PROPERTY TAXES AND POLITICIANS: EVIDENCE FROM SCHOOL BUDGET ELECTIONS

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Recent studies provide mixed evidence on whether electoral pressure influences policy choices. This study examines this question in a unique setting: local school districts where the policy outcome, property taxes, has unusually high visibility and salience. We exploit the sharp discontinuity created by the annual majority-rule school-budget elections in New Jersey's school districts. Using panel data from over 3,600 district-by-year elections, we find that local politicians are responsive to a modest change in support for a budget (around the 50 percent threshold) resulting in a reduction in contemporaneous property taxes (by $180 per pupil or 1.7 percent) as well as the proposed tax bill for the following year. However, these tax reductions do not generally persist because budget rejections also trigger subsequent increases in both voter turnout and support for school spending, suggesting the stability of Tiebout equilibria.

I. INTRODUCTION

In the United States, virtually all local governments tax residential property and rely on these levies as a major source of revenue. However, explicit limits on property taxes (i.e., constraints on revenues, tax rates, or the growth in tax rates) are also now quite common. These tax limitations could reflect a desire among voters to constrain a fiscal "Leviathan" or to acquire a form of insurance against future tax liabilities (Anderson, 2006). The imposition of such tax caps does appear to limit the growth in property taxes, particularly in the longer run (e.g., Dye, McGuire, and McMillen, 2005). In this study, we examine how property taxes respond to voter discontent that is instead expressed directly through referenda.

Specifically, we focus on the unique institutional circumstances in New Jersey school districts where, every spring, voters participate in a majority-rule election in which they

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can either approve their local district's proposed budget for the coming academic year or reject it. When voters reject a budget, their local governing body (e.g., boroughs and townships) can then choose to reduce the proposed budget. We examine whether elected officials respond to voter rejection of a school budget by reducing property taxes. We identify the causal effect of these rejections through a regression discontinuity (RD) design that effectively compares the aftermath of budgets that were just rejected to those that were just approved.

We find that local politicians are indeed responsive to modest changes in voter preferences, as property taxes in districts that just rejected their budgets are $180 per student lower than in districts that just passed their budgets. The availability of longitudinal data on budget elections and property taxes in school districts over several years also allows us to study the longer-term effects of a budget rejection. We find that a rejected budget increases voter turnout and support for school spending in the subsequent year (i.e., attenuating the prior reduction in property taxes). We also present a simple decomposition, which allows us to combine these results to infer how the budgets *proposed* by school boards respond to a prior rejection. Our results suggest that school boards do respond to a rejected budget by proposing more modest spending increases in the next year.

Overall, our study indicates that these school-budget elections have modest but short-lived effects on property taxes and public school budgets. Our study adds to an active, recent literature on whether elections influence policy choices (Lee, Moretti, and Butler, 2004; Leigh, 2008; Fredriksson, Wang, and Warren, 2009; Ferreira and Gyourko, 2009; Albouy, 2011). We argue that our findings are consistent with the view that elections have attenuated relevance in highly Tiebout-like settings in which voters have already "voted with their feet" to choose their preferred tax and spending bundle.

Our paper is organized as follows. Section II discusses the prior literature and relates the contribution of this study to that literature in more detail. Section III outlines the school budgeting and election procedures in New Jersey, and Section IV discusses the district-by-year panel data used in this study. Section V provides an overview of our econometric strategies. Section VI presents the main results, Section VII presents dynamic estimates, and Section VIII concludes.

II. ELECTIONS AND POLICY CHOICES

We examine whether elected officials adjust local tax burdens in response to credibly exogenous differences in voter support for such reductions. Traditional models of political economy, which suggest that politicians will adopt and implement moderate policies in order to maximize their likelihood of remaining in office, imply that elected officials will be responsive to such variation in voter preferences. This classical perspective on the policy choices made by elected representatives is based on two seminal theoretical contributions. One is that the most preferred choice of the median voter can dominate all other options in binary-choice elections based on majority rule (Black, 1948). The second is the claim that competitive politicians interested in winning elections will

then make credible commitments to policies that converge on those of the median voter (Downs, 1957). However, it has also been understood for some time that the extreme policy convergence implied by the Downsian model of competitive political behavior will be attenuated when politicians maintain personal ideological preferences in addition to their interest in electoral success (Wittman, 1983; Calvert, 1985).

A more recent literature outlines a broader critique of the Downsian paradigm and its implications for the policy choices made by elected representatives (Persson and Tabellini, 2000; Besley and Case, 2003). Part of this criticism simply turns on the validity of the assumptions necessary for the median voter's preferences to be decisive (e.g., single-peaked preferences, a single-dimensional issue). However, this literature has also raised concerns about the assumptions that politicians have motivations based primarily on remaining in office and that they have the capacity to commit to post-election policies credibly (Alesina, 1988; Glaeser, Ponzetto, and Shapiro, 2005). Certainly, in a one-shot election where commitments are not credible and politicians have private preferences about which policy is implemented, voters should not expect the policy convergence the Downsian model predicts (Alesina, 1988). However, Alesina (1988) also shows that, even in infinitely repeated elections, whether elected candidates choose policies that deviate from their ideological preference depends on key modeling assumptions such as the discount rates of the candidates, the divergence of their preferences, and the comparative popularity of the parties. More recently, Glaeser, Ponzetto, and Shapiro (2005) outline a model in which there is no policy convergence because politicians strategically adopt extreme positions in order to increase turnout and donations among core constituents.

A parallel literature examines the relevance of local politics given individuals' ability to "vote with their feet" (Tiebout, 1956). Tiebout's seminal model implies that elections are largely unnecessary because competition among jurisdictions will restrict the ability of politicians to pursue their self-interest. However, Epple and Zelenitz (1981) show that, in a model with fixed jurisdictional boundaries and immobile land, competition among localities is not sufficient to eliminate the potential monopoly power of local governments. This finding suggests that elections may have relevance for policy choices, even in Tiebout-like settings (i.e., "Tiebout does need politics"). Henderson (1985) questions the assumptions of this model, arguing instead that landowners and entrepreneurs play an active role in inter-jurisdictional land markets and that, by implication, politics are not relevant in a long-run Tiebout equilibrium.

The empirical literature on how electoral pressure influences the policy choices made by elected representatives has produced mixed results across different settings. Studies in political science find that elected officials who face similar constituencies but are from different parties vote very differently, implying a lack of convergence to the preferences of the median voter (e.g., Poole and Rosenthal, 1984). A more recent study by Lee, Moretti, and Butler (2004) examines these issues using data from elections to the U.S. House of Representatives and from the subsequent voting behavior of members of Congress. Specifically, they examine the effect of having a congressional seat won in a close election (a 50 to 52 percent share of the vote) by a particular party

in period *t* (e.g., the November 1992 election relevant for the 1993–94 congressional session) on the voting behavior of whoever held that seat in period *t* + 1 (i.e., during the 1995–96 congressional session). They argue that the credibly exogenous movement to just winning the prior election creates an incumbency advantage that could influence subsequent policy choices, in part, through an increase in electoral strength. However, an empirical decomposition of this overall effect indicates that House members do not alter their voting behavior in response to this increase in electoral strength. In other words, voters (at least in U.S. House elections) appear to "elect" rather than "affect" policies. Lee, Moretti, and Butler's leading explanation for the lack of policy convergence is the notion that politicians cannot make a credible commitment to pursuing specific policies in order to gain electoral advantage. They therefore choose to pursue their preferred policies regardless of variation in their electoral strength. In a regression-discontinuity study of Swedish local governments, Pettersson-Lidbom (2008) similarly finds that economic policies are indeed influenced by exogenous variation in party control (i.e., a lack of policy convergence).

However, in an extensive review of the earlier literature, Besley and Case (2003) identify the role of elections in driving accountability as one of the most robust findings in the empirical literature on policy choices. They underscore specifically the evidence that politicians who are not seeking reelection make different choices than those who are. Similarly, more recent, regression-discontinuity studies based on the policy choices made by governors present evidence that is more consistent with the classical Downsian perspective (Leigh, 2008; Fredriksson, Wang, and Warren, 2009). These studies suggest that electoral pressures drive some policy convergence across members of both political parties. Similarly, in a regression-discontinuity study of elections in larger U.S. cites (i.e., population over 25,000), Ferreira and Gyourko (2009) find no overall effect of partisan switches in mayoral control on municipal policies.

Finally, there is a set of papers that focus on school budget referenda (Romer and Rosenthal, 1979, 1982; Romer, Rosenthal, and Munley, 1992). Romer, Rosenthal, and Munley (1992) examine the degree to which referenda constrain spending using linked referenda and spending data in New York school districts during the 1975–76 school year.[1] Their study differs from ours in that the authors use the full range of vote share to identify the effects of interest. Because this vote share is endogenous, the authors model voting and district spending jointly. While they devote more time to the underlying structural parameters driving voter decisions, they also attempt to distinguish between districts that seek to maximize their budgets and those that attempt to satisfy the median voter. They find evidence that larger districts are more likely to be budget maximizers while smaller districts aim to satisfy the median voter. A more recent study examines a related question: does the presence of a referenda on a budget constrain spending?

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[1] Their setting is distinct from that of New Jersey in at least two ways. First, budget referenda could be put up for a vote multiple times. Second, a failed budget had more serious consequences in New York than in New Jersey, as a failure resulted in a reversion level of spending that was dramatically lower than the proposed level.

Using the introduction of school budget referenda for smaller school districts in New York, Nguyen-Hoang (2012) shows that the presence of a budget vote results in lower spending levels.

Other researchers have leveraged school budget votes to examine somewhat different outcomes. Ehrenberg et al. (2004) explore why school district budget referenda fail, finding that rejection in a prior year is a strong predictor of failure. Brunner and Ross (2010) explore whether school budget voting patterns tend to follow the preferences of the voter with the *median income*, finding that outcomes are more consistent with the preferences of an individual below the median income level. The authors suggest that this may be a result of the education spending preferences of very high-income individuals aligning with those of poor individuals. A recent paper uses school bond referenda and a regression discontinuity design to investigate the effect of school facility investments on area housing prices, finding that homeowners value these types of investments (Cellini, Ferreira, and Rothstein, 2010).

Our study presents new regression-discontinuity evidence on whether elections influence policy choices in local Tiebout-like settings. Specifically, we examine how the property-tax burdens chosen by local elected officials and school districts in New Jersey respond when voters reject the proposed annual school budget in favor of less spending. Our study leverages the plausibly exogenous variation in voter rejection generated by the sharp regression-discontinuity that occurs when the share of voters favoring rejection just reaches 50 percent or higher. We outline the key institutional features of this budgetary process in more detail below. However, in the context of political-economy models, we view voter rejection of a school budget as essentially having two proximate consequences.

First, voter rejection sends to local officials a clear indicator of negative voter sentiment towards the proposed budget. In the case of our regression-discontinuity design, where the vote share is evenly split, we argue that the relevant sentiment is that of the median voter. In this setting, a budget failure alerts local politicians and school board members that a bare majority disapproves of the budget. Our estimates then compare the local property taxes per students under a bare majority disapproval to a bare majority approval, essentially capturing the effect of moving a small share of voters from one side to the other. To be clear, this is a relatively weak message to elected officials; however, it is just that weak message that lies at the heart of the median voter model. If the budget rejection around the threshold lacks information, we should not observe an effect on the final budget adopted or the budget proposed in the following year.[2]

Second, as described below, a rejected budget also expands the set of choices available to elected officials, who can then reduce (but not increase) the school budget.[3] Theoreti-

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[2] Relatively low voter turnout in these elections (15 percent on average) weakens the signal further. However, this is an additional reason why we would not expect to observe an effect of a budget rejection on property taxes.

[3] In our dynamic analysis, we address the response of elected school board officials at time *t* to a rejection at time *t* − 1.

cally, we would expect property-tax burdens to be unresponsive to voter rejection of budgets if local officials tend to view the rejected budget (or even higher spending) as their preferred policy and if they are unresponsive to electoral pressure (e.g., because they cannot make credible policy commitments as suggested by the policy-divergence result in Lee, Moretti, and Butler, 2004). In contrast, we would expect voter rejection of a school budget to lead to policy changes if (1) local officials are responsive to voter preferences (i.e., as in classical Downsian models), or (2) the opportunity to decrease a school budget has made available to local officials their most preferred policy choice (i.e., lower spending), which they choose without regard to voter preferences.[4]

We can further explore the responsiveness of elected officials by examining the effect of a budget failure on the proposed budget in the following year. Presumably, the budget proposed by school board officials in year $t-1$ reveals a lower bound for their preferred level of spending.[5] If we can demonstrate that a budget failure results in a reduction in the proposed level of spending at time t, we have strong evidence of policy convergence. In Section VII, we outline a simple decomposition technique to explore this.

We view our study as making several distinct contributions to the recent and active empirical literature on whether elections influence policy choices. First, local school budget elections provide a powerful setting in which to test for policy convergence in response to elections by both municipal officials and elected school board members. In large part, this is because local officials are making a policy choice (i.e., local property taxes and school spending) that is highly visible and especially salient for voters. The majority of districts are extremely small (a median population of 10,275), with 80 percent of districts having a population below the lower bound population (25,000) in Ferreira and Gyourko (2009). Furthermore, these budget elections provide an unusually targeted expression of voter opinion because they focus on a *single* topic (i.e., in contrast to voting for a candidate who effectively bundles positions on multiple policy issues). However, we note that the capacity of our empirical study to inform these theoretical distinctions about politician behavior may be attenuated by the fact that, in this setting, the school finance choices observed in the aftermath of a budget rejection are the result of the decisions made by a *group* of local elected officials rather than a single politician. Therefore, we view our results as providing more general, reduced-form evidence on whether policy choices are responsive to expressions of voter preferences.

A second contribution of our study is that the frequency of school budget elections and the considerable demographic and socioeconomic heterogeneity across school districts make it possible for us to examine how characteristics of the district or election (e.g., community size, socioeconomic variables, and turnout rates) affect the impact of budget rejections on subsequent policy choices. Third, we also view our results as

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[4] In light of recent evidence that elected officials actually prefer higher spending than their constituents (e.g., Agren, Dahlberg, and Mörk, 2007), we view evidence of budget rejection leading to lower property taxes as suggesting policy convergence in the Downsian tradition.

[5] If the school board preferred a lower level of spending, it could just choose this level, as there is no opportunity for voters to vote against a budget for being too low.

contributing new evidence to a long-standing debate (Epple and Zelenitz, 1981; Henderson, 1985) about whether competition in Tiebout-like settings (i.e., across school districts) effectively eliminates the relevance of politics in influencing policy choices. Fourth, we view this study as timely from a policy perspective since it provides direct evidence on how local elections influence both property-tax levels and the resources available to local public schools both immediately and in the long run.

III. SCHOOL BUDGETING AND ELECTIONS IN THE GARDEN STATE

New Jersey has approximately 550 school district governments.[6,7] Elected school boards govern these local, regional, and consolidated districts, and each district can propose its own property-tax levies and bond issuances. However, New Jersey has unusual institutional circumstances in that the *annual* budgets proposed by these school districts are also subject to the approval of local voters. More specifically, in early March of each year, each school district prepares an itemized budget that is quickly followed by a public hearing (New Jersey Department of Education, 2010). The district is responsible for publishing a notice of the public hearing on the proposed budget in a local newspaper. The proposed budget that is voted upon may be revised by the school district in response to the public hearings but must be formally adopted no later than 18 days prior to the election, which takes place on the third Tuesday in April. If a simple majority of voters approves the budget, the county board of taxation is then informed of the property-tax levy that has been effectively certified by the election.

However, if the budget is defeated, the school district must deliver the "Defeated Budget Information" documents to the municipal government in their community (New Jersey Department of Education, 2011a, 2011b).[8] This local governing body (typically, the borough council or township committee and the mayor) must decide whether to reduce the proposed budget and, if so, by how much.[9] Interestingly, if these elected

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[6] "Lists and Structure of Government, 2007 Census of Government," U.S. Census Bureau, Washington, DC, https://www.census.gov/govs/go/historical_data.html.

[7] Around 20 of these Type 2 districts are non-operational (New Jersey Department of Education, 2009) and are excluded from our analysis. New Jersey also has an additional 57 dependent school systems (U.S. Census Bureau, 2011, http://www2.census.gov/govs/cog/all_ind_st_descr.pdf), which are not part of this analysis. These entities include county-level agencies that provide vocational and other special services, "jointure commissions" providing services to disabled students, and community college agencies. However, it should be noted that this category also includes 21 "Type 1" school districts, which have *appointed* school boards and whose budgets are not subject to the election procedures described here.

[8] Of the approximately 530 districts in our sample, around 450 directly overlap with one of New Jersey's 566 municipalities. The remaining regional or consolidated school districts are associated with several local governing bodies. When budgets fail in these districts, the associated local governing bodies either meet together or individually appoint representatives to a joint committee to certify possible budget reductions.

[9] There is a required minimum levy for each district. However, very few districts are effectively constrained with respect to lowering their levies. For example, data from 2009 indicate that only eight districts in 2009 implemented a final budget at the minimum levy amount (personal communication with New Jersey Department of Education).

officials decide to reduce the proposed budget, they must suggest specific spending reductions, increases in other budgeted revenues, or changes to the district's fund balance. However, once a lower budget (and an implied property-tax levy) has been certified for the coming year, school districts can rearrange the budget reductions as they see fit.[10] The budget does not go back to the voters for a second vote.

The key features of this institutional arrangement are that voter-rejected budgets provide elected municipal officials with: (1) clear information about negative, median-voter sentiment on a narrow and visible policy issue, and (2) the opportunity to adjust the relevant policy in response to that expressed sentiment (i.e., to lower the property-tax levy). Ex ante, one might expect that voter rejections of proposed district budgets are rare because school district officials are themselves elected and because the final budget proposed to voters can also reflect changes in the wake of public hearings. However, as we discuss below, budget rejections are actually quite common in our analytical sample (e.g., a third of budgets were rejected over the seven school years from 2002–2003 through 2008–2009). The prevalence of rejected budgets also varies considerably across districts (Figure 1).

IV. DATA

The data for the primary analyses come from two merged sources: the Local Education Agency (School District) Finance Survey (F-33) and annual district-level budget-election results reported by the New Jersey Department of Education. The combined data for the school years from 2002–2003 through 2008–2009 result in over 3,600 district-by-year observations. The F-33 is an annual financial survey of every school district in the United States. The F-33 survey instrument elicits information on several district traits (e.g., student enrollment and grade levels) as well as on revenues, expenditures, and other debt and asset measures. Restricting the F-33 to New Jersey school districts results in a sample of over 600 school districts. After removing vocational and special-service agencies, charter schools, and Type 1 school districts, this number is reduced to approximately 550, the number of school districts that hold school budget elections. Finally, an additional 20 non-operating or zero-enrollment school districts were eliminated, leaving a final analytical sample of approximately 530 districts over six years.

The F-33 separates revenues derived at the federal, state, and local levels and further refines them into numerous sub-categories. Data on local property tax revenues, the most salient feature of the school budget process for district residents, are combined with enrollment information to construct the outcome variable of interest, local property

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[10] If the municipal authorities fail to agree and have not certified tax levies by late May, the state Commissioner of Education can review the proposed budget and certify the tax levy to the county board of taxation. If a district's budgeted amount falls below a "thorough and efficient" per-pupil allocation, a review by the Commissioner is automatic. School boards can also petition the Commissioner to review budget cuts. However, full or partial restorations of reduced budgets by the Commissioner are uncommon; there appear to have been only 15 instances in our sample window (New Jersey School Board Association, 2008).

Figure 1

Proportion of Districts by Rejected Budgets — 2003 to 2009
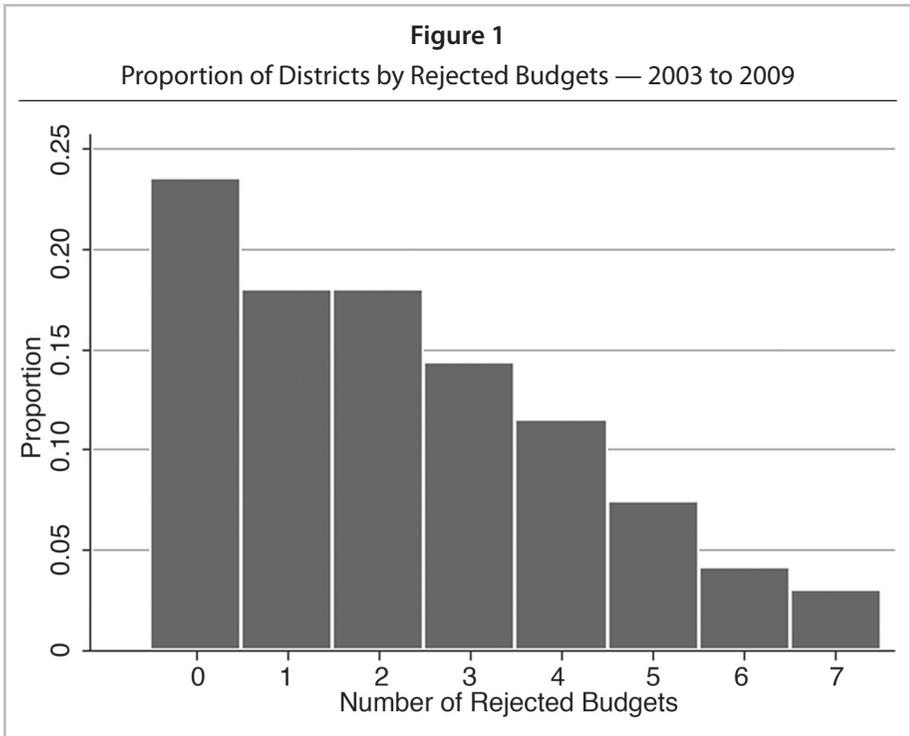
taxes per student. Budget rejections could influence other elements of district finances (e.g., fund balances). However, the focal point for voter rejection of school budgets is local revenue raised through property taxes. This is particularly true of school districts in New Jersey where property taxes are unusually high.[11] We matched these F-33 data with the corresponding district-year results from New Jersey school budget elections. For example, the financial data for the 2002–03 school year are matched with budget vote results from the April 2002 election. The election data consist of district-year observations on the number of votes for and against a budget as well as an indicator of whether or not the budget passed. The share of voters rejecting a proposed budget (PCT_REJECT_{dt}), as well as an indicator for budget failure ($REJECT_{dt}$) are constructed from these data. While we rely on the F-33 and election data for our main analysis, we also draw on data from the National Center on Education Statistics Common Core of Data[12] and the 2000 Census (aggregated at the school-district level) in order to test

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[11] In our sample, the average property tax revenue is $10,764 per pupil. To put this into perspective, property taxes per pupil in the United States averaged around $4,000 during this period.

[12] National Center on Education Statistics Common, "Core of Data," https://nces.ed.gov/ccd/ccddata.asp; and "Build Tables," https://nces.ed.gov/ccd/bat/.

our assumptions of quasi-random assignment and to explore treatment heterogeneity. Data on district demographics, educational attainment, population, income, and poverty status are used for this purpose.

V. ECONOMETRIC SPECIFICATIONS

Our goal is to identify the effect of a budget rejection on property taxes. A naïve analysis would simply compare property taxes in districts where the budget passed to property taxes in districts where the budget failed. However, it may be the case that districts with relatively high taxes consistently have more anti-budget sentiment. The simple approach would suggest that a budget rejection leads to higher property taxes! One candidate design to address these concerns is a difference in differences (DD) strategy that, using panel data on school districts, controls for district and year fixed effects. The implied identification strategy in the DD specifications is to compare the change in the property taxes per student in districts that move from budget approval to rejection relative to the contemporaneous variation in districts that experienced no change in the status of their budgets. We present these results in Table 1.

The DD results in Columns 1 and 2 suggest that budget rejections had no effect on property taxes per pupil. We note another possible explanation for these results that is rooted in a bias that might plague the DD specification. More specifically, DD estimates of the effect of budget rejections on property taxes would be biased downward if the districts experiencing budget rejections had, on average, an unobserved, relative tendency towards submitting larger budgets to voters during the years in which the district's voters decided to reject the budget. In other words, the DD approach would understate the impact of rejections in reducing property taxes if the districts whose budgets were being rejected tended to be those that requested relatively more tax revenue in a particular year (i.e., reverse causality).

Columns 3 and 4 in Table 1 illustrate this by presenting DD results where we allow for heterogeneous effects depending on the share voting against the budget; we represent these effects using dummy variables for intervals of 5 percentage points. The results presented here show that the within-district increases in the share of individuals voting against a budget imply larger rather than smaller property tax burdens. The reference category in these models is $PCT\_REJECT_{dt} < 0.35$. From that reference point, higher values of PCT_REJECT_{dt} imply consistently *higher* values of property taxes per pupil. However, these results also illustrate a downward shift in property taxes associated with budgets that just move across the 50 percent threshold. These results illustrate the weakness of the DD estimator in our setting, while also suggesting the potential value of the regression discontinuity approach.

A regression discontinuity (RD) design is a quasi-experimental method used to ascertain the effect of a treatment. In settings where an observed assignment variable crosses a specific threshold that assigns treatment, one can compare entities on either side of the threshold to ascertain the effect of treatment. In our current setting, when the share rejecting the budget (the assignment variable) crosses 50 percent (the threshold), the budget is rejected. One can estimate the effect of the budget rejection (treatment)

Table 1

Difference in Differences versus Regression Discontinuity Estimates

Variable	Difference in Difference				Regression Discontinuity
	(1)	(2)	(3)	(4)	(5)
PCT_REJECT > 0.5	34.26	24.08			−170.8***
	(46.77)	(45.99)			(57.9)
PCT_REJECT 0.35 to 0.40			166.5*	162.7*	
			(99.4)	(98.2)	
PCT_REJECT 0.40 to 0.45			273.6**	263.7**	
			(116.0)	(114.4)	
PCT_REJECT 0.45 to 0.50			313.2***	294.2**	
			(118.7)	(116.4)	
PCT_REJECT 0.50 to 0.55			219.3*	204.3*	
			(124.7)	(122.5)	
PCT_REJECT 0.55 to 0.60			335.4***	319.4**	
			(127.7)	(124.1)	
PCT_REJECT 0.60 to 0.65			363.2**	335.5**	
			(161.3)	(158.0)	
PCT_REJECT > 0.65			480.3**	407.8**	
			(207.3)	(204.1)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes
District-year covariates	No	Yes	No	Yes	Yes
Assignment variable	No	No	No	No	Yes

Note: These results are based on 3,669 district-by-year observations. The dependent mean is $10,764. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

by comparing entities (e.g., districts) in which the measure just passed or just failed. Our baseline regression discontinuity specification takes the following general form

$$(1) \quad Y_{dt} = \gamma(REJECT_{dt}) + f(PCT_{REJECT_{dt}}) + \beta X_{dt} + \alpha_d + \lambda_t + \varepsilon_{dt},$$

where Y_{dt} is the per-pupil property-tax revenue for district d in year t, $REJECT_{dt}$ is a binary indicator for whether the budget proposed for that year has been rejected, $f(PCT\_REJECT_{dt})$ is a smooth function of the share of voters who rejected that year's budget, and X_{dt} refers to observed traits varying within districts over time. The terms α_d

and λ_t are fixed effects unique to school districts and years, respectively. The parameter of interest, γ, identifies the shift in real local property taxes per pupil as a proposed district budget moves from having just been approved to being just rejected. The key identifying assumption is that the movement around the 50 percent threshold is conditionally random so that γ can be interpreted as the causal effect (at least locally) of voter rejection on the realized property taxes per student in closely contested elections.

Our preferred specification conditions on district fixed effects, which control for constant differences in preferences for spending across districts, and year fixed effects, which control for variation over time that is shared across all districts. The combination of an RD specification and a two-way fixed-effects model has an appealing and intuitive interpretation. This approach (i.e., a combined RD and DD) effectively involves comparing the change in property taxes per student *within* districts that move across the $REJECT_{dt}$ boundary relative to the contemporaneous variation within districts that do not. During our study window, nearly 24 percent of districts never rejected a budget while roughly 3 percent rejected all seven annual budgets (Figure 1). This implies that over 70 percent of the districts exhibit within-district variation in $REJECT_{dt}$ during our study window with most districts rejecting one-to-three budgets during this short period (Figure 1).[13]

VI. BASELINE RESULTS

We begin by providing graphical RD evidence of how median voter rejection of a budget influences real property taxes per pupil. We then complement this graphical evidence with regression analysis and several robustness checks that examine the internal validity of the RD results. We also examine how the impact of median voter sentiment varies with respect to district traits and turnout. Finally, we present evidence on the longer-term effects of narrowly rejected budgets and use a simple decomposition to infer the effect of a budget rejection at time $t - 1$ on proposed budget levels at time t.
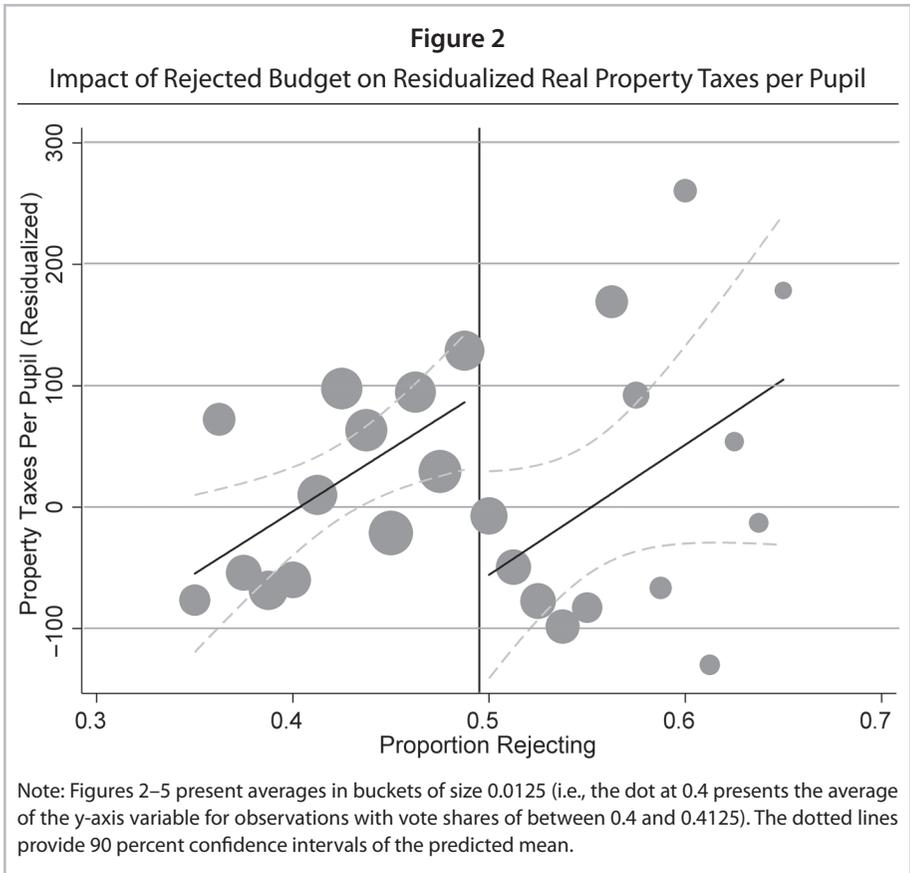
A. Graphical Evidence

Figure 2 illustrates the RD impact of rejected budgets graphically. To construct this figure in a manner consistent with the panel-based nature of our regression analysis, we regressed real property taxes per pupil on district and year fixed effects and recovered the residuals from these regressions.[14] We then averaged these residuals across bins of fixed width to the right and left of the 50 percent threshold.

Figure 2 presents the average values of the residualized property taxes per student for the relatively local observations where PCT_REJECT_{dt} is greater than 35 percent

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[13] Because we have only seven years of within-district variation, models that allow the assignment variable (vote share against the budget) to have different slopes above and below the threshold lead to a substantial loss of precision (i.e., increasing the standard error on the treatment effect by a factor of 10). Fortunately, specifications both with and without district fixed effects consistently reject the hypothesis that the slope of the assignment variable differs above and below the threshold (see also Figure 2).

[14] Figures based on the raw dependent variable are qualitatively similar but do not illustrate how the vote share against the budget varies with the outcome variable within districts over time.

Figure 2

Impact of Rejected Budget on Residualized Real Property Taxes per Pupil

Note: Figures 2–5 present averages in buckets of size 0.0125 (i.e., the dot at 0.4 presents the average of the y-axis variable for observations with vote shares of between 0.4 and 0.4125). The dotted lines provide 90 percent confidence intervals of the predicted mean.

and less than or equal to 65 percent. This graph suggests that the subsequent levels of real property taxes per pupil are increasing in the percent of voters who rejected the budget.[15] Interestingly, this graph also suggests that the vote share against a budget has an approximately linear effect on the local property taxes per pupil and that this linear slope is similar to the left and the right of the 50 percent threshold. This graph also suggests that median voter rejection of a budget implies a reduction in subsequent property-tax revenue. Specifically, at the 50 percent threshold for budget reject, real property taxes per pupil appear to fall by roughly $125.

B. Baseline Regression Results

Table 2 presents the key results from regressions that estimate the impact of $REJECT_{dt}$ on real property taxes per pupil. The specification in Column 1, which conditions on year fixed effects and the vote share against the budget, is consistent with the graphical

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[15] This result is consistent with the concerns raised in the discussion of the DD results in Table 1 (i.e., as districts move towards higher spending, voters move towards rejecting budgets).

Table 2

Regression Discontinuity Estimates of Impact of Rejected
Budget on Real Property Taxes per Pupil

Variable	(1)	(2)	(3)	(4)	(5)
REJECT	−118.5	−180.6***	−170.8***	−152.9**	−175.9***
	(365.7)	(58.3)	(57.9)	(62.3)	(63.9)
PCT_REJECT	−6,517**	1,660***	1,517***	3,565*	1,091
	(2,623)	(457)	(442)	(2,062)	(4,630)
PCT_REJECT²				−2,201	3,280
				(2,251)	(9,730)
PCT_REJECT³					−3,679
					(6,632)
R^2	0.040	0.967	0.967	0.967	0.967
Year fixed effects	Yes	Yes	Yes	Yes	Yes
District fixed effects	No	Yes	Yes	Yes	Yes
District-year covariates	No	No	Yes	Yes	Yes

Note: These results are based on 3,669 district-by-year observations. All property taxes are inflated to $(2009) using the BLS series CUUR0000SA0. The dependent mean is $10,764. The standard errors are adjusted for heteroscedasticity clustered at the district level. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

evidence in suggesting that budget rejection implies that subsequent property taxes are lower. However, this point estimate is highly imprecise. The remaining four specifications in Table 2 condition on district fixed effects, which soak up persistent variation across districts in preferences for spending.[16,17]

RD estimates that condition on both district and year fixed effects consistently imply that budget rejections in close contests led to statistically significant reductions in property taxes of $150 to $180 per pupil. These point estimates are robust across specifications that introduce the available controls that vary within districts over time (e.g., percent minority in the school district and the turnout rate in the budget election). They are also robust in specifications that control for quadratic and cubic terms of the vote share against the budget.[18]

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[16] Estimates using lagged spending per pupil are very similar to those using district fixed effects.

[17] The introduction of these fixed effects increases the R^2 substantially (from 0.036 to 9.68) and similarly improves the precision of the RD estimates (reducing the standard errors by over 80 percent).

[18] The point estimates on these polynomial terms confirm the intuition from Figure 2 suggesting that the within-district variation in property taxes is linear in the vote share against the budget.

Initially, the impact of a budget rejection appears to be relatively modest; it is equivalent to no more than 1.7 percent of the corresponding property tax burden.[19] However, it is important to remember that this impact is the result of a budget vote in which just over half of the population disapproved of the budget; this is convincing evidence that local politicians do respond to the median voter.[20] However, there is a concern that the response observed among municipal officials instead indicates a preference for lower spending. Although we argue that the available evidence suggests that this is not the case (e.g., Agren, Dahlberg, and Mörk, 2007), we later present evidence that *school board officials* also appear to be affected by a narrowly rejected budget tending to propose lower budgets in the subsequent year. First, we explore the robustness and heterogeneity of the RD results.

C. Potential Concerns and Robustness Checks

A concern frequently addressed in RD applications involves the potential manipulation of the assignment variable. For example, the concern in this context is that, in communities with outcome-relevant but unobserved traits, voters are able to mobilize during very close elections to influence the outcome. For example, if a league of pro-spending parents were able to manipulate the percentage voting for a budget in close contests, we might then observe reduced spending among failed budgets, not necessarily because of the failure, but because the assumption of effectively random assignment to either side of the cut point had been violated. Lee and Lemieux (2010) are careful to emphasize that a capacity to influence the assignment variable is not necessarily problematic for an RD design as long as individuals cannot precisely manipulate the variation *across* the threshold.

The identifying assumption that vote shares cannot be manipulated across the margin seems quite reasonable in these settings.[21] Figure A1 illustrates this result visually by presenting the histogram values and the estimated density associated with the assignment variable (vote share against the budget) on either side of the 50 percent rejection threshold. This density suggests that the distribution of vote shares for rejecting school budgets is smooth around the threshold.

Another potential concern is that budget votes far away from the rejection threshold are very different from those that are tightly contested. If this is the case, including observations with vote shares far from 50 percent may bias our estimate of the effect

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[19] However, it is important to note that if this tax reduction continues in perpetuity, the present discounted value of this reduction is substantially larger (e.g., roughly $3,600 at a 5 percent discount rate).

[20] It is likely that more emphatically rejected budgets will face more substantial reductions. Thus, we interpret our $180 per pupil estimate as a probable lower bound of the one-period impact of a budget rejection.

[21] The density test developed by McCrary (2008) provides an empirical approach to addressing the practical relevance of this concern. The density test proposed by McCrary indicates that the null hypothesis that the share of voters voting to reject their proposed school budgets is continuous at the 5 percent threshold cannot be rejected. Further details are in the online Appendix (http://people.tamu.edu/~abarr/ntj_schoolbudget_appendix.pdf).

of a budget rejection. A method to combat this concern involves narrowing the focus of this regression to smaller windows around the cut point (Hahn, Todd, and van der Klaauw, 2001; Lee and Lemieux, 2010).[22] In our study, this involves focusing on closer elections (i.e., excluding the data from elections that are not close).

In Table A1 in the online appendix, we present both full sample results (Column 2 from Table 2) and results that only utilize results from increasingly narrow bandwidths around the 50 percent threshold.[23] The results in Table A1 demonstrate that the construction of more narrow bandwidths implies a loss of precision as the sample size falls. However, the basic results in Table 2 are nonetheless replicated across these specifications. Even using the relatively narrow bandwidth of 10 percentage points indicates that budget reduction led to statistically significant reductions in real property taxes per pupil.[24]

In appendix Table A3, we present another complementary approach to examining the robustness of our RD results by estimating the effects of irrelevant placebo thresholds on property taxes per pupil. Each cell in Table A3 presents the RD estimate from a separate regression that identifies whether the property taxes per pupil shifts significantly at different thresholds of the vote share against the budget. If we were to find that property taxes per pupil shifted significantly at thresholds of the vote share against the budget that did *not* imply a change in the status of the budget (e.g., 35 percent, 45 percent, 55 percent, and 60 percent), it would suggest the existence of an undiagnosed specification error. The results in Table A3 are generally consistent with the assumption that the RD strategy is valid.[25]

D. Treatment Heterogeneity

The substantial heterogeneity across school districts and the frequency of these district-year budget elections makes it possible to explore heterogeneity in treatment effects. We present our full-sample results and results defined for sub-groups in

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[22] This involves limiting the sample to observations where the share voting against the budget is within a certain distance of 50 percent. This distance is referred to as the bandwidth.

[23] We also implemented a procedure developed by Imbens and Kalyanaraman (IK), (2012) to identify the optimal bandwidth in an RD design (i.e., one which minimizes the mean squared error of the shift parameter of interest). The IK procedure implies a much larger, negative effect of *REJECT* (and a relatively tight bandwidth of 0.06). However, this effect is highly imprecise because the IK procedure follows the standard practice of allowing the assignment variable to have different slopes on either side of the threshold, which leads to a substantial loss of precision as noted above.

[24] In appendix Table A2, we present another standard test of the validity of the RD. We evaluate the impact of budget rejection in RD specifications where district-year observables (i.e., percent of district students who are minorities, the natural log of district enrollments, and the turnout rate) are the dependent variables. If vote share has not been manipulated, there should not be a discontinuous change in the characteristics of districts at the 50 percent threshold. The results indicate that there are no statistically significant shifts in these observables at the 50 percent threshold for both the full sample and different bandwidths.

[25] In specifications that exclude polynomials of the vote share against the budget (Columns 1 and 2), we do see evidence of a weakly significant, positive shift in property taxes at the 30 percent threshold. However, apart from that, there are not consistent or statistically significant changes in the outcome variable except at the 50 percent threshold that actually implied a change in whether the budget was rejected.

Table 3. Specifically, we divided our sample by a variety of baseline traits such as whether baseline district enrollment (i.e., in the first year of our study window) was above or below the median. Smaller districts in this construction have a total baseline enrollment of no more than roughly 1,230 students. We also used district-level data drawn from the 2000 Census (i.e., prior to our study window) to identify districts that were above or below the median for various traits such as the percent of minorities in the population, the percent elderly, the percent of adults with a high school degree, and median family income. We also identified whether the budget election for each district-year observation had a turnout rate above or below the median.

The results across these different districts suggest a number of interesting patterns. In particular, the RD estimates imply that a change in median voter sentiment leads to particularly large reductions in property taxes in smaller school districts, in communities with higher levels of educational attainment, and in the wake of high-turnout elections. In contrast, the hypothesis that budget rejections have no effects cannot be rejected for larger school districts, for districts with higher concentrations of high school dropouts, and for lower-turnout elections. This striking heterogeneity is consistent with the claim that elections are more effective in shaping the policy choices made by elected officials in settings that support public awareness and monitoring. This suggests that more attention should be directed towards larger districts with lower average education levels and lower levels of civic participation. Individuals in these disadvantaged districts are less likely to receive the level of public schooling that they desire and less likely to be able to move to a district that provides what they want. Furthermore, these results suggest that recent efforts to combine districts and/or centralize school finance will limit individuals' abilities to obtain the level of services they desire.[26]

VII. DYNAMIC TREATMENT EFFECTS

Our baseline results have focused on how rejection of the proposed budget for a given academic year influenced the subsequent property tax burden for that year. However, an interesting and policy-relevant question involves whether these effects persist into later years. One channel relevant for these longer-term effects involves the budgets proposed by school districts. For example, a budget rejection in year $t-1$ could reduce property taxes in year t if school districts subsequently propose a more modest budget. Conversely, school districts may strategically seek to make up for revenue lost during a previous budget cycle by proposing somewhat larger budgets in the year after a voter rejection. We view this as an additional test of policy convergence among a different set of elected officials.

A second channel involves how a budget rejection in period $t-1$ influences the likelihood of rejection in period t. For example, voter turnout, which is generally low in these elections, may increase in the wake of a budget rejection, particularly among supporters

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[26] Whether or not the preferences of the median voter align with the socially optimal level of investment in public schooling is beyond the scope of this paper.

Table 3

Regression Discontinuity Estimates of Impact of Failed Budget
on Real Property Taxes per Pupil, by District Traits

Sample by Baseline Trait	Dependent Mean	(1)	(2)	(3)	N
Full	10,764	180.6***	170.8***	175.9***	3,669
	(4,754)	(58.3)	(57.9)	(63.9)	
District enrollment < median	11,863	263.1***	−246.6**	308.7***	1,829
	(5,304)	(97.1)	(97.73)	(112.0)	
District enrollment ≥ median	9,672	−85.84	−73.64	−54.90	1,840
	(3,838)	(56.69)	(56.90)	(61.25)	
Turnout rate < median	9,591	−42.01	−35.65	−9.259	1,829
	(5,086)	(72.12)	(72.28)	(87.73)	
Turnout rate ≥ median	11,931	−238.5**	233.6***	280.2***	1,840
	(4,079)	(92.6)	(89.9)	(100.6)	
% Minority < median	12,137	224.8***	−214.7**	−80.66	1,831
	(4,569)	(86.6)	(87.7)	(91.06)	
% Minority ≥ median	9,396	−114.3	−107.8	−182.3**	1,838
	(4,538)	(79.7)	(77.4)	(82.5)	
% Elderly < median	9,791	−146.0**	−116.1*	−169.6**	1,828
	(4,272)	(66.6)	(65.8)	(80.6)	
% Elderly ≥ median	11,731	−230.6**	−216.7**	−207.1**	1,841
	(5,006)	(95.5)	(94.7)	(101.4)	
Med. family income < median	8,849	−126.0	−109.5	−186.7*	1,829
	(4,362)	(91.6)	(90.1)	(96.5)	
Med. family income ≥ median	12,668	251.3***	244.4***	−134.7	1,840
	(4,347)	(72.9)	(71.7)	(83.2)	
% HS grad < median	8,712	−103.9	−95.36	−97.81	1,829
	(4,159)	(87.4)	(85.59)	(84.69)	
% HS grad ≥ median	12,804	268.4***	282.8***	242.3***	1,840
	(4,420)	(76.8)	(73.3)	(88.4)	

Table 3 (Continued) Regression Discontinuity Estimates of Impact of Failed Budget on Real Property Taxes per Pupil, by District Traits

Sample by Baseline Trait	Dependent Mean	(1)	(2)	(3)	N
% Renters < median	11,628	−157.0**	−153.5**	−129.8	1,831
	(4,597)	(70.0)	(68.7)	(84.0)	
% Renters ≥ median	9,904	−195.5**	−182.6*	−206.8**	1,838
	(4,753)	(92.7)	(93.8)	(88.8)	
Year fixed effects		Yes	Yes	Yes	
District fixed effects		Yes	Yes	Yes	
District-year covariates		No	Yes	Yes	
Polynomials of *PCT_REJECT*		No	No	Yes	

Note: *PCT_REJECT* is included in all specifications. The standard errors are adjusted for heteroscedasticity clustered at the district level. Regressions conditioning on additional covariates have six fewer observations due to a data error associated with a single district in the 2000 Census. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

of school spending. Furthermore, a budget rejection by those who voted in period $t − 1$ may lead to a change in how they cast their votes in period t, stemming from changes in either the proposed budget or preferences (e.g., regret over a previous rejection).

While we do not directly observe either the budgets proposed by districts or the votes cast by individuals over time, we can utilize the data that are available — along with our RD design — to assess how voters and school board officials respond in the year following a budget rejection. This then provides evidence on how school board officials, who set the initial budget in year $t − 1$, are affected by voter preferences. Figure 3 demonstrates graphically how voter rejection at time $t − 1$ affects the property taxes levied at time t. While somewhat noisy, the figures indicate at most a small reduction in property taxes. In the first row of Table 4, we present the corresponding RD estimate of how a budget rejection in period $t − 1$ influences the real property taxes per pupil in period t. Overall, this result supports the graphical evidence that budget rejection during the previous cycle implies slightly lower property taxes in the current year (a reduction of $104 per pupil). However, this smaller effect is not statistically significant and is also consistent with the hypothesis that districts experiencing budget rejections and the implied loss of property tax revenue are able to recover some of the lost revenue in the next year.

As noted above, the absence of a sustained reduction in property taxes could simultaneously reflect two broad channels: (1) school districts experiencing rejections at
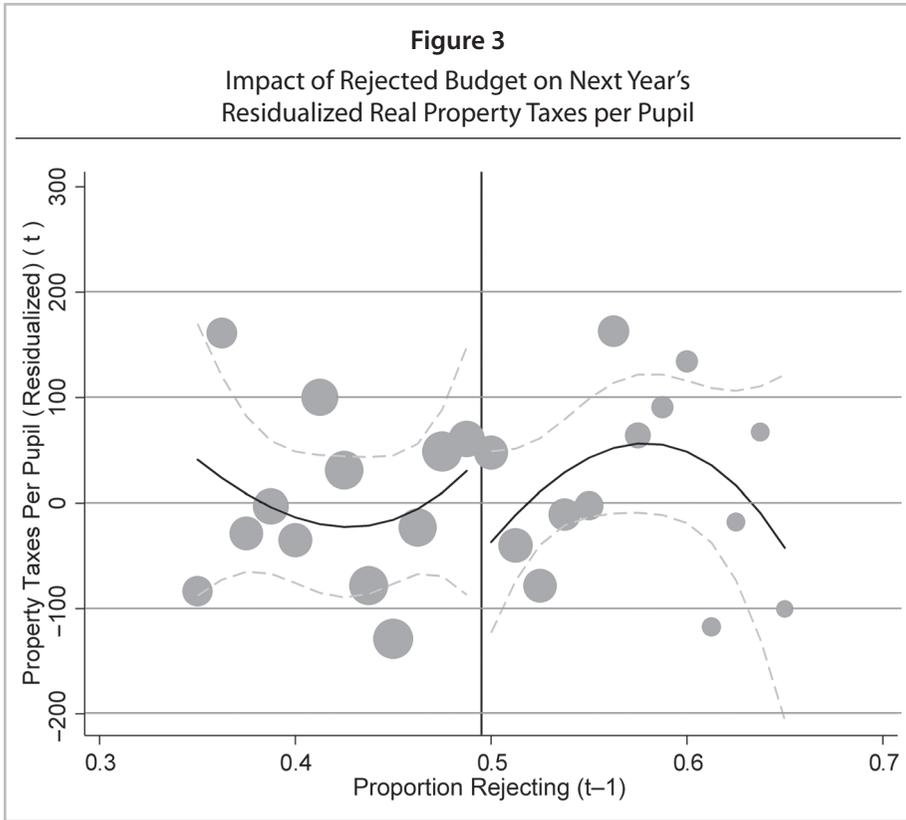
Figure 3

Impact of Rejected Budget on Next Year's
Residualized Real Property Taxes per Pupil

Table 4

Regression Discontinuity Estimates of Dynamic Impacts of Failed Budget

Dependent Variable	$REJECT_{t-1}$	PCT_REJECT_{t-1}	N
Real property taxes per pupil	−104.0	1,383**	3,143
	(66.3)	(592)	
REJECT	−0.258***	1.114***	3,147
	(0.028)	(0.152)	
PCT_REJECT	−0.0328***	0.185***	3,141
	(0.0052)	(0.033)	
TURNOUT RATE	0.00938***	0.0255*	3,147
	(0.00212)	(0.0147)	

Note: The standard errors are adjusted for heteroscedasticity clustered at the district level. Regressions conditioning on additional covariates have six fewer observations due to a data error associated with a single district in the 2000 Census. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

$t-1$ proposing larger budget increases at time t in order to move their budgets back in line with those that did not experience a rejection at time $t-1$, and (2) increased voter support for higher spending (i.e., through increased turnout and/or changes in voter opinions). A simple decomposition illustrates how we can use the observed data and RD estimates to assess the empirical relevance of these different channels. Specifically, a district's property tax per pupil in period t, Y_t, can be written as

$$(2) \quad Y_t = Y_t^P + \gamma \times P(REJECT_t),$$

where Y^P represents the per-pupil property tax *proposed* by the district, γ is the RD-estimated reduction in Y_t implied by voter rejection (see (1)), and $P(REJECT_t)$ is the probability of such a rejection. Using (2), it is straightforward to show that the effect of $REJECT_{t-1}$ on Y_t (i.e., the reduced-form effect in the first row of Table 5) also equals

$$(3) \quad E(Y_t^P \mid REJECT_{t-1} = 1) - E(Y_t^P \mid REJECT_{t-1} = 0) +$$
$$\gamma \times [E(P(REJECT_t \mid REJECT_{t-1} = 1)) - E(P(REJECT_t \mid REJECT_{t-1} = 0))].$$

In other words, the effect of a budget rejection in the prior year on current year property taxes reflects both the change in the budgets proposed by districts (i.e., the first bracketed term in (3)) and the change in the budget-rejection probability implied by budget rejection in the previous year (i.e., the second bracketed term in (3) multiplied by the amount of tax reduction conditional on rejection, that is, γ).[27]

The interpretation of the decomposition in (3) is straightforward. Consider the case implied by our full sample results. Voter rejection of a budget implies a modest reduction in *current* property taxes (i.e., $\gamma \approx -180$). However, we cannot reject the hypothesis that there is no detectable effect on property taxes just one year later (i.e., (3) equals zero). The decomposition in (3) tells us that the absence of an overall tax effect one year after a rejection could reflect some mix of a change in what school districts propose and a change in the likelihood of a second rejection. We cannot estimate the first bracketed term directly because we do not observe the budgets proposed by districts. However, we can estimate the second bracketed term: the effect of a budget rejection on the likelihood of a rejection in the next year.

As illustrated in Figure 4, rejection at $t-1$ makes it dramatically less likely that a budget will be rejected at time t. This could be the result of higher turnout or changing preferences or a response to school boards proposing smaller budgets in the wake of a rejection. The second row of Table 5 presents a regression-based estimate of the RD results in Figure 4. This estimate indicates that voter rejection in period $t-1$ reduces the probability of voter rejection in period t by a statistically significant 25.8 percentage points.

To summarize, we simultaneously observe (1) zero (or possibly negative) effects of budget rejection in period $t-1$ on property taxes in period t, and (2) dramatic *reductions* in period-t budget rejection rates as a result of a rejection in period $t-1$. This

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[27] It should be noted that we assume that $\tilde{a}$ is invariant to whether a rejection occurred in the previous year. We find that this assumption is not rejected by the data.

Table 5

Dynamic Impact of Failed Budget on Real Property Taxes per Pupil

| Sample by Baseline Trait	$REJECT_{t-1}$	PCT_REJECT_{t-1}	N
Full	−104.0	1,383**	3,143
	(66.3)	(592)	
District enrollment < median	−244.6**	2,359**	1,567
	(116.6)	(941)	
District enrollment ≥ median	37.23	193.7	1,576
	(50.44)	(364.5)	
Turnout rate < median	99.31	812.1	1,532
	(87.15)	(609.9)	
Turnout rate ≥ median	−178.2**	2,060***	1,611
	(83.2)	(794)	
% Minority < median	−176.2*	1,300	1,568
	(98.0)	(999)	
% Minority ≥ median	−29.90	1,465**	1,575
	(93.71)	(705)	
% Elderly < median	−74.52	1,385*	1,565
	(89.78)	(708)	
% Elderly ≥ median	−139.1	1,384	1,578
	(98.1)	(951)	
Med. family income < median	−41.67	869.4	1,567
	(81.34)	(746.8)	
Med. family income ≥ median	−185.2*	2,202**	1,576
	(108.8)	(960)	
% HS grad < median	12.68	432.7	1,567
	(74.59)	(742.9)	
% HS grad ≥ median	−245.2**	2,793***	1,576
	(112.8)	(977)	
% Renters < median	−64.55	682.4	1,568
	(70.07)	(522.0)	
% Renters ≥ median	−140.1	1,959*	1,575
	(111.0)	(1,013)	

Note: *PCT_REJECT* is included in all specifications. The standard errors are adjusted for heteroscedasticity clustered at the district level. Regressions conditioning on additional covariates have six fewer observations due to a data error associated with a single district in the 2000 Census. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Figure 4

Impact of Rejected Budget on Share Voting Against Budget Next Year
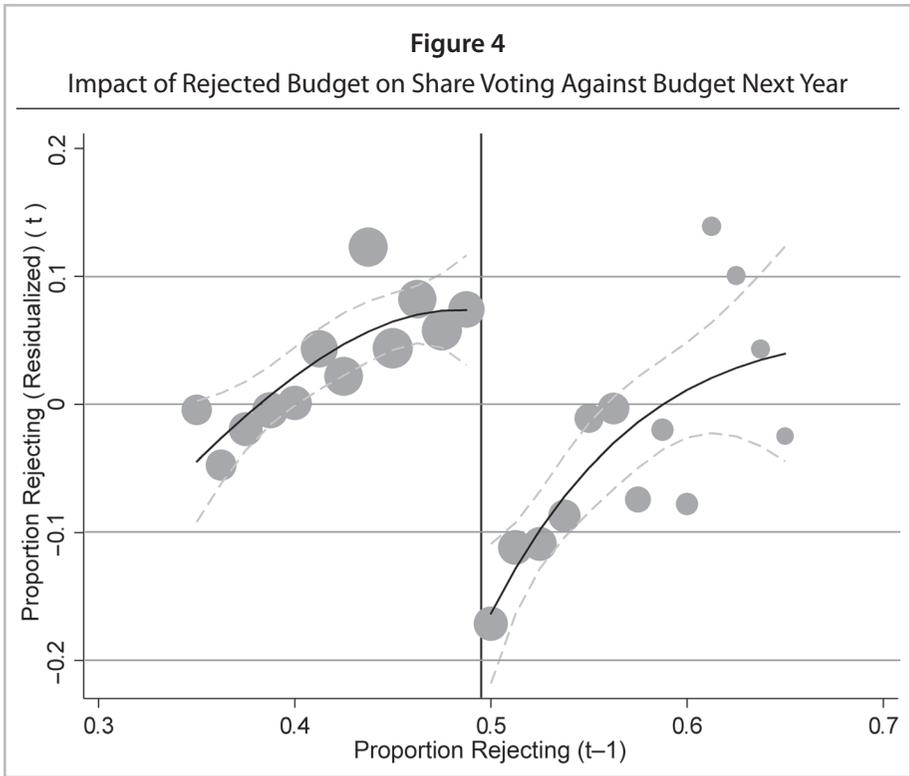
clear reduction in the probability of rejecting a budget in period t implies that per-pupil property taxes in period t increase by $46 in expectation (i.e., 180×0.258) through this channel. However, the only way for this positive effect to have occurred while the overall effect is nil (i.e., (3) equals zero) is for proposed budgets to have fallen by a corresponding amount.

Stated differently, the fundamental lesson from this decomposition exercise is that the only way we could observe zero (or even negative) effects on property taxes one year after a rejection while simultaneously observing such a large reduction in the chance voters will reject a budget is if districts propose more modest budgets.[28] Thus, school board officials are responsive to the median voter. This result turns on the sharp reduction in the likelihood that voters reject a budget when they just rejected a budget in the previous period. However, it does not provide evidence on the source of this sharp reversal in voter sentiment. The remaining results in Table 5 address this issue by presenting evidence on how voter rejection in period $t-1$ influences voter turnout in period t and the percent of voters choosing rejection in period t (i.e., PCT_REJECT_t). Interestingly, these results indicate that budget rejection in period $t-1$ leads to a statisti-

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[28] This reduction in proposed budgets appears to be even larger in smaller districts, with an implied reduction of several hundred dollars.

Figure 5

Impact of Rejected Budget on Next Year's Voter Turnout
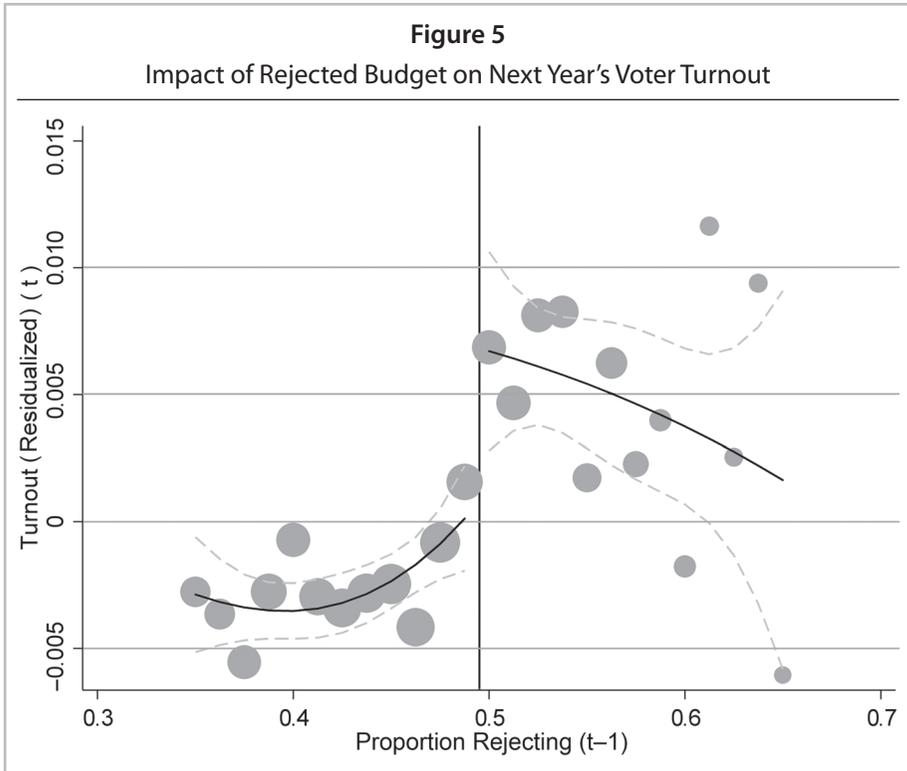
cally significant increase in voter turnout in period t and a decrease in PCT_REJECT_t. Figure 5 depicts this result graphically, with sharply higher turnout rates in districts that experienced budget rejections during the prior year.

A story consistent with these observations is that school boards are responding to budget rejections during a prior year by proposing more modest spending increases in the subsequent year. The resulting swing in pro-budget sentiment occurs to such an extent that, even though school districts propose more modest budgets, the likelihood of voter rejection is reduced enough that property taxes recover in expectation. Results based on subsamples of districts suggest patterns similar to those seen in Table 3. Smaller districts and higher turnout elections result in larger implied reductions in the budgets proposed by school boards in the year following a budget rejection (Table 5). Thus, it appears that both school board and local government officials are more responsive in districts and at times when monitoring of public figures is more likely to be occurring.

VIII. DISCUSSION

Our results indicate that voter rejection of a district budget in favor of lower spending leads to an immediate, though short-lived, reduction in property tax burdens. This

short-term effect occurs even with small shifts in voter sentiment around the 50 percent threshold. A decomposition combined with our RD evidence implies that local officials respond to budget rejections not only by reducing property taxes but also by reducing proposed budgets in the subsequent year. We argue that this latter piece of evidence strongly suggests that local politicians are immediately responsive to the median voter. This finding is consistent with the evidence from other settings indicating that electoral competition influences policy choices. For example, the RD study of large-city mayors by Ferreira and Gyourko (2009) also finds evidence consistent with the influence of elections on policy choices in that the close election of a Democrat is not associated with changes in the size of government. However, when there are few competing jurisdictions nearby (i.e., less Tiebout competition), electing a Democrat increases the size of government by 7 to 9 percent (Ferreira and Gyourko, 2009).

Our study similarly suggests the importance of such contextual factors, even in these highly localized school-district settings. In particular, we find that a budget rejection only has larger and sustained effects on policy outcomes in circumstances where voters are more likely to monitor policy choices or express an intensity of preferences (e.g., smaller districts and higher-turnout elections). These results imply that the question of whether voter sentiment affects policies is highly context-dependent. That is, the role of elections in shaping the policy choices made by elected officials can depend on how proximate the potential discipline provided by the ballot box actually is. This characterization is also consistent with the observation by Albouy (2011) that U.S. Senators choose to vote more moderately in the two years just prior to their re-election.

These results have implications for policy issues related both to local tax and spending decisions and to institutional design. Budget elections are intended to give district residents a role in the determination of school district finances. However, the tax and spending decisions by local school districts are, on average, unresponsive to budget rejections at the ballot box. In low-turnout budget elections, the fiscal effects of a rejected budget are undone by increased voter turnout and pro-spending sentiment within just one year. This pattern is consistent with the view that residential sorting largely renders the political economy of Tiebout settings irrelevant. However, the heterogeneity in our findings qualifies these policy implications. For example, the durability of policy changes in response to higher-turnout elections suggests that budget elections sometimes transmit intensely held voter preferences into enduring policy change. In other words, elections in certain types of districts have some relevance in terms of aligning local tax and spending decisions with community preferences. In combination, these results imply that these local budget elections provide an important check on local tax decisions in *certain communities*. This heterogeneity implies that policy efforts to inform voters and promote turnout can enhance this electoral function and that future research on elections and policy choices should be attentive to such contextual factors.

DISCLOSURE

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