School District Tax Referenda, Spending Cuts, and Student Achievement*

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Abstract

Many public school districts rely on revenue from local taxes approved via popular referendum. The dynamics of these referendum elections therefore could have a significant impact on school district administration and student learning. We estimated these effects using data on more than 4,200 referenda held in Ohio districts between 2003 and 2013. The results indicate that, in the years immediately following elections, referendum failure (instead of passage) led to decreases in district expenditures of around $200 per pupil and declines of approximately 0.005 standard deviations in student-level achievement, which corresponds to about 2-3 annual "days of learning." However, these initial impacts generally dissipate within 4-6 years, as districts eventually secure voter approval for tax levies and expenditures rebound. The analysis also examines administrative mechanisms that might explain these results and offers insights into district responses to fiscal stress, the causal impact of spending cuts on student achievement, and the transaction costs associated with using direct democracy to make school funding decisions.

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1. Introduction

Over 40 percent of all U.S. school district funding comes from local sources (Cornman et al., 2011). To collect such revenue, districts in many states must propose property and income tax rates directly to their residents via the referendum process. Raising local funds in this manner amounts to a repeated bargaining game between district leaders and residents—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. This latter scenario might lead districts to return to the ballot multiple times in the hope of eventually gaining voter approval. Importantly, such over-time dynamics of the referendum process could have significant impacts on school district administration and student learning.

We explore these potential impacts by analyzing more than 4,200 tax referenda proposed by Ohio school districts between 2003 and 2013. The analysis employs multiple identification strategies—including panel data methods and regression discontinuity (RD) designs—to estimate the impact of referendum failure or passage on school district revenues, expenditures, and student achievement up to six years after the vote. Specifically, we employ a panel RD method similar to Cellini et al. (2010) to estimate the dynamic impact of referenda outcomes, as well as difference-in-differences models to explore the generalizability of these estimates. The analysis focuses primarily on property tax levies that raise funds for district operations, but we also examine income tax measures and property tax levies used to finance long-term capital improvements. To our knowledge, this study is the first to use an RD design to estimate the impact of operational tax referendum outcomes that determine a significant proportion of school district revenues used to fund day-to-day activities.

The results indicate that Ohio districts where tax measures failed subsequently spent less per pupil and had lower student achievement than districts where measures passed. The negative effect is generally around 0.03-0.10 standard deviations in district-level educational quality (as measured by an achievement-focused performance index) and, at its worst, around 0.005 standard deviations in student-level achievement in math and reading, corresponding to 2-3 fewer “days of learning.” But the analysis also indicates that this negative impact of referendum failure decreases over time and often becomes statistically indistinguishable from zero four to six years after levy elections are held. This latter finding is explained by a heightened probability of subsequent levy passage and a corresponding rebound in district revenues. Indeed, districts in which a levy failed are likely to pass a tax measure the following year, which enables them to catch up to those districts where levies initially passed.

Further analysis provides insight into some of the mechanisms at work. Ohio districts that placed levies on the ballot were experiencing relative declines in operational expenditures. Districts where levies passed were able to stem further declines, but districts where levies failed implemented cuts on instruction-related expenditures (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). The sudden cuts in instructional spending coincided with the attrition of instructional staff—primarily teachers with under four years of experience—and higher student-teacher ratios.

Additionally, we detect modest declines in student attendance rates, which could be attributable to reduced spending on services such as transportation.

Finally, to quantify the link between spending cuts and student achievement—while accounting for the reality that initial levy failure was followed by passage of a subsequent tax referenda in most districts—we employed the recursive estimator introduced by Cellini et al. (2010) to estimate the "treatment on the treated" effects associated with levy failure. Our most conservative estimates indicate that every $1,000 cut in per-pupil spending is associated with student-level achievement declines of around 0.02 standard deviations per school year—about 12 fewer "days of learning" if one assumes a 180-day school year. It is important to emphasize, however, that these achievement declines are associated with failing tax referenda. Our analysis indicates that these effects are likely linked to sudden spending cuts, but we cannot attribute them completely to these cuts. Other disruptions associated with levy failure also may have an impact.

These results build on extant research that examines school district responses to fiscal stress and tax and expenditure limitations (e.g., see Berne and Stiefel, 1993; Downes and Figlio, 2015) and contribute to the debate regarding the link between per-pupil spending and student achievement (e.g., see Hanushek, 2006; Jackson et al., Forthcoming). What distinguishes this study is the strength of the empirical strategy for identifying the causal impact of fiscal stress on school district administration, as well as its insights on the consequences of direct democracy. For example, the results indicate that voters' inclination to punish low-achieving districts by rejecting their tax levies (Kogan et al., 2016) likely exacerbates achievement gaps between districts. The results also are consistent with the notion that the uncertainty inherent in the bargaining between districts and voters entails transaction costs—including declines in student achievement—as districts and voters settle on tax rates that they believe make them best off. That does not mean that direct democracy institutions are inferior to representative institutions (such as school boards or other local governments) when it comes to raising revenue. For example, there is evidence that the presence of direct democracy in U.S. cities can limit the influence of special interest groups and lower public spending on wages or employment (Matsusaka, 2009). But the results indicate that direct democracy may entail some significant transaction costs.

The paper proceeds as follows. Sections 2 and 3 provide background on K-12 school district finance in the U.S. and Ohio. Section 4 describes our empirical strategy, data, and statistical models. Section 5 presents our main results and explores several mechanisms that link the fiscal impacts of referendum failure to the subsequent declines in achievement. Finally, section 6 discusses some of the implications of our findings.

2. 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).[1] 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—shifted school district funding responsibilities from local sources to state general revenue financed primarily by statewide sales and income taxes. Nevertheless, local sources still account for approximately 44 percent of school district revenues (Cornman et al., 2011).

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

How local taxes are raised varies significantly across states. Thirty-six states have independent school districts that have the power to set property tax rates; districts in seven states rely on other local governments (cities, towns, or counties) for revenues; and the remainder employ some mix of these approaches (McGuire et al., 2015). Local property taxes account for about two-thirds of the revenue that districts raise directly (other taxes account for just over 3 percent), but they are also the primary source of funds for local governments providing revenues to dependent school districts (McGuire et al., 2015). Many school districts with taxing authority must obtain voter approval to set tax rates (U.S. Advisory Commission on Intergovernmental Relations 1995). The process typically entails elected officials making a "take it or leave it" offer to residents, who can vote for or against the proposed tax rate.[2] For school districts with discretion in the timing of tax referenda and the size of proposed tax rates, raising revenues in this manner amounts to a repeated bargaining game between district leaders and residents (Romer and Rosenthal, 1979; Figlio and O'Sullivan, 2001; Barseghyan and Coate, 2014).

3. Ohio School District Finance and Local Tax Referenda

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 (Jackson et al., Forthcoming). Ohio is unusual, however, in that state

[2] This is not always the case. For example, between 1939 and 1968, voters in Florida selected a millage rate and the median became the property tax rate (Holcome and Kenny, 2007).

law governing local property taxes effectively requires school districts to seek the approval of voters more frequently than districts in other states.

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, 2015). Districts may place tax measures on the ballot on four election dates in most years. 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. 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.[3] 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.[4]

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

[3] Since the passage of Proposition 13 in California, many other states have adopted similar property tax limitations (Martin 2008).
[4] 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.

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.

It is worth noting that property tax receipts can never drop below a state-mandated 20 mill[5] floor and that tax rates must exceed one percent to require voter approval. Additionally, districts can have multiple overlapping 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 levy need not lead to substantial declines in district revenues or expenditures.

4. Empirical Strategy

The purpose of this study is to estimate the causal impact of tax referendum failure (instead of passage) on school district administration and student achievement, as well as to gain insights into the underlying mechanisms that explain these effects. We frame the analysis as the impact of failure (as opposed to the impact of passage) because it better reflects Ohio districts' context, where levies are typically proposed to 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 levy failure exacerbating these declines. Thus, deeper cuts that follow levy failure appear to be one of the principal mechanisms that produce the differences between passing and failing districts in our analysis.

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

Our primary identification strategy is based on a regression-discontinuity (RD) design. The design takes advantage of the fact that the election outcome—failure or passage—is essentially random[6] 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 pupil, expenditures per pupil, and student achievement—as well as other variables that capture potential causal mechanisms—at the 50 percent vote cutoff determining levy failure instead of passage.

The models we report are estimated using Cellini et al.'s (2010) panel RD method. These models, described below, employ a panel of tax levies Ohio districts 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.[7] 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).

[6] 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).
[7] 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.

It is important to note that 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 RD model because it provides several advantages. First, the ability to include fixed-effects in the analysis increases 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 models to the more general average treatment effect (ATE) estimates from basic differences-in-differences models, which we also report as a robustness check. Third, panel methods facilitate our analysis and presentation of trends before and after levy failure or passage, which we 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.

4.1 Panel Regression Discontinuity Model

Using the proposal panel we describe above, the analysis compares within-district trends between districts where levies failed and those where levies passed. Specifically, the basic differences-in-differences model takes the following form:

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

where the outcome of interest Y for proposal i during calendar year t and the year relative to the election k is a function of fixed effects for proposals (α_i), calendar years (θ_t), and 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 changes relative to the year prior to the focal election year within each district[8], and the coefficient vector τ^k captures differences in these changes between districts that failed to pass a levy and those that succeeded.

The panel RD model accounts for the relationship between a proposal's vote share and the outcome Y in the differences-in-differences 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), our preferred specification features a 2nd order polynomial to capture the relationship between a proposal's vote share and outcome Y.[9] Additionally, we interacted this 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, our preferred OLS model 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.[10] Additionally, because the vote share is centered, the coefficients τ^k

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[8] The differences are actually within proposals. However, as we demonstrate in Table A6 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.

[9] However, the results are robust to using lower and higher order polynomials.

[10] 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.

capture the impact of levy failure (instead of passage) for each year relative to the year before the election.[11] 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[12] 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). As we discuss below, our tests of this assumption validate our use of the RD design. 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. [13]

Additionally, we validate the estimates from our preferred specification by demonstrating that the results are robust to linear specifications of the running variable and estimation based on a data sample within a restricted bandwidth of the cutoff, which we identified using the method proposed by Calonico, Cattaneo, and Titiunik (2014).[14] Finally, to address concerns regarding the generalizability of the local RD estimate away from the cutoff, we also report the estimates from the basic differences-in-differences model described by equation 1.

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[11] One can think of the RD estimates as representing 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. 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 at will in subsequent years. We have also estimated treatment-on-the-treated (TOT) effects (e.g., see Cellini et al., 2010; Isen 2014), which represent the causal effect of exogenously changing a referendum from passing to failing among the subset of districts that do not subsequently pass a tax referenda in future elections. We report these TOT estimates in the appendix (Table A6).

[12] Table A6 in the appendix reveals that the results are similar if we restrict the sample to one proposal per district in a given year (the proposal that received the highest vote share in that year).

[13] 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. We do not report that analysis in the interest of space.

[14] We also conducted these checks using higher order polynomials, as well as multiple other techniques for identifying bandwidths. The results are qualitatively similar to those we report below.

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 each tax proposal in our dataset, we coded the number of votes cast for and against it. For the period 2008 to 2013, we obtained the vote breakdowns from the Ohio School Board 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 percentage levies receive is bunched tightly around 50 percent support, with approximately two-thirds of levies receiving between 40 percent and 60 percent of votes in favor. It is also worth pointing out that the majority of the levies in our sample (79 percent) are temporary, "fixed length" levies with a median length of five years, and two-thirds are intended to raise funds for operational expenditures.

[Insert Table 1 about here.]

We obtained the primary dependent variables from a number of organizations. Data on school 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 Department of Education (ODE).[15] 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 services" (which includes transportation, counselors, instructional technology, professional development, and the like).

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

[15] All 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.

and captures aggregate proficiency levels on state exams in math and English language arts (administered in grades 3-8) and science and social studies (administered twice in grades 3-8).[16] The estimates of districts' annual value-added—which are available from 2007 through 2014[17]—compare the year-to-year gains a district's students made on state math and reading exams with those of all other students in the state. Unlike district performance measures based on achievement levels, which are confounded by student socioeconomic status and other differences in academic achievement unrelated to school and district quality, the value-added scores control for up to three years of students' previous test scores and, thus, account for student-level factors that may affect student performance (see Chetty, Friedman and Rockoff, 2014). We standardized both metrics to have a mean of zero and standard deviation of one to facilitate comparisons in district quality. Additionally, we estimated models using the unstandardized value-added metric reported as Normal Curve Equivalent (NCE) scores, as this enables us to discuss the results in terms of student-level achievement gains.

Table 2 presents descriptive statistics for these primary variables in the year immediately before each levy election ($f - 1$).[18] 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 spend more per pupil across all

[16] Compared to proficiency rates, the performance index captures wider variation in aptitude by assigning points for four different levels of student proficiency.

[17] 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. There will be some error in this calculation, as each year is not weighted equally in the ODE estimates. Additionally, 2007 value-added are based on scores in just one grade, whereas other years include all grades 4-8.

[18] Statistics in Table 2 are based on the administrative expenditures data file because it has the broadest coverage of our dependent variables.

categories (particularly on instruction), rely more on local revenue, and have 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 the administrative mechanisms that might explain our results, we obtained teacher counts from the Common Core of Data, measures of teacher experience and student counts from district "Cupp" reports available on the Ohio Department of Education website, and we used detailed payroll records available from the Ohio State Treasurer to calculate teacher attrition rates.

4.3 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 levy elections, 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 the 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 employ's 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. We find no such discontinuity.[19]

Another way to test the "as-if random" assumption of the RD design is to examine whether the districts on either side of that cutoff differ in terms of observable characteristics. We tested for such differences using 52 district-level covariates—including all of the 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 RD models we describe below. Only four of 52 regressions yielded a coefficient on the failure indicator that is significant at $p<0.10$, which is about what one expects by chance. [20]

[19] See Figure A1 in the appendix. 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.

[20] As a histogram and rootogram of p-values in the appendix reveal (Figure A2), there is essentially a uniform distribution of p-values between 0 and 0.8 and there are somewhat more results approaching p=1.0 than one would expect—a deviation from the uniform distribution that the hanging rootogram indicates is statistically significant.

5. Impact of Levy Failure on Revenues, Expenditures, and Achievement

We focus on the estimated impact of levy failure (instead of passage) on district revenues per pupil, expenditures per pupil, and student achievement, respectively. For each of these three sets of outcomes, we report the results from the panel regression discontinuity model (equation 2), 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, which we identified using the method proposed by Calonico, Cattaneo, and Titiunik (2014). Additionally, we present the results of the differences-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 of the difference in trends between districts where levies failed and where levies passed relative to the year before the focal election year, and we do so for up to two years prior to the election and up to six years after the election.

5.1 Impact on Per-pupil Revenues and Expenditures

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, correspond to percent differences in the outcomes between districts with failing and passing levies, after controlling for referendum 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 levies failed and where levies 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 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, these districts were essentially identical just prior to the levy election.

[Insert Table 3 about here.]

The results in Table 3 reveal that districts in which operational or capital levies failed had total per-pupil revenues that were approximately 4 percent lower (over $500 lower per pupil[21]) 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 decrease by a comparable magnitude. 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.[22]

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[21] Table A1 in the appendix reports the results in absolute 2010 dollars per pupil.

[22] These election year effects could be due to our matching of calendar year levies to district school and fiscal years. To explore this possibility we estimated models separately for levies that occurred after July (where the levy outcome is clearly after the fiscal year) and levies that occurred before July (when levy outcomes might influence state funding at the end of the fiscal year). But the election year effects remained. We also tested to see if this was related to state matching funds for capital expenditures, which could also explain declines in state funding in subsequent years. Specially, estimated revenue models based solely on a sample of operational levies (see Table A2 in the appendix). Again, however, the election-year effect remains for state expenditures.

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 similar negative effects on operational expenditures. However, the relative declines in per-pupil expenditures due to levy 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 during those years[23]), as opposed to 3-4 percent across two years. This indicates that districts smooth out the revenue shocks due to levy failure by translating the revenue loss into smaller cuts in expenditures that persist longer and continue to be felt even as revenues recover. The results also indicate that these overall negative 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). Districts clearly cut less essential services at a higher rate, which is consistent with the other findings in the literature on fiscal stress.

[Insert Table 4 about here.]

The results reveal something else that speaks to the generalizability of the RD estimates: the revenue effects persist longer in the difference-in-differences model than in the RD models. In other words, for those districts close to the treshold for passage, differences in expenditures taper off more quickly than they do when comparing all districts with failing and passing levies—which includes districts that must overcome a larger deficit in voter support to pass

[23] See Table A3 in the appendix for models estimated using expenditures in 2010 dollars per pupil.

subsequent levies. It is also worth noting that the spending cuts are similar if we limit the analysis to operational levies.[24]

Finally, Figure 1 presents figures summarizing the per-pupil revenue and expenditure results. The figures present trends in total revenues (panels a and b) and operational expenditures (panels c and d) separately for districts in which tax measures passed (solid lines) and those in which tax measures failed (dashed lines). The predicted values from the RD models (panels a and c) illustrate how the spending in districts where levies failed immediately falls behind but that eventually catches up (after districts pass a subsequent levy). The trends are similar for the differences-in-differences models (panels b and d). Finally, the figure illustrates how relative to all districts in the dataset (whether or not they had a levy on the ballot)[25] districts in which levies passed merely staved off further expenditure declines instead of increasing their relative expenditures. Indeed, additional analyses indicate that districts in which levies failed were 4-6 percentage points more likely to be identified as at risk of financial insolvency by the state.[26]

[Insert Figure 1 about here.]

5.2 The Dynamics of Levy Passage and Defeat

Table 5 presents helps explain the dynamics in per-pupil revenues and expenditures presented in Tables 3 and 4. It presents the results of models estimating the probability that districts propose a levy and the probability that districts pass a levy. The results reveal that districts where levies failed were far more likely than districts where they passed to propose and secure passage of a subsequent measure in the year after levy failure. Indeed, districts where levies failed were over

[24] See Table A4 in the appendix.
[25] These trends for districts with levies on the ballot are relative to all 580 districts included in the analysis.
[26] These results are available from the authors upon request.

50 percentage points more likely to pass a levy the following year. Additionally, the results reveal that the estimated effects are more pronounced in the RD models—that is, when comparing districts where the election was decided by narrow margins.

[Insert Table 5 about here.]

Thus, it appears that the temporary revenue and expenditure effects we detect in the RD analysis are partly attributable to these districts having approximately a 50-50 chance of passing a subsequent levy—which, we should note, is just below the mean probability of passing a levy across all Ohio districts. For the larger sample of districts examined in the differences-in-differences models, we still see an increase in the probability of passing a subsequent levy, but this effect is roughly half the size of the RD estimates. This likely reflects the reality that districts where initial levies fail by larger margins have a harder time overcoming this electoral disadvantage in future elections. Interestingly, the DID models also indicate that districts in which levies failed are also less likely to pass levies four and five years after the election.

One initially puzzling finding in Table 5 is 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 prior 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 $f - 1$. In other words, there is a ceiling effect that limits the increase in probability of levy proposal but not passage—because many districts propose a levy in the baseline year, but very few of them pass one.

The dynamics of levy proposal, initial defeat, and subsequent passage we document in Table 5 are consistent with three possible political scenarios. First, districts may change the nature of the tax proposals they make to voters after observing a defeat by, for example, reducing the magnitude of the levy. Second, voters might alter their behavior. For instance, voters who initially oppose a levy may observe the resulting cuts to services that follow its defeat and decide to change their vote to support a subsequent tax measure that appears on the ballot a year or two later. Alternatively, elections themselves may be stochastic—the composition of voters and the level of turnout can change significantly between elections—and districts can take advantage of this over-time variation by repeatedly proposing levies election after election, until favorable electoral conditions help them achieve victory.

Our data do not allow us to convincingly isolate these mechanisms, but descriptive analyses yield results consistent only with the last explanation.[27] In the aggregate, we find no evidence that districts generally reduce the size of their tax proposal following defeat. Nor do we see that districts that reduce the size of their proposals improve their odds of passing a subsequent levy compared to districts that make no reductions. We also observe no association between the nature and magnitude of spending cuts following initial levy defeat and the probability of passage for a subsequent tax measure. These results suggest that differences in outcomes across elections are not driven by strategic behavior by districts or voters and may be due simply to the unpredictability of these local elections. In other words, if a district puts a tax

[27] These analyses are not presented here to conserve space but are available from the authors upon request.

measure on the ballot often enough, it has a good chance of eventually riding a favorable electoral wave to passage.

5.3 Impact on Student Achievement

The results above indicate that the failure of a district tax measure has a significant, negative short-term impact on district revenues and expenditures per pupil. A substantial portion of these expenditure effects are concentrated on instructional spending. Thus, there is reason to believe that levy failure might have an impact on the quality of the education provided by the affected districts. We explore this possibility by applying the same empirical strategy to models using district-level student achievement as dependent variables. Specifically, we estimated models that employ a district-level measure of yearly student-level achievement gains (the "value added" measure) and models that employ the state's district "performance index." 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. It also is worth noting that we conducted additional analyses of enrollment trends and found no evidence of significant changes in the composition of the student body following levy failure, so the state "performance index" may be an adequate measure given our focus on within-district changes.

[Insert Table 6 about here.]

Table 6 presents the results for 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, the value-added results provide less statistical power and often fail to reach conventional levels of statistical significance, but effect sizes across both achievement measures generally range from about 0.03 to 0.10 districts standard deviations in the years immediately following an election year.[28]

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.[29] 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 with failing levies.[30]

[Insert Table 7 about here.]

5.4. Mechanisms Linking Spending Cuts and Achievement Declines

The results above provide strong evidence that levy failure has a significant, albeit substantively modest, negative impact on student achievement, and they provide suggestive evidence that cuts to instructional and other expenditures (such as transportation) are responsible. We explore these mechanisms more thoroughly in this section. Specifically, we consider whether cuts to

[28] As Table A5 in the appendix reveals, these estimates increase in size if we limit the analysis to operational levies (see Table A5 in the appendix)
[29] We obtained these numbers by dividing the coefficient estimates by 21.063.
[30] 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.

instructional expenditures led to teacher attrition and bigger student-teacher ratios. Additionally, we examine whether student attendance rates suffered, possibly as a result of cuts in other district services such as transportation. To focus on tax levy elections that had the most direct connection to such district operations, we limit this analysis to tax measures for operational funding.[31] 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.]

The results in Table 8 indicate that, in both RD and DID models, levy failure is associated with teacher attrition rates up to 1.5 percentage points higher (columns 1-2) and 0.3-0.4 more students per teacher (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 is up to 3 percentage points lower in districts with failing levies, whereas the proportion of teachers with more than ten years of experience is up to 2.7 percentage points higher, suggesting that districts responded to their resource constraints by letting go of less experienced teachers.[32] The results also indicate that student attendance rates were up to 0.9 percentage points lower in districts with failing levies—perhaps because transportation and other services were less available to those students. Finally, the table reveals that many of these effects persist through the sixth post-election year. Given strong evidence in the existing literature that both teacher turnover (e.g., Ronfeldt, Loeb, and Wyckoff, 2013) and student attendance (e.g.,

[31] 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.

[32] Since research indicates that teacher performance, as measured by student test score value-added, improves with experience (e.g., Harris and Sass 2011), these compositional effects likely increase average teacher quality, mitigating some of the negative consequences of levy failure. 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.

Marcotte, 2007; Marcotte and Hemelt, 2008) affect student achievement, these results document some of the likely mechanisms through which the negative fiscal impacts of levy failure likely harm student learning.

5.5. Spending Cuts and Student Achievement

To quantify the impact of spending cuts on student achievement more precisely, we re-estimated these models using Cellini et al.'s (2010) "recursive" TOT estimator that accounts for non-compliance—that is, districts passing a levy after an initial failure.[33] Non-compliance is very high in our application: More than 60 percent of districts where a tax referendum initially fails pass a subsequent measure within a year, and this rises to more than 90 percent within three years. Ignoring these dynamics thus understates the impact of initial failure for the small subset of districts that fail to secure passage for a subsequent levy. The TOT estimates focus on effects in this subset of districts.

Table 9 presents the coefficient estimates from models employing linear specifications (top panel) and quadratic specifications (bottom panel) of the running variable. Specifically, we present estimates of cuts in per-pupil spending in 2010 dollars and student-level achievement effects in student-level standard deviations. The table also reports these effects in terms of "days of learning," which we calculated using the procedure we describe above. As the table indicates, the results from the linear specification indicate that five years after the election—after the disruptions from levy failure have likely occurred—every $1,000 in spending cuts is associated with about 12 fewer annual days of learning, whereas the model employing a quadratic specification indicates that such cuts are associated with 26 fewer annual days of learning.

[33] The right panels in Table A6 and Table A7 in the appendix presents the results of TOT models we estimated using the recursive method derived in Cellini et al. (2010).

[Insert Table 9 about here.]

6. Summary and Conclusion

The analysis employed RD and panel methods to estimate the impact of levy failure (instead of passage) across nearly all Ohio districts that placed a tax measure on the ballot between 2003 and 2013. Importantly, Ohio's heavy reliance on local tax measures to fund public schools enabled us to identify this impact using a sample of districts that is representative of districts throughout the country in terms of their reliance on local sources of revenue, spending per-pupil, and demographic characteristics. The results indicate that levy failure led to large relative declines in district revenues and expenditures per pupil—and ultimately, modest declines in student achievement. Further analysis reveals that the relative cuts to instructional spending corresponded to teacher attrition—concentrated among teachers with four years of experience or less—as well as increases in student-teacher ratios. Additionally, the cuts to other district services correspond to lower student attendance rates, which is consistent with the possibility that cuts to services such as transportation funding also affected the ability of districts to educate students.

Our results are similar to those produced by research on fiscal stress and tax and expenditure limits. We find that electorally induced fiscal stress leads districts to cut services—and that these cuts are disproportionately targeted at what are perceived to be less essential services (Berne and Stiefel, 1993). Additionally, like much of this literature, we find that fiscal stress is associated with cuts to instructional spending, higher student-teacher ratios, and declines in student achievement (e.g., see Downes and Figlio, 2015, Figlio 1997). However, contrary to some of this research (e.g., Nguyen-Hoang, 2010), we do not find that districts spared spending on district administration. We cannot rule out the possibility that the negative achievement effects are driven by electorally induced revenue instability or uncertainty (e.g., see Loeb, Bryk, and Hanushek, 2008; Lavertu and St. Clair, 2015) as opposed to lower spending levels, but we do find that trends in expenditures correspond to trends in achievement and various mechanisms linking spending to student achievement, just as Jackson et al. (Forthcoming) do.

What distinguishes our study from others is the strength of our identification strategy and our focus on local levy elections. Although, we know of two other studies that have examined the impact of school district bond measures on student achievement using an RD design (see Cellini et al., 2010; Martorell et al., 2015), we are the first to do so in a state that relies heavily on local democracy to fund school operations as opposed to capital spending—and, as our analysis reveals, these local decisions have a significant impact on state and federal funding.[34] Thus, not only does this study provide an important contribution to the existing literature on tax and expenditure limitations and cut-back budgeting, it also illustrates the significant transaction costs of direct democracy. Districts in which levies failed were very likely to ultimately get approval of a subsequent levy of sufficient size to close the revenue gap.

We also provide suggestive evidence that this occurred for reasons that had little to do with how the districts responded to initial levy failure: Voters ultimately changed their mind and approved subsequent tax measures regardless of whether districts reduced the magnitude of the tax increase, and irrespective of the cuts to spending made in the meantime. 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

[34] Isen (2014) uses Ohio data to examine the impact of bond measures on revenues but not on expenditures and student achievement. Other research looks at the impact of facilities spending but does not use an RD design (e.g., Jones and Zimmer, 2001).

influence of special interest groups and lower public spending on wages or employment (Matsusaka, 2009), we find that it can impose some significant costs in the process.[35]

[35] This study cannot distinguish whether these cuts are strategic ploys by districts who wish to secure approval of subsequent levy proposals (Figlio and O'Sullivan, 2001). It should be noted, though, that we find no evidence that the depth of these cuts affects the probability of subsequent voter approval. But the results are consistent with the notion that districts have insufficient slack resources to maintain services, contrary to what voters tend to think. (e.g.,see Ladd and Wilson, 1982; Stein et al., 1983).

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Tables & Figures

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

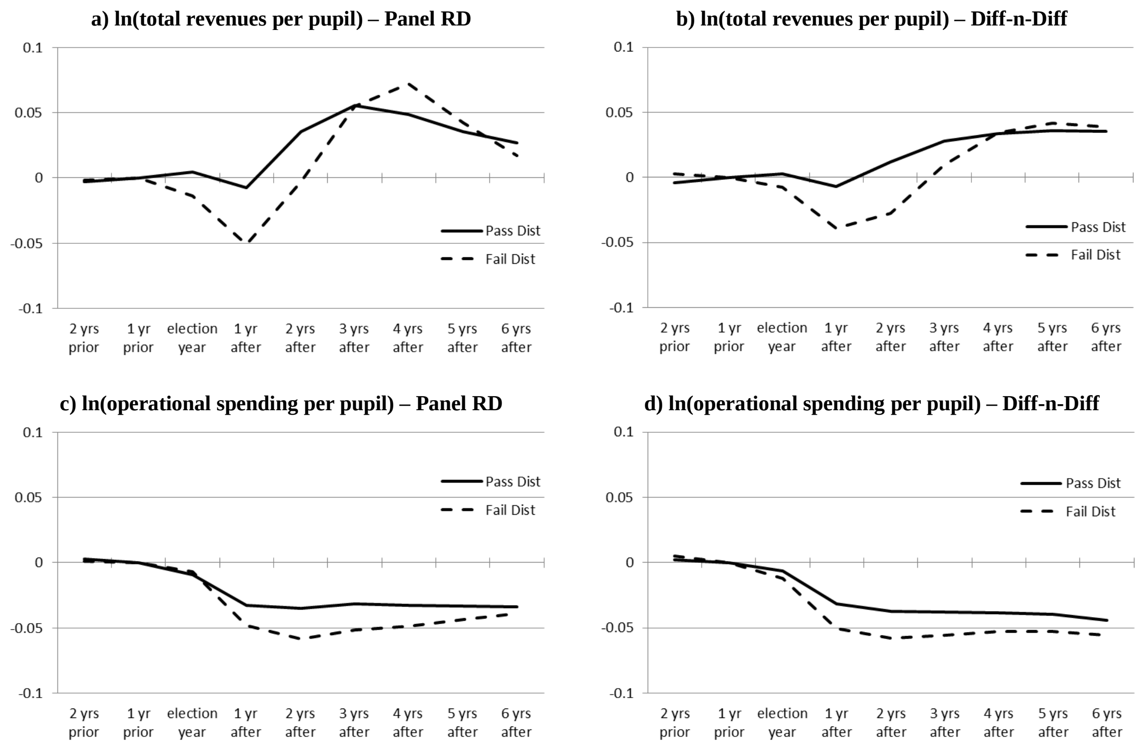
Note. The results above are from models estimating the impact of levy failure (as opposed to passage) on logged district revenues. Standard errors clustered by district are presented in parentheses below the estimated coefficients. P-values were calculated using a two-tailed test: ^p<0.10; * p<0.05;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

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.

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. POTENTIAL MECHANISMS

	Teacher attrition rate		% teachers w/ less than 4 years of experience		% teachers w/ more than 10 years of experience		Student-teacher ratio		Student attendance rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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)	0.0119 (0.0330)	0.0320* (0.0163)
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)	0.0103 (0.0297)	-0.0167 (0.0166)
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)	-0.0311 (0.0413)	-0.0770*** (0.0217)
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)	-0.0692 (0.0424)	-0.0863*** (0.0244)
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)	-0.0137 (0.0484)	-0.0680** (0.0253)
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)	0.0210 (0.0494)	-0.0756* (0.0311)
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)	0.0742 (0.0567)	-0.0438 (0.0307)
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)	0.0563 (0.0620)	-0.0794* (0.0332)
N	19,307	19,307	18,206	18,206	18,206	18,206	16,404	16,404	20,069	20,069
District Cnt	526	526	527	527	527	527	526	526	527	527
Levy Cnt	2,771	2,771	2,848	2,848	2,848	2,848	2,771	2,771	2,848	2,848
Mean DV	8.26	8.26	22.44	22.44	58.5	58.5	16.13	16.13	95.15	95.15
MODEL	RD	DID	RD	DID	RD	DID	RD	DID	RD	DID
Polynomial	Quad.	N/A	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	No	N/A
Levy Type	Operat.	Operat.	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.

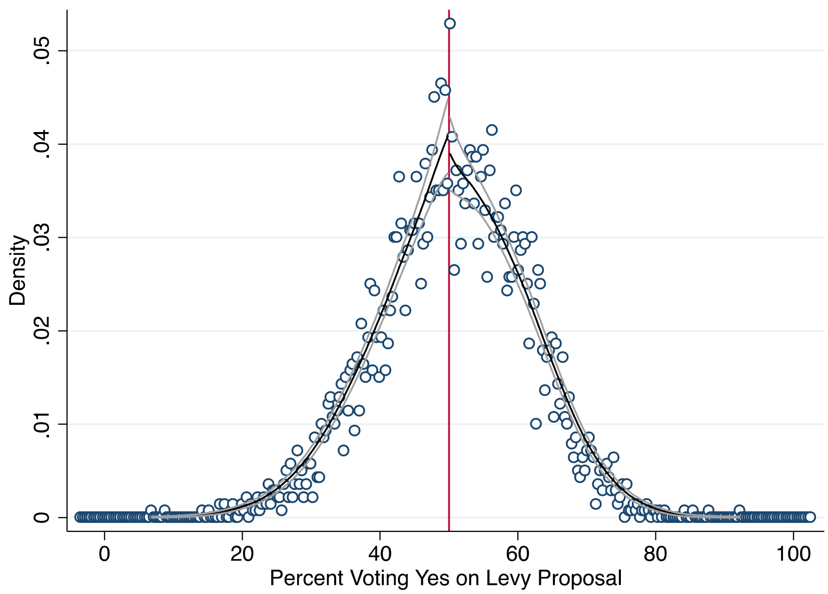
Table 9. Treatment-on-the-treated (TOT) Estimates of Levy Failure

	Expenditures (2010 Dollars)	Annual Achievement Gains (Standard Deviations)	Annual Achievement Gains (Days of Learning)	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.

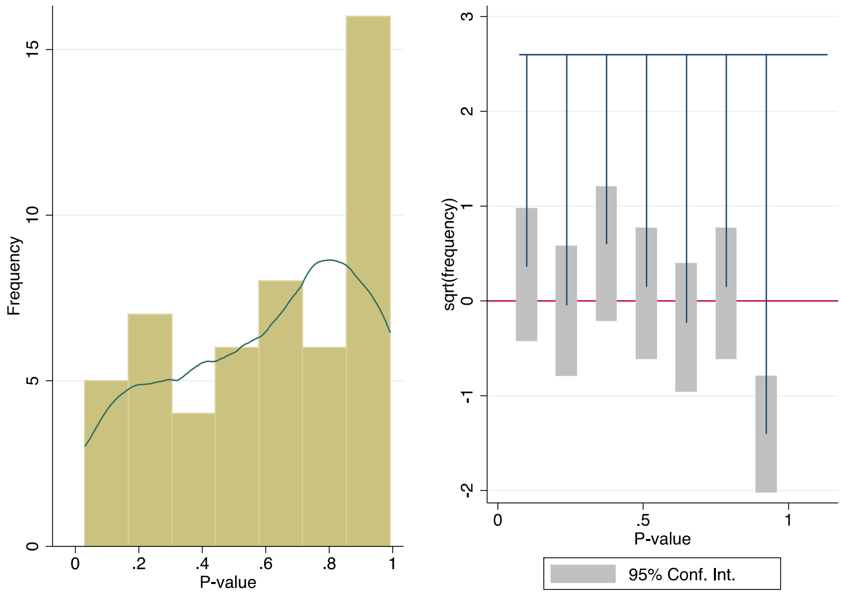
Appendix

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.

FIGURE A2. DISTRIBUTION OF P-VALUES FROM BALANCE TESTS



Note. The histogram and hanging rootogram plot the frequencies of p-values from the covariate balance tests. The rootogram provides insights as to whether the deviation from the expected uniform distribution is statistically significant.

TABLE A1. IMPACT OF PROPERTY & INCOME TAX LEVY FAILURE ON REVENUES PER PUPIL(2010 Dollars)

	Primary Specification				Sensitivity Checks			
	(1) Local	(2) State	(3) Federal	(4) Total	(5) Total	(6) Total	(7) Total	(8) Total
2 YRS PRIOR	13.35 (86.97)	-39.85 (107.5)	-19.00 (23.63)	-45.51 (146.3)	78.60 (275.9)	-21.90 (107.6)	-199.7 (197.6)	71.65 (67.53)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-34.93 (66.02)	-221.4* (111.4)	-25.51 (25.61)	-281.8* (135.9)	-134.0 (280.8)	-196.4^ (100.1)	-347.0^ (191.9)	-142.6* (66.22)
1 YR AFTER	-235.8** (78.02)	-254.2^ (148.7)	-44.39 (27.07)	-534.3** (178.5)	-782.2* (365.3)	-478.6*** (135.8)	-603.7* (246.5)	-418.7*** (101.9)
2 YRS AFTER	-199.0* (94.10)	-307.9^ (182.9)	-39.43 (28.01)	-546.3* (221.5)	-567.0 (471.4)	-583.5*** (170.3)	-623.9* (309.1)	-557.8*** (105.8)
3 YRS AFTER	-29.14 (108.9)	83.70 (215.0)	-43.23 (30.36)	11.33 (252.2)	-122.2 (483.7)	-93.70 (195.7)	-34.17 (333.9)	-278.1* (122.0)
4 YRS AFTER	-0.589 (106.1)	374.0^ (218.1)	-49.59 (30.54)	323.8 (256.2)	-442.2 (448.0)	182.7 (200.3)	171.6 (336.9)	-12.91 (127.1)
5 YRS AFTER	-0.931 (113.7)	97.02 (220.1)	-36.07 (33.17)	60.01 (264.2)	-819.4^ (477.8)	99.97 (207.9)	-257.2 (360.1)	57.22 (136.5)
6 YRS AFTER	41.74 (128.6)	-177.0 (241.7)	-51.43 (37.56)	-186.7 (290.2)	-599.6 (515.0)	12.07 (219.9)	-173.4 (374.3)	16.13 (148.4)
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	5,638.5	5,057.0	739.6	11,434.9	11,483.7	11,434.9	11,494.0	11,434.9
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 operational and capital levy failure (as opposed to passage) on per-pupil revenue in 2010 dollars. 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 A2. 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	0.0173	0.00116	-0.00293	0.00841	0.0307	0.00395	0.00237	0.00850
	(0.0105)	(0.0148)	(0.0181)	(0.0119)	(0.0201)	(0.00899)	(0.0145)	(0.00572)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	0.000177	-0.0326*	0.0209	-0.0185^	-0.00725	-0.0145^	-0.0184	-0.0126*
	(0.00911)	(0.0149)	(0.0170)	(0.0111)	(0.0223)	(0.00814)	(0.0153)	(0.00543)
1 YR AFTER	-0.0123	-0.00968	-0.0218	-0.0140	-0.0109	-0.0190^	-0.00500	-0.0226**
	(0.0108)	(0.0195)	(0.0188)	(0.0142)	(0.0266)	(0.0106)	(0.0190)	(0.00797)
2 YRS AFTER	-0.0118	0.0304	0.0184	0.00825	0.00523	-0.00706	0.00822	-0.0196*
	(0.0121)	(0.0222)	(0.0208)	(0.0162)	(0.0310)	(0.0124)	(0.0215)	(0.00804)
3 YRS AFTER	0.0118	0.0344	0.000177	0.0228	0.0274	0.00437	0.0232	-0.00773
	(0.0148)	(0.0253)	(0.0210)	(0.0178)	(0.0331)	(0.0131)	(0.0229)	(0.00862)
4 YRS AFTER	0.0149	0.0470	-0.0174	0.0308	0.00620	0.0158	0.0350	0.00227
	(0.0166)	(0.0300)	(0.0228)	(0.0204)	(0.0374)	(0.0151)	(0.0266)	(0.00993)
5 YRS AFTER	0.0121	0.0119	0.000865	0.0160	-0.0439	0.0171	0.00875	0.00534
	(0.0195)	(0.0328)	(0.0243)	(0.0224)	(0.0419)	(0.0160)	(0.0302)	(0.0112)
6 YRS AFTER	0.0179	0.00792	0.0161	0.0117	-0.0129	0.0262	0.0394	0.00896
	(0.0239)	(0.0366)	(0.0283)	(0.0252)	(0.0436)	(0.0171)	(0.0317)	(0.0127)
N	20,104	20,104	20,094	20,104	10,412	20,104	10,154	20,104
District Count	523	523	523	523	457	523	456	523
Levy Count	2,819	2,819	2,819	2,819	1,448	2,819	1,412	2,819
Mean of DV	8.6	8.44	6.44	9.33	9.33	NA	9.33	9.33
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	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 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 A3. IMPACT OF PROPERTY & INCOME TAX LEVY FAILURE ON SPENDING PER PUPIL (2010 Dollars)

	Primary Specification				Sensitivity Checks			
	(1) Instruction	(2) Admin.	(3) Other	(4) Total	(5) Total	(6) Total	(7) Total	(8) Total
2 YRS PRIOR	24.57 (18.06)	-21.83^ (12.76)	4.392 (17.50)	0.524 (32.15)	-51.87 (53.59)	14.07 (24.61)	-6.623 (36.15)	25.66 (15.76)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-14.99 (17.56)	-4.440 (10.04)	-3.008 (18.20)	-32.14 (34.09)	-17.31 (55.79)	-35.15 (26.90)	-23.64 (37.91)	-43.39* (17.24)
1 YR AFTER	-55.93* (26.12)	-12.88 (11.78)	-90.10*** (24.79)	-161.2*** (47.15)	-188.6* (75.06)	-131.0*** (36.59)	-129.7* (52.48)	-175.8*** (25.20)
2 YRS AFTER	-83.55** (30.18)	-10.28 (12.03)	-114.6*** (31.71)	-212.3*** (58.12)	-278.0** (92.04)	-172.3*** (45.68)	-179.5** (64.94)	-196.7*** (32.53)
3 YRS AFTER	-57.39+ (32.70)	-21.84 (13.42)	-87.67** (32.88)	-170.6** (60.49)	-167.2^ (91.44)	-162.1*** (47.69)	-132.5* (65.26)	-170.1*** (34.78)
4 YRS AFTER	-49.10 (35.35)	-21.49 (15.53)	-73.12* (32.12)	-137.8* (61.22)	-72.14 (93.69)	-127.1* (49.83)	-94.60 (67.74)	-118.2** (38.86)
5 YRS AFTER	-47.35 (41.65)	-24.99 (17.32)	-29.95 (37.98)	-90.76 (74.40)	-19.22 (112.5)	-97.97^ (55.13)	-43.38 (74.89)	-98.76* (42.28)
6 YRS AFTER	7.058 (46.48)	-25.46 (21.11)	-22.05 (42.82)	-32.48 (83.02)	-106.3 (143.0)	-61.12 (61.47)	-4.470 (84.85)	-86.67^ (44.78)
N	29,507	29,518	29,507	29,527	17,716	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	2,512	4,218	2,512	4,218
Mean of DV	5,344.3	1,189.3	3,048.9	9,577.8	9,623.2	9,577.8	9,623.2	9,577.8
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 operational and capital levy failure (as opposed to passage) on per-pupil district expenditures in 2010 dollars. 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 A4. 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 A5. IMPACT OF TAX LEVY FAILURE ON STUDENT ACHIEVEMENT (OPERATIONAL LEVIES ONLY)

	State "Value Added" Estimate (District SDs)				State Performance Index (District SDs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2 YRS PRIOR	-0.231 (0.162)	-0.0864 (0.115)	-0.282 (0.192)	-0.00442 (0.0671)	0.00147 (0.0273)	-0.0167 (0.0219)	0.0124 (0.0320)	0.00710 (0.0151)
1 YR PRIOR	--	--	--	--	--	--	--	--
ELECTION YR	-0.233^ (0.141)	-0.121 (0.102)	-0.345* (0.161)	-0.0614 (0.0665)	0.0385 (0.0248)	0.00622 (0.0192)	0.0459^ (0.0278)	-0.0159 (0.0120)
1 YR AFTER	-0.154 (0.144)	-0.114 (0.107)	-0.163 (0.169)	-0.0879 (0.0669)	-0.0291 (0.0269)	-0.0481* (0.0207)	-0.0381 (0.0322)	-0.0524*** (0.0150)
2 YRS AFTER	-0.236^ (0.122)	-0.140 (0.0911)	-0.260^ (0.142)	-0.0977 (0.0632)	-0.0269 (0.0289)	-0.0560* (0.0233)	-0.0254 (0.0347)	-0.0480** (0.0171)
3 YRS AFTER	-0.259^ (0.135)	-0.188^ (0.0985)	-0.365* (0.155)	-0.0165 (0.0653)	-0.0401 (0.0330)	-0.0812** (0.0256)	-0.0577 (0.0377)	-0.0498** (0.0179)
4 YRS AFTER	-0.173 (0.132)	-0.0366 (0.103)	-0.210 (0.150)	0.0209 (0.0689)	-0.0118 (0.0364)	-0.0567* (0.0276)	-0.0296 (0.0415)	-0.0407* (0.0194)
5 YRS AFTER	-0.237^ (0.141)	-0.121 (0.112)	-0.258^ (0.156)	-0.0119 (0.0757)	-0.0401 (0.0403)	-0.0733* (0.0290)	-0.0592 (0.0473)	-0.0449* (0.0216)
6 YRS AFTER	-0.227 (0.153)	0.00810 (0.122)	-0.210 (0.168)	-0.0213 (0.0816)	-0.0203 (0.0438)	-0.0382 (0.0321)	-0.0399 (0.0497)	-0.0381 (0.0239)
N	16,380	16,380	9,507	16,380	22,161	22,161	12,348	22,161
District Cnt	527	527	471	527	527	527	466	527
Levy Cnt	2,848	2,848	1,663	2,848	2,848	2,848	1,585	2,848
Mean DV	0.03	0.03	0.02	0.03	0.06	0.06	0.03	0.06
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	Operational	Operational	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.

TABLE A6. 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 (146.4)	22.74 (30.52)	-0.0138 (0.114)	-0.00302 (0.0213)	--	--	--	--	--
1 YR PRIOR	--	--	--	--	--	--	--	--	--
ELECTION YR	-301.8* (139.7)	-54.95 (36.17)	-0.0495 (0.0945)	-0.0129 (0.0181)	-301.6306* (139.6999)	-55.5671 (36.2098)	-0.0495 (0.0946)	-0.0662 (0.0965)	-0.0129 (0.0181)
1 YR AFTER	-674.1*** (187.4)	-144.8** (46.57)	-0.00170 (0.104)	-0.0418* (0.0211)	-935.5254** (289.864)	-192.9232** (69.2296)	-0.0445 (0.1636)	-0.0634 (0.1673)	-0.053 (0.0335)
2 YRS AFTER	-832.1*** (224.5)	-216.9*** (53.36)	-0.159^ (0.0954)	-0.0457^ (0.0233)	-1764.069*** (498.8148)	-405.1929*** (110.3296)	-0.2171 (0.245)	-0.2604 (0.2487)	-0.0968^ (0.0556)
3 YRS AFTER	-255.7 (273.2)	-238.8*** (58.26)	-0.0822 (0.0995)	-0.0575* (0.0253)	-2278.127** (817.986)	-688.9621*** (174.4476)	-0.3071 (0.3822)	-0.3815 (0.389)	-0.1677^ (0.0881)
4 YRS AFTER	81.06 (249.8)	-194.4** (65.79)	0.0458 (0.106)	-0.0336 (0.0272)	-3065.448* (1262.536)	-1047.416*** (275.088)	-0.3401 (0.5922)	-0.4382 (0.6021)	-0.2426+ (0.1379)
5 YRS AFTER	146.7 (276.9)	-159.5* (72.49)	-0.0890 (0.117)	-0.0472 (0.0293)	-4519.793* (1983.924)	-1583.279*** (434.545)	-0.6217 (0.9217)	-0.7847 (0.9368)	-0.384^ (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
Restricted Bandwidth	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 A7. 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
Restricted Bandwidth	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.