenditure limitations* takes value 1 if the state has a tax limitation rule in that year, and value 0 otherwise. *Supermajority requirements* takes value 1 if the state in that year requires a supermajority to vote for a tax increase. The forcing variable is *Democratic control*, which is the percentage of seats in the state House of Representatives that belongs to the Democratic party. The discontinuity is estimated at *Democratic control* = 50% with a 3-degree polynomial on each side of the cutoff. Theoretical cluster-robust standard errors by state are in parentheses.

In row 8 of Table 6 I look at an institutional feature that has been adopted by some of the states in our sample: tax and expenditure limitations. The majority of these limitations restrict expenditure growth to increases in income per capita or, in some cases, to inflation and population growth. Some of these limitations also restrict the size of appropriations to a percentage of state income; whereas some have statutory bounds on expenditure growth rates.[17] I use an indicator variable that takes value 1 should such a rule be in place within a state during that year, and 0 otherwise. As shown in Table 6 the incidence of observations with such rules is on average similar on both sides of the cutoff. The same is true in row 9 for the incidence of another institutional feature: super majority requirements.[18]

In Row 10, I treat the lagged tax level as another covariate. I lag the tax level twice. This means that for an observation at the current year t, I look at the tax level at year $t-2$. I do so because of the nature of the data. Each election cycle for the state House of representatives is two years. The political variables therefore only change every two years, whereas the tax level changes every year. This means that regressing the current *Democratic control* on the tax level lagged once $(t-1)$ will for half of our observations be the same as regressing the current *Democratic control* on the current tax level. Such an estimation would be partly a repetition of the contemporaneous regression and therefore would not be a good test of the validity of the design. Finding no discontinuity in the lagged tax level is an indication that the design works well, and that we can interpret the lack of a contemporaneous discontinuity in the tax level as the lack of a causal relationship between *Democratic control* and the tax level.

[17] For more details, see Waisanen (2008).

[18] In principle, when such a requirement is adopted, it is no longer enough to hold 50% of seats to formally raise the tax level, which makes dealing with the observations that have supermajority requirements more problematic than dealing with other covariates. One option for dealing with the 240 observations with supermajority requirements is to drop them entirely, which does not change the results. These results are available on request. Another option would be to define the forcing variable as the distance from the cutoff so that the 66.6% cutoff is pooled with the 50% cutoff. However, in the states with supermajority requirements, the budget is still approved by a simple majority. The two cutoff points are not directly comparable. I prefer to keep the observations with supermajority requirements and treat it as another covariate. For an analysis of their adoption and the effect on the tax level, see Knight (2000).

3.4 Senate

The interpretation of the result in Section 3.1 as the lack of a causal relationship between partisan political control and the tax level hinges on the assumption that representatives will vote according to party lines, particularly in budget matters. If representatives do not vote according to party lines our results may simply express that parties are weak.[19]

In Table 7, however, we can see that Democratic control over the state Senate is positively correlated with the tax level. A Democratic controlled Senate implies a 10% increase in the the state tax level. As I have mentioned before this result can not be interpreted as a causal relationship,[20] but it suggests that parties do have an influence over the tax level, even if this influence is determined by an unobservable variable such as preferences.

The positive correlation between Democratic control and the tax level can be seen not only in Table 7, but also in Figure 3, where the estimates indicate an increasing function between Democratic control and the tax level. Both of these results suggest that there is indeed a positive correlation between Democratic political control and the tax level. This relationship, however, can not be interpreted as causal.

[19] Political scientists tend to agree that parties influence the policy making process. They disagree on the mechanisms, strength, and domain of this influence. An example of this can be seen in Wright and Schaffner (2002), who compare the unicameral non-partisan Nebraska legislature with the Kansas Senate. They claim that these two chambers are comparable in almost all aspects, with the exception of Nebraska being officially run as non-partisan. Wright and Schaffner (2002) use roll-call data from 1996 to 1998 to determine the ideological location of each representative in a spatial voting model. They also find that although the main dimension, usually identified with a liberal-conservative line, does well in predicting how the partisan senators in Kansas will vote, it does not help to predict how the non-partisan members of the Nebraska legislature will vote. Wright and Schaffner (2002) conclude that this is an indication of the influence of parties on representatives' behavior. Similarly, Aldrich and Battista (2002) look at roll-call data and spatial analysis in order to measure the polarization of different state legislatures. They find a strong positive relationship between slim majorities and the more polarized Legislatures. Aldrich and Battista (2002) finding is important for the purposes of this paper, as the assumption that representatives vote according to party line has to hold around the 50% cutoff point in particular.

[20] See Section 2.2 and 2.3.

Table 7: State tax level and *Democratic control* in the Senate

Method	Jump at 50%	Bootstp mean	SE
Polynomials	0.76	-	(0.31)**
LLR(bandwidth 5)	0.51	0.55	(0.35)
LLR(bandwidth 8)	0.58	0.56	(0.28)**
LLR(bandwidth 13)	0.52	0.52	(0.22)**
LLR(bandwidth 18)	0.50	0.49	(0.23)**
LLR(bandwidth 20)	0.52	0.46	(0.22)**

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 2006. Each observation represents a state within a year. The dependent variable is the total sum of a state's income, sales, and corporate taxes divided by state GDP and is shown as a percentage. The forcing variable is *Democratic control* - the percentage of seats in the state Senate that belongs to the Democrats. The discontinuity is estimated at *Democratic control* = 50%. Row 1 shows the results for a 3-degree polynomial on each side of the cutoff. Rows 2-6 shows the result for a local linear regression specification with a triangular kernel and varying bandwidth. Theoretical cluster robust standard errors are provided for the polynomial regression together with bootstrapped cluster-robust standard errors by state for the nonparametric regression (wild bootstrap with 1,000 draws each).

4 Concluding remarks

The results in this paper are in line with the results of Lee et al. (2004), who find that voters elect policy instead of affecting policy choices by politicians. If voters want a bigger government they will vote for the people who will implement it. In a close election however, under the identifying assumption of the regression discontinuity design, voters's preferences are on average the same whether the Democrats have the majority or not. And as we have seen, there is no discontinuity in the tax level around the cutoff *Democratic control*=50%. This result supports the preference hypothesis put forward by Krehbiel (1993) over the party hypothesis. When the preferences are held fixed, party identity has no effect on the size of government.

In the state Senate we do observe a jump in the tax level when the Democrats have a majority - this is the case even when the Democrats gain the majority. But since we have seen that the design is not valid for the state Senates, we can not infer that voters' preferences are the same on both sides of the cutoff. Voters seem able to manipulate their choice at the cutoff and therefore the may choose a Democratic majority for the Senate when they want a bigger government. In this case we can not distinguish between the preference and party hypothesis.

The results presented here for the American states are similar to Ferreira and Gyourko (2009)'s for the American cities. Even though both papers observe a positive correlation between democratic influence and the tax level, the relationship is not causal.

If parties do not influence the size of government, what do they do? Glaeser and Ward (2006), for example, argue that partisan differences are mostly based on religion and culture and less oriented along economic issues. On similar lines, de Magalhães and Ferrero (2011) propose a model for the American states in which parties exist but have no preferences over the size of government per se - parties are only interested in maximizing the utility of their supporters. I their model the tax level is not driven by partisan identity but by the degree of alignment between the Governor and the Legislature. They show that such a model is able to explain the empirical relationship between political control of the Legislature and the tax level.

A Robustness Check

A.1 *Democratic Control* and the State Tax Level: All States

Figure 4: Histogram - *Democratic control in the House* - States with the Block Veto

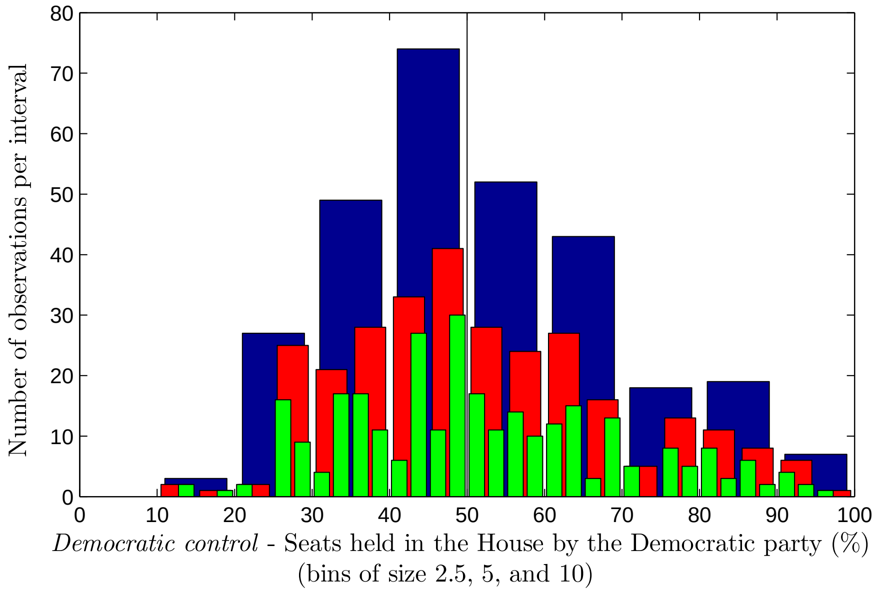


Table 8: State tax level, Covariates, and *Democratic Control*: all states

Variable	Jump at 50%	SE
State tax level	-0.32	(0.27)
State taxes per capita	8.82	(67.93)
Democratic control Senate	-0.01	(0.11)
Democratic Governor	0.03	(0.12)
Turnout	-0.01	(0.02)
Midterm election	0.08	(0.09)
Population	0.84	(1.08)
Income per capita	0.89	(0.91)
Local property taxes	0.19	(0.24)
Tax and expenditure limitations	0.06	(0.09)
Supermajority requirements	0.02	(0.06)
State tax level lagged twice	-0.22	(0.28)

Note: This sample comprises 2004 observations of states with both the line item veto and the block veto from 1960 to 2006. Each observation represents a state within a year. *State tax level* is the total sum of a state's income, sales, and corporate taxes divided by state GDP in percentage terms. *State taxes per capita* is the total sum of a state's income, sales, and corporate taxes per capita in 1982-dollars. *Democratic control Senate* takes value 1 if the state Senate is controlled by the Democratic party, and value 0 otherwise. *Democratic Governor* takes value 1 if the Governor is a Democrat, and value 0 otherwise. *Turnout* is defined as the fraction of the population that turned out to vote in the last election. *Midterm election* takes value 1 if the election for that observation was a midterm election, and value 0 if the Governor was also chosen in that election. *Population* is the state population in millions for a given year. *Income per capita* is the state income per capita in thousands of 1982-dollars. *Local property taxes* is the percentage of a state average property tax in a year divided by state GDP. *Tax and expenditure limitations* takes value 1 if the state has a tax limitation rule on that year, and value 0 otherwise. *Supermajority requirements* takes the value 1 if the state in that year requires a supermajority to vote a tax increase. The forcing variable is *Democratic control*, which is the percentage of seats in the state House of Representatives that belongs to the Democratic party. The discontinuity is estimated at *Democratic control* = 50% with a 3-degree polynomial on each side of the cutoff. Theoretical cluster-robust standard errors by state are in parentheses.

A.2 Alternative bandwidths

Table 9: State tax level and *Democratic control*

Method	Jump at 50%	Bootstp mean	SE
LLR(bandwidth 5)	0.11	0.06	(0.34)
LLR(bandwidth 8)	0.04	-0.06	(0.28)
LLR(bandwidth 13)	-0.07	-0.07	(0.25)
LLR(bandwidth 18)	-0.05	-0.04	(0.22)
LLR(bandwidth 20)	-0.04	-0.02	(0.22)

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 2006. Each observation represents a state within a year. The dependent variable is the total sum of a state's income, sales, and corporate taxes divided by state GDP and is shown as a percentage. The forcing variable is *Democratic control* - the percentage of seats in the state House of Representatives that belongs to the Democrats. The discontinuity is estimated at *Democratic control* = 50%. Rows one and two show the result for a local linear regression specification with a triangular kernel and varying bandwidth. Theoretical cluster robust standard errors are provided for the polynomial regression together with bootstrapped cluster-robust standard errors by state for the nonparametric regression (wild bootstrap with 1,000 draws each).

A.3 Excluding decades

Table 10: Tax level and *Democratic control*: one decade excluded at a time

Excluded decade	Jump at 50%	SE
1960s	-0.10	(0.27)
1970s	-0.12	(0.31)
1980s	-0.30	(0.36)
1990s	0.16	(0.34)
2000s	-0.11	(0.32)

Note: This sample comprises state-years with the line item veto from 1960 to 2006. We exclude one decade at a time. Each regression is run with 1369, 1342, 1342, 1346, and 1449 observations, respectively. The dependent variable is the percentage of the sum of income, sales, and corporate taxes in a state divided by state GDP and shown as a percentage. The forcing variable is *Democratic control*, the percentage of seats in the state House of Representatives that belong to the Democratic party. The discontinuity is estimated at *Democratic control* = 50%. Each row shows the results for a 3-degree polynomial on each side of the cutoff. Theoretical cluster-robust standard errors by state are in parentheses.

A.4 Excluding One State at a Time

Table 11: Tax level and *Democratic control*: one state excluded at a time

Excluded	Jump at 50%	Cluster robust-SE	Excluded	Jump at 50%	SE
AL	-0.08	(0.30)	AZ	-0.13	(0.29)
CO	-0.04	(0.30)	CT	-0.02	(0.29)
DE	-0.08	(0.30)	FL	-0.11	(0.30)
GA	-0.10	(0.30)	IA	-0.04	(0.30)
IL	-0.18	(0.30)	KS	-0.10	(0.30)
KY	-0.05	(0.30)	LA	-0.07	(0.30)
MA	-0.14	(0.29)	MD	-0.09	(0.30)
MI	-0.19	(0.30)	MO	-0.18	(0.29)
MS	-0.03	(0.30)	MT	-0.13	(0.31)
ND	-0.03	(0.29)	NJ	-0.17	(0.29)
NM	-0.01	(0.29)	NY	-0.02	(0.29)
OH	-0.08	(0.30)	OK	-0.10	(0.30)
OR	0.00	(0.29)	PA	-0.05	(0.33)
SC	-0.11	(0.30)	SD	-0.11	(0.30)
TN	-0.08	(0.30)	TX	-0.03	(0.29)
UT	-0.06	(0.30)	VA	-0.07	(0.30)
WA	-0.07	(0.31)	WI	-0.07	(0.31)
WV	-0.05	(0.30)	WY	-0.08	(0.30)

Note: This sample comprises state-years with line item veto from 1960 to 2006. Each regression is run with 1665 observations. The first exception is the regression excluding Connecticut, that has 1669 observations, as Connecticut had fours years with an independent Governor dropped. The regressions excluding Iowa, Washington and West Virginia have 1674 observations each, as these states adopted the line item veto in 1969. The dependent variable is the percentage of the sum of income, sales, and corporate taxes in a state divided by state GDP and shown as a percentage. The forcing variable is *Democratic control*, the percentage of seats in the state House of Representatives that belong to the Democratic party. The discontinuity is estimated at *Democratic control* =5 0%. In each entry, we exclude from the sample the state in columns 1 or 3. Each row shows the results for a 3-degree polynomial on each side of the cutoff. Theoretical cluster-robust standard errors by state are in parentheses.

A.5 Alternative Measure: State Taxes Per Capita

Table 12: Taxes per capita and *Democratic control*

Method	Jump at 50%	Bootstp mean	SE
Polynomials	55.13	-	(82.56)
LLR(bandwidth 7)	37.75	33.90	(64.88)

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 2006. Each observation represents a state within a year. The dependent variable is the total sum of a state's income, sales, and corporate taxes per capita in 1982-dollars. The forcing variable is *Democratic control*, which is the percentage of seats in the state House of Representatives that belong to the Democratic party. The discontinuity is estimated at *Democratic control* = 50%. Row 1 shows the results for a 3-degree polynomial on each side of the cutoff. Theoretical cluster robust standard errors are provided for the polynomial regression together with bootstrapped cluster-robust standard errors by state for the nonparametric regression (wild bootstrap with 10,000 draws each).

A.6 Alternative Measure: State Expenditures over GDP

Table 13: Taxes per capita and *Democratic control*

Method	Jump at 50%	Bootstp mean	SE
Polynomials	-0.01	-	(0.56)
LLR(bandwidth 12)	-0.44	-0.32	(0.57)

Note: This sample comprises 1712 observations of states with the line item veto from 1960 to 1998. Each observation represents a state within a year. The dependent variable is total state expenditure divided by state GDP. The forcing variable is *Democratic control*, which is the percentage of seats in the state House of Representatives that belong to the Democratic party. The discontinuity is estimated at *Democratic control* = 50%. Row 1 shows the results for a 2-degree polynomial on each side of the cutoff. Theoretical cluster robust standard errors are provided for the polynomial regression together with bootstrapped cluster-robust standard errors by state for the nonparametric regression (wild bootstrap with 10,000 draws each).

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