# Different Models or Measures? *

# PRELIMINARY

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**Abstract**

Recent papers have documented strong party effects consistent with policy divergence at the national level in the United States, but have generally failed to find such effects locally. These differences have generally been attributed to Tiebout sorting and government competition at the local level. Using data on both roll call voting scores and fiscal policy outcomes at the state legislative level, I find large party divergence in roll call voting (nearly identical to that documented in Congress) and no measurable effects on fiscal policy outcomes (the outcome used in studies of local government party divergence). These patterns hold even when roll call votes are restricted to those likely to have affected the fiscal policy of interest suggesting it is not simply a product of the different nature of the issues in the policy space at the local and national levels. These results are also robust to restricting the sample to states which historically have larger (or more diverse) than average legislative districts.

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# 1 Introduction

There has been a proliferation of empirical work using regression discontinuity in political settings. In particular it is frequently used to test theoretical predictions about the effect of politician identity and voter preferences on policy choices The results of such studies vary widely. For example, in the United States, Lee, Moretti and Butler (2004) find strong evidence that Democrat and Republican Congressmen are not moderated by their electorates' preferences, while Ferreira and Gyourko (2009) and Leigh (2008) find closely elected Democrat and Republican mayors and governors enact extremely similar policies. This is in contrast to Pettersson-Lidbom (2008), which finds differences in economic policies and outcomes across closely elected left and right wing governments in Swedish municipalities. It is tempting to infer differing roles of party and politician preferences from the varied empirical results or to attribute their differences to sorting of electorates (Tiebout competition), differences in the electoral accountability of executives and legislators, or any of the institutional factors that differ across the empirical settings. Unfortunately, the myriad dimensions on which key institutional features and outcome measures differ across the existing studies make it extremely difficult to compare results across studies and gain more general insight into the underlying theoretical mechanisms and theories. US state legislatures provide the opportunity to assess many of these issues in a setting with a constant institutional backdrop. Using data on US state legislatures and state fiscal policy, I find evidence for both complete divergence in roll call voting by political party and complete convergence in fiscal policy outcomes. These effects persist even when the roll call voting measures are limited to bills related to the relevant fiscal policies and are constant across district size and heterogeneity.

These results are not theoretically contradictory, but do beg the question: if the policies proposed by the parties converge after conditioning on the electorate, why is the roll call voting so divergent? This highlights the importance of the theoretical difference in the outcome measures employed and the difficulties in drawing theoretical inferences across the existing empirical studies. Conceptually, roll call votes need not translate directly into outcomes. Policy outcomes are the aggregate of choices in multiple bills and potentially include relevant off-setting side deals and compromises

based on the legislators' relative bargaining strengths and interests. A roll call vote, in contrast, is simply an indication of whether a particular bill along with unobserved ancillary pressures or inducements cross the legislator's reservation threshold. Unobserved side payments and the fact that the content of bills proposed is negotiated prior to their introduction (and bills modified to increase their likelihood of passing), mean one cannot conclude that two Democrats who vote the same way on a bill necessarily have the same policy preference or even the same effect on policy. In addition, unlike spending, additional roll call votes need not translate into a more extreme policy. A bill passes once the majority of the legislature votes for it. The content of the bill and any side deals, not the size of the majority voting for it determines the policy outcome. Once a majority of votes are cast, any additional vote for or against the bill are irrelevant to the outcome. They are, however, a way for politicians and parties to signal effort and intent to supporters when outcomes may be outside their control or not contemporaneously observable. Taken together the roll call voting and fiscal allocations suggest extreme partisan coaltions (or signaling) and moderate outcomes. Finally, the results demonstrate that differences in the choice of unidimensional indexes used to rank politicians (roll call voting vs. direct measures of policy outcomes) are capable of generating the range of results seen in the literature on their own. The institutional differences as well as differences in the degree of political competition across the bodies of government studied in the existing literature and the nature of the policies considered are theoretically capable of altering the amount of political divergence. However, they are not necessary.

## 2 Existing Literature:

The role of political parties and their effects on policy outcomes are widely debated in the political science and political economy literatures. The literature has two strands, the first seeks to isolate the effects of party preferences (and internal bargaining) on politician behavior while the second approaches the issue from the perspective of political competition. This work situates itself in the later literature. In addition to considering political parties generally, researchers have also considered them within the context of state legislatures. Alt and Lowry (1994) find that Democrat

majorities are correlated with higher targets for state government size (as measured by spending as a share of personal income). More recently, Reed (2006) found that five years of Democrat control of the state legislature is correlated with 3 to 5 percent growth in state and local governments. Rogers and Rogers (2000) also find some evidence that the size of government is increasing in the Democrats' strength in the state house. Both Reed (2006) and Rogers and Rogers (2000) fail to find significant correlations between government size and the governor's political affiliation. Leigh (2008) also fails to find dramatic differences between policies in states with Democrat and Republican governors, although he does find statistically significant correlations in a handful of the 32 measures he considers.

While suggestive, the primary challenge in interpreting these results as causal is the well known endogeneity of election outcomes and party control. Voters are aware of candidates' political affiliations when they vote. Parties go to great lengths to create their brands and differentiate themselves from the opposing party. Therefore, voters may choose the party to support based partly on the social and economic policies favored by that party. It is highly unlikely that Democrat and Republican control are independent of voters' policy preferences. Absent that independence any observed correlation between fiscal policy patterns and politicians' political affiliations could be due to voter or politician preferences. Disentangling the policies that arise from voter preferences from those due to the parties themselves is the over-arching challenge in this literature. Early work in this area used a large set of controls to attempt to address the endogeneity of politician selection (See, for example, Levitt (1996), and Besley and Case (2003)). Recently regression discontinuity estimators have emerged as the favored approach to addressing these endogeneity challenges particularly when considering individual politicians (Lee, Moretti and Butler (2004); Ferreira and Gyourko (2007); Petterson-Lidbom (2003)). Petterson-Lidbom find that marginally elected left wing local governments impose significantly higher taxes and spend significantly more than their right wing counterparts in Sweden. However, in the United States those looking at outcomes associated with local executives' political affiliations fail to find overwhelming differences between the policies implemented by Democrats and Republicans. Ferreira and Gyourko (2009) fail to find any differences in the size of government under Democrat and Republican mayors.

The appeal of regression discontinuity is that it enables the researcher to compare politicians in districts with voters who are likely to have similar preferences. In addition extremely close races usually have some element of chance in the outcome generating random assignment in which of the extremely competitive candidates is ultimately successful. A politician who barely wins election has an electorate who did not have a strong preference between him and his opponent. Comparing politicians with comparable electorates predates the use of regression discontinuity in this setting by decades. Poole and Rosenthal (1984) compared the voting patterns of Senators of different parties who represent the same state In contrast to the predictions of the classic Median Voter Theorem, they found the senators' roll call voting was more similar to their fellow party members than their counterpart from their state. Lee, Moretti and Butler (2004) is move beyond testing for complete convergence to test more subtle theories of policy choice. Lee, Moretti and Bulter (2004) note that the party that wins a close race exogenously receives the incumbency advantage in the next election. That exogenous shift in the potential safety of the seat enables them to identify how policy choices change as the seat becomes safer. They note that in an Alesina (1988) full divergence model, this shock to the safety of the seat would have no effect on policy choices. In contrast, a partial convergence model assumes politicians are at least partially moderated by their voters and would imply that politicians will move towards their true preferred policies as their seat becomes more secure. More formally their test consists of the following decomposition of policy changes over time into voter and politician components. Following Lee, Moretti and Butler's (2004) notation with $t$ indexing the election, $S$ representing the roll call voting score, $D$ an indicator for the Democrats control of the seat, $P_i$ the percent of voters supporting voting for the candidate from party $i$. and $P^*$ the unobservable voter preferences for each party:

$$E[S_{t_{+1}}|D_t = 1] - E[S_{t_{+1}}|D_t = 0] = \pi_0(P^*_{D_{t+1}} - P^*_{R_{t+1}}) + \pi_1(P_{D_{t+1}} - P_{R_{t+1}}) \quad (1)$$

with

$$\begin{aligned} \pi_1 &= E[S_t|D_t = 1] - E[S_t|D_t = 0] \\ P_{D_{t+1}} - P_{R_{t+1}} &= E[D_{t+1}|D_t = 1] - E[D_{t+1}|D_t = 0] \end{aligned} \tag{2}$$

If $\pi_1 = 0$, Democrats and Republicans implement different policies even after controlling for voter preferences (the simple test any divergence). Full divergence would imply that underlying voter preferences have not effect on policy resulting in:

$$\frac{E[S_{t_{+1}}|D_t = 1] - E[S_{t_{+1}}|D_t = 0]}{(P_{D_{t+1}} - P_{R_{t+1}})} = E[S_t|D_t = 1] - E[S_t|D_t = 0] \tag{3}$$

Each of the terms in equation 3 can be directly estimated by a separate regression discontinuity. This decomposition can be applied to both roll call voting and direct measures of policy outcomes.

The remainder of the paper proceeds by first presenting the data, methods and results for the roll call voting analysis. It then presents the data method and results for direct policy outcomes.

# 3 Roll Call Voting

## 3.1 Data and Methods

There is a vast political science literature devoted to analyzing roll call voting and devising methods to derive politician placements within the ideological spectrum based upon them (see, for example, Buchanan and Tullock (1962); Groseclose, Levitt and Snyder (1997); Heckman and Snyder (1997); Poole and Rosenthal (1997)) One of the challenges in using roll call votes is inferring what it means when a politician votes for and against a bill. For example, a Democrat who could deviate from his party and vote against a bill either because he finds it too extreme (he is more moderate than his party) or too conservative (he is more liberal than his party). Furthermore, there is also the question of how to aggregate votes on abortion and national defense and collapse the multi-dimensional policy space into a single index. I will use two measures to capture the differences

in roll call voting by closely elected politicians. Following Lee, Moretti and Butler (2004), I use interest group scores of the legislators based on the roll call votes on the bills most important to the interest group. This measure has the advantage of highlighting bills seen as being high profile and on which voters are likely to be informed as to how the candidate voted. However, the use of roll call voting scores in election materials could also be problematic. The bills included in the score are selected ex-post by interest groups with partisan affiliations. They may be strategically chosen (by the interest group) to emphasize differences between the parties or even particular candidates. Therefore, I repeat the roll call voting analysis using all of the votes cast during a legislative session, not just those selected by an interest group. Specifically, I calculate the percent of the time that legislators deviated from the Democratic party overall and across specific bill categories. Neither measure is perfect, but they encompass two ends of the spectrum of ways one could aggregate roll call votes and produce very similar results.[1]

Unlike Congress, at the state legislative level, there are very few interest groups that consistently publish voting scores for state legislators in every state. One such group is the National Federation of Independent Business' (NFIB). The NFIB is a small business lobby, which judging by its roll call voting scores, tends to be more aligned with Republicans than with Democrats. Each state NFIB publishes a summary of the 6-18 bills it considered most important in the previous legislative session. The summary provides a brief description of the bill's content and the NFIB's preferred vote on the bill. The guide also contains a table listing how each legislator voted on the selected bills and assigns a score based on the percent of the time the legislator voted in agreement with the NFIB's position on the selected bills. Figure 1 identifies the 30 states from which I was able to obtain voting guides from the NFIB for both the 2002 and 2004 elections (i.e. scores for individuals who were elected in 1998 and 2000).[2]

Overall roll call voting data are from the database compiled by Gerald Wright. Wright's data cover the cohort elected in 1998 (i.e., data on votes which took place in 1999 and 2000).[3] Wright's

[1] Another measure common in the political science literature is Nominate scores - essentially a triangulation measure based on one's voting of one's colleagues. However, with only a single year of roll call voting data, the relatively small number of roll call votes in some of the states complicates the implementation.

[2] Unfortunately, some state NFIB offices did not preserve their past NFIB voting guides and others were unwilling to provide past guides.

[3] Data on Colorado and Idaho's roll call votes are not available. Arkansas was excluded as its roll call votes were

data includes all roll call votes in which at least five percent of the legislators dissented. Wright has also assembled bill summaries for the bills included in the roll call voting data. While not providing a great deal of detail on the bills, it is possible to broadly categorize the bills based on key words included in the summaries. For example using the descriptions contained in the bill summaries one can distinguish bills that are primarily concerned with health from those primarily concerned with education. Bill summaries were searched for various key words and the bills classified as: prison, health, education, children, or other.[4] These classifications were chosen both to be consistent with standard state budget expenditure categories and the literature on the areas in which male and female voters' preferences differ. Roll call voting data are available for the 1999-2000 state legislative house sessions.

Both sets of roll call voting measures were then merged with state legislative election returns from Carsey et al's (2006) data.

## 3.2 Incumbency Advantage

Like Congress, state legislatures have a large incumbency advantage. As one can see in Figure 2, a party that just barely wins a state house seat is approximately 50 percentage points more likely (than the losing party) to control the seat after the next election. Figure 2 plots the probability of Democrat control separately for states for which NFIB scores were and were not available. There is no perceptible difference in the incumbency advantage across the two groups.

available for 2001 only. Kentucky was included even though its roll call votes are available for only 2000 becuase as in other states the relevant legislators were elected in 1998.

[4] Bills whose summaries contained the words child, children, or mother were classified as "child related" Those containing the words college, education, school, teacher and university were classified as "education related." Those containing the words: health, hospital, maternity, medicine, Medicaid, medical, prenatal and prescription drug were coded as "health related." Those containing the words and phrases correctional facility, correctional institution, corrections institution, department of correction, jail, juvenile detention and prison were coded as "prison related." Words containing the listed keywords as roots (e.g., healthy, children) were also counted as containing the word and classified accordingly. With the exception of the "other" category, bills were allowed to be classified as more than one category. However, bills were classified in the "other" category only if they contained *none* of the listed keywords in their entirety, or as roots. Excluding bills containing the education key words from the health cateogry and vice versa produces the same pattern of results.

### 3.3 Roll Call Voting after Initial Election ($\pi_1$)

The average NFIB score for politicians elected at each vote margin (1 percentage point bins) are plotted in Figure 3 There is a large discontinuity at the point where the seat flips from Democrat to Republican control with Democrats receiving significantly lower NFIB voting scores. In addition, there is very little slope as one moves away from the discontinuity suggesting very little heterogeneity in voting within the two parties as the safety of the seat varies. Panel A of Table 1 contains the regression analogue of Figure 3 . Closely elected Democrats are approximately 35 percentage points less likely to vote in accordance with the NFIB than Republicans who win close elections.

As noted above, one might be concerned that interest groups "rig" their voting scores (through bill selection) to emphasize the differences between candidates, particularly those likely to be in highly contested races. Figure 4 performs the same exercise for the percent of the time legislators vote with their parties on all bills (not just those the NFIB deemed important). If closely elected politicians were more moderate or attempting to appeal to the half of the electorate that favored a politician from the other party, one might see them breaking party ranks to vote with the other party more often than those in electorates that overwhelming favor their party. Figure 4 looks strikingly similar to the graph of NFIB voting scores despite being estimated on a completely different sample of votes. This suggests that the observed discontinuity in NFIB scores is not purely the result of cherry picking and that the differences in voting patterns are not limited to high profile legislation. Table 2 (column 1) contains the point estimates based on polynomial control functions and simple differences in narrow bandwidths immediately surrounding the discontinuity. The different estimation methods produce nearly identical results, as one would expect with very little slope away from the discontinuity.

Given the heterogeneity in the dispersion of preferences across issues and the fact that some policy areas are more divisive than others one might be interested in how the gap between the parties varies across bill types. Figure 5 and Table 2 (columns 2-5) show the gap in the percent of the time closely elected politicians vote with their parties across bill types. There is surprisingly little difference in the results across policy areas. The consistency of the point estimates across

outcome measures does make one wonder if politicians are constrained by their parties or receiving side payments to offset the disparate . If that is the case, then these are still accurate estimates of the divergence between the parties, but their theoretical interpretation would change. Instead of being the result of individual politicians' optimal location decisions, the divergence and homogeneity within the party would represent the policy choice chosen by the party leadership to maximize the party's strategic objectives rather than politicians who lack credible commitment devices choosing to locate at their bliss points.

### 3.4 Tests for Partial vs. Full Divergence

Just as Poole and Rosenthal (1984) and others have found, the results above resoundingly reject the straw man of full convergence to the median voter by the parties. However, the lack of full convergence does not mean that voters are unable to exert any influence on their representatives' positions. Panel B of Table 1 estimates the remaining component of equation 3, the relationship between NFIB scores and policy two periods after the election ($E[S_{t_{+1}}|D_t = 1] - E[S_{t_{+1}}|D_t = 0]$).[5] Panel C combines the estimates with the incumbency effect to decompose the gap between the parties over time into the gap in the politicians' preferences (what Lee, Moretti and Butler term the "elect" component) and the influence of the voters (the "affect" component). Complete divergence implies that all of the gap between the parties comes from the elect component (equation 3 holds). Any gap between the two sides of equation 3 is attributed to the moderating effect of the voters. However, as we can see in Panel C, there is almost no change in roll call voting as marginally won seats become safer due to the incumbency advantage. The affect component is less than 10 percent of the gap between parties and is below 2, the value found Lee, Moretti and Butler (2004). Thus, on roll call voting, state legislative districts look identical to the US Congress even though they are much closer to cities in their homogeneity and size. In fact, the degree of divergence and the influence of voters on their politicians over time look very similar if one splits the sample by district size (Table 3). If anything, the point estimates suggest that voters have a larger moderating role in

[5] Since only one legislative class of complete roll call voting data is available, it is unfortunately only possible to perform the exercise for the NFIB scores.

small districts and that if anything politicians moderate their voters in large districts. In addition, the parties are initially farther apart in the smaller districts, the opposite of what one would expect if the smaller districts were more sorted and had a narrower range of preferences.

Finally, the outcome measures used in the upcoming analysis and for local governments are primarily budgets. While the NFIB scores are generally based on economic bills and those of interest to businesses, they do include some health insurance and labor regulation bills. One might be concerned that the degree of divergence differs on the more limited subset of bills related to fiscal policy. Since the NFIB provides summaries of all the bills it includes in its scores it is possible to create one's own score based solely on the subset of bills identified by the NFIB that are related to fiscal policy. Table 4 replicates the exercise for these self-created NFIB scores. The raw gap between the parties is similar to that on other bills. However, the voters appear to have an even more negligible effect on their politicians on this subset of bills than they did overall. The estimates of the amount of partial convergence (the "affect" component) are virtually zero. Thus the divergence in voting does not appear to be driven only by social issues such as abortion.

### 3.5 Balance Around the Discontinuity

The identifying assumption in regression discontinuity is that while outcomes change discontinuously at the threshold, individual characteristics do not. Carpenter et al (2010) have called the appropriateness of this assumption into question in US Congressional House Races. Specifically, they find that those who just barely win close Congressional races have better funded campaigns and are more likely to come from the party that controls the state political aparatus. They argue that even at very small margins, candidates who are successful in close races have higher political capital than those who lose. This does not appear to be an issue in state legislative races. As Figures 6 and 6 show, those who win close state legislative house races do not appear to have a financial advantage nor are they more likely to come from the same party as the state's governor.[6] In addition, districts in which Democrats just win and lose elections appear to be identical in census

[6] Figure 6 is constructed using data from Opensecrets.org matched to election return data. The data on fundraising are drawn from public election filings. The total for each candidate was calculated by the author by summing all the contributions the candidate received in the year of the election and the preceeding year.

characteristics (poverty, income, single parentood rates, demographics, etc). This is not surprising. State legislative office is far less powerful than Congress and the campaigns involve far less money (the average candidate raised less than $50,000).

# 4 State Fiscal Outcomes

The preceding analysis considered the degree of policy divergence across parties as measured by roll call voting. In the analysis that follows I test whether complete divergence in roll call voting translates into divergence in the policy outcomes typically studied in the literature. One challenge in moving from roll call votes to outcomes, is that budgets and program rules are the product of the entire legislature and are not within the control of a single identifiable individual. Instead, they represent the aggregated preferences of the legislature as a whole. One can match each politician to the fiscal policies enacted while the individual was in office and estimate equations 1, 2, and 3 with regression discontinuity with the standard errors clustered by state-year to account for each budget being matched to multiple legislators. For example, equation 2 becomes:

$$\widetilde{\pi_1} = E[\Delta Y_{st}|D_{ist} = 1] - E[\Delta Y_{st}|D_{ist} = 0] \tag{4}$$

with $\Delta Y_{st}$ representing the two year change in spending (the average length of state house sessions). Table 5 contains estimates of equation 4 using local linear regressions. State spending data are from the Annual Survey of Governments. The top panel of Table 5 contains the estimates from all years for which both state spending and state legislative election data are available (1978-2002). The bottom panel restricts the analysis to the years for which the NFIB and roll call voting data are available. As one can see, in addition to being insignificant, the point estimates are generally extremely small (around 0.001) suggesting that closely elected democrats have no measurable effect on state spending overall or on areas of the budget associated with social spending.

However, while $\widetilde{\pi_1}$ is the reduced form effect of a Democrat winning a close race against a Republican, one can only infer convergence in implemented policies, not necessarily platforms.

A politician's vote is entirely within each politician's control, but the ultimate budget is not. Therefore, the effect of any one Democrat winning a close race on policy ($\widetilde{\pi_1}$) is the product of the difference between the policy preferences of the Democrat and the Republican who could have won in his district ($\pi_1$) and the individual legislator's weight in the policy process ($\varpi$). If having one more Democrat in the legislature is irrelevant to the bargaining and policy outcome, then one would estimate that $\widetilde{\pi_1}$ =0 even when the gap in preferences and platforms is as large as those suggested by the roll call voting. Therefore, the analysis below looks at legislatures that are closely divided between the two parties, the case where the effect of adding an additional Democrat to the legislature is likely to be largest.

## 4.1 State Legislative Control and Fiscal Outcomes

The estimates above show that even though closely elected politicians vote the same way as those with safe seats, the outcomes of close state house races have no measurable impact on state fiscal policy. However, one might be concerned that fiscal policy is unaffected due to the average weight of these politicians in the policy process rather than convergence. For example, some closely elected politicians are in legislatures where their party or the opposition party has overwhelming control and outcomes are unlikely to be dramatically effected by slight changes in the degree of lopsidededness of the chamber. However, small changes in the number of seats held by either party are of critical importance in closely divided legislatures. In United States legislatures, the political party that has the majority of seats controls the organizational details of the legislative process. This control includes, but is not limited to: committee chairmanships and additional committee seats, larger staffs and the scheduling of votes on legislation. The majority party is literally the agenda setter. Therefore in fairly evenly divided legislatures, small changes in a party's numerical representation can result in large changes in legislative power.[7] The analysis that follows therefore estimates the effect of a democrat winning a close election in legislatures where the result is pivotal

[7] Breaking fillibusters and over-riding vetos often require more than a simple majority. Even after crossing the 50 percent threshold, legislative power is therefore still increasing in the number of seats held by the majority. That the majority's power is not unlimited does not diminsh the fact that there is a large increase in legislative power upon reaching majority status.

to determining the control of the chamber. If the standard interpretation of the roll call voting results is correct (and the majority has the power to set policy), then marginally elected Democrats enact the same policies as those in safe districts and policy should shift from R* (the Republicans' prefered policy) to D* (the Democrats' prefered policy) when a Democrat wins a close race in an evenly divided legislature. This large change in legislative power resulting from small changes in house and senate seats suggests a regression discontinuity approach to isolating the effects of political party control of state legislatures with the fraction of seats held by the Democrats as the running variable.

$$Y = \alpha + \beta_H I\left[DH > 0.5\right] + f(DH) + v$$

where, DH is the Democrats' share of House seats and and $f(*)$ is a higher order polynomial estimated separately for chambers with Democrat majorities and minorities. A similar approach is employed by Petterson-Lidbom (2003) to study the effects of liberal control in Swedish local governments. Like this paper, Petterson-Lidbom use the share of seats won in the local election instead of voteshare as the running variable for the analysis. Control of Sweden's local governments is determined by proportional representation.

The average state has 111 state house members and 40 state senate members. On average there are 5.13 house races and 3.67 senate races that are 48.5 to 51.5 percent or closer in each state-year, or almost 5 percent of house seats and 10 percent of senate seats. Therefore, any state house that is divided 48 to 52 or closer likely had its majority determined by the idiosyncratic outcomes of close races as does any senate divided 45 to 55 or closer.

One potential impediment to the majority implementing its prefered policy is the state Senate. As one can see in Table 6, divided control of the legislative branch (between the Democrats and Republicans) has not been particularly popular in the states. Instead, one party has control of both chambers over 75 percent of the time and that party is twice as likely to be the Democrats as the Republicans. This Democrat dominance is largely driven by the South and its unique political history. Outside of the South, the Democrats had control of state houses and senates 55 and 52 percent of the time, respectively. However, in 1980 all of the southern state legislatures (upper

and lower chambers) were controlled by the Democrats. In 2000, Democrats still controlled three quarters of the southern state senates and 85 percent of the southern lower legislative chambers. Even in the chambers not controlled by the Democrats, they had at least 40 percent of the legislative seats. Still, there is far more variation in political control in the state legislatures than in the United States Congress.[8] The second and third quadrants in Table **??** represent the number of times each party has lost or gained control of a state legislative chamber since 1977. State houses and senates have changed hands over 120 times in the period, with some states changing hands far more frequently than others. For example, Pennsylvania's state house changed hands 6 times during the period while Mississippi had none. Not surprisingly, more closely divided legislatures are more likely to change hands in the short term as fewer seats need to change hands to tip the balance of power. Legislatures that are divided 55-45 or closer change hands in the next election a third of the time while less divided legislatures only change hands in the next election less than 7 percent of the time.

With the exception of Nebraska, state legislatures in the United States contain upper and lower chambers which have at least partially synchronized elections.[9] State budgets are a function of the decisions made by and the negotiations between both chambers of the legislature. State houses and senates have overlapping districts and elections making it likely that the extent of party control of the two bodies are correlated. Therefore even in a regression discontinuity setting, failing to jointly estimate the effect of party control of each chamber could lead to omitted variables bias in the discontinuity estimates. In addition, controlling both legislative chambers may have an effect in and of its own right suggesting the inclusion of interactions even if both joint estimation of the extent of party control in both legislative chambers. The multi-dimensional regression discontinuity

[8] Political control of the United States House Congress flipped back and forth in the immediate post-war period (1946, 1948, 1952, and 1954). However, the House of Representatives has only changed twice since then (1994 and 2006) while the Senate has changed hands six times (1980, 1986, 1994, 2000, 2002, and 2006). With only one observation per year, it is difficult to separate time effects from changes in control. [Information on party control in the United States Senate is derived from information on majority leaders available at: http://www.senate.gov/artandhistory/history/common/briefing/Majority_Minority_Leaders.htm

[9] Nebraska is unique in having a unicameral legislature.

approach developed in Rehavi (2008) is therefore employed. The estimation equation is:

$$Y = \alpha + \beta_H I\left[DH > 0.5\right] + \beta_s I\left[DS > 0.5\right] + \gamma I\left[DH > 0.5, DS > 0.5\right] + f(DH) + f(DS) + h\left(DH * DS\right) + \upsilon$$

Where:

$$\begin{aligned} DH &= \text{Democrats' share of the House} \\ DS &= \text{Democrats' share of the Senate} \end{aligned}$$

and $f(DH)$ and $f(DS)$ are higher order polynomials estimated separately for Democrat majorities and minorities. State and year fixed effects as well as state gross domestic product are also included as controls. Finally, the political control variables are appropriately lagged so that legislatures are matched to the budgets on which they voted.

$\beta_H$ and $\beta_s$ capture a combination of the effect the Democrat needed to obtain control of the legislative chamber exerts on policy outcomes (through log-rolling, legislative bargaining and his own voting) *and* the effect of Democrats gaining control of the legislature with all the administrative advantages that come with that control. If the effect of an additional Democratic seat does not vary with the number of seats already held by the Democrats$\left(\frac{\partial \beta_H}{\partial DH} = 0\right)$, then $\beta$ is the combination of the effect of the marginal Democrat and the gaining of party control. It is therefore an upper-bound for the effect of the marginal Democrat as well as an estimate of the effect of party control. However, if there are heterogenous effects then it is the effect of one additional Democratic seat evaluated near the threshold for party control and one cannot make general inferences about the marginal Democrat from the effects of party control.

Alternately, if the other branches of government present significant policy constraints, one should focus on the instances where the parties gain complete control of the government. That is the effect of a Democrat majority in the House may only be observable when the Senate and Governor's seat are also controlled by the Democrats. At a minimum, the majority party would be the

least constrained in that instance. A party obtains complete control of the Legislature when the chamber on which they have the weakest hold is won. This suggests estimating the effect of full control through a regression discontinuity in the margin of majority in the chamber with the slimest majority. While the effect of having Democrat as governor is not identified, one can still test whether the magnitude of and Democrat legislative control effects vary when the party also holds the governor's office. Specifically, the estimation equation would be:

$$Y = \alpha + \beta_H I\left[DH > 0.5\right] + \beta_s I\left[DS > 0.5\right] + \gamma I\left[DH > 0.5, DS > 0.5\right] + \gamma_2 I\left[DH > 0.5, DS > 0.5\right] * Dem\_Gov + f(\min(DH, DS)) + \upsilon \quad (5)$$

Where:

$$\begin{aligned} DH &= \text{Democrats' share of the House} \\ DS &= \text{Democrats' share of the Senate} \end{aligned}$$

# 5 Empirical Results

## 5.1 Allocation of State Spending

Table 8 displays the results of estimating equation 5 for changes in state budget shares over the budets the legislators voted on. There do not appear to be any significant changes in the allocation of state budgets across the major spending categories. It may, however, be the case that Democrats in different states prioritize different types of social spending leading these estimates to fail to find any systematic changes. Table 9 shows the effects of Democrat control on the share of the

budget devoted to social programs overall.[10]. While there are several more statistically significant specifications in this category, Democrat control does not appear to be substantially increasing the amount of resources devoted to social spending. If anything, the estimates in Table 9 suggest that social spending declines when Democrats take control of state senates. However, this effect appears to be driven by the handful of state legislative chambers that changed hands in the South. Southern state houses and senates only changed hands half a dozen times during the period with most of the changes from Democrat to Republican majorities. Regression discontinuity estimates are identified in the limit and the density around the discontinuity is extremely small when the sample is restricted to the South, making one hesitant to place much weight on the estimates for that subsample (Figure **??**).

## 5.2 State Tax Policy

Tax policy is a particularly politically charged area of the budget. Table 10 tests whether Democrat control of the legislature affects state tax revenues as a share of gross state product. This measure is used in lieu of the statutory rate in order to allow it to capture changes in the tax base (exemptions and credits) that affect effective tax rates. There does not appear to be any noticeable short-run change in the size of tax revenues relative to the economy in states marginally controlled by Democrats. Another place party rhetoric diverges is in beliefs about the optimal progressivity of the tax code with Democrats generally favoring a more progressive tax code. The NBER's TAXSIM program was used to calculate the average taxes individuals at different points in the income distribution would owe in each state in each year. Estimates of the effect of Democrat control of the legislature on the tax burden throughout the income distribution are presented in Table 11. Taxes for the lowest income groups do appear to decrease more than those at higher income levels when Democrats control the legislature (particularly the house). However, it should be emphasized that the effects are quite small.

[10] For the purposes of this exercise "social spending" is defined as spending enumerated under the education, health and hospital, and public welfare portions of the state budget.

# 6 Conclusion

If one takes the roll call voting and state fiscal policy results at face value, they are consistent with signalling models in which the party in power (or its individual members) is constrained in its ability to change observable policy outcomes in the short space in between elections. For example, roll call votes can then serve as a signalling device to indicate the party or politician's effort or desire to implement a more extreme agenda. In such a model, compelling the members representing marginally held districts to vote in line with the party even when their vote will not affect the outcome of the bill could be seen as a form of money burning. The party is potentially increasing the difficulty of holding onto seats in order to demonstrate its dedication to its "brand" in the absence of measurable short-run policy change. In such a model party-line voting is beneficial to both parties as any cross-over votes would dilute the signal value. This pattern of results is also consistent with bargaining models where parties move policy towards the center in order to gain the votes of enough members of their coaltion to pass the bill. Such models could produce the pattern of results seen there, a high degree of party cohesion in voting coupled with relatively moderate policies.

Like those who have studied executives, I generally fail to find any large differences in the fiscal policies and the distribution of tax-burden when Democrats marginally have control of state legislatures. These effects even fail to emerge when Democrats control both chambers of the state legislature and the Governor's mansion. These are of course the short-run effects of marginal party control of state legislatures. That there are no such effects in the short run does not rule out the existence of an effect for long term control of the state government. Estimates of *both* complete divergence in roll call voting and complete convergence in fiscal policy can both be present at the same time in the same body of government. The absence of significant party effects in state outcomes in spite of near complete divergence in roll call voting measures suggests that the institutional differences between legislatures and executives and differences in political competition are not solely responsible for the differences in findings across the existing literature.

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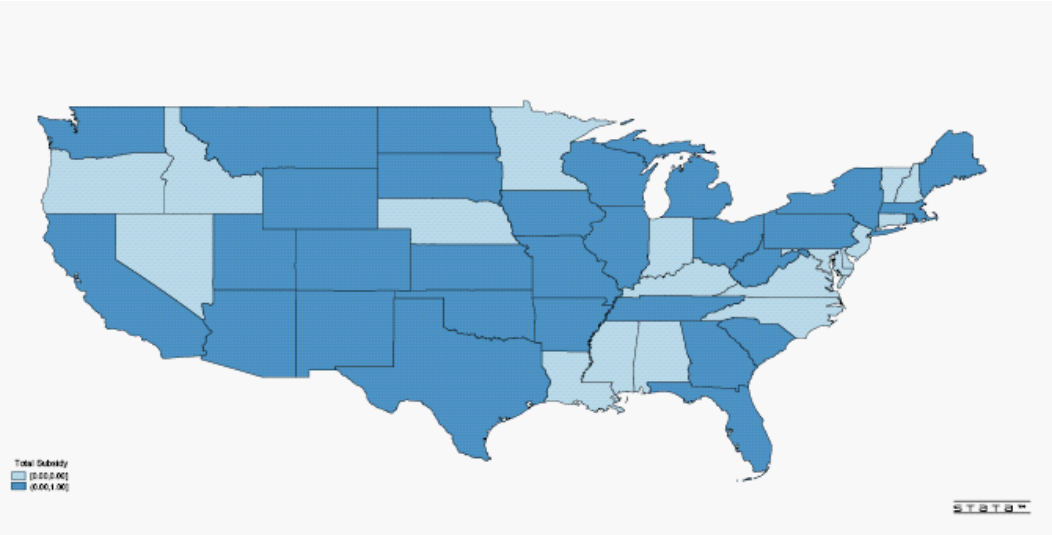
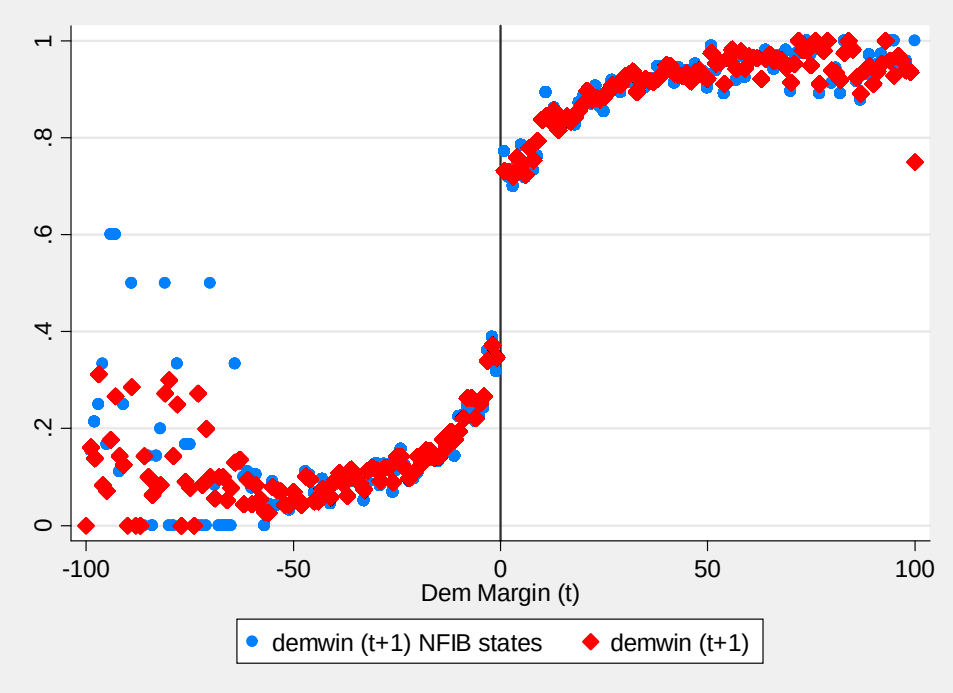
Figure 1: States with NFIB Scores



Note: The dark states are those for which the author was able to obtain NFIB scores for all of the relevant years (the states included in the regression analysis). Nebraska has a unicameral legislature and was therefore excluded. Similarly states such as Louisiana which have elections in odd calendar years were also excluded.

Figure 2: Probability Democrats Continue Holding the District after the Next Election




Note: The graph shows the probability that Democrats control the legislative seats in the next election based on the Democrat margin of victory in the most recent election. Red diamonds are probabilities estimated for the states with NFIB scores available. Blue circles are the estimates based on all states.

Figure 3: NFIB Scores by Most Recent Election

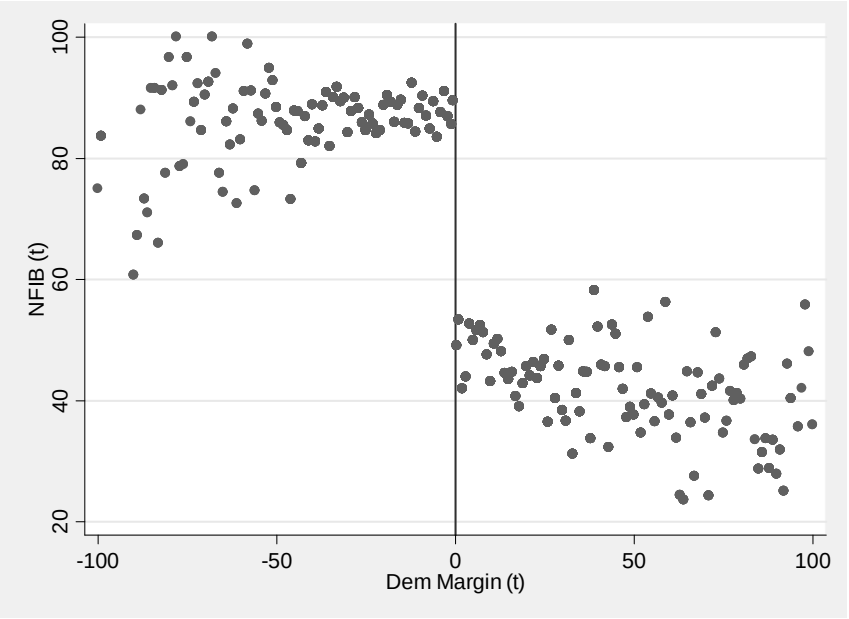



Figure 4: Percent Legislators Vote with their Party

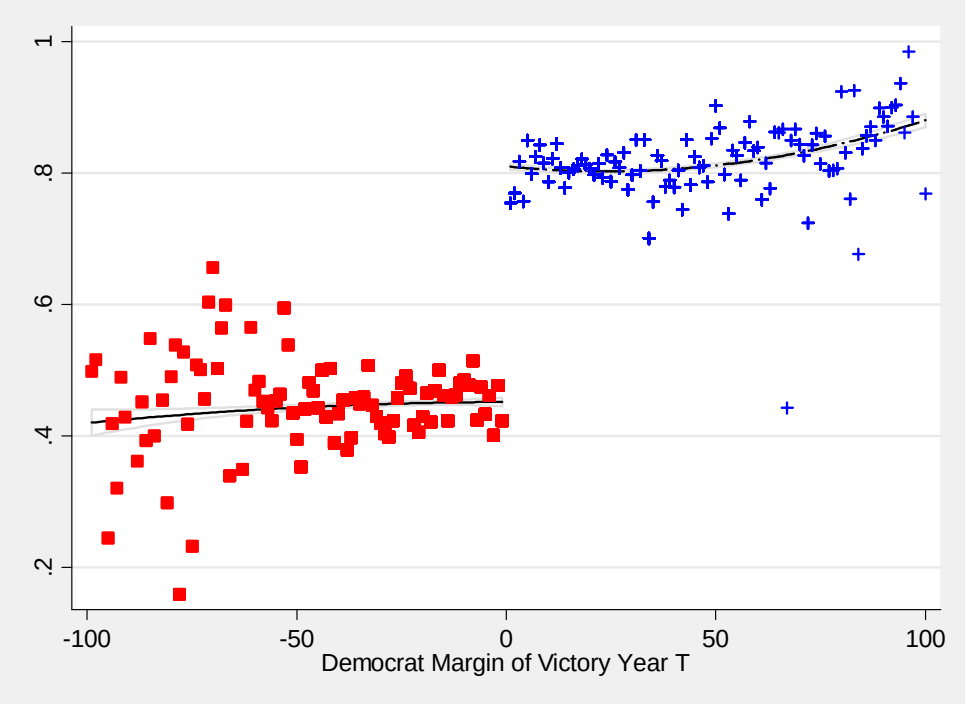


Figure 5: Percent Legislators Vote with their Party by Bill Type

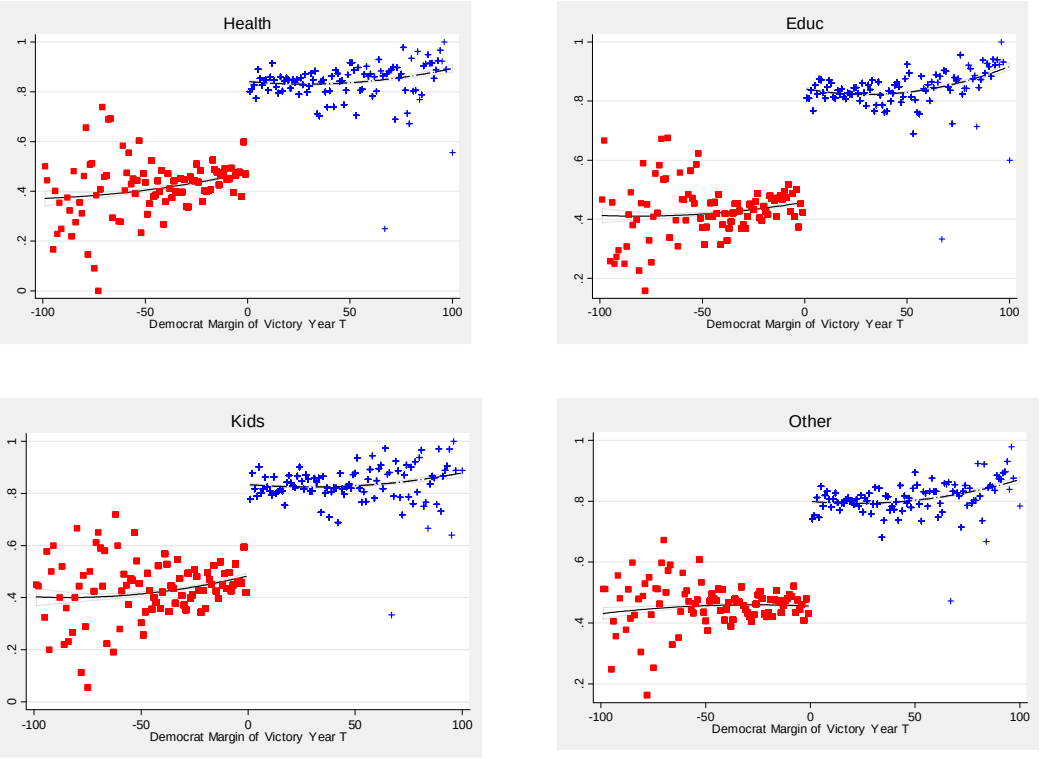
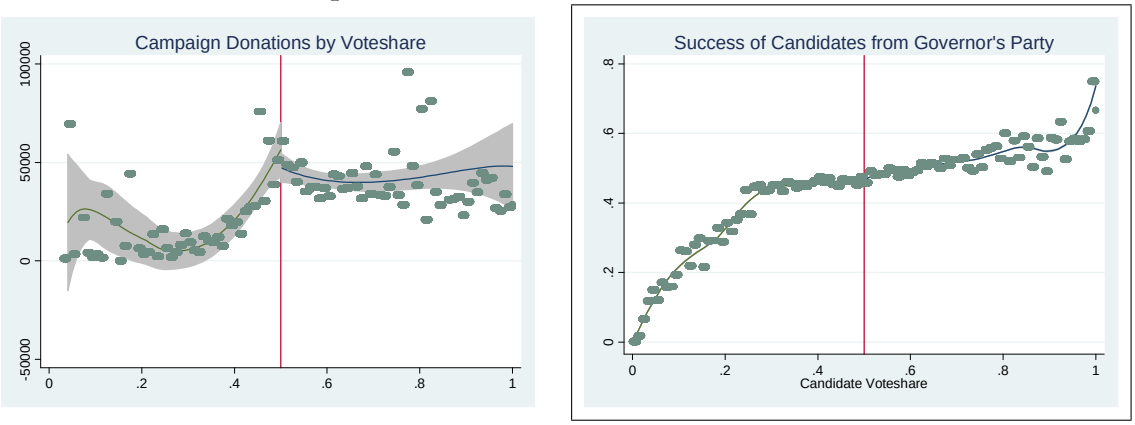



Figure 6: Characteristics of Close Winners



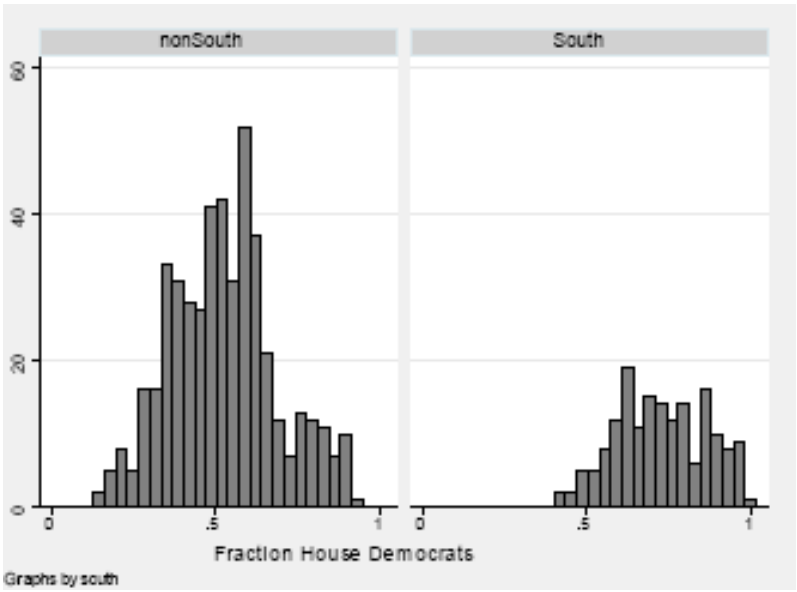



Figure 7: Distribution of Democrat Held Seats in State Houses

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Table 1: Tests of Full and Partial Divergence (NFIB Scores

| | | | | | | |
|---|---|---|---|---|---|---|
| | **Panel A: NFIB (t)** | | | | | |
| Dem Win (t) | -43.63*** | -35.73*** | -32.62*** | -34.00*** | -32.53*** | -33.34*** |
| | [3.26] | [3.59] | [3.86] | [3.96] | [4.31] | [6.55] |
| Polynomial | | 3rd Order | 5th Order | 5th Order | 3rd Order | No |
| Year Fe | Yes | Yes | Yes | Yes | Yes | No |
| State FE | No | No | No | Yes | Yes | No |
| Margin | All | All | All | All | 20 | 1 |
| | **Panel B: NFIB (t+1)** | | | | | |
| Dem Win (t) | -36.02*** | -19.63*** | -17.68*** | -15.60** | -12.05** | -15.92*** |
| | [4.35] | [4.47] | [4.76] | [5.82] | [5.55] | [4.95] |
| Polynomial | No | 3rd Order | 5th Order | 5th Order | 3rd Order | No |
| State FE | No | No | Yes | Yes | Yes | No |
| Margin | All | All | All | All | 40 | 5 |
| | **Panel C: Decomposition** | | | | | |
| Implied Elect | -21.815 | -17.865 | -16.31 | -17 | -16.265 | -16.67 |
| Implied Affect | -14.205 | -1.765 | -1.37 | 1.4 | 4.215 | 0.75 |

Note: Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 2: Divergence in Party Line Voting in the First Term Post-Election

| | All | Health | Education | Kids |
|---|---|---|---|---|
| **Simple Difference** | 0.36 | 0.40 | 0.40 | 0.39 |
| | [0.026] | [0.038] | [0.033] | [0.033] |
| **Linear Control** | 0.33 | 0.35 | 0.36 | 0.35 |
| | [0.027] | [0.041] | [0.03] | [0.039] |
| **Quadratic** | 0.32 | 0.33 | 0.35 | 0.32 |
| | [0.027] | [0.043] | [0.033] | [0.040] |
| **5 pt margin** | 0.28 | 0.24 | 0.28 | 0.31 |
| | [0.067] | [0.087] | [0.0845] | [0.105] |
| **1 pt margin** | 0.31 | 0.29 | 0.32 | 0.35 |
| | [0.098] | [0.132] | [0.10] | [0.171] |
| **Cubic** | 0.33 | 0.34 | 0.36 | 0.33 |
| | [0.023] | [0.040] | [0.030] | [0.036] |

Note: Each cell is an estimate of the gap in the propensities of cosely elected Democrats and Republicans to vote with their parties. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 3: Estimates of Full and Partial Divergence by District Size

| | NFIB (t) | | NFIB (t+1) | | Dem (t+1) | |
|---|---|---|---|---|---|---|
| | -32.81*** | -29.09*** | -22.01** | -13.18** | 0.44*** | 0.53*** |
| **Dem Win (t)** | [4.39] | [3.81] | [10.08] | [6.01] | [0.12] | [0.11] |
| **Polynomial** | 5th order | 5th order | 5th order | 5th order | 3rd Order | 3rd Order |
| **Year Fe** | Yes | Yes | Yes | Yes | Yes | Yes |
| **State FE** | Yes | Yes | Yes | Yes | Yes | Yes |
| **District Size** | Small | Large | Small | Large | Small | Large |
| **Implied Voter Effect** | | | 7.6 | -2.2 | | |

Note: Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 4: Estimates of Full and Partial Divergence by for Budget Related NFIB Bills

| | NFIB Budget (t) | | NFIB Budget (t+1) | |
|---|---|---|---|---|
| | -34.11*** | -35.00*** | -17.71** | -17.03* |
| Dem Win (t) | [8.61] | [12.11] | [4.98] | [9.56] |
| Polynomial | 1st | 1st | 1st | 1st |
| Year Fe | Yes | Yes | Yes | Yes |
| Margin | All | 1 ppt | All | 1 ppt |
| Implied Voter Effect | | | 0.21 | -0.47 |

Note: All regressions are robust to the inclusion of state fixed effects. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 5: Effect of Marginal Democrats on State Spending

| | Two Year Change in Log Spending | | | | | | |
|---|---|---|---|---|---|---|---|
| | Total | Health and Hospital | College | Education | Elementry Ed | Prisons | Public Welfare |
| All Years | | | | | | | |
| Dem Win (t) | -0.001 | -0.001 | -0.002 | 0.001 | 0.018 | 0.004 | -0.009 |
| | [0.002] | [0.004] | [0.004] | [0.003] | [0.028] | [0.007] | [0.006] |
| Dem Win (t-2) | 0.001 | -0.006 | 0.005 | 0.001 | -0.020 | 0.004 | -0.005 |
| | [0.002] | [0.004] | [0.003] | [0.005] | [0.028] | [0.007] | [0.006] |
| | | | | | | | |
| 1998-2002 | | | | | | | |
| Dem Win (t) | -0.006 | -0.027* | -0.002 | 0.000 | -0.003 | 0.004 | -0.029 |
| | [0.004] | [0.016] | [0.008] | [0.005] | [0.007] | [0.010] | [0.019] |
| Dem Win (t-2) | -0.004 | -0.026 | 0.010 | -0.005 | -0.014 | 0.000 | -0.035 |
| | [0.005] | [0.017] | [0.007] | [0.008] | [0.023] | [0.011] | [0.021] |

Note: All regressions are robust to the inclusion of state fixed effects. Bootstrapped standard errors clustered by state-year are in brackets. All were estimated with local linear regression. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 6: Distribution of Party Control of State Legislative Chambers

| | Democrats Control Senate | |
|---|---|---|
| Democrats Control House | No | Yes |
| No | 314 | 93 |
| Yes | 139 | 624 |

Table 7: Persitence and Turnover in Party Control

| | Democrats Control House | | | Democrats Control Senate | |
|---|---|---|---|---|---|
| Democrats Control House (t-2) | No | Yes | Democrats Control Senate (t-2) | No | Yes |
| No | 171 | 39 | No | 172 | 35 |
| Yes | 26 | 352 | Yes | 22 | 306 |

Table 8: Spending Allocation Over the Legislative Session

| | Two Year Change in Budget Share | | | | | | |
|---|---|---|---|---|---|---|---|
| | Health and Hospital | Education | Elementary | College | Prisons | Police | Public Welfare |
| Full Legislative Control (t-2) | -0.001 | -0.005 | -0.01 | 0.001 | 0.001 | 0 | 0 |
| | [0.002] | [0.004] | [0.006] | [0.001] | [0.001] | [0.000] | [0.004] |
| Dem Gov & Full Leg Control (t-2) | 0.001 | 0.002 | 0 | 0 | 0 | -0.000* | -0.001 |
| | [0.001] | [0.003] | [0.003] | [0.001] | [0.001] | [0.000] | [0.002] |
| Dem Majority Senate (t-2) | 0 | 0 | 0.002 | 0 | 0 | 0 | 0 |
| | [0.001] | [0.002] | [0.002] | [0.001] | [0.001] | [0.000] | [0.002] |
| Dem Majority House (t-2) | 0 | -0.001 | 0.002 | 0 | -0.001* | 0 | 0.001 |
| | [0.001] | [0.003] | [0.003] | [0.001] | [0.001] | [0.000] | [0.003] |
| Democrat Governor (t-2) | 0 | 0 | -0.002 | 0.001 | -0.001 | 0.000** | -0.001 |
| | [0.001] | [0.003] | [0.002] | [0.001] | [0.001] | [0.000] | [0.002] |
| Constant | 0.028 | -0.07 | -0.081* | -0.001 | 0.011 | -0.002 | 0.006 |
| | [0.022] | [0.047] | [0.045] | [0.008] | [0.012] | [0.004] | [0.037] |
| Observations | 1070 | 1070 | 1070 | 1070 | 1070 | 1070 | 1070 |
| R-squared | 0.07 | 0.14 | 0.8 | 0.1 | 0.09 | 0.09 | 0.37 |

Each regression contains a 3rd degree Polynomial in the Democrts' weakest seat margin. Standard errors clustered by state are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Note: All regressions contain log real gross state product as well as state and year fixed effects. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 9: Effects of Democratic Control on Social Spending

| | *Log Real Social Spending* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| House Democrat Majority | 0 | -0.032 | 0.012 | -0.06 | -0.007 | 0.027 | -0.015 | 0.026 | 0.002 | 0.023 | 0.008 | 0.015 |
| | [0.028] | [0.064] | [0.028] | [0.055] | [0.024] | [0.044] | [0.048] | [0.045] | [0.032] | [0.035] | [0.033] | [0.083] |
| Senate Democrat Majority | -0.048 | -0.078 | -0.047 | -0.128** | -0.082* | -0.031 | -0.034 | -0.099** | -0.043 | -0.047 | -0.045 | -0.382*** |
| | [0.034] | [0.049] | [0.044] | [0.048] | [0.042] | [0.054] | [0.064] | [0.041] | [0.054] | [0.052] | [0.048] | [0.089] |
| Interaction | | | 0.023 | 0.108* | 0.051** | -0.005 | -0.002 | 0.060* | 0.033 | 0.011 | 0.024 | 0.255** |
| | | | [0.032] | [0.058] | [0.024] | [0.039] | [0.048] | [0.031] | [0.034] | [0.035] | [0.036] | [0.106] |
| Democrat Governor | 0.017* | 0.044*** | 0.018** | | 0.026*** | 0.008 | | | | 0.016* | 0.020* | 0.019** |
| | [0.009] | [0.014] | [0.009] | | [0.009] | [0.009] | | | | [0.008] | [0.011] | [0.008] |
| Constant | -1.376 | -1.879 | -1.561 | -1.93 | -2.513** | -1.073 | -0.6 | -2.932** | -2.000* | -1.833* | -1.489 | -2.829* |
| | [0.942] | [1.281] | [0.966] | [1.271] | [0.994] | [0.904] | [1.161] | [1.110] | [1.013] | [1.025] | [1.207] | [1.364] |
| Observations | 1215 | 339 | 1215 | 339 | 790 | 965 | 627 | 635 | 630 | 585 | 890 | 325 |
| Polynomials | | | | | | | | | | | | |
| Share Democrats House | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Share Democrats Senate | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Interactions | No | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | All | 60-40 or closer | All | 60-40 or closer | More Professional | Less Professional | 1980s | the 1990s | Odd | Even | Non-South | South |

Note: All regressions contain log real gross state product as well as state and year fixed effects. Social spending is the sum of spending on education, public welfare, and heath and hospitals. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 10: Effects of Democratic Control on Tax Revenues

| | Tax Revenue as a Percent of GSP | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| House Dem Majority | 0.002 | | 0.002 | 0.005** | | | 0.002 | 0.003* | -0.003 |
| | [0.001] | | [0.001] | [0.002] | | | [0.001] | [0.001] | [0.003] |
| Senate Dem Majority | | -0.001 | | | -0.002 | -0.003 | -0.002 | -0.002 | -0.009 |
| | | [0.002] | | | [0.002] | [0.002] | [0.002] | [0.002] | [0.008] |
| Interaction | | | 0.001 | 0.003** | 0.003** | 0.002 | 0.002 | 0.001 | 0.005* |
| | | | [0.001] | [0.001] | [0.001] | [0.001] | [0.001] | [0.002] | [0.003] |
| Constant | 0.270** | 0.283** | 0.229** | -0.989 | 0.281** | 0.628 | 0.272** | 0.246* | 0.87 |
| | [0.118] | [0.113] | [0.106] | [0.612] | [0.112] | [0.439] | [0.110] | [0.124] | [2.626] |
| Sample | All | All | All | 60-40 | All | 60-40 | All | Non-South | South |
| Polynomials: | | | | | | | | | |
| Dem House | Yes | No | Yes | Yes | No | No | Yes | Yes | Yes |
| Dem Senate | No | Yes | No | No | Yes | Yes | Yes | Yes | Yes |
| Observations | 1127 | 1119 | 1119 | 435 | 1119 | 434 | 1119 | 820 | 299 |
| R-squared | 0.82 | 0.83 | 0.82 | 0.83 | 0.83 | 0.82 | 0.83 | 0.81 | 0.94 |

Note: All regressions contain state and year fixed effects. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Table 11: Effects of Democratic Control on Personal Taxes (as a percent of income)

| | $30,000 of Real Income | | | $60,000 of Real Income | | | $100,000 Real Income | | $120,000 Real Income | |
|---|---|---|---|---|---|---|---|---|---|---|
| Dem house | -0.010* | -0.011* | -0.016** | -0.008* | -0.008* | -0.012** | -0.007 | -0.008* | -0.006 | -0.008* |
| | [0.006] | [0.006] | [0.007] | [0.004] | [0.004] | [0.006] | [0.004] | [0.004] | [0.004] | [0.004] |
| Dem senate | 0.008 | 0.007 | 0.013** | 0.005 | 0.005 | 0 | 0.007 | 0.007 | 0.008 | 0.008 |
| | [0.005] | [0.005] | [0.005] | [0.005] | [0.005] | [0.007] | [0.005] | [0.006] | [0.006] | [0.006] |
| Dem H & S | -0.003 | -0.004 | | | -0.004 | -0.006 | -0.003 | -0.003 | -0.003 | -0.003 |
| | [0.004] | [0.004] | | | [0.005] | [0.004] | [0.005] | [0.005] | [0.005] | [0.005] |
| Dem Gov | | 0.001 | -0.001 | 0 | 0 | 0 | | 0 | | -0.001 |
| | | [0.002] | [0.002] | [0.002] | [0.002] | [0.002] | | [0.002] | | [0.002] |
| Constant | 0.028*** | -0.013 | -0.003 | -0.016 | -0.019 | 0.055*** | 0.024*** | -0.021 | 0.023*** | -0.022 |
| | [0.005] | [0.010] | [0.009] | [0.012] | [0.014] | [0.010] | [0.005] | [0.015] | [0.005] | [0.015] |
| Observations | 1217 | 1170 | 930 | 1170 | 1170 | 762 | 1217 | 1170 | 1217 | 1170 |
| R-squared | 0.73 | 0.74 | 0.75 | 0.79 | 0.8 | 0.81 | 0.81 | 0.81 | 0.81 | 0.81 |
| | All | All | South | All | All | South | All | All | All | All |

Note: All regressions contain state and year fixed effects. Standard errors clustered by state are in brackets. One, two, and three stars denote significance at the 10%, 5%, and 1% levels respectively.

Figure 8: Density of Partisan Control