

THE EFFECT OF PARTISANSHIP AND POLITICAL INSTITUTIONS ON DEBT ISSUES

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Abstract: We investigate the effect of gubernatorial partisanship and political institutions on municipal bond issues in the United States using a regression discontinuity design. Our unique dataset of individual bond issues allows us to precisely measure the issuing behavior of different entity types – states, state authorities, and localities – and examine how the response of debt issues to gubernatorial partisanship varies across jurisdictions. We argue that debt issuance decisions are shrouded from electoral oversight and, as a result, are ripe for partisanship to exert an effect. We find that, unlike other policy areas where the effects of gubernatorial partisanship are minimal, state government bond issues are responsive to gubernatorial partisanship. The election of Democratic governors results in higher levels of debt issuance, particularly in states that lack the institution of public debt referenda. We find that debt referenda requirements are not circumvented by issuing debt through state authorities or local governments. Consistent with our perspective that debt issuances are shrouded from electoral oversight, we show that debt issuances do not vary with proximity to the next gubernatorial election.

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Introduction

Measurable policy impacts of government officials' partisanship at the state and local levels have been surprisingly elusive (Leigh 2008, Ferreira and Gyourko 2009, Gerber and Hopkins 2011). Scholars have posited that the median voter theorem and electoral accountability explain the relatively minor differences between the policies that different parties enact when in office. In policy areas that are shrouded from electoral oversight (Glaeser and Ponzetto 2014) or fall into an electoral blind spot (Bawn et al. 2012, Anzia and Moe 2015), politicians have more scope to pursue their partisan goals.¹ One policy area that is substantively important and particularly hidden from public awareness is the issuance of new debt. Debt issues receive relatively little scrutiny from the media and public and, unlike a tax increase, the costs of repaying the issues are borne by future generations of voters and citizens. This lack of systematic electoral oversight raises the possibility that partisanship, working through the channel of personal ideology or the desire to reward favored interest groups, manifests itself in debt policy changes that do not occur in more electorally salient policy domains.

We investigate the policy effects of gubernatorial partisanship on new issuances of debt using a regression discontinuity design. Our empirical approach allows us to examine how debt issuing behavior varies as Democratic governors just win election relative to just losing election. In some of the regression discontinuity specifications, we also control for unobserved time-invariant state level factors using state fixed ef-

¹Recent scholarship has begun to call into question the absence of a relationship between gubernatorial partisanship and policy outcomes. For example, Caughey, Warshaw and Xu (Forthcoming) find that Democratic governors lead to more liberal policy outcomes.

fects as a robustness check. We first examine how state government's debt issues change with gubernatorial partisanship. An important additional consideration in the issuance of municipal debt is the existence of fiscal constraints. Scholars have argued that state political institutions that constrain the ability to issue debt may have an important role in affecting state fiscal policy. We examine how one particularly important constraint on debt issuance, referenda requirements on the issuance of new general obligation debt (Bohn and Inman 1996), mediates the relationship between Democratic gubernatorial partisanship and debt issuance decisions. We then compare states' responses to changes in gubernatorial partisanship with the responsiveness of state authorities and local municipalities. State authorities are organizations created by state legislatures and governed by independent boards of directors. Governors may be able to circumvent state level debt referenda requirements by encouraging state authorities and localities to issue debt instead (Bunch 1991, Kiewiet and Szakaly 1996). Examining how state authorities and localities' debt issuance behavior changes with gubernatorial partisanship in states with debt referenda requirements allows us to investigate this hypothesis. Finally, to further investigate the role of electoral oversight in state bond issues, we examine how bond issue responsiveness to partisanship varies over the gubernatorial electoral cycle.

Our analysis makes three primary contributions. First, we document a significant change in debt issuance policy outcomes as a consequence of gubernatorial partisanship. In states that lack debt referenda, the estimated treatment effect of Democratic partisanship on debt issues is \$55.46 per capita² and statistically dis-

²This quantity is the estimated treatment effect from the regression discontinuity specification with state fixed effects.

tinguishable from 0 at conventional levels of significance. In states that possess debt referenda, the point estimate of Democratic gubernatorial partisanship is only \$5.30 and statistically insignificant. Second, we find that the effects of gubernatorial partisanship on debt issuance throughout the governmental hierarchy are not consistent with the circumvention or devolution hypotheses. When Democratic governors are constrained by debt referenda requirements, neither state authorities nor localities issue additional debt to make up for the foregone debt issuance due to the constraint.³ These findings illustrate that state authorities and localities are not successfully used to advance the political objectives of governors. Instead, the observed variation in the debt issuing behavior of state authorities and localities is more consistent with the idea that these entities are responding to their own professional and political objectives. Third, we show that debt issuance for states, as well as for state authorities and localities, is stable over the course of the election cycle. This finding is consistent with the perspective that debt issuances are low salience and shrouded from voter oversight. We show that our results are not due to higher levels of debt refinancing by Democratic than Republican governors. The findings are robust to alternative specifications of the vote share polynomial in the regression discontinuity specification, exclusion of Southern states where Democratic gubernatorial candidates may be more conservative than Republican candidates, and reclassifying the timing of debt issuances.

Our analysis speaks to a large literature on the effect of partisanship and polit-

³State authorities are entities established through legislative mandate for the purpose of furthering public interest. They typically have some degree of autonomy from the state and have the ability to issue municipal debt without voter approval.

ical institutions on economic policy outcomes in the U.S. states. Besley and Case (2003) survey much of this literature and use state fixed effects regressions to show that the effects of Democratic governors on state total tax revenue and spending are quite small. Leigh (2008) uses a regression discontinuity design to assess the effect of gubernatorial partisanship on a wider variety of policy outcomes and also finds little evidence of systematic differences in policy outcomes across Democratic and Republican governors. In contrast to these findings, we show that there are economically significant differences in debt issuing behavior of Democrats and Republicans. An important strand of this literature examines how elected and appointed government officials affect policy outcomes (Besley and Coate 2003, Iaryczower, Lewis and Shum 2013, de Benedictis-Kessner and Warshaw 2016). For instance, de Benedictis-Kessner and Warshaw (2016) document a significant impact of mayoral partisanship on the amount of municipal expenditures. They further show that this additional expenditure appears to be financed by increased levels of taxation and debt. These findings give us reason to expect that partisanship *might* play a role in state debt issuance decisions, though there are also significant institutional differences between state and local governments that prevent one from simply extending the results of prior research to the state setting. One such difference relates to the existence of state authorities; many states relegate the task of debt issuance to state authorities which are likely to be more shielded from partisan effects, but by and large city governments lack an analogous counterpart. The existence of state authorities makes it plausible that the effects of partisanship on state debt issuance could be markedly different from that of local debt issuance. Our results on state authorities are consis-

tent with Whalley's (2013) finding that appointed city treasurers are more efficient financial managers than elected city treasurers. Politically-insulated authority managers might be more concerned with their professional reputation than pursuing the partisan or ideological goals that are more prominent in state debt issues. As a result, state authorities' behavior is less responsiveness to the partisanship of the governor.

Our findings provide new insights into a long-standing debate on the role of state authorities and localities in municipal finance. One perspective on state authorities argues that they enable politicians to circumvent constitutional and statutory restrictions on the issuance of state guaranteed debt. Politicians can then use debt issued by authorities to pursue their political interests, be they ideological or patronage-based. A competing perspective emphasizes the independence of authorities from politics and argues that authorities issue debt in a politically-neutral fashion that is motivated by the merits of a project. The existing evidence on these debates is somewhat contradictory. Bunch (1991) finds in a cross-sectional setting that states with more restrictive debt limitations use public authorities more extensively than states with less constraining debt limitations and Nice (1991) finds that more restrictive debt limitations are correlated with lower levels of general obligation debt, but not total debt. In contrast, Kiewiet and Szakaly (1996) find that debt referenda and constitutional prohibitions are effective in reducing debt issuances and that the presence of these limitations do not result in circumvention through the use of state authorities. Consistent with the view that authorities are relatively independent of politics, we find that in states with Democratic governors and debt referenda requirements, there is no evidence that state authorities increase their debt issuance. This suggests that

governors are unable to use state authorities to circumvent the constraints imposed by debt referenda requirements.

We also investigate how local governments change their debt-issuing behavior in response to gubernatorial partisanship. The results of Kiewiet and Szakaly (1996) suggest that when there are binding debt constraints at the state level, localities will issue more debt to substitute for the loss of state support. In contrast, we find that localities in states with Democratic governors and debt referenda requirements do not issue additional debt to compensate for the foregone state-level debt financing.

This research contributes to a burgeoning political science literature on the political causes and consequences of state and local financial obligations. Both Alt and Lowry (1994) and Poterba (1994) find that divided state governments do not respond effectively to negative fiscal shocks relative to unified government. Using a simultaneous equations model of fiscal adjustment, Alt and Lowry (1994) also show that Democratic governors prefer higher levels of public spending than Republicans. In contrast, our identification strategy allows us to account for unobserved heterogeneity in states' fiscal preferences through random variation in gubernatorial vote share and within-state variation over time to examine the debt consequences of Democratic and Republican governors. Kiewiet and McCubbins (2014) document political responses to the Great Recession and the ensuing fiscal challenges in the states. Anzia and Moe (2017) study how state lawmakers partisanship affects public pension financing and show that, after the Great Recession, pension contributions became a partisan issue with Democratic state lawmakers advocating for more generous public financing of pensions and Republican opposing these efforts. Our findings are similar in

that we document a higher incidence of future financial obligations under Democratic governors and lawmakers.

Background on Municipal Bonds

The term “municipal debt” refers to borrowing by states and local governments and government entities to finance operations and capital projects. Along with federal transfers and tax revenue, it is one of the main sources of financing for state and local governments. As of December 2016 there is nearly \$4 trillion of municipal debt outstanding in the United States. Municipal bonds are politically and financially distinct from bonds issued by the U.S. federal government. In particular, this means that the federal government does not insure municipal bonds in the event of default. Most municipal bonds are exempt from federal income taxation and bonds that are issued by an individual’s state of residence are also typically exempt from the state income tax.⁴ Due to this favorable tax treatment, municipal bond yields are lower than pre-tax yields of similar credit quality corporate debt. As a result of the favorable tax treatment relative to other income-producing investments, municipal debt is a particularly attractive investment opportunity for taxable entities and also represents a large federal tax expenditure.

Municipal entities primarily issue either general obligation bonds or revenue bonds. Revenue bonds are issued for the purpose of funding a specific project or entity. These are usually long-term infrastructure projects like toll roads or transportation hubs. Additionally, revenue bonds are exclusively repaid using the revenues

⁴Similarly, bonds issued by a city of one’s residence are generally exempt from city income tax.

from that specific project and do not constitute a financial obligation on the part of the state. In other words, the revenue bond will only be repaid to the extent that the underlying project is successful in generating revenue. If the revenue-bond financed project fails, then the state is not obligated to pay the outstanding principal so revenue bonds limit the liability of the state.

In contrast, general obligation bonds are not tied to a specific project and can be used to finance a variety of different endeavors. Holders of a general obligation bond may look for repayment to all sources of revenue received by the municipal entity, as general obligation bonds are backed by the full faith and credit of the state and its taxing authority. Historically, general obligation bonds were the bulk of the market, but since the 1970s, revenue bonds have become a much larger portion of newly-issued debt. In our main specification, we employ total proceeds from both general obligation and revenue bonds.

A strength of our analysis over previous studies is that we are able to directly and separately measure the issuing behavior of states, state authorities, and localities instead of relying on more distant measures such as the total amount of state general obligation or revenue debt. State authorities may have different debt issuance behavior from states because state authorities are relatively autonomous of state governments. State authorities are created by state legislatures and are governed by independent boards of directors. These directors are generally professionals who are often appointed by the governor with legislative input. Some state authorities are responsible for managing debt instruments for specific projects or functional policy areas while others are designed to help finance a wider array of capital projects. State

authorities operate in a number of public sectors including transportation (New Jersey Turnpike Authority), natural resource provision (Massachusetts Water Resources Authority), healthcare (North Carolina Hospital Authority), and education (South Carolina State Education Assistance Authority).

Both state and state authorities issue general obligation and revenue bonds, but states utilize general obligation debt at higher rates than state authorities while state authorities are more likely to use revenue bonds.⁵

Role of the Governor in Debt Issues

One of the key responsibilities of the governor of every state is to propose an annual or biennial budget (White 2015). This budget includes a detailed outline of fund use as well as proposed sources of funding, including potential debt issuances. Although these requests are subject to legislative approval, the right to propose held by the governor suggests that the governor's initial proposal will influence the final outcome (Baron and Ferejohn 1989, Berry, Burden and Howell 2010). Moreover, governors can also influence the budgetary process through the veto and, in many states, the line-item veto. Kousser and Phillips (2012) argue that governors are relatively more effective at implementing their budgetary preferences compared to their objectives in other policy domains.

In addition to the theoretical prediction that governors influence debt issuance, there is also substantial evidence of such an effect in popular press accounts. For instance, Republican Governor George Pataki publicly opposed a \$5 billion municipal

⁵In the complete SDC sample, 75.93 percent of state issues are general obligation debt and 7.24 percent of state authority issues are general obligation debt.

bond issuance proposed by New York City (Steinhauer August 5, 2003). Similarly, Democratic Governor Jerry Brown expressed dismay over California’s “wall of debt” when he took office in 2011 and made limiting new debt issuances a priority of his first budget proposal (Jensen May 20, 2011). There are also numerous examples of governors proactively seeking additional debt issuance, including Democratic Governor Roy Barnes. In 2000, he proposed issuing \$540 million of new debt to finance rural economic and infrastructure development in Georgia (Whalen January 12, 2000). Likewise, Republican Governor Mitt Romney authorized \$8.2 billion of state borrowing for school improvements (Piazza January 30, 2004)

As these examples make clear, both Democratic and Republican governors use the powers of their office to increase and decrease the level of debt issuance. Our goal is to move beyond such anecdotes to determine if there is a systematic effect of partisanship on municipal debt issuance. Prior literature on municipal debt issues tends to abstract away from the political considerations that inform the decision to offer new debt issues. Of the sparse literature that examines the effects of politics on bond issues, none are able to identify a causal relationship between partisanship and debt issuance. Clingermayer and Wood (1995) investigate political and economic correlations with state debt levels. They find that increased electoral competition is associated with higher levels of debt. Interestingly, Republican legislatures with and without Republican governors are associated with lower debt levels. The specifications do not include state fixed effects or an identification strategy to isolate the effect of partisanship on the state debt stock.

There are not unambiguous *ex ante* reasons to believe that Democratic governors

would have a strong preference for debt issues relative to Republican officeholders. On the one hand, Democratic officeholders generally have a preference for higher infrastructure spending and are closely aligned with labor unions (Anzia and Moe 2015). These factors suggest that Democrats might support higher levels of debt issues than Republicans for the purpose of financing infrastructure and other public projects. The low mass electoral salience of debt issues relative to alternative financing mechanisms may allow Democratic governors to reward politically-important interest groups at low or no electoral cost. On the other hand, Republicans may be particularly averse to tax increases. As a result, Republican officeholders might prefer debt issues over sales, income, and other tax increases because debt issues are shrouded from public oversight relative to tax increases.

Research Design

Our research design employs the regression discontinuity design (Lee and Lemieux 2010) to estimate the effect of gubernatorial partisanship on debt issuing behavior. We exploit random variation in whether the Democratic or Republican gubernatorial candidate was victorious and use vote share polynomials to allow the conditional mean of the debt outcome to vary based on the amount of electoral support for the Democratic gubernatorial candidate. The running variable in our context is the two-party vote share of Democratic general election candidates for governor. Because we do not have expectations on the general effect of third-party governors on new debt issues, we restrict the sample to state-years where either a Democrat or Republican

is serving in office.⁶ Thus, all of our partisanship results should be interpreted as the effect of having a Democratic governor relative to a Republican governor. We calculate the vote share variables on the basis of two-party vote share and exclude third-party candidates from the calculation. As a result of these coding decisions, the Democrat mechanically wins the election when the Democratic vote share variable is greater than 0.5 and the Republican wins when this variable is less than 0.5. The regression discontinuity design exploits variation in the electoral performance of Democratic gubernatorial candidates to identify the effect of a Democratic governor on new debt issues. Our identification strategy assumes that the potential outcomes of new debt issues are a smooth function in the vicinity of the 0.5 vote share threshold where treatment status changes. We allow the conditional mean of the debt issuance function to vary by including linear and quadratic vote share polynomials in the specifications and we further allow the parameters of the quadratic function to vary to the left and the right of the cutoff by interacting the vote share polynomials with an indicator for whether the Democratic candidate won the election. Gelman and Imbens (2014) show that including third and higher-order polynomials of the running variable in a regression discontinuity design can lead to excessive weighting of observations that are far from the cutoff and can lead to artificially small confidence intervals. We focus on results that use a quadratic polynomial, but examine the robustness of this assumption in the Online Appendix.

In some specifications, we further account for unobserved heterogeneity across

⁶As we discuss below, there are very few instances where a third-party gubernatorial candidate wins in our sample. The results are very similar if we include these third-party governors in the estimation and these results are available upon request.

states and time by including state and year fixed effects as a robustness check. Pettersson-Lidbom (2012) employs a similar research design to investigate the effect of legislative size on total government spending. The advantages of this approach are that it strengthens the credibility of the research design and improves the precision of the estimates. We also report specifications without state fixed effects to show that the results are not driven by this modeling assumption.

The magnitude of bond issues may also respond to nationwide conditions such as the interest rate environment, changes in the federal tax code, and the financial viability of municipal bond insurers (Cuny 2016). To account for these national time-varying factors, we include year fixed effects in all specifications. In addition to time-invariant state factors and national time varying factors, states also differ in their underlying population changes and economic conditions. States differ dramatically in their population sizes and growth rates and these demographic factors might affect both the demand for new debt issues and the partisanship of the governor. To control for these differences, we include a battery of control variables that we describe more completely in the next section. Letting i subscript states and t the year, the main estimating equation is then:

$$Issues_{it} = \alpha \text{Dem Win}_{it} + \beta_1 \text{Dem Vote Share}_{it} + \beta_2 \text{Dem Vote Share}_{it}^2 + \beta_3 \text{Dem Vote Share}_{it} \times \text{Dem Win}_{it} + \beta_4 \text{Vote Share}_{it}^2 \times \text{Dem Win}_{it} + \theta_i + \gamma_t + Controls_{it} \boldsymbol{\delta} + \epsilon_{it}$$

The election variables refer to the election outcome in the most recent gubernatorial election.⁷ The controls variables are a row vector of additional state character-

⁷For example, the value of Democrat win in 1999, 2000, 2001, and 2002 in a state with guber-

istics that include demographic and economic variables. We describe these variables more completely in the Data section below. For statistical inference, we employ heteroskedasticity robust standard errors that are clustered at the state level. The clustering allows for the error terms to be arbitrarily correlated within states over time.

We also investigate how additional political institutions, such as the requirement that general obligation debt issues be approved by voters via a referendum, affect the responsiveness of debt issuing behavior to gubernatorial partisanship. We investigate this hypothesis by interacting the treatment effects with an indicator for whether a state has a general obligation debt referendum requirement.

In our base specifications, we employ all of the data in the sample to estimate the treatment effect of a Democratic governor on debt issues. As a result, the estimate of the vote share polynomial depends on observations far from the 50 percent vote cutoff where a Democratic candidate goes from just losing to just winning the election. It is surely the case that state-years where the Democratic candidate won 20 percent of the two-party vote are systematically different from states where the Democratic candidate wins just more than 50 percent of the vote. One way we address this issue in our main specifications is by including state and year fixed effects and a battery of demographic and economic controls. To additionally alleviate this concern, we also employ the Calonico, Cattaneo and Titiunik (2014) automated method for selecting the bandwidth to include in the estimation sample. We use this method to select a bandwidth and to estimate the bias-corrected treatment effect at the cutoff.

natorial elections in 1998 and 2002 would be 1 if the Democratic candidate won the election in 1998.

One concern with any regression discontinuity design is the possibility that the running variable is manipulated. For example, if agents have precise control over the realization of the running variable then there is a possibility that the observations to the left and the right of the cutoff differ in unobservables and treatment status. Evidence of manipulation would call into question the credibility of the regression discontinuity design. We test for manipulation of the running variable by conducting the McCrary (2008) test to examine whether the density of the vote-share running variable changes discontinuously at the cutoff where a Democratic candidate goes from just winning to just losing. We find that the density is smooth around the cutoff and we present the graphical results in the Online Appendix. We also conduct a number of placebo tests to further probe the internal validity of the regression discontinuity design in our setting. We estimate model specifications where the dependent variable is defined as lagged bond proceeds during the previous gubernatorial term.⁸ If the research design is valid, current gubernatorial partisanship should not have a systematic relationship with this lagged dependent variable. We estimate our four basic specifications using the full sample for state, state authorities, and localities. We show in the Online Appendix that none of the 24 coefficient estimates for the Democratic governor and Democratic governor interacted with state debt referenda are statistically different from 0 at the 5 percent significance level. These results provide additional assurance on the validity of the empirical approach.

We also examine how debt issuing behavior responds to partisanship over the

⁸In the case of states with four-year gubernatorial terms, the dependent variable is defined as the fourth-lagged bond proceeds while in states with two year gubernatorial terms it is the twice-lagged dependent variable.

course of the electoral cycle. Healy and Lenz (2014) show that voters exhibit myopia in their evaluation of government performance. This myopia could motivate government officials to strategically time debt issues in accordance with voter preference. That is, if voters have a distaste for debt issues (Peltzman 1992), we would expect to see debt issues frontloaded in a governor’s term when the election is far off in the future. Conversely, if voters have a preference for debt issues, we would expect to see debt issues backloaded to the end of a governor’s term.⁹ Alternatively, if bond issues are not a salient issue for voters, we would expect bond issuance be constant across the electoral cycle. To investigate these possibilities, we interact the treatment effect for Democratic gubernatorial partisanship with an indicator for the number of years until the next election and we include indicators for the number of years until the election.

Data

In the next three subsections we briefly describe the data sources used for the analysis. We first describe the construction of the bond issues data that is used as the main dependent variable in the empirical analysis. We then describe the political variables, consisting of both election outcomes and political institutional variation across states. Finally, we elaborate on the various demographic and economic control variables that we employ in the analysis. We report a table of summary statistics for the variables in the Online Appendix.

⁹Shepsle et al. (2009) find evidence that appropriations, which improve incumbents’ electoral fortunes (Levitt and Snyder 1997), are backloaded in the Senate due to the electoral pressures of Senators.

Bond Issues

Our dataset is constructed from individual issues recorded in the SDC Platinum database. We sum the individual issues within a year for each state and state authority to generate aggregate amounts of debt issued each year. The SDC Platinum database records the type of issuer for each bond.¹⁰ SDC records both the par amount at maturity and the net proceeds (the par amount minus the original issuing discount) for each issue in the database. Because there is variation in the coupon payments and time to maturity the par amount is not directly comparable across bond issues. We employ the net proceeds

We transform nominal debt issue amounts to 2009 real dollar values using the gross domestic product implicit price deflator from the Bureau of Economic Analysis. Due to the tremendous variation across states in the population and size of the economy, we define the dependent variable in per capita terms. We classify the year of issue based on the sale date of the bond issue. In robustness checks, we examine the sensitivity of the results to perturbations in the timing. The first debt issue sale date we employ occurs in 1980 and the most recent year is 2014. The earliest debt issue in SDC Platinum extend back to the 1960s, but coverage appears to be somewhat haphazard. As a result, we restrict attention to debt issues that begin in 1980.

¹⁰Bonds that have issuer code “10” are classified as state issues and those with issuer code “15” are classified as state authority issues. We remove county, city, town, and village, university, board of education, local authority, American Indian tribe, and not for profit issues from the sample.

Political Variables

Our gubernatorial election data are from the *Congressional Quarterly* Voting and Elections Collection. We calculate the two-party vote share of the Democratic candidate as the ratio of the Democratic candidate's total votes to the sum of the Democratic and Republican candidates' total votes.¹¹ Because we are interested in the treatment effect of a Democratic governor relative to a Republican governor, when a third-party candidate wins election¹² we remove the state from the sample until the year following the next election where a Democrat or Republican wins election. There are also three additional instances (Illinois in 1986, Colorado in 2010, and Maine in 2010), where a third-party candidate finishes in second place ahead of either the Democratic or Republican candidate. This raises the concern that the regression discontinuity design recovers the treatment effect of the winning candidate's partisanship relative to the the third-party candidate. We include these state years in our basic results. In the Online Appendix, we examine the robustness to dropping these state-years from the sample and find that the results are quite similar. We generally classify the partisanship of the governor based on the year the new governor is inaugurated. For example, California changed from a Democratic governor to a Republican governor when Democrat Jerry Brown was replaced by Republican George Deukmejian on January 3, 1983. Because inauguration day is typically very close to January 1, this poses little practical problem for our measurement of gubernatorial

¹¹We center this vote-share variable by subtracting 0.5 from each realization in order to make the cutoff for the Democratic gubernatorial candidate winning 0.

¹²A successful third-party gubernatorial campaign is rare in our sample. Of the 432 gubernatorial elections in our sample, a third-party candidate wins in only six instances.

partisanship and bond issues.¹³

As with much of the literature on state and local public finance, we are interested in the potential role of the political institution of debt referenda requirements in constraining government behavior. Some states maintain a statutory or constitutional requirement that voters approve all new general obligation debt issues through a referenda. We use the classification of Bohn and Inman (1996) to determine which states possess these constraints on borrowing.

Our main focus is on the effect of gubernatorial partisanship because the executive office is extremely important in state policymaking and because we can cleanly use a regression discontinuity design to identify the effect of a change in gubernatorial partisanship on debt issues. However, other officeholders may also affect debt issuance decisions through their influence on the political process. We investigate the possibility that partisan control of the state legislature affects municipal bond issues. We use the Klarner (2003) index of Democratic legislative control, which takes on the value 1 if there are Democratic majorities in both chambers, 0.75 if Democrats control one chamber and split control of the other chamber, 0.5 if Democrats and Republicans each control a chamber, 0.25 if Republicans control one chambers and control is split over the other chamber, and 0 if Republicans control both chambers. Because partisan control of a legislative chamber is the result of many simultaneous

¹³We deviate from our classification rule in two instances. In Arizona the 1990 gubernatorial election advanced to a runoff due to no candidate receiving a majority of the vote. The runoff was held in February 1991 and the Republican candidate Fife Symington III was victorious. We classify 1991 as a Republican year in Arizona. Similarly, as a result of the successful 2003 California recall election, California went from a Democratic to Republican governor when Republican Arnold Schwarzenegger was inaugurated on November 17, 2003. We classify 2003 as a Democratic year in California because the Democrat served in office for the majority of the year and 2004 as a Republican year.

election outcomes, we do not have the discontinuous vote share induced changes in legislative partisanship that we employ to identify the effect of gubernatorial partisanship.

State Demographic and Economic Variables

In some specifications, we include several demographic and economic variables to capture other factors that might affect both the partisanship of the governor and debt issuing behavior of the state. While a valid regression discontinuity design will recover the causal effect of gubernatorial partisanship on debt issues, there is still some risk that state-years are not balanced across treatment and control. The inclusion of these additional controls also has the advantage of improving the precision of the estimates.

We control for the age structure of the state by including the proportion of total state residents 17 years old or younger and the proportion of residents 65 years old or greater as two separate independent variables. These data are from the U.S. Census' Population Estimates files.¹⁴ The demand for public expenditures and the partisanship of the governor may both covary with the age structure of the state so it is potentially important to control for age structure. For example, states with relatively young populations might be more supportive of public expenditures to finance education and infrastructure improvements and these demands may also lead to Democratic governors. Alternatively, young populations may impose some fiscal discipline because these citizens will have to bear a disproportionate share of the

¹⁴Available online at: <https://www.census.gov/popest/data/historical/index.html>

burden of repaying the debt relative to the elderly (Song, Storesletten and Zilibotti 2012). We include the total state population from the Population Estimates files as an additional control variable to capture changes in the demand for debt issues that may be due to changes in population. We include real state per capita income¹⁵, from the Bureau of Economic Analysis' Regional Economic Accounts, and annual state unemployment rate, from the Bureau of Labor Statistics, to capture time-varying local economic conditions that could affect both voting behavior and the demand for new debt issues. The strength of the state economy has the potential to affect debt issues through a number of channels. Municipal bond interest payments are typically tax-exempt for state residents so the market exhibits higher levels of segmentation by state than would exist in the absence of this asymmetric tax treatment (Pirinsky and Wang 2011). Poor economic performance in the state can limit the demand for municipal bonds among state residents and these residents can play a disproportionate role in the market for state-issued debt.

Bond Issue Results

Before presenting the coefficient estimates, we first show graphical evidence on how debt issuing varies with Democratic partisanship for the three types of debt issuing entities. We plot quadratic fits of the dependent variable as a function of the Democratic candidate's centered vote share, allowing the slope of the conditional mean function to vary to the left and the right of the cutoff. We also display a scatter plot

¹⁵As with all other dollar variables in this study, we transform nominal per capita income to real 2009 per capita income through the GDP deflator.

of the local average of bond sale proceeds for bins of Democratic vote share.¹⁶

Figure 1: Regression Discontinuity Plots

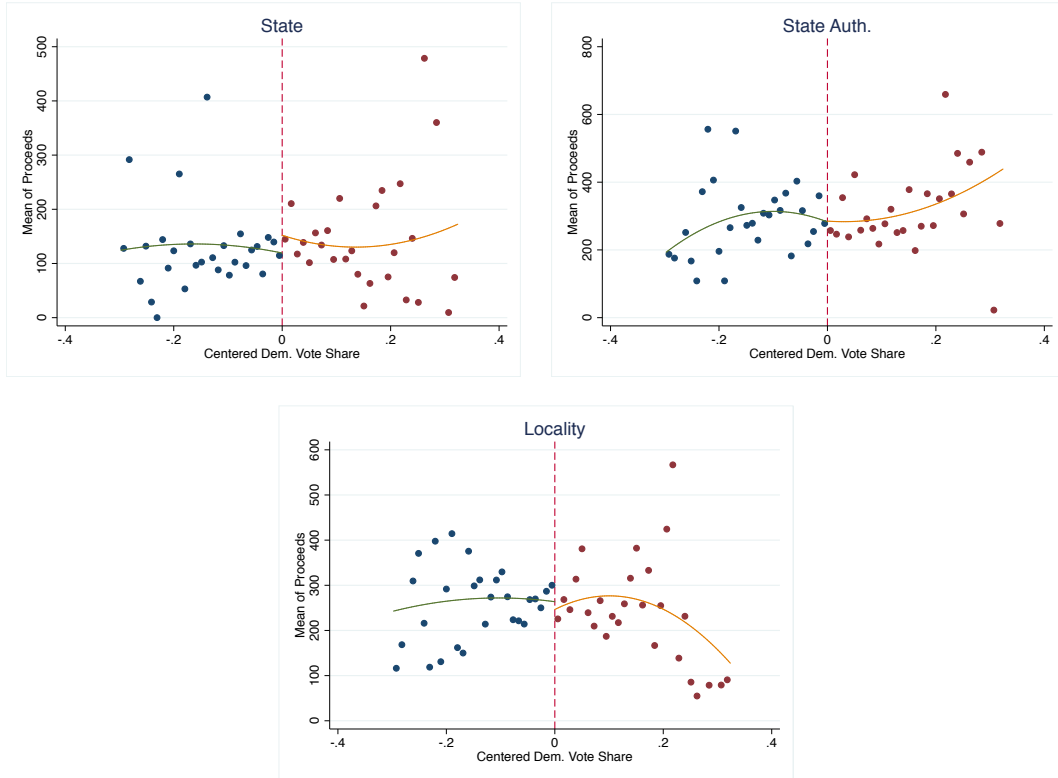
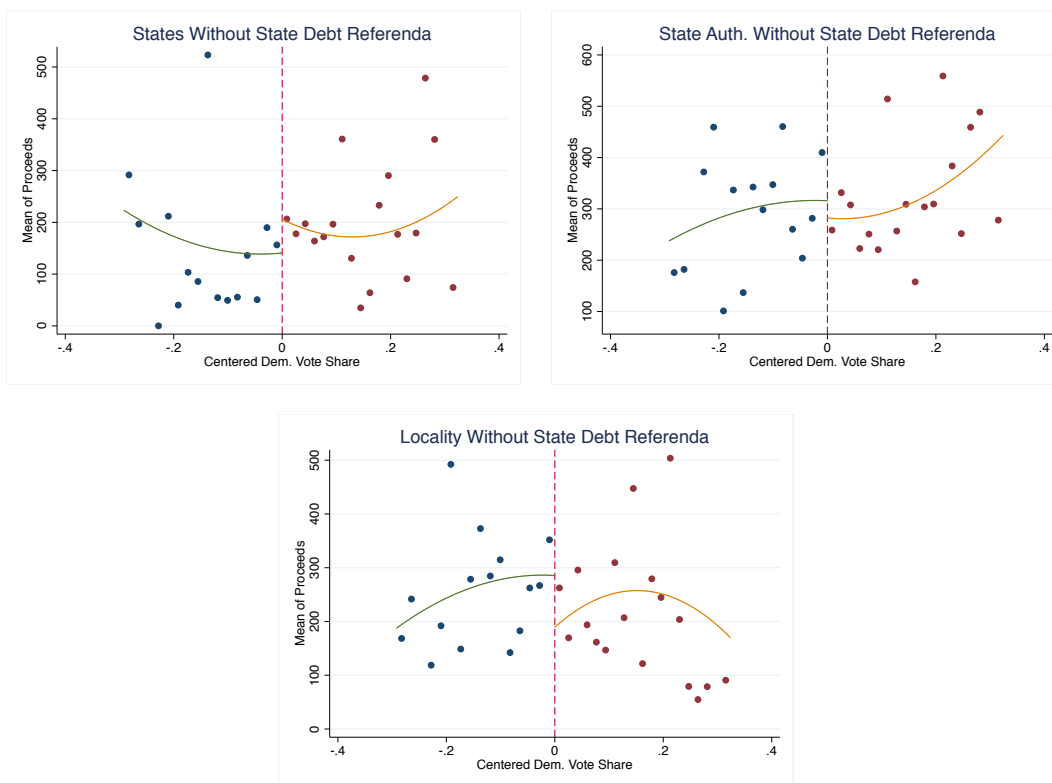


Figure 1 present the fitted regression lines using our full sample of states with and without debt referenda. They reveal a small increase in state per capita bond proceeds when a Democratic governor is in power, no change for state authorities, and a slight decrease for localities. Since debt referenda vary across states and are likely to be an important factor in our analyses, we also recreate these plots for the subset of states that lack debt referenda. Theory suggests that these states are

¹⁶We employ the default bin sizes in the cmogram package in State, which results in bins of approximately 0.01 length.

likely to experience the greatest partisan effect on debt issuance since governors in these states are the least constrained. Figure 2 reveals that the discontinuities at the threshold are more pronounced in these states. There is a sizable increase in state bond proceeds at the cutoff, a decrease for state authorities, and a larger decrease for localities.

Figure 2: Regression Discontinuity Plots



We first present the most basic regression discontinuity design specification that only employs state debt issues. In the first column, we estimate the regression discontinuity specification with the proceeds from new debt issues in millions of real 2009 dollars as the dependent variable and we estimate the simplest RDD model

that lacks state fixed effects. In this basic specification, we include an indicator for whether the Democratic gubernatorial candidate won election, linear and quadratic Democratic candidate vote share polynomials that are permitted to vary to the left and the right of the cutoff. In the second column, we add state and year fixed effects to the specification. In the third column, we introduce demographic and political controls into the specification. Finally, in the fourth column we include state specific linear time trends in the specification. One potential confounder in our design is that states with increasing demand for public goods, perhaps due to a growing economy and in-migration, may be more likely to elect Democratic governors and issue bonds to finance these needs. While the regression discontinuity aspect of our research design and the inclusion of demographic covariates partially obviates these concerns, we examine the sensitivity of our findings by controlling for state specific trends in bond issuing behavior during the sample period. We allow the mean of the dependent variable to evolve flexibly over time by including state linear time trend for each of the states in our regression specifications.

In all four specifications, the effect of a Democratic governor on debt issues is substantively large. In the base specification, the presence of a Democratic governor in states that lack debt referenda requirements increases debt issues by \$73.26 per capita per year. Over the course of a four-year term¹⁷, the local average treatment effect of a Democratic governor is over \$293 in additional per capita state bond proceeds. The estimated coefficient estimate for Democratic governors in states with voter oversight of general obligation debt issuance is much smaller at \$23.15 per

¹⁷Every state, except New Hampshire and Vermont, currently have a four-year gubernatorial term. Arkansas had a two-year term until 1986.

Table 1: State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	73.26*	55.46*	59.24*	39.12 ⁺
	(35.75)	(25.98)	(26.51)	(21.30)
Dem. Won \times State Debt Ref.	-50.11	-50.17 ⁺	-54.15*	-44.03*
	(34.93)	(26.55)	(24.71)	(21.76)
Observations	1714	1714	1714	1714
R^2	0.029	0.480	0.498	0.557
Dem. Effect for Debt Ref.	23.15	5.298	5.092	-4.912
SE Dem. Effect for Debt Ref.	(35.64)	(19.99)	(19.57)	(16.85)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

capita and this effect is statistically indistinguishable from 0. The results are quite similar for the specifications in columns 2, 3, and 4, which add state fixed effects to the regression discontinuity design. There is a large positive estimated effect of Democratic gubernatorial partisanship in states without debt referenda requirements and effects that are substantively close to 0 and statistically insignificant in states with voter oversight of debt issuance decisions.

Table 2: CCT Estimates of State Per Capita Bond Proceeds

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	41.35**	146.9***	28.08 ⁺
	(14.027)	(30.729)	(16.532)
Bandwidth	0.0870	0.0500	0.0730
Observations	1714	657	1057

Robust standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Our basic regression discontinuity design specifications use the entire sample to estimate the effect of gubernatorial partisanship on new debt issues, regardless of the vote share of the Democratic gubernatorial candidate. As a result, we are using data far from the cutoff to identify the discontinuous change in the conditional mean at the 50 percent vote threshold. To examine whether the results are similar when we only use election returns that are in the neighborhood of the 50 percent vote threshold we use the Calonico, Cattaneo and Titiunik (2014) method. Calonico, Cattaneo and Titiunik (2014) derive a data-driven method for selecting the optimal bandwidth to include in the data and recovering bias-corrected point estimates of the treatment effect. As a result, the sample only employs data near the running variable cutoff. We use a common mean-squared error optimal bandwidth across the cutoff.

The results corroborate our findings from the parametric specification that employs the full sample of election outcomes. In the sample of all states and the subset of those without debt referenda requirements, there are large estimated effects of \$70.71 and \$128.1 per capita, respectively. For states with debt referenda requirements, the estimated effect is much smaller in magnitude at \$33.65 per capita and the estimate is statistically indistinguishable from 0.

Effects on State Authorities and Localities

Investigating the responsiveness of state authorities' and localities debt issuance decisions to gubernatorial partisanship allows us to determine whether governors use state authorities and localities to evade constraints on debt issuance imposed by referenda. We estimate our main specifications with all state authority and locality

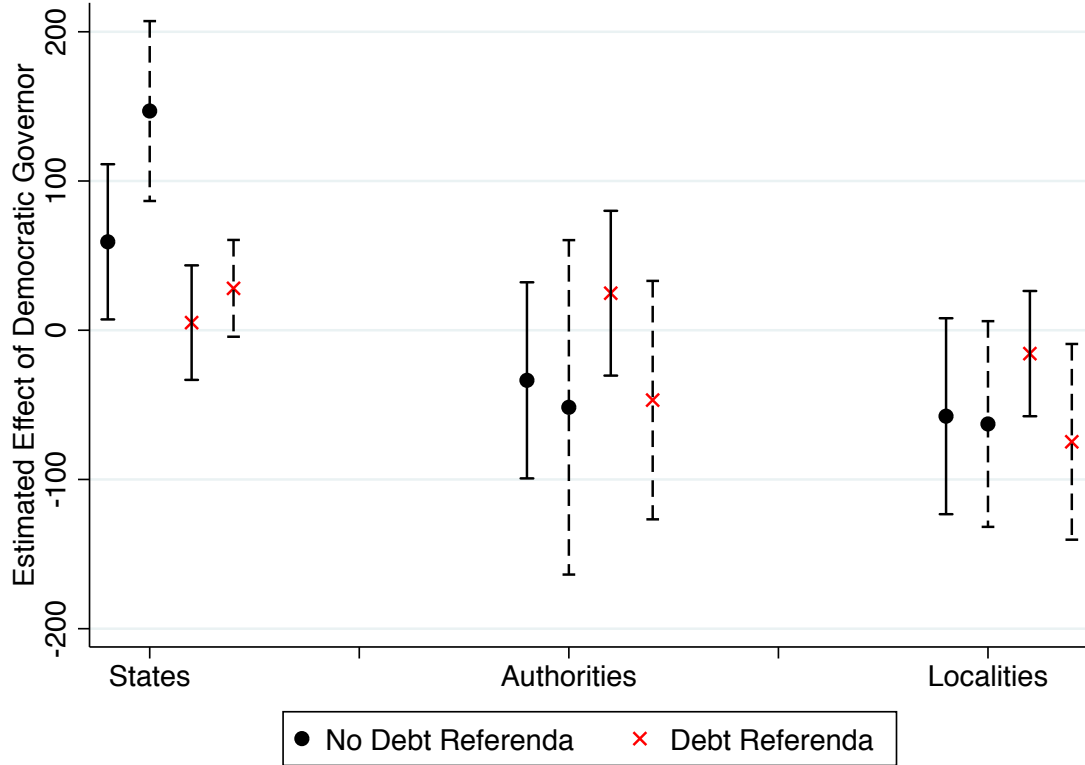
debt issuances in a given state-year as the dependent variable. In Figure 3 we report a plot of coefficient estimates and confidence intervals for the effect of a Democratic governor on states, state authorities, and localities for states with and without debt referenda¹⁸ If the circumvention hypothesis is correct, we expect that in state-years where Democratic governors are constrained by debt referenda requirements, state authorities will issue more debt to offset the foregone debt financing imposed by the constraint. Our results cast doubt on this hypothesis. We find that, in states with debt referenda requirements, state authorities do not issue substantially more debt under Democratic governors. The point estimate is positive in the parametric specification and negative in the Calonico, Cattaneo and Titiunik (2014) specification and statistically insignificant in both. Even the positive point estimate is insufficiently large to fully offset the reduction of state debt issuance associated with debt referenda.

Similarly, the devolution hypothesis predicts that in state-years where Democratic governors are constrained by debt referenda requirements, localities will issue more debt. Our results are also inconsistent with this hypothesis. We find that localities in debt-referenda states issue *less* debt when there is a Democratic governor. The point estimates are negative in both specifications and are precise enough to rule out even modest positive effects.

Both the circumvention and devolution hypothesis are premised on the assumption that state authorities and localities are essentially agents of state officials (Bunch 1991). Our results paint an alternative picture. We find that, at least with regard

¹⁸In the Online Appendix, we report the full set of regression results with state authority and locality debt issues each replacing state' debt issues as the dependent variables.

Figure 3: Effects of Democratic Partisanship by Entity Type and Debt Referenda Requirements



Note: The figure reports coefficient estimates and 95 percent confidence intervals for the effect of Democratic governors on per capita debt issues by states, state authorities, and localities for states with and without debt referenda requirements. The solid line estimates are derived from the parametric RDD specification with state fixed effects and the full set of state economic and demographic controls. The models are separately estimated for states, state authorities, and localities. The dashed line estimates are from the Calonico, Cattaneo and Titiunik (2014) specification using the subsamples of the three entity types in states with and without debt referenda.

to debt issuance decisions, state authorities appear insulated from the partisanship of state officials. State authorities do not adjust their debt issuances in response to the preferences of governors. This insulation is robust to scenarios where governors

are limited by voter referenda in their issuance authority and have strong incentives to encourage state authorities to make up the slack. As Whalley (2013) argues, appointed officeholders are typically motivated by the desire to cultivate a positive reputation amongst their professional peers and potential post-office employers. Developing a reputation for prudent financial management and competence is likely to be a much more important consideration for state authorities than pursuing the electoral goals of governors.

Similarly our results with regards to localities suggest that governors are not able to pursue their debt issuance goals by relying upon local cooperation. Rather, we find that local governments act independently.¹⁹ Localities are governed by mayors, city councils, managers, and bureaucrats who have their own objectives, such as re-election and career concerns, that may be independent of the governor's objectives. Contrary to the devolution hypothesis, which predicts that localities are vehicles for executing the governor's wishes, we find that localities operate as distinct entities.

Effects Over the Election Cycle

We now investigate how the responsiveness of debt issues to partisanship varies over the course of the election cycle. There is a compelling body of evidence from the political behavior literature that voters are myopic and tend to overweight recent events in their evaluation of politicians (Healy and Lenz 2014). As a result, politicians have incentives to frontload electorally undesirable outcomes early in their terms and

¹⁹We do find negative point estimates for the effect of Democratic gubernatorial partisanship on the issuance of debt by localities, regardless of the existence of debt referenda requirements. This is consistent with the idea that localities free-ride off of state-issued debt.

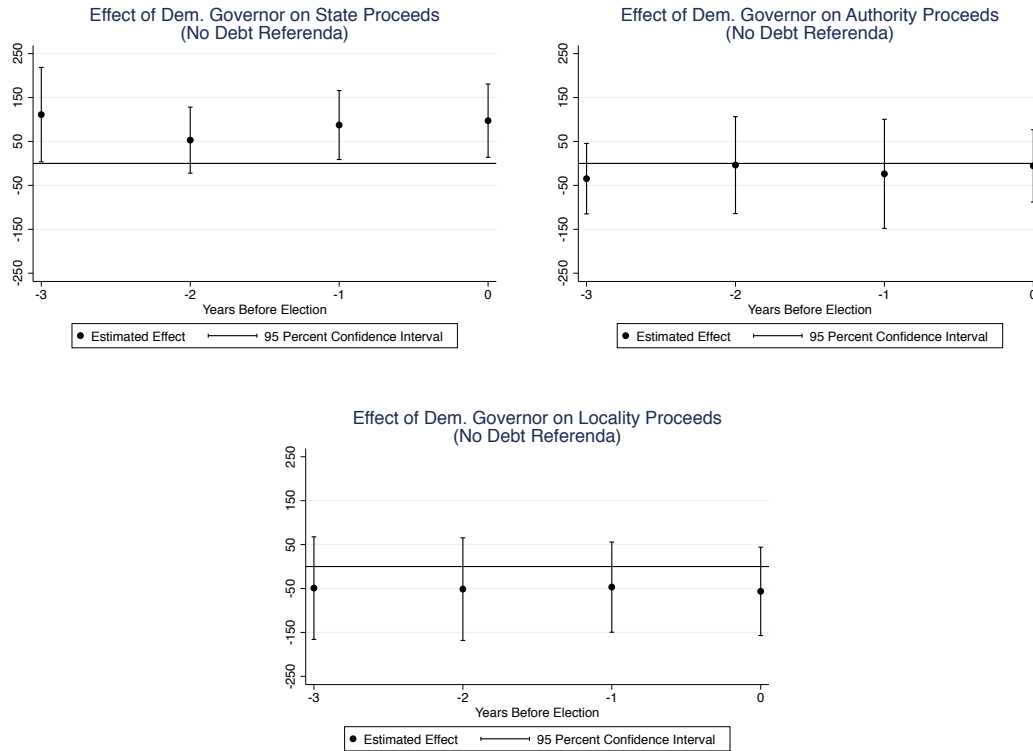
to backload electorally desirable outcomes late in their terms. We also examine how debt issuance varies over the course of the electoral cycle for state authorities and localities to further investigate potential circumvention on behalf of governors.

To investigate the effect of how the Democratic governor effect varies over the course of the electoral cycle, we interact our treatment effect with an indicator for whether there are 3, 2, 1, or 0 years remaining before the next election.²⁰ We then estimate the main regression discontinuity design specification and include these interactions and indicators for whether there are 3, 2, or 1 years remaining before the next election. We separately estimate these models for states, state authorities, and localities and plot the resulting coefficient estimates and 95 percent confidence intervals. For brevity, we restrict attention to the sample of states that lack debt referenda requirements in the main text and report the results for states without debt referenda in the Online Appendix.

The plot illustrate that the effect of a Democratic governor on state debt issue amounts is relatively stable over the course of the election cycle. There is little variation across years and no evidence that bond issuances systematically vary over the electoral cycle. The pattern is quite similar for both state authority and locality issues with neither type of entity changing their debt issuance behavior over the course of an electoral cycle. Unlike electorally-salient activities such as Senate appropriations (Shepsle et al. 2009) there is no evidence that politicians time debt issues in order to improve their electoral prospects. These findings are consistent with the perspective that bond issues are shrouded from voter awareness.

²⁰We code the year of the election as taking place 0 years before the election.

Figure 4: Effects by Years Elapsed Since Election



Democratic Legislative Control

While governors have many tools to affect the debt-issuing behavior of the state, our theoretical perspective suggests that other political actors may also be able to influence the state’s debt issuing behavior. In particular, we expect that the partisan composition of the state legislature may also have an effect on new debt issues. We measure partisanship using the Klarner (2003) index of legislative control. As mentioned earlier, this index takes on the value 1 when the Democratic party has majorities in both legislative chambers and 0 when the Republicans have majorities

in both chambers. We do not have a regression discontinuity design to investigate the effect of legislative partisanship on state debt issues. Instead, the causal interpretation of the legislative partisanship variable depends on state partisanship being independent of unobservables after conditioning on state and year fixed effects and state time trends. This is a less plausible assumption than that used to justify the regression discontinuity design so more caution is warranted in interpreting these results.

Table 3: State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	58.51 (40.67)	43.34 (28.49)	46.89 ⁺ (27.35)	39.81 ⁺ (22.30)
Dem. Won × State Debt Ref.	-47.61 (39.40)	-55.69 ⁺ (29.19)	-60.99* (25.89)	-55.83* (24.37)
Dem. Leg. Control Index	74.67* (28.21)	50.71* (22.44)	55.14** (20.25)	62.09** (19.66)
Observations	1541	1541	1541	1541
R^2	0.050	0.477	0.498	0.562
Dem. Effect for Debt Ref.	10.90	-12.35	-14.10	-16.02
SE Dem. Effect for Debt Ref.	(34.82)	(20.73)	(20.76)	(18.49)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In all four specifications, the coefficient estimate on the Democratic legislative control index are positive, statistically distinguishable from 0 and roughly the same magnitude as the Democratic governor coefficient estimate.²¹ The results are con-

²¹We fail to reject the null that the coefficients on Democratic governor and Democratic legislative

sistent with the perspective that partisanship influences state debt issuance through both the executive and legislative branches and that the effect sizes are similar to one another.

Refunding As An Alternative Explanation

An alternative interpretation of our results is that states with Democratic governors are issuing higher levels of debt in order to take advantage of refinancing opportunities made available by the decrease in interest rates during the sample period. Macroeconomic variables, such as decreases in interest rates, that vary over time but not across states are likely to drive a large portion of this refunding behavior and the inclusion of year fixed effects in our regression specifications flexibly accommodate these changes over time. However, some of the residual variation in refunding behavior might be the result of gubernatorial partisanship.

To investigate this possibility we employ Census data on debt refunding behavior by the states from the State Government Finance reports as the dependent variable in our analysis.²² The comparison with our main results is not perfect because of differences in how SDC and the Census classify state governments. In particular, the Census data preclude separating the refunding behavior of states and state authorities because both types of entities are aggregated together in the Census' compilation of the reports.²³ If the interpretation of our main result is that Democrats are issuing

control are equal in all four specifications.

²²We have data from 1980 to 2008 for the purpose of this analysis.

²³The Census defines state governments as follows: "For the purpose of Census Bureau statistics, the term 'state government' refers not only to the executive, legislative, and judicial branches of a given state, but it also includes agencies, institutions, commissions, and public authorities that operate separately or somewhat autonomously from the central state government but where the

higher levels of debt than Republicans in order to refund previous debt issues and not finance new projects, we would expect to see higher levels of refunding behavior when Democratic governors are in office.

Table 4: State Refunded Debt Per Capita

	(1)	(2)	(3)	(4)
	Refunds	Refunds	Refunds	Refunds
Dem. Won	-37.30*	-39.69**	-39.88**	-24.77 ⁺
	(15.86)	(12.34)	(12.59)	(13.17)
Dem. Won \times State Debt Ref.	16.31	34.20**	36.35***	22.19*
	(13.05)	(9.785)	(9.930)	(9.951)
Observations	1426	1426	1426	1426
R^2	0.026	0.269	0.278	0.319
Dem. Effect for Debt Ref.	-20.99	-5.491	-3.533	-2.576
SE Dem. Effect for Debt Ref.	(13.28)	(12.38)	(12.59)	(12.52)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Contrary to these expectations and consistent with our main interpretation that debt is being used for new projects, the point estimates on the Democratic governor indicator are negative in all four specifications and are even statistically different from 0 in some of the specifications. While the results do not offer definitive evidence that Democratic governors engage in *lower* levels of debt refunding than Republican governors, they do help rule out the possibility that Democrats refund debt at higher rates than Republicans. Due to the negative point estimates and relatively precise standard errors, we can reject reasonably low positive values of the Democratic gov-

state government maintains administrative or fiscal control over their activities as defined by the Census Bureau.”

error effect on refunding at conventional significance levels.

Table 5: CCT Estimates of State Refunded Debt Per Capita

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	-17.29	-15.70	-9.065
	(12.83)	(33.469)	(11.969)
Bandwidth	0.0690	0.0760	0.0520
Observations	1426	543	883

Robust standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results of the CCT regression discontinuity estimator are similar. The point estimates are negative in all three specifications, but are not statistically distinguishable from 0 in any of the specifications. Again, the results do not show that Democratic governors are issuing more debt due to financial management.

Sensitivity Checks

In the Online Appendix, we consider a number of robustness checks to examine the sensitivity of the findings to alternative constructions of the dependent variable and modeling assumptions. We first examine how the results change with alternative specifications of the vote share polynomial. While we follow the suggestions of Gelman and Imbens (2014) and focus on quadratic polynomials of the running variable in the main results, we report specifications with linear and cubic vote share polynomials in the Online Appendix. We also report the results of the pure differences-in-differences research design that does not include any controls for the Democratic gubernatorial candidate's vote share. This is the specification that we would estimate if we lacked information on the electoral support of gubernatorial candidates.

The results are qualitatively similar to the main findings. Interestingly, the point estimate is largest for the specification with the cubic vote share polynomial, which is consistent with the warnings of Gelman and Imbens (2014) that point estimates can increase dramatically when higher-order polynomials are used.

We also examine the robustness of our main findings to alternative classifications of the timing of debt issues. During the transition period between gubernatorial election and inaugurations, state governments and authorities may no longer be responsive to the incumbent governor's partisanship. We consider two alternative operationalizations of the dependent variable to investigate whether these dynamics are affecting the results. First, we omit all bond sales that occur in November and December of gubernatorial election years from the construction of the dependent variable. Second, we reclassify bond sales that occur in November and December of an election year as occurring in the subsequent year. This second approach allocates responsibility for the bond issues to the partisanship of the elected, but not yet inaugurated, governor. In the Online Appendix, we report our main specification with each dependent variable and find similar point estimates and standard errors.

Throughout most of the analysis we have implicitly assumed that the local average treatment effect of a Democratic governor on debt issues only varies with whether the state has debt referenda requirements or not. However, there is also variation in the meaning of a Democratic governor across states due to regional differences in party structure. In Southern states during the early part of the sample period, it was not uncommon to have ideologically conservative Democratic gubernatorial candidates. In the Online Appendix, we re-estimate the state bond proceeds regres-

sions excluding the sample of Southern states. We use Farhang and Katznelson's (2005) 17-state definition of the South based on whether the state required segregated schools before the *Brown v. Board of Education* decision. Consistent with the perspective that southern Democratic governors are likely to be more conservative than non-Southern Democratic governors, the treatment effects of a Democratic governor on debt proceeds are even larger when Southern states are omitted from the sample.

Conclusion

While electoral incentives induce relatively small policy differences between Democratic and Republican governors on salient policy issues, the scope for politicians to pursue ideologically-preferred policies expands in the absence of electoral oversight. We document policy responsiveness to gubernatorial partisanship in the context of state bond issues. When a Democrat serves as governor, there is a statistically and substantively significant increase in state bond issues. In contrast, politically insulated state debt authorities do not change their debt issuing behavior in response to the partisanship of the governor.

The results open up several promising avenues for future inquiry. While we have documented statistically and substantively important differences in the debt issuing behavior of states as a function of gubernatorial partisanship, we have yet to investigate how the characteristics of the debt issues varies with partisanship. Do Democratic governors employ debt with longer maturities? Are Democratic gover-

nors more likely to employ negotiated rather than competitive bidding when pricing debt issues for the market? Are there differences across the parties in how debt proceeds are used?

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Appendix

Table A.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Proceeds	133.748	187.179	0	2084.107	1714
Refunds	42.449	92.712	0	1038.967	1426
Debt Referenda Requirement	0.617	0.486	0	1	1714
Dem. Leg. Control Index	0.597	0.434	0	1	1541
Dem. Won	0.506	0.5	0	1	1714
Centered Dem. Vote Share	0.003	0.11	-0.297	0.324	1714
Proportion of State Pop. 17 and Under	0.258	0.026	0.15	0.385	1714
Proportion of State Pop. 65 and Older	0.126	0.022	0.029	0.261	1714
State Population (1000s)	5458.084	5991.255	405.315	38802.5	1714
State Real Per Capita Income	32043.056	7934.214	16046.239	57476.859	1714
State Unemployment Rate	6.075	2.118	2.3	17.8	1714

Placebo Tests

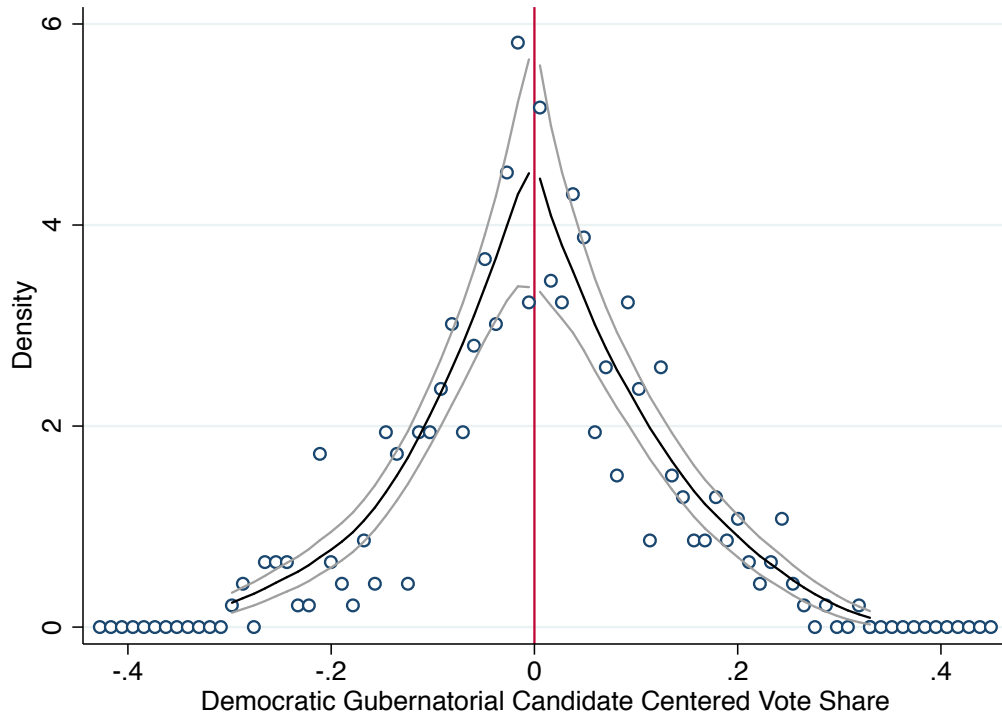
Table A.2: Placebo: Lagged State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds
Dem. Won	43.81 (44.07)	4.218 (27.41)	6.246 (28.30)	-18.90 (25.66)
Dem. Won \times State Debt Ref.	-24.90 (41.79)	-6.872 (25.45)	-4.254 (25.87)	10.17 (25.15)
Observations	1520	1520	1520	1520
R^2	0.033	0.475	0.489	0.553
Dem. Effect for Debt Ref.	18.91	-2.655	1.992	-8.728
SE Dem. Effect for Debt Ref.	(37.83)	(23.64)	(23.49)	(24.71)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A.1: McCrary Test



State Authority and Locality Regression Results

Table A.3: Placebo: State Authority Lagged Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds
Dem. Won	-22.47 (66.83)	-69.76 (47.40)	-63.10 (45.66)	-69.12 (41.80)
Dem. Won \times State Debt Ref.	-26.99 (39.36)	43.63 (28.87)	41.43 (30.80)	32.18 (23.41)
Observations	1520	1520	1520	1520
R^2	0.009	0.494	0.503	0.594
Dem. Effect for Debt Ref.	-49.46	-26.12	-21.67	-36.94
SE Dem. Effect for Debt Ref.	(83.7)	(44.86)	(46.02)	(45.25)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Placebo: Locality Lagged Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds	Lagged Proceeds
Dem. Won	9.714 (61.75)	4.934 (30.42)	18.75 (31.90)	16.01 (29.95)
Dem. Won \times State Debt Ref.	30.40 (42.44)	26.52 (33.14)	18.32 (27.34)	-12.37 (20.37)
Observations	1520	1520	1520	1520
R^2	0.015	0.581	0.610	0.687
Dem. Effect for Debt Ref.	40.12	31.46	37.07	3.634
SE Dem. Effect for Debt Ref.	(83.02)	(50.36)	(46.53)	(27.56)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5: State Authority Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-11.10 (61.11)	-46.21 (34.16)	-33.57 (33.50)	-23.78 (32.10)
Dem. Won \times State Debt Ref.	16.01 (46.72)	66.47 ⁺ (35.30)	58.39 ⁺ (32.26)	37.35 (31.12)
Observations	1714	1714	1714	1714
R^2	0.005	0.506	0.520	0.603
Dem. Effect for Debt Ref.	4.914	20.27	24.82	13.57
SE Dem. Effect for Debt Ref.	(63.63)	(30.77)	(28.15)	(26.47)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: CCT Estimates of State Authority Per Capita Bond Proceeds

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	-47.52 (34.3)	-51.67 (57.13)	-46.83 (40.728)
Bandwidth	0.0490	0.0800	0.0510
Observations	1714	657	1057

Robust standard errors in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7: Locality Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-55.37 (48.39)	-63.47 (38.54)	-57.62 ⁺ (33.48)	-55.91 ⁺ (29.97)
Dem. Won × State Debt Ref.	58.31 (36.44)	50.57 ⁺ (28.25)	41.95 (26.54)	26.78 (22.86)
Observations	1714	1714	1714	1714
R^2	0.008	0.581	0.616	0.677
Dem. Effect for Debt Ref.	2.939	-12.90	-15.67	-29.13
SE Dem. Effect for Debt Ref.	(51.38)	(25.12)	(21.4)	(21.27)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: CCT Estimates of Locality Per Capita Bond Proceeds

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	-63.64** (22.558)	-62.84 ⁺ (35.143)	-74.78 ** (33.431)
Bandwidth	0.0520	0.0700	0.0360
Observations	1714	657	1057

Robust standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Alternative Vote-Share Polynomials

Table A.9: Linear Vote Share Polynomial State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	53.48 ⁺ (31.80)	35.59 (22.67)	37.72 ⁺ (22.44)	33.10* (14.07)
Dem. Won \times State Debt Ref.	-50.20 (34.88)	-49.78 ⁺ (26.59)	-53.41* (24.75)	-44.06* (21.64)
Observations	1714	1714	1714	1714
R^2	0.028	0.478	0.496	0.556
Dem. Effect for Debt Ref.	3.283	-14.19	-15.69	-10.96
SE Dem. Effect for Debt Ref.	(25.91)	(16.12)	(16.21)	(15.81)
Linear Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: Linear Vote Share Polynomial State Authority Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-50.75 (42.34)	-83.72* (31.34)	-79.13** (28.57)	-50.41* (23.97)
Dem. Won \times State Debt Ref.	15.54 (46.91)	65.31 ⁺ (35.20)	56.63 ⁺ (32.35)	37.02 (31.32)
Observations	1714	1714	1714	1714
R^2	0.003	0.504	0.518	0.602
Dem. Effect for Debt Ref.	-35.21	-18.40	-22.50	-13.40
SE Dem. Effect for Debt Ref.	(40.15)	(26.41)	(26.09)	(23.77)
Linear Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11: Linear Vote Share Polynomial Localities Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-37.19 (35.94)	-20.00 (31.14)	-25.42 (27.43)	-26.16 (22.94)
Dem. Won \times State Debt Ref.	57.97 (35.92)	49.48 ⁺ (27.71)	40.50 (26.06)	27.08 (22.27)
Observations	1714	1714	1714	1714
R^2	0.005	0.578	0.614	0.675
Dem. Effect for Debt Ref.	20.78	29.48	15.07	0.913
SE Dem. Effect for Debt Ref.	(37.29)	(21.74)	(15.91)	(13.98)
Linear Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.12: Cubic Vote Share Polynomial State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	66.11 ⁺ (33.75)	46.10 ⁺ (24.24)	49.58* (24.47)	34.37 ⁺ (18.34)
Dem. Won × State Debt Ref.	-50.24 (34.95)	-50.46 ⁺ (26.66)	-54.53* (24.82)	-44.32* (21.73)
Observations	1714	1714	1714	1714
R^2	0.029	0.479	0.497	0.557
Dem. Effect for Debt Ref.	15.87	-4.359	-4.943	-9.949
SE Dem. Effect for Debt Ref.	(31.65)	(17.22)	(16.96)	(15.28)
Cubic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.13: Cubic Vote Share Polynomial State Authority Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-22.38 (53.48)	-51.94 ⁺ (30.59)	-42.71 (29.68)	-32.95 (28.35)
Dem. Won × State Debt Ref.	15.74 (46.77)	65.89 ⁺ (34.98)	57.63 ⁺ (32.00)	36.98 (31.12)
Observations	1714	1714	1714	1714
R^2	0.005	0.506	0.520	0.603
Dem. Effect for Debt Ref.	-6.645	13.95	14.92	4.027
SE Dem. Effect for Debt Ref.	(54.65)	(27.37)	(24.71)	(25.28)
Cubic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.14: Cubic Vote Share Polynomial Localities Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	-48.06 (44.64)	-47.75 (35.45)	-46.47 (30.92)	-48.06 ⁺ (27.37)
Dem. Won \times State Debt Ref.	58.50 (36.37)	51.04 ⁺ (28.29)	42.23 (26.53)	27.35 (22.83)
Observations	1714	1714	1714	1714
R^2	0.009	0.581	0.616	0.676
Dem. Effect for Debt Ref.	10.44	3.287	-4.240	-20.71
SE Dem. Effect for Debt Ref.	(47.59)	(23.83)	(19.31)	(18.58)
Cubic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Pre-Inauguration Sensitivity Check

Table A.15: State Per Capita Bond Proceeds

	(1)	(2)	(3)	(4)
	Proceeds (Pre-Nov.)	Proceeds (Pre-Nov.)	Proceeds (Pre-Nov.)	Proceeds (Pre-Nov.)
Dem. Won	69.56* (33.72)	52.38* (25.68)	55.78* (26.36)	30.82 (20.83)
Dem. Won \times State Debt Ref.	-43.84 (34.17)	-44.32 (26.73)	-47.94 ⁺ (24.83)	-31.76 (21.43)
Observations	1714	1714	1714	1714
R^2	0.026	0.469	0.487	0.545
Dem. Effect for Debt Ref.	25.72	8.059	7.840	-0.941
SE Dem. Effect for Debt Ref.	(34.73)	(19.5)	(19.18)	(16.35)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.16: CCT Estimates of State Per Capita Bond Proceeds

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	52.92 *** (13.769)	115.7 *** (25.518)	26.99 (16.632)
Bandwidth	0.0600	0.0550	0.0660
Observations	1714	657	1057

Robust standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Non-Southern States

Table A.17: State Per Capita Bond Proceeds (Excluding South)

	(1)	(2)	(3)	(4)
	Proceeds	Proceeds	Proceeds	Proceeds
Dem. Won	78.43 ⁺ (42.94)	93.35* (36.68)	96.01* (39.02)	82.31* (34.52)
Dem. Won \times State Debt Ref.	-48.27 (49.92)	-67.47 ⁺ (38.38)	-78.69* (33.78)	-78.13* (29.35)
Observations	1119	1119	1119	1119
R^2	0.062	0.462	0.483	0.542
Dem. Effect for Debt Ref.	30.17	25.88	17.32	4.189
SE Dem. Effect for Debt Ref.	(45.57)	(30.34)	(30.96)	(26.17)
Quadratic Dem. Vote Poly	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes
Entity Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Entity Time Trends	No	No	No	Yes

Heteroskedasticity robust standard errors clustered at the state level in parentheses
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.18: CCT Estimates of State Per Capita Bond Proceeds (Excluding South)

	(1)	(2)	(3)
	All States	No Debt Referenda	Debt Referenda
Bias-Corrected RD Estimate	112.3 *** (23.293)	433.5 *** (58.316)	5.759 (23.612)
Bandwidth	0.0350	0.0270	0.0650
Observations	1119	377	742

Robust standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Effects by Year For States with Debt Referenda Requirements

Figure A.2: Effects by Years Elapsed Since Election

