Direct Democracy and Administrative Disruption*

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Abstract

Direct democracy is often touted as a means of reining in the administrative state, but it could also hinder the performance of public organizations. In particular, we argue that bargaining dynamics between voters and government officials can lead to costly administrative disruptions. We explore this issue by estimating the impact of Ohio tax referenda on school district administration using a regression discontinuity approach. The results suggest that administrators in districts where referenda failed sought to insulate core functions from revenue declines. Nonetheless, referendum failure (as opposed to passage) led to lower instructional spending, teacher attrition, and lower student achievement growth. Spending and performance generally rebounded within a few years, however, as districts eventually secured approval for a subsequent tax proposal. These results illustrate how involving citizens in decision-making can entail short-term transaction costs in the form of decreased administrative performance, which in this case may have had lasting achievement effects for students attending school in the wake of a referendum failure.

Keywords: direct democracy, agency performance, external shocks, cutback management

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1. Introduction

U.S. Progressive reformers sought to root out political corruption and enhance the efficiency of the administrative state (e.g., see Wilson 1887). One of their primary strategies for accomplishing the former was to establish institutions of direct democracy[1], particularly at local levels where most government services are delivered. By voting directly on policy matters, the logic went, citizens could counter organized interests that capture public organizations (Goebel 2002). Although there are some concerns that direct democracy enables interest groups to set the agenda and manipulate voters[2], empirical research generally confirms that involving citizens directly in decision-making improves the alignment of policy with public preferences (e.g., Gerber 1996; Matsusaka 2010).[3] With regard to reformers' goal of promoting administrative efficiency, there is also some evidence that direct democracy generally lowers the cost of public services.[4] However, despite the growing prevalence of direct democracy[5], there is little

[1] The term refers to two institutions in particular—the direct initiative and the referendum—that together give voters direct control over policymaking. The direct initiative and the referendum share important similarities—both give voters the final say over whether a proposed policy is adopted—but these institutions differ in one important respect: access to the political agenda. With the initiative, an individual constituent can herself theoretically propose any policy for consideration to fellow voters, with few constraints. With the referendum, by contrast, voters are limited to saying only "yes" or "no" to a policy that has been proposed by elected officials.

[2] The high cost of qualifying ballot initiatives, which almost certainly requires paid signature collectors, means that access to the ballot is generally limited to the wealthiest or most organized interest groups (Gerber, 1999). There is also evidence that spending on campaign ads can influence voter behavior (e.g., see de Figueiredo, Chang, and Kousser. 2011; Rogers and Middleton, 2015).

[3] There are some concerns that direct democracy may come at some cost to minority rights, but the magnitude of these costs has been disputed in the empirical literature (see Gamble 1997; Hajnal et al., 2002).

[4] Matsusaka (2004; 2009) reports that the initiative process reduces spending at the state level and provides a check on the compensation of public employees in city government. Shifting focus from the initiative to the referendum, Feld and Matsusaka (2003) similarly find lower spending among Swiss cantons that are required to put their budgets before voters. Hinnerich and Pettersson-Lindbom (2014) also find that access to direct democracy reduces government spending, at least in certain categories of expenditures.

[5] Eighty percent of the largest 1,000 U.S. cities now allow citizen initiatives, and the institution is spreading quickly across the world (Matsusaka 2009).

systematic research on its implications for the performance of public organizations (Matsusaka 2009). We argue that the introduction of a veto player via direct democracy could have a negative impact on the performance of public organizations. Indeed, we argue that any democratic process that gives citizens a veto over public decision-making could have such an effect.

Direct democracy may disrupt the administration of public programs in part because it forces public officials to engage in an uncertain and extended bargaining process with voters. To test this possibility, we consider the case of U.S. local school districts that must gain voter approval to levy taxes to generate revenue. Raising funds in this manner amounts to a repeated bargaining game between district leaders and voters—one that features school district agenda-setting power, voter uncertainty over the tax revenues necessary to realize their preferred educational outcomes, and school district uncertainty over the taxes voters will approve (Romer and Rosenthal 1979; Figlio and O'Sullivan 2001; Barseghyan and Coate 2014). Consequently, voters might agree to tax rates that are excessive relative to the services they desire, or they might reject proposals that would have generated the revenues necessary to support student learning, leading to unwanted declines in service quality. Or perhaps referendum failure occurs because districts misjudge voters' willingness to pay for public services. Whatever the reason, an initial failure might require districts to make budget cuts and return to the ballot until they gain voter approval. Although this process might deliver certain benefits—such as forcing school districts and residents to enlighten one another about the implications of changing tax rates (Smith and Tolbert, 2004) and perhaps enhance the legitimacy of public school districts (e.g., see Fung 2006)—these bargaining dynamics also could disrupt school district administration.

We estimate the administrative impact of voter-district bargaining over tax rates by analyzing more than 4,200 tax referenda proposed by Ohio school districts between 2003 and 2013. Specifically, we examine the impact of referendum failure, relative to passage, on district revenues and spending, district budget allocations and staffing decisions, and the probability of districts proposing and voters approving a subsequent measure. We also examine the impact of these dynamics on districts' "value added," which captures district contributions to annual student achievement gains. This metric accounts for multiple prior years of student-level test scores and thus, unlike measures of agency performance generally used in public administration research, should capture the causal impact of school districts on student learning (see Deming, 2014; Chetty, Friedman, and Rockoff, 2014). Ohio is an ideal case for this analysis because districts in the state must frequently consult voters just to maintain revenues, providing much needed statistical power.

The analysis employs a regression discontinuity (RD) design (see Lee 2008) to estimate the causal impact of bargaining dynamics on administrative behavior and outcomes. The RD design leverages the fact that districts where referenda received just under 50 percent of the vote should be essentially identical to those where referenda received just over 50 percent of the vote—except for the outcome of their referenda. In other words, if the assumptions of the design are met (assumptions we test in the analysis below), the approach enables us to identify the causal impact of failure, as if we had randomly allocated treatment status to districts (i.e., randomly assigned districts to referendum passage or failure). Our focus on districts near the 50 percent threshold is particularly valuable in this analysis, as we wish to explore bargaining dynamics that are more likely in districts whose referenda failed by small margins. The analysis also employs panel data methods to enhance the statistical power of the RD analysis (see Cellini, Ferreira, and Rothstein, 2010) and to examine the generalizability of the RD results across all Ohio school districts.

The results indicate that districts that placed tax proposals on the ballot were experiencing relative declines in operational expenditures at the time. Districts where tax referenda passed were able to stem these declines, but districts where referenda failed had further expenditure declines of about $200 per pupil. Consistent with cutback management strategies (e.g., see Berne and Stiefel, 1993; Levine 1978; Meier and O'Toole, 2009 Nguyen-Hoang, 2012), relative spending declines were smaller when it came to core instructional functions (cuts of 1-2 percent, or $50-$85 per pupil) and administrative functions (cuts of 1-2 percent, or $10-$20 per pupil), as opposed to less critical functions such as staff support, student support, building services, and transportation (cuts of 3-4 percent, or $70-$115 per pupil). District administrators spread expenditure declines associated with sudden revenue losses across several years, but lower instructional expenditures were nonetheless associated with the attrition of instructional staff—primarily teachers with under four years of experience. Although this latter effect is consistent with union-negotiated teacher contracts that require districts to implement layoffs according to seniority, it also is consistent with a strategy to minimize impacts on student learning because more experienced teachers tend to be more effective (e.g., see Harris and Sass 2011).

Importantly, these organizational disruptions were generally temporary. The analysis indicates that, on average, the expenditures of districts that experienced referendum failure subsequently rebounded and that, within 4-6 years, their expenditures were just as they would have been had their initial referenda passed. Consistent with this finding, the analysis reveals that soon after failure, districts were likely to secure approval for a subsequent, similar tax measure.

Thus, on average, initial failures did not have lasting consequences on school district spending and staffing. Yet, the short-term consequences were significant. The analysis reveals referendum failure had a significant negative impact on district contributions to student learning. Assuming a 180-day school year, and using Hill et al.'s (2008) estimates of how much students learn from year to year, we find that the negative achievement effects correspond to about 2-3 fewer annual "days of learning"—annual achievement deficits that accumulate over multiple years after the initial referendum failure. Interestingly, districts that did not pass a subsequent referendum did not experience a rebound in spending and performance, which suggests a causal link between expenditure declines and performance. Indeed, the "treatment on the treated" effect for districts that did not pass a subsequent referendum imply that every $1,000 in cuts to educational spending per pupil led to declines in annual student achievement of between 10 and 20 "days of learning."[6]

Thus, this study identifies a short-term—yet significant—transaction cost associated with school district tax referenda in Ohio. Voters ultimately approved referenda, but it sometimes took multiple, costly rounds of bargaining to do so. These costs came in the form of administrative disruptions that adversely affected performance, apparently due to expenditure declines that district managers were unable to absorb completely. These results contribute to research on how public organizations deal with external shocks (e.g., see Aldrich, 1979; Dess & Beard, 1984; Haveman, 1992), particularly revenue declines and the strategies managers employ to minimize their effects on performance (e.g., see Andersen & Mortenson, 2010; Berne &

[6] This estimate of the impact of spending on educational delivery is similar in magnitude to Lafortune, Rothstein, and Shazenbach's (2016) estimate. We both find that $1,000 in spending per pupil is associated with about 0.02 student-level standard deviations in annual achievement gains (i.e., 0.2 standard deviations over 10 years). Because of the RD design, our approach provides one of the most convincing estimates of the impact of operational spending on student achievement (also see Jackson, Johnson, and Persico, 2016).

Stiefel, 1993; Bozeman, 2010; Levine, 1978; Levine, 1979; Meier & O'Toole, 2009; Nelson & Balu, 2014; Pandey, 2010). Our contribution to this literature is particularly strong because our empirical approach enables us to identify causal effects—something which extant studies cannot claim.

But we see our primary contribution to be the exploration of the costs of direct democracy and, more broadly, citizen involvement in governance—something which scholars and thought leaders tend to ignore (Lynn, 2002). Although anecdotes abound—such as those associated with the recent Brexit referendum or the tax revolts of the 1970s and 1980s—this study is one of the few to provide a convincing account of the administrative disruptions that direct democracy can induce. And the dynamics we examine can be generalized to other types of reforms that involve citizens and public officials more directly in the administration of public programs. As such, this study helps fulfill recent calls to consider the management implications of state institutional designs (e.g., see Milward et al., 2016).

This paper proceeds as follows. Section 2 reviews empirical research on direct democracy and offers a theory of how it might disrupt the administration of public programs. Section 3 provides relevant background on Ohio school district finance. Section 4 describes our empirical strategy, tests its assumptions, and describes our data. Sections 5-8 present the results. Finally, Section 9 discusses the study's implications.

2. Direct Democracy and Administrative Disruption

Policymaking via direct democracy is subject to greater aggregate uncertainty than policymaking through representative democracy. One reason is that actors proposing policies for voter consideration must make an educated guess about the distribution of voter preferences in order to craft their proposal such that the median voter prefers it over the status quo. Even those who invest in high-quality polling cannot take all of the guesswork out of this process, as the preferences of the median voter cannot be pinned down with precision because of fluctuations in voter turnout and other idiosyncratic conditions unique to each election.[7]

The informational challenge is perhaps even greater for voters, as they typically have a limited understanding of the policy and administrative issues at hand. Although there is a good deal of evidence that voters draw on available information (Boehmke et al. 2012), make cost-benefit calculations for each proposal (Gerber and Phillips 2003), and close remaining knowledge gaps by using cues from political elites (Lupia 1994), research also indicates that voters are sometimes led astray by cognitive biases and misinformation (Bowler and Donovan 1998). In the context of budgetary measures in particular, there is ample evidence that voters fail to discern the true cost of the government services they consume (Sears and Citrin 1982) and routinely over-estimate the efficiencies that can be found by eliminating wasteful government spending.[8] There is also evidence that voters sometimes cut taxes in order to punish governments that they perceive to be performing poorly, even if that perception is flawed (e.g., see Kogan, Lavertu, and Peskowitz 2016).

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[7] Matsusaka (2014) shows, for example, that the order in which ballot measures appear on the ballot may, under certain conditions, affect voter support for each proposal.

[8] Consider California as an example: In October 2009, in the midst of a state budget crisis, the Field Poll asked a random sample of California voters if the state government could cut $20 billion to $25 billion out of the state budget—roughly one quarter of the total—without affecting service levels simply by eliminating "waste, fraud, and abuse." Almost two-thirds of the voters agreed that it could (Field Poll 2009). Even if voters are correct in their beliefs that certain cuts could be made without dramatically reducing service quality, there is no guarantee that government officials, who must implement the will expressed by voters, will faithfully respond to voter rejection of tax increases by targeting the cuts to the areas of spending that their constituents value the least (Gerber et al. 2001; Gerber, Lupia and McCubbins 2004).

Uncertainty among both government officials and voters has important consequences. In the case of referenda, when government officials miscalculate, they may propose policies that stray too much from the median voter's ideal relative to the status quo policy, resulting in their defeat. Similarly, voters might sometimes err by rejecting proposals that represent an improvement over the status quo, leading to policy that makes them worse off. Consequently, the process by which voters and governments come to agree on policy—which can entail the repeated rejection of policy proposals—might involve temporary administrative disruptions.

One particularly likely scenario in the case of tax referenda—the focus of our analysis—is that voters will reject initial proposals assuming that public organizations can make due with lower revenue. If organizations lack slack resources (e.g., Hou and Moynihan, 2008) and are unable to manage cutbacks so as to spare core organizational functions (e.g., see Berne & Stiefel, 1993; Levine 1978; Meier & O'Toole, 2009), then negative revenue shocks could affect performance. Additionally, the mere uncertainty that the bargaining process introduces can hamper strategic planning and could negatively affect staff morale and performance (Kiefer et al., 2015; Levine 1978). For example, in the education context, researchers have convincingly identified the negative student achievement effects associated with teacher movement between grades and schools (Ronfeldt, Loeb, and Wyckoff, 2013; but see Adnot, Dee, and Wyckoff, 2016). Indeed, the mere prospect of layoffs due to revenue declines can lead to such teacher "churn" (Goldhaber, Strunk, Brown, and Knight, 2016). Thus, even if spending levels have no impact on performance, sudden changes in those levels could (e.g., see Andersen & Mortenson, 2010; Lavertu & St. Clair, 2016). That said, recent research has found convincing evidence that spending levels can have a significant impact on educational delivery (see Jackson et al., 2016; Lafortune et al., 2016).

In summary, we argue that by injecting another veto player in the decision-making process, direct democracy introduces bargaining dynamics that could disrupt the administration of public programs—whether through the uncertainty they introduce or the revenue shocks that follow. Indeed, we argue that any process that injects citizens as a veto player in policymaking and administration could yield such dynamics.

3. Ohio School District Finance and Local Tax Referenda

U.S. school districts in general are heavily reliant on local revenue to fund their operations. The vast majority of this local revenue comes from property taxes (McGuire et al., 2015).[9] The centralization of school funding during the 20th century—prompted in large part by concerns over relying on local property taxes to fund public education—significantly increased the share of school funding distributed at the state level. Nevertheless, local sources still account for approximately 44 percent of school district revenues (Cornman et al., 2011).

Ohio is in many ways a typical state in terms of school district finance. District spending per pupil is just under $12,000 and local and state revenue sources each account for approximately 44 percent of total district revenues—both of which are close to nationwide averages (Cornman et al., 2011). And, like many other states, Ohio distributes state funds via a formula that combines a foundation component (to ensure "adequate" school district funding) and an equalization component. Ohio is unusual, however, in that state law governing local property taxes effectively requires school districts to seek the approval of voters more frequently than districts in other states, thus making it an ideal state to study bargaining dynamics.

[9] The remainder comes from local government contributions (intergovernmental transfers), other local taxes (e.g., sales and income taxes), various service charges, and investment returns.

Ohio law allows districts to supplement state aid by levying local property and income taxes that, respectively, account for over 90 percent and 4 percent of local revenue (Lavertu and St. Clair, 2016). Districts may place tax measures on the ballot on four election dates in most years.[10] Importantly, the vast majority of local taxes for operational funds are temporary, effectively requiring districts to propose a tax renewal or replacement after a set period of time. In addition, since the mid-1970s, state laws have prevented property taxes from growing automatically when property values increase, requiring that school tax referenda appear on the ballot quite frequently as districts pursue additional revenues to cover rising costs.[11] Of the approximately 615 school districts operating during the study period (2003-2013), 580 placed at least one funding measure on the ballot in these years.[12]

It is worth noting that property tax receipts can never drop below a state-mandated 20 mill[13] floor and that tax rates must exceed one percent to require voter approval. Additionally, districts can have multiple overlapping tax levies that expire in different years, and most districts carry fund balances to help them weather sudden dips in funding. Thus, the failure of a single

[10] These include November general elections, primary elections held in May, and special elections in February and August. In presidential election years, the primary is held in March, and no February special election takes place, so only three election dates are available in these years. Placing a tax proposal before voters—either a change in the tax rate or an extension of an expiring tax—requires a two-thirds vote of the local school board, which must adopt a resolution declaring that existing revenues, combined with state and federal aid, are expected to fall short of funding district operations in the coming years (Ohio Revised Code 5705.199). The resolution, and eventual language used to describe the measure on the ballot, must specify the amount of money to be raised each year by the tax. This amount is fixed over the life of the tax and does not increase with inflation. Each district may place tax measures on the ballot up to three times each calendar year (Ohio Revised Code 5705.214), with simple majority support among voters necessary for passage.

[11] Since the passage of Proposition 13 in California, many other states have adopted similar property tax limitations (Martin 2008).

[12] The remaining districts are in counties where local property tax rates do not exceed the 1 percent threshold that triggers mandatory voter approval for tax increases or operate at Ohio's minimum statutory tax rate floor, set at 20 mills, which means that district property taxes revenues can automatically increase with local property values without a public vote.

[13] A mill is equal to 1/10th of a cent of assessed valuation.

levy need not lead to substantial declines in district revenues or expenditures. However, in practice, political forces that threaten to reduce funding if districts maintain large balances likely lead districts to maintain relatively small balances.

4. Empirical Framework

The purpose of this study is to estimate the causal impact of tax referendum failure (relative to passage) on school district administration. We frame the analysis as the impact of failure (as opposed to the impact of passage) because it better reflects Ohio districts' context, where tax levies are supposed to help districts maintain current expenditure levels or to meet projected expenditures that exceed revenue forecasts. Additionally, our analysis shows that inflation-adjusted, per-pupil operational expenditures follow a downward trajectory relative to other Ohio districts just prior to districts placing levies on the ballot, with the failure of proposed tax levies exacerbating these declines. Thus, the relative decline in expenditures that follows levy failure appears to be one of the principal mechanisms producing differences between districts with passing and failing tax referenda.

Our primary identification strategy is a regression-discontinuity (RD) design. The design takes advantage of the fact that the election outcome—failure or passage—is essentially random[14] for levy proposals close to the 50 percent vote threshold, provided that there is no precise manipulation of the vote percentage near that threshold (Lee 2008; Eggers et al., 2015). Thus, our primary empirical strategy entails estimating discontinuities in district revenues per

[14] We recognize that this description is not entirely accurate, but, consistent with much existing work using the RD design, we use this simplified characterization throughout the paper. Formally, our analysis requires only that potential confounders change continuously at the threshold (e.g., see Cattaneo, Frandsen, and Titiunik, 2015).

pupil, expenditures per pupil, staffing, and student achievement—as well as other outcomes of interest—at the 50 percent vote cutoff determining referendum failure instead of passage. If the assumptions of the RD design are met—assumptions which we test below—then we can state with some confidence that the estimated impacts are causal.

The RD estimates we feature below are primarily from panel models that account for time-invariant differences between districts. Specifically, our statistical models, described below, employ a panel of tax levies Ohio districts proposed between 2003 and 2013. The proposal-level panel is structured so that the time dimension is captured by years relative to the election date for each tax proposal.[15] Specifically, for each calendar year, we identified all school district tax proposals across the state and merged in data associated with the district that placed each measure on the ballot. These district data span up to two years prior to the election year and up to six years following the election year. Thus, for each focal election year f, we created a proposal-level panel spanning up to two years prior ($f-2$) and up to six years after ($f+6$) district residents voted on the tax measures. We then stacked the 11 panel datasets (corresponding to each calendar election year) into the single dataset that we used for the analysis. Structuring the dataset this way enabled us to implement the RD design using a panel framework, as per Cellini et al. (2010).

The results of the RD analysis are not dependent on our employing the panel design. The results are robust to analyzing the data one year at a time. We focus on the panel models because they provide several advantages. First, the ability to capture district fixed-effects in the analysis should increase the statistical precision of our RD estimates. Second, employing panel methods enables a clear comparison of the local average treatment effect (LATE) estimates of the RD

[15] The analysis also accounts for calendar year fixed effects. One can discuss the results in terms of district-level effects because each proposal is associated with exactly one district.

design to the more general average treatment effect (ATE) estimates of the basic differences-in-differences design, which we also report. Third, employing panel methods facilitates our presentation of trends before and after referendum failure or passage, which we also use to test the RD assumption of "as-if random" treatment assignment near the 50 vote threshold, to test the common trends assumption of the supplementary differences-in-differences models, and to examine the potential mechanisms underlying the results.

It is important to reiterate that the RD design generates "local" estimates. In this case, we are focusing on the effect of levy failure, relative to passage, for districts where referenda received close to 50 percent of the vote. As we note above, focusing on this subsample makes sense if we wish to examine the transaction costs of bargaining dynamics associated with direct democracy. After all, districts close to the 50 percent threshold could, by random chance, have realized a different outcome. For these districts, it also is quite possible that an initial failure is followed by the success of an identical proposal, in which case consulting voters most clearly entails an administrative cost. Nevertheless, we also present difference-in-differences estimates to explore whether the effects for districts near the threshold are indeed different than for the average district with a referendum on the ballot.[16]

4.1 Statistical Models

Using the proposal panel we describe above, the analysis entails estimating pre- and post-referendum differences in outcomes within districts, and comparing those within-district differences between districts where tax proposals failed and those where proposals passed. Specifically, the basic difference-in-differences model takes the following form:

[16] This analysis of course cannot speak to what the effect of referendum failure (relative to passage) would have been in the 30 districts that never pursued a tax referendum.

$$Y_{itk} = \alpha_i + \theta_t + \partial_k + \tau^k(Fail_i \times \partial_k) + \epsilon_{itk} \qquad (1)$$

where the outcome of interest Y for proposal i during calendar year t and the year relative to the election k—the difference between the calendar year in which the election was held and the calendar year of a given observation $(t^* - t)$—is a function of fixed effects for proposals (α_i), calendar years (θ_t), relative years (∂_k), and an interaction between a variable indicating whether or not a proposal ultimately failed $(Fail_i)$ and the fixed effects for years relative to the election year (∂_k).

Note that the proposal fixed effects (α_i) subsume district fixed effects and that the relative year fixed effect (∂_k) is captured through the inclusion of indicator variables for all relative years except the year preceding the focal election year (i.e., $k = -1$). Thus, the model captures differences relative to the year prior to the focal election year within each district[17], and the coefficient vector τ^k captures differences in these differences between districts that failed to pass a levy and those that succeeded. Because there is no variation in referendum outcomes within proposals, the identifying variation comes from the comparison of within-proposal differences between proposals that did and did not obtain voter approval.

To implement the RD design, we account for the relationship between a proposal's vote share and the outcome Y in the panel model described in equation 1. Specifically, we centered the vote share variable at the 50 percent cutoff to create the running variable X_i and, following Gelman and Imbens (2014), we employed a first or second order polynomial to capture the relationship between a proposal's vote share and outcome Y.[18] Additionally, we interacted this

[17] The differences are actually within proposals. As we show in the appendix, the results are similar if we restrict the sample to one proposal per district in a given year—specifically, the proposal that received the highest vote share in that year.
[18] However, the results are robust to using higher order polynomials.

polynomial with the failure indicator to allow the relationship to differ on either side of the cutoff for each relative focal year (captured by ∂_k). Specifically, the model employing a quadratic specification is the following:

$$Y_{itk} = \alpha_i + \theta_t + \partial_k + \tau^k(Fail_i \times \partial_k) + \beta_1(X_i \times \partial_k) + \beta_2(X_i^2 \times \partial_k) + \beta_3\big(Fail_i \times (X_i \times \partial_k)\big) + \beta_4\left(Fail_i \times (X_i^2 \times \partial_k)\right) + \epsilon_{itk} \qquad (2)$$

By controlling for the share of votes cast in favor of each tax proposal in this way, we allow the conditional mean of our outcomes of interest to vary flexibly as a function of realized voter support for each tax levy. [19] Additionally, because the vote share is centered, the coefficients τ^k capture the impact of levy failure (relative to passage) for each year relative to the year before the election. To estimate this model, we demeaned the data to get rid of the proposal fixed-effects parameter (α_i), and we clustered standard errors at the district level to account for multiple proposals in some districts and within-district error correlation over time.

The RD design is only valid if there is no precise manipulation of the running variable near the 50 percent vote threshold (Lee, 2008). Our tests of this assumption validate our use of the RD design (see Appendix A). We do not find imbalances in district covariates near the threshold, and there is no discontinuity in the density of the running variable (the percent of votes cast in favor of each tax referendum) at the 50 percent threshold. We also conducted placebo tests by looking for discontinuities in our dependent variables at arbitrary vote thresholds other than the 50 percent threshold. There were no such discontinuities.

Additionally, we validate the estimates by demonstrating that the results are robust to linear specifications of the running variable and estimation based on a data sample within a

[19] Note that the time-invariant constituent terms $Fail_i$ and X_i in the interactions are implicitly included in the regression through the proposal fixed effect α_i.

restricted bandwidth of the cutoff (usually within plus or minus 0.07 of the 0.50 vote share cutoff), which we identified using the method proposed by Calonico, Cattaneo, and Titiunik (2014).[20] Finally, to address concerns regarding the generalizability of the RD estimate away from the cutoff, we also report the estimates from the basic differences-in-differences model described by equation 1.

4.2 Data

The analysis employs data from over 4,200 tax referenda that 580 unique districts placed on the ballot between 2003 and 2013. For the period 2008 to 2013, we obtained the vote breakdowns from the Ohio School Boards Association. For earlier years, we located the election results in archived records maintained by the Ohio Secretary of State. As Table 1 indicates, for every year of the analysis, the vote percentages that tax referenda received are bunched tightly around 50 percent support, with approximately two-thirds of proposals receiving between 40 percent and 60 percent of votes in favor. It is also worth pointing out that the majority of the proposals in our sample (79 percent) were temporary, "fixed length" tax levies with a median length of five years, and two-thirds were intended to raise funds for operational expenditures.

[Insert Table 1 about here.]

We obtained the primary dependent variables from a number of sources. Data on school district revenues are from the Common Core of Data at the National Center for Education Statistics, and we obtained detailed breakdowns of district per-pupil expenditures from the Ohio

[20] The Calonico, Catteneo, and Titiunuk procedure selects the bandwidth based on minimizing the asymptotic mean squared error of the RD treatment effect estimator. We identified this quantity using the rdrobust package in Stata. For most models, the procedure recommended bandwidths very close to +/-0.07, although there are some models for which the recommended bandwidth was around +/-0.09.

Department of Education (ODE).[21] Expenditure categories include instruction (e.g., pay for teachers, instructional aids, and instructional materials), administration (e.g., school and central office staff), and what we call "other" functions (e.g., transportation, counselors, instructional technology, and professional development).

To examine the impact of referendum failure on student achievement, we obtained two district-level student achievement measures—a "performance index" and a "value added" estimate—from the Ohio Department of Education. The performance index ranges from 0 to 120 and captures aggregate achievement levels on state exams in math and English language arts (administered in grades 3-8 and 10) and science and social studies (administered twice in grades 3-8 and 10). Compared to proficiency rates, which capture the percent of a district's students reaching the state's "proficient" threshold, the performance index captures wider variation in aptitude by assigning points for five different levels of student achievement.[22] In practice, however, a district's average proficiency rate in math and reading is very highly correlated with the performance index. Thus, in the analysis below, the standardized performance index can be thought of as comparing districts according to the average proficiency level of their students.

The estimates of districts' annual value-added—which were available from 2007 through 2014[23]—compare the year-to-year gains a district's students made on state math and reading

[21] All revenue and expenditure variables were adjusted for inflation using the state and local government implicit price deflator (Bureau of Economic Analysis series A829RD3) and are expressed in real year 2010 dollars.

[22] Specifically, the ODE took the percent of district students that reached each of five performance levels ("limited," "basic," "proficient," "accelerated," and "advanced") and multiplied those percentages by 0.3, 0.6, 1.0, 1.1, and 1.2, respectively. No points were given to the proportion of students who were not tested. The performance index is determined by summing across those weighted percentages.

[23] Value-added estimates for 2013 and 2014 are based on a three-year average, so we backed out the 2013 estimates using the 2011 and 2012 totals and repeated the procedure for the 2014 estimates. Additionally, 2007 value-added estimates are based on scores in just one grade, whereas other years include all grades 4-8.

exams with those of all other students in the state. Unlike district performance measures based on achievement levels (e.g., the performance index), which are confounded by student socioeconomic status and other differences in academic achievement unrelated to school and district quality, the value-added scores account for up to five years of students' previous test scores and, thus, account for student-level factors that may affect their performance.

The ODE reports the value-added estimates in Normal Curve Equivalent (NCE) units that compare the average annual achievement gains of district students.[24] The NCE scale is an equal-interval scale with a mean of 50 and a standard deviation of 21.063, and it ranges from 0 to 100 like a percentile scale. Districts whose students displayed average achievement growth compared to all students in the state would get a value-added score of 0 (50 minus 50), and districts whose students had achievement gains that placed them 1 standard deviation above mean growth would get a value-added score of 21.063. The benefit of this raw scale is that it allows us to compare the achievement impact of referendum failure to other impact estimates reported in the economics and education literatures. Moreover, this scale—which one can easily translate to student-level standard deviations—enables us to compare the effect sizes to average learning gains in math and reading for students in grades 3-8. Specifically, Hill et al. (2008) found that the average annual achievement growth of students in these grades and subjects is 0.368 student-level standard deviations. Thus, if one assumes a 180-day school year, we can convert the annual value-added effects into a more intuitive "days of learning" metric.

The bulk of the analysis examines the impact of referendum failure (relative to passage) in terms of district-level standard deviations in performance. Specifically, for each year of our

[24] It is important for scholars to employ the correct value-added data, as the ODE typically disseminates the "gain index" (which is basically a t-statistic), and it sometimes labels both this index and the value-added estimate as "gain scores."

panel, we standardized the performance index and the value-added metric to have a mean of zero and standard deviation of one to facilitate comparisons in district quality.[25] But we also estimated models using the unstandardized value-added metric (reported in NCE units) so that we could discuss the substantive significance of the results in terms of student-level achievement growth.

Table 2 presents descriptive statistics for these primary variables in the year immediately before each levy election ($f-1$). The first three columns provide the mean and standard deviation (in brackets) for the revenue, expenditure, and achievement variables. The final column presents the difference between column 2 and column 3, as well as the p-value (in brackets) from a two-tailed difference of means t-test. The table reveals that, compared to districts where proposals failed, districts with passing proposals spent more per pupil across all categories (particularly on instruction), relied more on local revenue, and had higher-achieving students (although they do not learn more annually according to math and reading exams). Additionally, the table illustrates how instructional expenditures—which are primarily for teacher labor costs—are by far the largest category of expenditures, and it reveals that total per-pupil revenues far outpace expenditures, as our data are for operational expenditures only.

[Insert Table 2 about here.]

Finally, to explore administrative disruption, we obtained teacher counts from the Common Core of Data, measures of teacher experience and student counts from district "Cupp"

[25] The state tests used to create the performance index change over time in terms of subjects and standards tested. Thus, one cannot standardize across all years simultaneously. The value-added metric is calculated using state test scores standardized by year, grade, and subject to allow comparison between and within students over time. Although the math and reading tests used to calculate the value-added metric remain the same across all years (2007-2014), for consistency we standardized at the district level separately for each year. Standardizing across all years simultaneously has little effect on the results.

reports available on the Ohio Department of Education website, and we used payroll records available from the Ohio State Treasurer to calculate teacher attrition rates.

5. Impact of Tax Referendum Failure on Revenues and Expenditures

We begin with the estimated impact of referendum failure (relative to passage) on district revenues and expenditures per pupil. For each of these sets of outcomes, we report the RD estimates based on the model in equation 2, as well as variants of that model estimated with linear specifications of the running variable or using a subset of the sample restricted to a narrow bandwidth around the 50 percent vote threshold.[26] Additionally, we present the results of the difference-in-differences model (equation 1), primarily to provide insights into the generalizability of the RD estimates away from the 50 percent threshold. Across all of these models and for each outcome of interest, we provide estimates that compare differences within proposals (using the year before the election as the baseline) between proposals that did and did not obtain voter approval.

Table 3 presents the results for models estimated using the natural log of per-pupil revenues as the outcome of interest. Thus, the coefficients, multiplied by 100, capture percent differences in the outcomes between districts with failing and passing levies, after controlling for proposal fixed effects, calendar and focal-year fixed effects, and referendum vote share. For example, the first column reveals that the within-district percent change in per-pupil revenues leading up to a referendum was essentially the same for districts where proposals failed and those where proposals passed. Local revenues two years prior to the election were approximately 0.7 percent higher in districts where levies ultimately failed than in districts where levies

[26] The appendix reports the results of non-parametric estimates.

ultimately passed, but that difference does not approach conventional levels of statistical significance. This is to be expected. As we note above, we failed to detect any baseline differences between districts where levies barely passed and those where levies just failed. In other words, the assumptions of the RD design are met in that these districts were essentially identical in terms of their revenue trends leading up to the levy election.

[Insert Table 3 about here.]

Overall, the results in Table 3 reveal that districts in which operational or capital tax proposals failed had total per-pupil revenues that were approximately 4 percent lower (over $500 lower per pupil[27]) two years following the election. The results are similar when we use a linear specification of the running variable or a local sample within a restricted bandwidth, and they obtain using the difference-in-differences model. The results also indicate that state and federal funding decreased by a comparable magnitude.[28]

Table 4 presents the results of models in which the outcome of interest is the log of various operational expenditures per pupil. The table reveals that the negative revenue effects we describe above correspond to relative declines in operational expenditures. However, the negative effects on per-pupil expenditures due to referendum failure are smaller in each year and are spread across four post-election years. Specifically, the relative expenditure declines are between 1.5 and 2 percent across four years (about $140-$215 less per pupil during those years), as opposed to 3-4 percent across two years. This pattern is consistent with district administrators

[27] We also estimated models using dollars per pupil, as opposed to the natural log of dollars per pupil. We report those figures in parentheses throughout.

[28] This may be because Ohio's state formula rewards local tax effort and federal grants to districts are often tied to state funding levels. It is noteworthy, however, that there are election-year effects for state funding. When a district levy fails during a calendar year (for example, 2008), it experiences a decline in state funds during the corresponding fiscal year (for example, during FY2008, between July 2007 and June 2008)—before local funds are ever collected beginning in January 2009. We suspect that this result is attributable to how districts account for state advances when they have levies on the ballot.

attempting to absorb revenue shocks by spreading cuts over a longer period, such that districts experience lower expenditures even after revenues recover.

[Insert Table 4 about here.]

The results also indicate that these overall effects on per-pupil expenditures are attributable to lower spending on instruction (by roughly 1-2 percent, or $50-$85 per pupil), administration (by roughly 1-2 percent, or $10-$20 per pupil), and other functions such as staff support, student support, and transportation (by roughly 3-4 percent, or $70-$115 per pupil). Thus, it appears that district administrators sought to protect expenditures on core instructional functions. However, it is worth noting that models limited to operational levies indicate no declines in administrative expenditures. Those results, which are otherwise similar to those we report here, appear in Appendix B.

Figure 1 illustrates the trajectory of revenues and expenditures before and after districts placed tax referenda on the ballot. As the figure reveals, the expenditure effects persist longer in the difference-in-differences models, which should capture the average effect across all districts that placed tax levies on the ballot. In other words, for those districts close to the passage threshold, differences in expenditures taper off more quickly than they do in districts where the initial referendum vote placed them far from the passage threshold.

[Insert Figure 1 about here.]

6. The Dynamics of Proposal Passage and Defeat

Table 5 presents the results of models that estimate the probability of districts proposing or passing a subsequent tax levy after an initial failure. The results reveal that districts where tax proposals failed were far more likely than districts where they passed to propose and secure passage of a subsequent measure. Indeed, districts where tax referenda failed were over 50 percentage points more likely to pass a tax levy the following year. Unsurprisingly, the results reveal that the estimated effects are more pronounced in the RD analysis—that is, the analysis that focuses on district referenda that narrowly failed or passed. Thus, it appears that the temporary revenue and expenditure effects we detect in the RD analysis may be attributable to these districts having approximately a 50-50 chance of passing a subsequent tax proposal—which, we should note, is just about the mean probability of passing a tax levy across all Ohio districts.[29]

[Insert Table 5 about here.]

There are a number of potential explanations for the high rate with which districts where referenda failed quickly obtained approval for a subsequent referendum. Our data do not allow us to convincingly isolate these mechanisms, but descriptive analyses suggest that districts keep proposing comparable tax levies until they pass.[30] We find no evidence that districts generally reduce the size of their tax proposals following a defeat. Nor do we find evidence that reducing

[29] Table 5 indicates that the probability of levy passage increases significantly more than the probability of levy proposal in the year following the initial failure. This gap is driven by differences in the baseline year ($f-1$) probabilities of observing these two outcomes. Many of the tax measures in our sample directly follow a levy put on the ballot in the previous election year. In other words, the probability of levy proposal in year $f-1$ was already quite high in our sample, so the capacity for the probability of levy proposal to increase further was limited. However, the vast majority of these earlier levies fail, so it is very rare to observe a levy in the current year if a district won a referendum one year earlier. As a result, the potential is much greater for a subsequent increase in the probability of passage relative to the baseline rate of passage in year $f-1$.

[30] These analyses are not presented here to conserve space but are available upon request.

the proposed tax rate or cutting expenditures increases the probability of passage. But we are unable to rule out other strategic efforts that could increase the probability of passage. For example, districts might simply engage in more electioneering the second time around.[31]

7. Impacts of Referendum Failure on District Performance

The results above indicate that the failure of a district tax referendum had a negative short-term impact on district revenues and expenditures per pupil. Although expenditure declines disproportionately affected less essential functions, they nonetheless affected instructional functions. Thus, there is reason to believe that the failure of tax referenda might have had an impact on the quality of education districts provided. We explore this possibility by applying the same empirical strategy to models that feature district-level student achievement as dependent variables. Specifically, we estimated models that employ a measure of districts' annual student-level achievement gains (the "value added" measure) and models that employ the state's district "performance index."[32]

[Insert Table 6 about here.]

Table 6 presents the results for standardized versions of both the value-added measure (columns 1-4) and the performance index (columns 5-8). The coefficients for many of the post-election years are either statistically significant or approach conventional levels of statistical significance across both sets of models. As expected given that they are estimates and available for fewer years, the value-added results provide less statistical power and often fail to reach

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[31] We thank a reviewer for suggesting this possibility.

[32] Although the value-added measure is preferable because it accounts for student-level educational histories via multiple years of prior test scores—and thus accounts for potential student movement in and out of the district—the statistical power of models using that measure is limited because it is available only for the latter half of the panel.

conventional levels of statistical significance. But the effect sizes across both achievement measures generally range from about 0.03 to 0.10 district standard deviations in the years immediately following an election year.[33]

To get a sense for the size of these effects in terms of student learning, we re-estimated value-added models using the Normal Curve Equivalent (NCE) scale on which the district value-added metric is based. Table 7 reveals that the value-added losses associated with levy failure generally peak two years after the failure and that the most conservative estimates for this year are coefficients with magnitudes of around 0.10-0.14, which translates to approximately 0.005-0.006 student-level standard deviations in math and reading achievement.[34] Assuming a 180-day school year and using Hill et al.'s (2008) estimates of the typical amount of learning in grades 3-8—the grades on which the value-added metric is based—these results translate to approximately 2-3 fewer "days of learning" among students in districts where tax referenda failed.[35]

[Insert Table 7 about here.]

One can think of these RD estimates as capturing the intent-to-treat (ITT) effect, in the sense that there is imperfect compliance with treatment assignment. Districts that receive the "referendum failure" treatment can fail to comply with their treatment assignment by approving a tax referendum in a subsequent election. Thus, the ITT effect can be interpreted as the causal effect of exogenously changing the outcome of a tax referendum from passing to failing and then allowing district voters to consider and potentially pass tax measures in subsequent years. We also calculated treatment-on-the-treated (TOT) effects using the recursive estimator that Cellini et al. (2010) developed. These estimates capture the causal effect of exogenously changing a

[33] These estimates increase in size if we limit the analysis to operational levies.
[34] We obtained these numbers by dividing the coefficient estimates by 21.063.
[35] We generated these estimates by dividing 0.10 and 0.14 by the average yearly gains in math and reading between grades 3-8 (0.368 standard deviations) and multiplying that number by 180.

referendum from passing to failing among the subset of districts that do not subsequently pass a tax referendum in future elections. (Essentially, the method entails rescaling the estimates by the rate at which districts failed to pass subsequent proposals.) The results indicate that neither the expenditures nor the performance of school districts rebounds if districts do not pass a subsequent referendum. The results (which we present in Appendix D) imply that every $1,000 in operational expenditures is associated with 10-20 fewer annual "days of learning" for each and every district student. These results provide evidence that there is a causal link between levy passage, district expenditures, and student achievement.

8. Impacts of Referendum Failure on District Staffing

The results above provide strong evidence that the failure of tax referenda had a significant negative impact on districts' educational delivery, and they provide suggestive evidence that cuts to instructional and other expenditures are responsible. We explore these potential administrative disruptions more thoroughly in this section. Specifically, we consider whether expenditure declines are accompanied by teacher attrition and higher student-teacher ratios. To focus on tax referenda that had the most direct connection to such district operations, we limit this analysis to tax measures for operational funding.[36] Table 8 presents the results of these analyses. In the interest of space, we limit our focus to the quadratic RD specification and the difference-in-differences models.

[Insert Table 8 about here.]

[36] Some of the tax measures included are combined levies that cover both operational and capital expenditures. We could not separate out how funds were used for those measures.

The results in Table 8 indicate that referendum failure is associated with teacher attrition rates that are up to 1.5 percentage points higher (columns 1-2) and increases in student-teacher ratios of 0.3-0.4 (columns 7-8). Additionally, the results reveal that failure led to a more experienced group of teachers: the percent of teachers with less than four years of experience was up to 3 percentage points lower in districts with failing tax measures, whereas the proportion of teachers with more than ten years of experience was up to 2.7 percentage points higher. These results suggest that districts responded to resource constraints by letting go of less experienced teachers (or hiring fewer additional inexperienced teachers).

Finally, it is worth noting that, for some districts, the failure of a tax proposal resulted in their receiving a label of "fiscal caution", "fiscal watch", or "fiscal emergency." These labels come with various state interventions, including the requirement that districts develop and implement plans to address their financial situations. It appears that these interventions themselves cause administrative disruptions that might negatively affect performance (see Thompson, 2016).[37] Our own analysis reveals that, in the year immediately following tax referendum defeat, the probability of receiving a state fiscal insolvency designation increased by five percentage points.

9. Summary and Discussion

The analysis employed an RD design and panel methods to estimate the impact of referendum failure (relative to passage) across nearly all Ohio districts that placed a tax measure on the ballot between 2003 and 2013. The results indicate that the failure of tax proposals led to large relative

[37] For a look at the impact of state monitoring and interventions from a public administration perspective, see Rutherford (2014).

declines in district revenues and expenditures per pupil—and, ultimately, declines in student achievement. Further analysis revealed that relative declines in instructional spending corresponded to teacher attrition—concentrated among teachers with four years of experience or less—as well as increases in student-teacher ratios. Importantly, the analysis reveals that these average affects were generally temporary—a product of a bargaining process that, on average, ended with districts spending, managing staff, and performing as if their initial tax proposals had passed.

One contribution of this study is to the management literature that considers how organizations cope with shocks from their external environments (e.g., see Aldrich, 1979; Dess & Beard, 1984; Haveman, 1992) and, more specifically, how organizations engage in “cutback management” to deal with revenue declines (e.g., see Bozeman 2010; Pandey 2010). The results suggest that district administrators used classic strategies to absorb the revenue shocks (e.g., see Berne and Stiefel, 1993; Levine 1978; Meier and O’Toole, 2009). First, although revenue declines were concentrated in the first and second post-failure years, district administrators spread expenditures declines over the following four (perhaps six) years. Second, less essential services bore the brunt of the expenditure declines. Although instruction and administration budgets were affected—indeed, most of the savings came from instruction-related declines, as instructional expenses are by far districts’ largest—it appears that managers sought to minimize the impact of revenue declines on instruction as a percentage of total spending. Third, the apparent cuts to instructional staff targeted the least experienced teachers. Although this pattern is consistent with teacher contracts that stipulate that senior teachers should be the last subjected to layoffs, it also is consistent with a strategy meant to minimize the negative impact of layoffs on performance.[38]

That district managers were unable to completely adapt to these shocks and maintain performance on the most prioritized dimensions—math and reading achievement—may be attributable to a number of factors characteristic of public organizations (e.g., see Bozeman, 1987; Bozeman 2010; Moulton 2009; Pandey 2010). First, Ohio law stipulates that districts can place tax levies on the ballot only if they project deficits. Although there is significant room for districts to maneuver—for example, by inflating expenditure forecasts—the process nonetheless constrains district options in ways that should make fiscal stress more likely. Second, there are political pressures against maintaining large fund balances. Both voters and elected officials have shown that they are less willing to provide additional funds if they think that districts are hoarding money. Third, as we note above, district administrators are constrained by collective bargaining contracts when it comes to managing human resources. In this case, managers may have further mitigated the impact of labor cuts on student achievement if they could have more easily let go of ineffective senior teachers, for example. Indeed, school districts are severely limited in how they manage their finances and staff in general. For example, many federal and state funds are designated for specific purposes and cannot be used to shore up deficits in other areas (Roza, 2013).

The above constraints certainly make public organizations more vulnerable to fiscal shocks and less able to respond to them. That may help explain why the relatively minor

[38] Research indicates that teacher performance, as measured by student test score value-added, improves with experience (e.g., Harris and Sass 2011). If districts reduced their workforces by instead providing early retirement incentives to the most experienced teachers (e.g., Fitzpatrick and Lovenheim 2014), it is possible that the negative achievement effects we observe would be even larger. On the other hand, it could also be that this practice resulted in retaining inferior teachers and, thus, exacerbated the achievement declines.

budgetary shocks of 2-3 percent—far lower than the 10 percent shocks that school districts absorbed in the Meier and O'Toole's (2009) study—had such an impact on performance. Another reason for the differences in results between this study and Meier and O'Toole's could simply be related to the empirical strategy. The regression discontinuity design we employ convincingly captures the counterfactual—what would have happened had districts' referenda passed instead of failed. Our tests of the RD design's assumptions confirm that the only apparent difference between districts near the 50 percent vote threshold is whether or not their tax referenda failed. Even the difference-in-differences approach sometimes showed some indication of pre-treatment trends, which suggests that those estimates cannot be interpreted as being causal.

Public administration research could benefit from exploiting discontinuities at vote thresholds—particularly as they look to estimate the impact of political transitions on public management. Since elections with a strict vote threshold generally provide an exogenous source of variation in political regimes (see Eggers et al., 2015) the discontinuity can be used to identify the causal impact of transitions on a variety of organizational behaviors and outcomes. Moreover, using an instrumental variables framework, the vote threshold can be used to identify the impact of other organizational behaviors, such as the impact on employees of moving from one agency to another.

This study's primary contribution, however, is that it illustrates how introducing more democracy—in this case, by inserting voters as veto players in school finance policy—can lead to administrative disruptions that adversely affect the performance of public organizations. Voters often changed their minds and approved subsequent tax measures, apparently regardless of whether districts implemented service cuts or adjusted their tax proposals. In many cases, if districts and voters could have agreed on a tax rate one year prior, significant multi-year losses in student achievement would have been averted. Thus, although direct democracy might limit the influence of special interest groups and lower public spending on wages (Matsusaka, 2009), we find that it can impose some significant administrative costs in the process. It remains an open question whether or not these costs are worth the benefits—for example, in the form of the increased legitimacy that might emanate from the deliberation between districts and their residents (see Fung 2006; Smith and Tolbert, 2004).

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Tables

TABLE 1. SUMMARY STATISTICS FOR SCHOOL DISTRICT TAX MEASURES

	Count of Tax Referenda	Fraction operational (vs. capital)	Fraction passed	Approval Vote Share			
				Mean	Standard Deviation	Minimum	Maximum
2003	411	0.635	0.516	0.508	0.115	0.089	0.878
2004	601	0.740	0.456	0.484	0.101	0.170	0.753
2005	483	0.720	0.524	0.508	0.103	0.230	0.805
2006	417	0.674	0.525	0.499	0.104	0.069	0.735
2007	401	0.616	0.509	0.506	0.105	0.213	0.855
2008	415	0.614	0.530	0.503	0.098	0.173	0.922
2009*	261	0.655	0.658	0.539	0.119	0.231	0.841
2010	412	0.738	0.534	0.504	0.104	0.183	0.775
2011	361	0.700	0.532	0.509	0.114	0.152	0.814
2012	331	0.535	0.580	0.514	0.095	0.141	0.739
2013*	144	0.257	0.590	0.533	0.112	0.238	0.788
Total	*4237*	*0.656*	*0.529*	*0.506*	*0.106*	*0.069*	*0.922*

Note. The descriptive statistics above are for all tax referenda included in the analysis. The data were compiled by the Ohio School Boards Association and the authors. Note that the statistics for 2013 are based on only the first two elections (special elections and primary elections) of that year and that we were unable to obtain vote totals for the special elections held in 2009.

TABLE 2. SUMMARY STATISTICS FOR SCHOOL DISTRICT VARIABLES

	All Proposals Mean [s.d.]	Proposals that passed Mean [s.d.]	Proposals that failed Mean [s.d.]	Difference (passed minus failed)
Total Revenues Per Pupil (in 2010 dollars)				
Local	5,657 [2,184]	5,864 [2,284]	5,425 [2,042]	439 [p=0.000]
State	4,967 [2,331]	4,861 [2,292]	5,087 [2,370]	-226 [p=0.002]
Federal	666 [476]	649 [428]	686 [524]	-37 [p=0.010]
Total	11,290 [2,592]	11,373 [2,599]	11,197 [2,581]	176 [p=0.026]
Operational Expenditures Per Pupil (in 2010 dollars)				
Instructional	5,457 [819]	5,491 [888]	5,418 [731]	73 [p=0.004]
Administrative	1,214 [292]	1,220 [305]	1,207 [277]	13 [p=0.144]
Other Services	3,113 [653]	3,113 [709]	3,114 [583]	-1 [p=0.947]
Total	9,781 [1,508]	9,824 [1,635]	9,732 [1,350]	92 [p=0.048]
Student Achievement (standardized by year)				
Performance Index	0.036 [0.942]	0.110 [0.968]	-0.047 [0.906]	0.157 [p=0.000]
Value-Added	0.045 [0.965]	0.055 [0.958]	0.031 [0.975]	0.023 [p=0.588]

Note. The descriptive statistics are based on observations one year prior to the proposal year ($f - 1$) for all proposals employed in the analysis. The first three columns provide the mean and standard deviation (in brackets) for the revenue, expenditure, and achievement variables. The final column presents the difference between column 2 and column 3, as well as the p-value (in brackets) from a two-tailed difference of means t-test.

TABLE 3. IMPACT OF PROPERTY & INCOME TAX LEVY FAILURE ON LN(REVENUES PER PUPIL)

	Primary Specification				Sensitivity Checks			
	(1) Local	(2) State	(3) Federal	(4) Total	(5) Total	(6) Total	(7) Total	(8) Total
2 YRS PRIOR	0.00737 (0.00797)	-0.00318 (0.0115)	-0.00521 (0.0161)	0.00139 (0.00907)	0.00311 (0.0170)	0.00187 (0.00677)	-0.00908 (0.0119)	0.00687 (0.00439)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-0.00366 (0.00749)	-0.0282* (0.0113)	0.0113 (0.0170)	-0.0187* (0.00852)	-0.0150 (0.0174)	-0.0136* (0.00636)	-0.0223^ (0.0115)	-0.0103* (0.00438)
1 YR AFTER	-0.0361*** (0.00946)	-0.0383* (0.0161)	-0.0298^ (0.0179)	-0.0435*** (0.0119)	-0.0633** (0.0238)	-0.0382*** (0.00916)	-0.0454** (0.0157)	-0.0320*** (0.00673)
2 YRS AFTER	-0.0327** (0.0123)	-0.0308 (0.0210)	-0.0100 (0.0197)	-0.0385* (0.0154)	-0.0497 (0.0306)	-0.0421*** (0.0117)	-0.0424* (0.0205)	-0.0394*** (0.00739)
3 YRS AFTER	-0.00427 (0.0139)	0.00983 (0.0245)	-0.0282 (0.0210)	-0.000332 (0.0173)	-0.0198 (0.0323)	-0.00782 (0.0134)	-0.00148 (0.0222)	-0.0185* (0.00849)
4 YRS AFTER	0.00227 (0.0150)	0.0496^ (0.0260)	-0.0504* (0.0216)	0.0235 (0.0180)	-0.0230 (0.0319)	0.0129 (0.0141)	0.0153 (0.0232)	0.000568 (0.00902)
5 YRS AFTER	0.0000462 (0.0161)	0.0160 (0.0279)	-0.0326 (0.0235)	0.00705 (0.0189)	-0.0565^ (0.0339)	0.00856 (0.0151)	-0.0139 (0.0250)	0.00554 (0.00977)
6 YRS AFTER	0.00800 (0.0187)	-0.0149 (0.0311)	-0.0267 (0.0264)	-0.00951 (0.0208)	-0.0440 (0.0360)	0.00206 (0.0159)	-0.00757 (0.0259)	0.00314 (0.0107)
N	29,912	29,912	29,901	29,912	15,456	29,912	14,831	29,912
District Count	568	568	568	568	518	568	514	568
Levy Count	4,289	4,289	4,289	4,289	2,190	4,289	2,100	4,289
Mean of DV	8.58	8.45	6.44	9.32	9.33	9.32	9.33	9.32
MODEL	RD	RD	RD	RD	RD	RD	RD	DID
Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Linear	Linear	N/A
Restricted Bandwidth	No	No	No	No	Yes	No	Yes	N/A
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on logged district revenues. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE 4. IMPACT OF PROPERTY & INCOME TAX LEVY FAILURE ON LN(SPENDING PER PUPIL)

	Primary Specification				Sensitivity Checks			
	(1) Instruction	(2) Admin.	(3) Other	(4) Total	(5) Total	(6) Total	(7) Total	(8) Total
2 YRS PRIOR	0.00533 (0.00346)	-0.0150* (0.00748)	0.00146 (0.00543)	-0.00148 (0.00386)	-0.00672 (0.00605)	0.000608 (0.00247)	-0.00236 (0.00407)	0.00256 (0.00166)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-0.00321 (0.00332)	-0.00561 (0.00726)	-0.00404 (0.00642)	-0.00584 (0.00455)	-0.00149 (0.00577)	-0.00410 (0.00346)	-0.00408 (0.00457)	-0.00577** (0.00190)
1 YR AFTER	-0.0104* (0.00497)	-0.0106 (0.00868)	-0.0302*** (0.00774)	-0.0160** (0.00486)	-0.0198* (0.00815)	-0.0128*** (0.00379)	-0.0136* (0.00541)	-0.0187*** (0.00258)
2 YRS AFTER	-0.0166** (0.00605)	-0.00843 (0.0104)	-0.0380*** (0.00976)	-0.0235*** (0.00642)	-0.0297** (0.00977)	-0.0163** (0.00511)	-0.0195** (0.00667)	-0.0208*** (0.00387)
3 YRS AFTER	-0.0133* (0.00675)	-0.0197^ (0.0104)	-0.0309** (0.0104)	-0.0201** (0.00669)	-0.0203* (0.00970)	-0.0155** (0.00555)	-0.0172* (0.00721)	-0.0180*** (0.00420)
4 YRS AFTER	-0.0122 (0.00765)	-0.0232* (0.0117)	-0.0294** (0.0103)	-0.0164* (0.00675)	-0.0107 (0.00987)	-0.0130* (0.00563)	-0.0130^ (0.00735)	-0.0146*** (0.00402)
5 YRS AFTER	-0.0119 (0.00854)	-0.0246^ (0.0140)	-0.0118 (0.0120)	-0.0106 (0.00809)	-0.00138 (0.0119)	-0.0107^ (0.00603)	-0.00645 (0.00804)	-0.0130** (0.00464)
6 YRS AFTER	-0.00298 (0.0104)	-0.0253 (0.0197)	-0.00868 (0.0142)	-0.00553 (0.00954)	-0.0116 (0.0148)	-0.00537 (0.00725)	-0.00303 (0.00924)	-0.0114* (0.00468)
N	29,507	29,518	29,507	29,527	17,593	29,527	17,716	29,527
District Count	572	572	572	572	534	572	534	572
Levy Count	4,217	4,218	4,217	4,218	2,494	4,218	2,512	4,218
Mean of DV	8.57	7.06	8	9.16	9.16	9.16	9.16	9.16
MODEL	RD	RD	RD	RD	RD	RD	RD	DID
Specification	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Linear	Linear	N/A
Restricted Bandwidth	No	No	No	No	Yes	No	Yes	N/A
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on logged district expenditures. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE 5. PROBABILITY OF LEVY PROPOSAL AND PASSAGE

	Levy Proposal		Levy Passage	
	(3)	(4)	(5)	(6)
2 YRS PRIOR	-0.0154 (0.0384)	-0.0624*** (0.0186)	0.0363 (0.0567)	0.00937 (0.0262)
1 YR PRIOR	--	--	--	--
ELECTION YR	0.0218 (0.0295)	-0.0581*** (0.0157)	-0.914*** (0.0393)	-0.959*** (0.0200)
1 YR AFTER	0.346*** (0.0412)	0.247*** (0.0199)	0.526*** (0.0519)	0.221*** (0.0186)
2 YRS AFTER	-0.0476 (0.0462)	-0.0493* (0.0234)	0.0673 (0.0585)	0.0441 (0.0292)
3 YRS AFTER	-0.0624 (0.0439)	-0.0939*** (0.0215)	0.0577 (0.0512)	0.0199 (0.0276)
4 YRS AFTER	-0.0323 (0.0431)	-0.146*** (0.0235)	-0.0235 (0.0523)	-0.112*** (0.0250)
5 YRS AFTER	-0.00103 (0.0495)	-0.169*** (0.0265)	-0.0163 (0.0571)	-0.174*** (0.0289)
6 YRS AFTER	0.0966^ (0.0536)	-0.0432 (0.0271)	0.166* (0.0659)	0.0199 (0.0294)
N	31,911	31,911	21,400	21,400
District Count	571	571	577	577
Levy Count	4,216	4,216	2,926	2,926
Mean of DV	0.57	0.57	0.38	0.38
MODEL	RD	DID	RD	DID
Polynomial	Quadratic	N/A	Quadratic	N/A
Restricted Bandwidth	No	N/A	No	N/A
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on the probability of levy proposal and the probability of levy passage. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE 6. IMPACT OF TAX LEVY FAILURE ON STUDENT ACHIEVEMENT

	State "Value Added" Estimate (District SDs)				State Performance Index (District SDs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2 YRS PRIOR	-0.102 (0.129)	-0.00960 (0.0909)	-0.144 (0.167)	-0.00754 (0.0577)	-0.00593 (0.0217)	-0.0127 (0.0174)	-0.0118 (0.0230)	-0.00703 (0.0121)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-0.0926 (0.113)	-0.0242 (0.0831)	-0.155 (0.140)	-0.0429 (0.0552)	0.0295 (0.0197)	-0.00163 (0.0150)	0.0271 (0.0212)	-0.0141 (0.00998)
1 YR AFTER	-0.0894 (0.117)	-0.0342 (0.0873)	-0.0666 (0.147)	-0.0486 (0.0548)	-0.0187 (0.0220)	-0.0351* (0.0167)	-0.0299 (0.0240)	-0.0356** (0.0123)
2 YRS AFTER	-0.199^ (0.103)	-0.124 (0.0782)	-0.178 (0.146)	-0.0913^ (0.0524)	-0.0198 (0.0245)	-0.0419* (0.0190)	-0.0251 (0.0265)	-0.0312* (0.0138)
3 YRS AFTER	-0.179^ (0.108)	-0.126 (0.0826)	-0.168 (0.148)	-0.00385 (0.0551)	-0.0274 (0.0273)	-0.0630** (0.0204)	-0.0317 (0.0290)	-0.0281^ (0.0145)
4 YRS AFTER	-0.0195 (0.108)	0.0722 (0.0868)	0.0678 (0.154)	0.0115 (0.0585)	0.0123 (0.0296)	-0.0281 (0.0220)	-0.0160 (0.0309)	-0.0181 (0.0156)
5 YRS AFTER	-0.120 (0.117)	-0.0701 (0.0955)	-0.0344 (0.156)	-0.00224 (0.0614)	-0.00610 (0.0326)	-0.0394 (0.0246)	-0.0364 (0.0341)	-0.0148 (0.0173)
6 YRS AFTER	-0.150 (0.123)	0.00258 (0.0975)	-0.0456 (0.156)	-0.0312 (0.0667)	-0.00776 (0.0354)	-0.0213 (0.0270)	-0.0339 (0.0363)	-0.0126 (0.0194)
N	24,796	24,796	10,936	24,796	33,199	33,199	21,660	33,199
District Cnt	571	571	509	571	571	571	541	571
Levy Cnt	4,324	4,324	1,916	4,324	4,324	4,324	2,812	4,324
Mean DV	0.03	0.03	0.03	0.03	0.09	0.09	0.06	0.09
MODEL	RD	RD	RD	DID	RD	RD	RD	DID
Specif.	Quad.	Linear	Linear	N/A	Quad.	Linear	Linear	N/A
Restricted Bandwidth	No	No	Yes	N/A	No	No	Yes	N/A
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on district performance measures standardized by year. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE 7. IMPACT OF TAX LEVY FAILURE ON STUDENT ACHIEVEMENT (STUDENT-LEVEL GAINS)

	State "Value Added" Estimate (NCE)			
	(1)	(2)	(3)	(4)
2 YRS PRIOR	-0.126 (0.129)	-0.0239 (0.0902)	-0.171 (0.167)	-0.00997 (0.0586)
1 YR PRIOR	--	--	--	--
ELECTION YR	-0.109 (0.115)	-0.0333 (0.0844)	-0.206 (0.141)	-0.0493 (0.0559)
1 YR AFTER	-0.0999 (0.119)	-0.0400 (0.0895)	-0.0932 (0.145)	-0.0526 (0.0567)
2 YRS AFTER	-0.222* (0.106)	-0.136^ (0.0800)	-0.208 (0.149)	-0.0997^ (0.0538)
3 YRS AFTER	-0.201^ (0.111)	-0.140 (0.0849)	-0.217 (0.149)	-0.00786 (0.0570)
4 YRS AFTER	-0.0307 (0.110)	0.0750 (0.0888)	0.0355 (0.151)	0.00652 (0.0599)
5 YRS AFTER	-0.135 (0.120)	-0.0776 (0.0983)	-0.0441 (0.155)	-0.00668 (0.0631)
6 YRS AFTER	-0.163 (0.127)	0.00584 (0.101)	-0.0801 (0.157)	-0.0407 (0.0692)
N	24,796	24,796	11,326	24,796
District Cnt	571	571	514	571
Levy Cnt	4,324	4,324	1,980	4,324
Mean DV	0.01	0.01	0.01	0.01
MODEL	RD	RD	RD	DID
Specif.	Quad.	Linear	Linear	N/A
Restricted Bandwidth	No	No	Yes	N/A
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. The results above are from models estimating the impact of operational and capital levy failure (as opposed to passage) on student achievement using value-added gains measured in terms of normal curve equivalent (NCE) scores. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

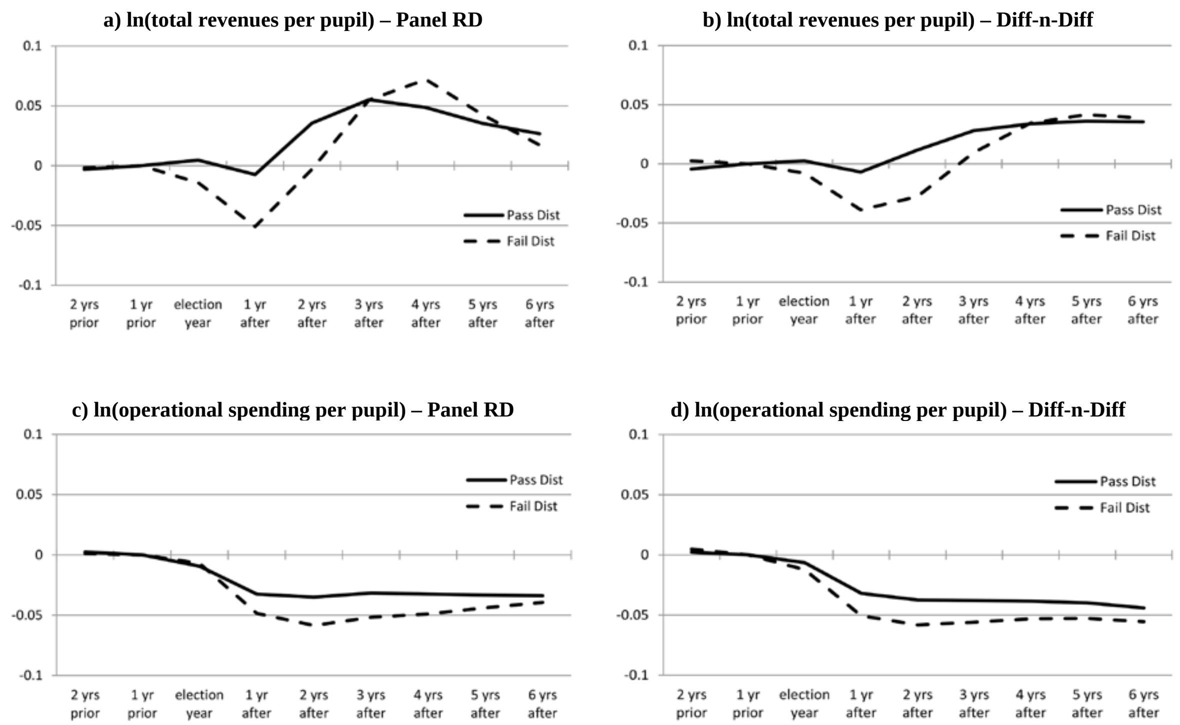
TABLE 8. IMPACT OF REFERENDUM FAILURE ON DISTRICT STAFFING

	Teacher attrition rate		% teachers w/ less than 4 years of experience		% teachers w/ more than 10 years of experience		Student-teacher ratio	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2 YRS PRIOR	0.622 (0.647)	0.214 (0.285)	-0.632 (0.804)	0.496 (0.405)	0.602 (0.770)	-0.128 (0.405)	0.171 (0.120)	0.0486 (0.0610)
1 YR PRIOR	--	--	--	--	--	--	--	
ELECTION YR	0.917 (0.569)	0.362 (0.281)	-0.405 (0.759)	-1.252** (0.398)	0.568 (0.656)	1.150** (0.366)	0.137 (0.116)	0.152** (0.0582)
1 YR AFTER	1.516** (0.572)	1.014** (0.312)	-1.014 (0.959)	-2.085*** (0.508)	1.097 (0.888)	2.018*** (0.493)	0.194 (0.140)	0.308*** (0.0745)
2 YRS AFTER	0.744 (0.645)	1.539*** (0.336)	-2.461* (1.050)	-2.988*** (0.602)	2.185* (0.996)	2.737*** (0.584)	0.285^ (0.163)	0.393*** (0.0881)
3 YRS AFTER	1.240* (0.571)	1.502*** (0.317)	-2.021^ (1.142)	-2.395*** (0.626)	1.981^ (1.134)	2.400*** (0.619)	0.286^ (0.163)	0.395*** (0.0950)
4 YRS AFTER	0.336 (0.691)	0.655^ (0.347)	-2.222^ (1.223)	-2.157*** (0.651)	2.202^ (1.216)	2.368*** (0.645)	0.245 (0.165)	0.320** (0.0979)
5 YRS AFTER	1.245* (0.616)	0.635 (0.334)	-1.933 (1.428)	-1.868** (0.710)	1.558 (1.383)	2.052** (0.709)	0.260 (0.176)	0.212* (0.106)
6 YRS AFTER	1.297^ (0.745)	0.447 (0.387)	-1.648 (1.517)	-1.675* (0.768)	2.361 (1.478)	1.738* (0.779)	0.354^ (0.183)	0.292** (0.105)
N	19,307	19,307	18,206	18,206	18,206	18,206	16,404	16,404
District Cnt	526	526	527	527	527	527	526	526
Levy Cnt	2,771	2,771	2,848	2,848	2,848	2,848	2,771	2,771
Mean DV	8.26	8.26	22.44	22.44	58.5	58.5	16.13	16.13
MODEL	RD	DID	RD	DID	RD	DID	RD	DID
Polynomial	Quad.	N/A	Quad.	N/A	Quad.	N/A	Quad.	N/A
Restricted Bandwidth	No	N/A	No	N/A	No	N/A	No	N/A
Levy Type	Operat.	Operat.	Operat.	Operat.	Operat.	Operat.	Operat.	Operat.

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on the teacher attrition rate, the percent of teachers with less than four years of experience, the percent of teachers with more than 10 years of experience, the student-teacher ratio, and the student attendance rate. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

Figures

FIGURE 1. WITHIN DISTRICT TRENDS – DISTRICTS WHRE LEVIES PASSED VS. FAILED



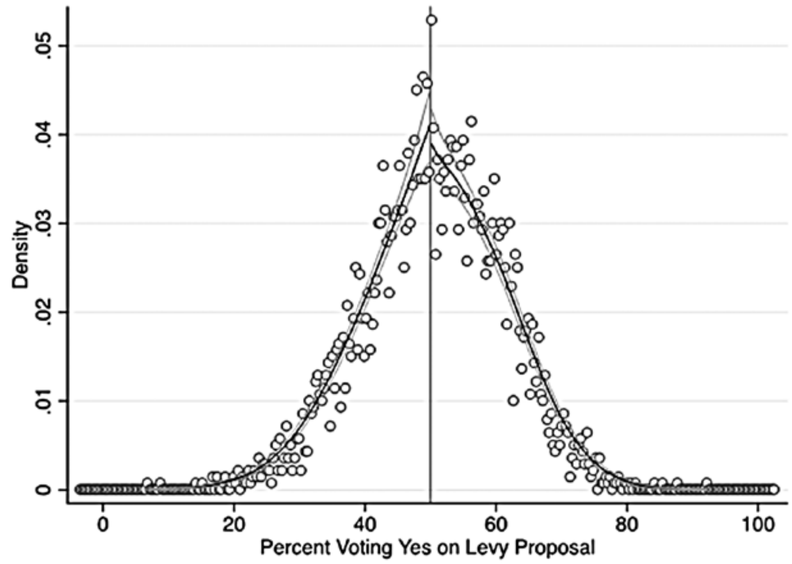
Note. The figures present trends separately for districts where levies passed (solid line) and districts where levies failed (dashes). These within district changes are presented separately for the panel RD and differences-in-differences models presented in tables 3-4.

Appendix A: Testing the Validity of RD Assumptions

A regression discontinuity design recovers the causal effect of an election outcome under the identifying assumption that this outcome is essentially random in the neighborhood of the 50 percent vote threshold determining passage or failure. However, if school boards or district officials can manipulate the results of tax referenda, then the design is invalid because the outcome of each levy vote might be correlated with unobservable confounders. For example, if more competent superintendents have an ability to precisely manipulate vote share to reach the necessary 50 percent of votes, our estimated treatment effect might be biased by unobserved superintendent competence. The incidence of manipulation in U.S. elections is extremely rare (Eggers et al, 2015), but we nonetheless checked for violations of RD assumptions.

Our first validity check employs McCrary's (2008) test for detecting manipulation of the running variable (i.e., the percent of votes cast in support of each levy, which we center at 50 in the analysis). Under the assumption of no manipulation, the density of the running variable will be smooth across the 50 percent vote threshold. Manipulation, on the other hand, should lead to a density that is greater just to the right of the threshold than it is just to the left of it. In other words, if district officials can precisely manipulate the vote share near the 50 percent vote threshold, then we expect to observe more districts with levies that just passed than districts with levies that just failed. As Table A1 indicates, we find no such discontinuity.

FIGURE A1. TEST OF DISCONTINUITY IN DENSITY OF VOTE SHARE



Note. The figure presents the results of the McCrary (2008) test for discontinuity in the density of the running variable near the cutoff. The red vertical line is the 50 percent vote threshold that determines whether a levy passes or fails. The open circles are locally weighted densities of the running variable, and the local estimates of the density on either side of the cutoff are displayed with bolded black lines. The associated 95 percent confidence intervals of these estimates are displayed with the lighter lines and indicate that there is no statistically significant difference in the density at the cutoff.

Another way to test the “as-if random” assumption of the RD design is to examine whether districts on either side of the cutoff differ in terms of observable characteristics. We tested for such differences using all district-level variables we feature in this study, as well as additional variables capturing the characteristics of districts’ teachers and students. We used values of these variables measured in the year before the election $(f-1)$ and employed the specifications of the running variable we feature in the main text. The results confirm the validity of the RD design, as the number of significant differences between districts with failing and passing referenda is actually below what one would expect by random chance. These results are present in Table A1 below.

Table A1. Covariate Balance Tests						
	Linear (Full Sample)		Quad (Full Sample)		Linear (Local Sample)	
	N	Coef (SE)	N	Coef (SE)	N	Coef (SE)
Admin. Expenditures Per Pupil	29518	6.645	29518	21.83^	15746	26.15
		(8.341)		(12.76)		(16.16)
Building Supp. Expend. Per Pupil	29516	1.592	29516	0.335	14024	23.56
		(13.51)		(11.08)		(22.36)
Federal Revenue Per Pupil	29912	19.00	29912	0.0241	17003	2.746
		(23.63)		(7.463)		(16.67)
Instructional Expend. Per Pupil	29507	-24.57	29507	-17.01	18073	-22.80
		(18.06)		(13.40)		(31.68)
Local Revenue Per Pupil	29912	-13.35	29912	5.356	15415	58.54
		(86.97)		(63.95)		(124.2)
Other Expenditures	29507	-4.392	29507	-3.578	16370	28.54
		(17.50)		(14.06)		(28.36)
Property Tax Revenue Per Pupil	29912	-11.00	29912	-22.69	18583	10.12
		(25.84)		(19.20)		(52.08)
Pupil Support Expend. Per Pupil	29521	-3.923	29521	-4.489	16836	7.381
		(7.800)		(6.003)		(12.61)
Staff Support Expend. Per Pupil	29496	-2.289	29496	0.534	16592	-2.107
		(4.378)		(3.215)		(8.632)
State Revenue Per Pupil	29912	39.85	29912	16.52	15817	-129.9
		(107.5)		(83.36)		(225.8)
Total Revenue Per Pupil	29912	45.51	29912	21.90	15695	-61.56
		(146.3)		(107.6)		(268.7)
Total Expenditures Per Pupil	29527	-0.524	29527	-14.07	16725	38.91
		(32.15)		(24.61)		(56.45)
Instruct. Aide-Student Ratio	29188	2.481	29188	-1.709	18056	-2.465
		(33.58)		(27.38)		(39.42)
Insolvency Risk Indicator	29672	0.0133	29672	0.0177	16770	0.0186
		(0.0133)		(0.0108)		(0.0231)
Student Attendance Rate	29760	0.00772	29760	-0.0163	14921	-0.0545
		(0.0290)		(0.0226)		(0.0507)
Counselor-Student Ratio	32158	9.000	32158	2.660	20983	13.66
		(8.792)		(6.779)		(14.05)
Elementary Teacher- Student Ratio	32300	0.313^	32300	0.0791	18676	0.408^
		(0.163)		(0.130)		(0.226)
Teacher experience: 4-10 years (%)	27498	-0.315	27498	0.239	17099	-0.564
		(0.398)		(0.308)		(0.594)
Teacher Experience: >10 years (%)	27498	-0.0515	27498	0.230	17178	-0.321
		(0.609)		(0.479)		(0.987)
Teacher Experience: <4 years (%)	27498	0.370	27498	-0.521	16822	0.595
		(0.657)		(0.512)		(1.107)
Value-Added (ODE)	24796	0.126	24796	0.0239	12271	0.0805
		(0.129)		(0.0902)		(0.230)
Levy Passage	21400	-0.0348	21400	-0.0405	12302	-0.122
		(0.0569)		(0.0416)		(0.0910)
Performance Index	33199	0.00163	33199	0.00859	20050	-0.0182
		(0.0225)		(0.0177)		(0.0422)

Levy Proposal	31911	0.0154	31911	0.0377	18298	0.0105
		(0.0384)		(0.0282)		(0.0627)
Secondary Teacher-Student Ratio	32315	-0.143	32315	-0.0213	18369	-0.485^
		(0.169)		(0.118)		(0.257)
Staff-Student Ratio	27510	0.792	27510	1.926	16061	3.013
		(2.567)		(2.004)		(4.166)
Teacher Attendance	24796	-0.767	24796	-0.413	13624	-3.222
		(1.129)		(0.688)		(2.484)
Average Teacher Experience (Yrs)	24796	0.285	24796	0.135	14724	0.639*
		(0.189)		(0.148)		(0.318)
Teachers with Master’s Degree	24796	-0.918	24796	-0.353	11639	-1.383
		(0.663)		(0.445)		(0.997)
Teacher Salaries (average)	26772	-41.72	26772	-90.06	15242	-96.16
		(246.5)		(185.7)		(319.5)
Teacher Salaries (median)	26772	-176.3	26772	98.97	15020	-441.9
		(275.4)		(194.8)		(559.2)
Number of Teachers (FTEs)	24738	-1.114	24738	0.617	14867	0.387
		(1.695)		(1.360)		(2.632)
Teacher Attrition (Building)	28945	-0.980	28945	-0.320	16297	-0.814
		(0.884)		(0.692)		(1.476)
Teacher Attrition (District)	28945	0.0855	28945	0.455	15877	-0.184
		(0.554)		(0.410)		(0.945)
New Teachers (District)	28947	0.932*	28947	0.458	15972	0.449
		(0.413)		(0.309)		(0.694)
New Teachers (Building)	28947	-0.184	28947	-0.331	16746	-0.260
		(0.791)		(0.626)		(1.319)

Note. The table presents coefficient estimates and standard errors for the indicator of scoring above the 50 percent vote threshold in the year prior to the election. The first and second columns present results using the full sample of referenda from separate OLS regressions that use linear and quadratic polynomials to model the running variable. The third column presents balance tests based on a restricted sample. Standard errors clustered at the district level are in parentheses below coefficient estimates: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

Appendix B: Results for Operational Levies Only

TABLE B1. IMPACT OF PROPERTY & INCOME TAX LEVY FAILURE ON LN(SPENDING PER PUPIL)

	Primary Specification				Sensitivity Checks			
	(1) Instruction	(2) Admin.	(3) Other	(4) Total	(5) Total	(6) Total	(7) Total	(8) Total
2 YRS PRIOR	0.00997* (0.00415)	-0.00599 (0.00947)	0.00856 (0.00676)	0.00446 (0.00450)	-0.00549 (0.00777)	0.00360 (0.00335)	0.00153 (0.00546)	0.00112 (0.00207)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-0.00515 (0.00389)	-0.00937 (0.00838)	-0.00267 (0.00743)	-0.00572 (0.00428)	-0.00288 (0.00702)	-0.00536 (0.00334)	-0.000870 (0.00492)	-0.00794*** (0.00213)
1 YR AFTER	-0.0138* (0.00569)	-0.00937 (0.0119)	-0.0301** (0.00933)	-0.0166** (0.00596)	-0.0240* (0.0110)	-0.0172*** (0.00488)	-0.0143* (0.00643)	-0.0270*** (0.00319)
2 YRS AFTER	-0.0178** (0.00664)	0.000953 (0.0121)	-0.0433*** (0.0111)	-0.0231** (0.00703)	-0.0278* (0.0122)	-0.0261*** (0.00560)	-0.0166* (0.00782)	-0.0330*** (0.00375)
3 YRS AFTER	-0.0165* (0.00721)	-0.00969 (0.0118)	-0.0309** (0.0114)	-0.0199** (0.00712)	-0.0263* (0.0114)	-0.0247*** (0.00566)	-0.0175* (0.00778)	-0.0290*** (0.00402)
4 YRS AFTER	-0.00862 (0.00776)	-0.0101 (0.0142)	-0.0210^ (0.0119)	-0.00903 (0.00772)	-0.0114 (0.0120)	-0.0156* (0.00616)	-0.00622 (0.00841)	-0.0239*** (0.00535)
5 YRS AFTER	-0.00950 (0.00917)	-0.00143 (0.0156)	-0.00767 (0.0131)	-0.00322 (0.00940)	-0.00681 (0.0144)	-0.0126^ (0.00652)	-0.00219 (0.00920)	-0.0220*** (0.00638)
6 YRS AFTER	0.00747 (0.00989)	-0.00170 (0.0219)	-0.00118 (0.0157)	0.00500 (0.00967)	-0.0124 (0.0173)	-0.00751 (0.00691)	0.0111 (0.00987)	-0.0207*** (0.00591)
N	19,853	19,857	19,853	19,861	11,476	19,861	11,619	19,861
District Count	526	526	526	526	468	526	469	526
Levy Count	2,770	2,771	2,770	2,771	1,596	2,771	1,617	2,771
Mean of DV	8.58	7.06	8.01	9.16	9.17	9.16	9.17	9.16
MODEL	RD	RD	RD	RD	RD	RD	RD	DID
Specification	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Linear	Linear	N/A
Restricted Bandwidth	No	No	No	No	Yes	No	Yes	N/A
Levy Type	Operational	Operational	Operational	Operational	Operational	Operational	Operational	Operational

Note. The results above are from models estimating the impact of operational levy failure (as opposed to passage) on logged per-pupil district expenditures. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE B2. IMPACT OF TAX LEVY FAILURE ON STUDENT ACHIEVEMENT (OPERATIONAL LEVIES ONLY)

	State "Value Added" Estimate (District SDs)			State Performance Index (District SDs)		
	(1)	(2)	(4)	(5)	(6)	(8)
2 YRS PRIOR	-0.0895 (0.298)	-0.0864 (0.115)	-0.00442 (0.0671)	0.0260 (0.507)	-0.0167 (0.0219)	0.00710 (0.0151)
1 YR PRIOR	--	--	--	--	--	--
ELECTION YR	-0.128 (0.240)	-0.121 (0.102)	-0.0614 (0.0665)	0.0809* (0.0390)	0.00622 (0.192)	-0.0159 (0.0120)
1 YR AFTER	-0.0095 (0.247)	-0.114 (0.107)	-0.0879 (0.0669)	-0.0183 (0.0465)	-0.0481* (0.0207)	-0.0524*** (0.0150)
2 YRS AFTER	-0.104 (0.209)	-0.140 (0.0911)	-0.0977 (0.0632)	0.0339 (0.0515)	-0.0560* (0.0233)	-0.0480** (0.0171)
3 YRS AFTER	-0.197 (0.260)	-0.188^ (0.0985)	-0.0165 (0.0653)	-0.0116 (0.0546)	-0.0812** (0.0256)	-0.0498* (0.0179)
4 YRS AFTER	-0.118 (0.253)	-0.0366 (0.103)	0.0209 (0.0689)	0.0462 (0.0654)	-0.0567* (0.0276)	-0.0407* (0.0194)
5 YRS AFTER	0.113 (0.254)	-0.121 (0.112)	-0.0119 (0.0757)	0.0395 (0.0763)	-0.0733* (0.0290)	-0.0449* (0.0216)
6 YRS AFTER	-0.0691 (0.254)	0.00810 (0.122)	-0.0213 (0.0816)	0.0645 (0.0811)	-0.0382 (0.0321)	-0.0381 (0.0239)
N	7,791	24,796	16,380	12,595	22,161	22,161
District Cnt	453	571	527	469	527	527
Levy Cnt	1,372	4,324	2,848	1,617	2,848	2,848
Mean DV	0.0124	0.0251	0.0285	0.0279	0.0607	0.0607
MODEL	RD	RD	DID	RD	RD	DID
Specif.	Quad.	Linear	N/A	Quad.	Linear	N/A
Restricted Bandwidth	Yes	No	N/A	Yes	No	N/A
Levy Type	Operational	Operational	Operational	Operational	Operational	Operational

Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on district performance measures standardized by year. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

Appendix C: Cross-sectional Local Estimates

As a comparison to our panel approach that employs proposal and year fixed effects, we estimated the effect of referendum failure on our outcomes of interest using a cross-sectional regression discontinuity design that does not control for any differences between passing and failing proposals—that is, without accounting for proposal and year fixed effects. We separately estimated the local linear regression of the outcome on the percentage voting yes and the failure indicator for the election year and the six years following the election. We used the Calonico, Catteneo, and Titiunuk local RDD estimator, which selects the bandwidth to minimize asymptotic mean squared error. Using the rdrobust package in Stata, we used a triangular kernel, a linear polynomial to compute the point estimate, a quadratic polynomial to correct for the bias, and scaled the regularization factor by 1. Note that the results below show effects consistent with the results we provide in the main text, but the estimates are very noisy—particularly (and understandably) when it comes to the value-added results. Thus, the parametric approach we feature in the text is important for estimating the quantities of interest with greater precision.

TABLE C1. Cross-sectional Analysis using Local RDD Estimator

	N (From Revenue Model)	Total Per Pupil Revenue (2010 Dollars)	Total Per Pupil Expenditures (2010 Dollars)	Value-Added (District SDs)	Perf. Index (District SDs)
ELECTION YR	2,045	-63.88 (199.0)	97.30 (117.6)	-0.184 (0.125)	0.0507 (0.0709)
1 YR AFTER	1,984	-369.8^ (216.1)	-12.18 (121.2)	0.164 (0.121)	-0.0117 (0.0659)
2 YRS AFTER	1,705	-293.0 (282.2)	-84.74 (126.7)	-0.0674 (0.0880)	-0.0172 (0.0638)
3 YRS AFTER	1,399	428.9 (321.8)	1.612 (144.4)	-0.132 (0.0944)	-0.0298 (0.0624)
4 YRS AFTER	1,609	379.3 (242.1)	140.7 (159.8)	0.120 (0.0837)	0.0261 (0.676)
5 YRS AFTER	1,305	73.43 (248.0)	105.9 (173.3)	0.0612 (0.0979)	-0.00702 (0.0727)
6 YRS AFTER	821	12.35 (320.9)	61.47 (177.6)	0.00930 (0.0888)	0.0104 (0.787)
Levy Type		Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. Each coefficient was estimated using a separate local linear regression model. The models account for no covariates. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

Appendix D: Highest Vote Share Only – ITT and TOT effects

The tables below compare the "intent to treat" (ITT) effects to "treatment on the treated" (TOT) effects, which we estimated using Cellini et al.'s (2010) recursive estimator.

TABLE D1. ITT vs. TOT Using Levy w/ Highest Vote Share if a District Had More Than One Proposal on the Ballot in a Year

	Intent to Treat (ITT)				Treatment on the Treated (TOT)				
	Total Per Pupil Revenue (2010 Dollars)	Total Per Pupil Expenditures (2010 Dollars)	Value-Added (District SDs)	Perf. Index (District SDs)	Total Per Pupil Revenue (2010 Dollars)	Total Per Pupil Expenditures (2010 Dollars)	Value-Added (District SDs)	Value-Added (District NCEs)	Perf. Index (District SDs)
2 YRS PRIOR	71.66	22.74	-0.0138	-0.00302	--	--	--	--	--
	(146.4)	(30.52)	(0.114)	(0.0213)					
1 YR PRIOR	--	--	--	--	--	--	--	--	--
ELECTION YR	-301.8*	-54.95	-0.0495	-0.0129	-301.6306*	-55.5671	-0.0495	-0.0662	-0.0129
	(139.7)	(36.17)	(0.0945)	(0.0181)	(139.6999)	(36.2098)	(0.0946)	(0.0965)	(0.0181)
1 YR AFTER	-674.1***	-144.8**	-0.00170	-0.0418*	-935.5254**	-192.9232**	-0.0445	-0.0634	-0.053
	(187.4)	(46.57)	(0.104)	(0.0211)	(289.864)	(69.2296)	(0.1636)	(0.1673)	(0.0335)
2 YRS AFTER	-832.1***	-216.9***	-0.159^	-0.0457^	-1764.069***	-405.1929***	-0.2171	-0.2604	-0.0968^
	(224.5)	(53.36)	(0.0954)	(0.0233)	(498.8148)	(110.3296)	(0.245)	(0.2487)	(0.0556)
3 YRS AFTER	-255.7	-238.8***	-0.0822	-0.0575*	-2278.127**	-688.9621***	-0.3071	-0.3815	-0.1677^
	(273.2)	(58.26)	(0.0995)	(0.0253)	(817.986)	(174.4476)	(0.3822)	(0.389)	(0.0881)
4 YRS AFTER	81.06	-194.4**	0.0458	-0.0336	-3065.448*	-1047.416***	-0.3401	-0.4382	-0.2426+
	(249.8)	(65.79)	(0.106)	(0.0272)	(1262.536)	(275.088)	(0.5922)	(0.6021)	(0.1379)
5 YRS AFTER	146.7	-159.5*	-0.0890	-0.0472	-4519.793*	-1583.279***	-0.6217	-0.7847	-0.384^
	(276.9)	(72.49)	(0.117)	(0.0293)	(1983.924)	(434.545)	(0.9217)	(0.9368)	(0.2152)
N	20,444	20,126	17,395	22,925					
District Cnt	568	572	571	571					
Levy Cnt	2,999	2,944	3,019	3,019					
Mean DV	11,491.60	9,600.30	0.02	0.12					
MODEL	RD	RD	RD	RD	RD	RD	RD	RD	RD
Specif.	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Quadratic
Rest. Band.	No	No	No	No	No	No	No	No	No
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. TOT estimates are based on the recursive estimator from Cellini et al. (2010). Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

TABLE D2. ITT vs. TOT Using Levy w/ Highest Vote Share if a District Had More Than One Proposal on the Ballot in a Year

	Intent to Treat (ITT)				Treatment on the Treated (TOT)				
	Total Per Pupil Revenue (2010 Dollars)	Total Per Pupil Expenditures (2010 Dollars)	Value-Added (District SDs)	Perf. Index (District SDs)	Total Per Pupil Revenue (2010 Dollars)	Total Per Pupil Expenditures (2010 Dollars)	Value-Added (District SDs)	Value-Added (District NCEs)	Perf. Index (District SDs)
2 YRS PRIOR	9.395 (202.8)	-36.40 (40.40)	-0.191 (0.159)	0.00248 (0.0280)	--	--	--	--	--
1 YR PRIOR	--	--	--	--	--	--	--	--	--
ELECTION YR	-417.6* (200.3)	-55.33 (46.58)	-0.136 (0.125)	0.00762 (0.0242)	-418.1317* (200.2414)	-55.5948 (46.6292)	-0.1365 (0.1246)	-0.1537 (0.1274)	0.0076 (0.0242)
1 YR AFTER	-804.6** (257.4)	-168.6** (63.31)	-0.153 (0.141)	-0.0366 (0.0275)	-1167.016** (408.7274)	-216.2411* (92.6022)	-0.2713 (0.2141)	-0.2987 (0.2186)	-0.03 (0.0443)
2 YRS AFTER	-804.0* (315.7)	-273.3*** (70.05)	-0.226^ (0.127)	-0.0289 (0.0298)	-1981.937** (708.2944)	-481.2002** (150.4027)	-0.5157 (0.3212)	-0.5719^ (0.3263)	-0.0518 (0.0719)
3 YRS AFTER	-31.02 (377.8)	-232.9** (76.04)	-0.207 (0.130)	-0.0293 (0.0334)	-2377.177* (1158.933)	-757.9249** (240.4505)	-0.8151 (0.5017)	-0.915^ (0.5108)	-0.0833 (0.1144)
4 YRS AFTER	275.8 (348.0)	-159.5* (79.32)	-0.0639 (0.132)	-0.00558 (0.0364)	-3167.498^ (1787.859)	-1111.32** (369.3943)	-1.1255 (0.7737)	-1.2666 (0.7867)	-0.1077 (0.179)
5 YRS AFTER	9.381 (378.3)	-114.0 (99.19)	-0.170 (0.144)	-0.0125 (0.0402)	-4977.68^ (2794.746)	-1657.562** (574.9968)	-1.8042 (1.2037)	-2.0292^ (1.2237)	-0.1667 (0.2793)
N	20,444	20,126	17,395	22,925					
District Cnt	568	572	571	571					
Levy Cnt	2,999	2,944	3,019	3,019					
Mean DV	11,491.60	9,600.30	0.02	0.12					
MODEL	RD	RD	RD	RD	RD	RD	RD	RD	RD
Specif.	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic
Rest. Band.	No	No	No	No	No	No	No	No	No
Levy Type	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap	Op. & Cap

Note. TOT estimates are based on the recursive estimator from Cellini et al. (2010). Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05; ** p<0.01; *** p<0.001.

Table D3. Treatment-on-the-treated (TOT) Estimates of Levy Failure

	Expenditures (2010 Dollars)	Annual Achievement Gains (Standard Deviations)	Annual Achievement Gains (Days of Learning)	Implied Annual Days of Learning per $1,000 in cuts
Linear Specification				
1 year after levy failure	-$193	-0.003 SDs	-1 day	-8 days
2 years after levy failure	-$405	-0.012 SDs	-6 days	-15 days
3 years after levy failure	-$689	-0.018 SDs	-9 days	-13 days
4 years after levy failure	-$1,047	-0.021 SDs	-10 days	-10 days
5 years after levy failure	-$1,583	-0.037 SDs	-18 days	-12 days
Quadratic Specification				
1 year after levy failure	-$216	-0.014 SDs	-7 days	-32 days
2 years after levy failure	-$481	-0.027 SDs	-13 days	-28 days
3 years after levy failure	-$758	-0.043 SDs	-21 days	-28 days
4 years after levy failure	-$1,111	-0.060 SDs	-29 days	-26 days
5 years after levy failure	-$1,658	-0.096 SDs	-47 days	-28 Days

Note. TOT effects estimated using recursive estimator from Cellini et al. (2010) using linear and quadratic specifications of the running variable. A complete set of results is available in Table A6 and Table A7 in the appendix.