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Published in the
American Economic Journal:
Economic Policy 2022

Duke University and NBER (email: jason.baron@duke.edu). C. Kirabo Jackson was coeditor for this article. I would like to thank Patrick Bayer; John Bound; Sewin Chan; Gordon Dahl; Susan Dynarski; Michael Gilraine; Ezra Goldstein; Tatiana Homonoff; Joshua Hyman; Brian Jacob; Shawn Kantor; Carl Kitchens; Julien Lafortune; Kevin Lang; Nicholas Lovett; Adrienne Lucas; Eric Ohrn; Leslie Papke; Luke Rodgers; Jesse Rothstein; Christopher Ruhm; Anastasia Semykina; Kevin Stange; Leanna Stiefel; Juan Carlos Suárez Serrato; Cullen Wallace; Ebonya Washington; David Welsch; Cathy Xue; numerous conference participants at the AEFP, APPAM, Urban Economics Association, National Tax Association, and ASSA Annual Meetings; and numerous seminar participants at Florida State University, Duke University, University of Michigan (supported by training grant award R305B170015 from the Institute of Education Sciences, U.S. Department of Education), New York University, Michigan State University, University of Delaware, Marquette University, Colby College, Bates College, San Diego State University, and Georgia Institute of Technology for valuable feedback and support. I also thank three anonymous referees for helpful comments and suggestions. All mistakes and conclusions are my own.

American Economic Journal: Economic Policy 2022, 14(1): 1–39
https://doi.org/10.1257/pol.20200226

School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin†

By E. Jason Baron*

This study examines the impacts of two distinct types of school spending on student outcomes. State-imposed revenue limits cap the total amount of revenue that a school district in Wisconsin can raise unless the district holds a referendum asking voters to exceed the cap. Importantly, Wisconsin law requires districts to hold separate referenda for operational and capital expenditures, which allows for estimating their independent effects. Leveraging close elections in a dynamic regression discontinuity framework, I find that increases in operational spending have substantial positive effects on test scores, dropout rates, and postsecondary enrollment, but additional capital expenditures have little impact. (*JEL* D72, H75, I21, I22, I28)

In an effort to improve the quality of public schools, the United States has dramatically increased the resources devoted to them. Total per-pupil expenditures on elementary and secondary education have nearly doubled in real terms from roughly \$7,000 in 1980 to approximately \$14,000 in 2015.[1] There is a growing consensus in the economics of education literature that increases in school funding generally improve student outcomes. Specifically, recent quasi-experimental studies primarily relying on variation from court-ordered school finance reforms (SFRs) have shown that additional school resources improve short- and medium-term outcomes such as test scores and educational attainment (Brunner, Hyman, and Ju 2018; Candelaria and Shores 2019; Hyman 2017; Johnson and Jackson 2019; Lafortune, Rothstein, and Schanzenbach 2018) and longer-term outcomes

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† Go to https://doi.org/10.1257/pol.20200226 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1] Author's calculations from National Center for Education Statistics (NCES) data. These expenditures are reported in constant 2017–2018 dollars based on the Consumer Price Index for All Urban Consumers (CPI-U) adjusted to a school year basis. Total expenditures include current operational expenditures, capital outlays, and interest on school debt.

such as wages, employment, and income mobility (Biasi 2019; Jackson, Johnson, and Persico 2016).[2]

While all of these recent studies find that "money matters" in public education, the optimal allocation of resources across expenditure types remains an open empirical question. Estimating the causal effect of various expenditure types within the same institutional context is challenging due to the need of a source of exogenous variation for each expenditure type. However, in an era where policymakers grapple with tight budget constraints and question the return to investments in public education, understanding which types of spending are most productive has considerable significance for economic policy.

This study seeks to fill this gap in the literature by examining the causal impact of two distinct types of expenditures—operational (e.g., teacher compensation and class sizes) and capital (e.g., new buildings or renovations)—within the same institutional context. Specifically, I leverage a detailed administrative dataset along with a credible research design and a novel source of quasi-experimental variation in Wisconsin's school finance. Wisconsin's 421 school districts are primarily financed through a combination of state aid and local property taxes. Beginning with the 1993–1994 academic year, state-imposed revenue limits cap increases in school district revenue from the combination of these two sources. If a district wishes to exceed revenue limits, then it must ask for voter approval to increase property taxes in a local referendum. A simple majority vote of district residents is required for the initiative to pass.

Importantly, state law requires school districts to hold separate referenda for operational purposes and for bond issues targeted to fund capital projects, which allows for estimating the independent effects of additional operational and capital expenditures. While districts that pass a referendum are likely to differ along observable and unobservable characteristics from districts where the initiative is defeated, these differences can be mitigated by focusing only on narrow elections. For instance, a district that passes an operational referendum by a small margin (e.g., 50.1 percent of the vote) is likely to have similar preferences for educational spending to a district where the initiative is defeated by a similar margin. I leverage close operational and capital bond elections in a regression discontinuity (RD) framework to identify the causal impact of additional operational and capital spending on student outcomes.

The standard RD design, however, is complicated by the dynamic nature of treatment in this setting. A school district may attempt (and pass) multiple referenda of each type throughout the sample period, which complicates identification of the dynamic treatment effects of each type of referendum. To see this, suppose there are two school districts, A and B, that attempt an operational referendum in time *t*. Further suppose that A narrowly passes the operational referendum, while B narrowly loses. In a setting where districts may attempt and pass multiple referenda

[2] Other studies in the recent school spending literature that do not rely on SFR-induced variation exploit discontinuities in state funding formulas (Kreisman and Steinberg 2019), examine the effects of school funding cuts during the Great Recession (Jackson, Wigger, and Xiong 2021), and estimate increases in operational funds from local tax elections (Abott et al. 2020; Lee and Polachek 2018). Another set of studies examine the impacts of additional federal Title I funds (Cascio, Gordon, and Reber 2013; Johnson 2015; Matsudaira, Hosek, and Walsh 2012; van der Klaauw 2008). See Jackson (2018) for a detailed literature review.

of each type, it would be difficult to draw inferences from a simple comparison of outcomes between districts A and B in subsequent years. For instance, if district A also passes a capital bond referendum in $t + 3$, then differences in student outcomes between the two districts in $t + 5$ will not solely be due to the operational referendum passed in t.

To isolate only the direct effects of a particular successful referendum, I adapt the "one-step" dynamic RD estimator developed by Cellini, Ferreira, and Rothstein (2010), who examine the effects of a successful bond referendum (targeted to fund school facility investments) on local house prices in California. My approach is identical to theirs but allows for two distinct types of referenda.[3] I use the dynamic RD approach to estimate "treatment on the treated" (TOT) effects. Estimates of TOT effects yield the causal impacts of successful referenda, holding subsequent election outcomes constant. Thus, in the example above, this approach would directly control for the districts' intermediate behavior (from t to $t + 5$). Intuitively, the dynamic RD approach compares the outcomes of school districts in which a particular referendum (operational or capital) at some point in time was narrowly successful to districts where the initiative was narrowly defeated—but the sequence of prior and subsequent types of initiatives, votes, and successful referenda is similar.

I apply this estimator to a rich administrative dataset combining information on nearly two decades of Wisconsin referenda, annual detailed measures of district-level finances and student outcomes, and an individual-level dataset containing information on the universe of Wisconsin public school teachers. I first examine the impact of increasing operational expenditures on student outcomes. Estimates of TOT effects indicate that operational referendum approval in a narrow election leads to an increase in operational expenditures of roughly \$300 (3 percent) per pupil each year in the 10 years following the election.

Using detailed administrative data, I show that narrowly winning districts allocate most of the additional resources, roughly \$200 per pupil, to instructional expenditures in the form of additional teachers and teacher aides as well as increases in average teacher compensation. They spend the remaining \$100 per pupil on support services for students in the form of additional guidance counselors, school psychologists, and social workers. There is no evidence that school districts allocate any of the additional resources to school administrators. Furthermore, all of the additional spending from operational referenda sticks in operational expenditure accounts and is not associated with increases in capital outlays.

Increases in operational spending result in substantial improvements in student outcomes. Specifically, I find that narrowly passing an operational referendum leads to an increase in test scores of approximately 8 percent of a standard deviation on the state's standardized exam, a 9 percent reduction in the district's dropout rate, and a 10 percent increase in the number of high school completers in the district who subsequently enroll in postsecondary education. I show that these results are not sensitive to the choice of RD estimator (parametric versus nonparametric), vote share

[3] Other studies that have employed the one-step estimator include Rauscher (2019); Hong and Zimmer (2016); and Martorell, Stange, and McFarlin Jr. (2016)—all of which examine the impacts of capital bond referenda on student outcomes.

specification, or bandwidth selection, and are not the result of endogenous sorting just above the 50 percent threshold. Finally, given that I use aggregate district-level data, one may be concerned that student migration from losing to winning school districts could be driving the results. However, I am able to rule out even small changes in the income and racial composition of district students in the years following a narrow operational referendum win.

I next document the impact of additional capital expenditures on academic outcomes. First, I show that narrowly approving a capital bond referendum leads to a sharp and immediate increase of roughly \$4,000 (200 percent) per pupil in capital outlays. All of the additional resources induced by a successful capital bond referendum stick in the capital outlay account and are not reallocated to operating expenditures, which allows me to isolate capital expenditure effects. I find that bonds are frequently used for the repair, maintenance, and modernization of existing structures and are not associated with improvements in measured school inputs such as class size, teacher experience, teacher compensation, or teacher attrition. Furthermore, I find little evidence that school facility investments impact student outcomes. TOT estimates of the impact of capital bond passage on test scores, dropout rates, and postsecondary enrollment are close to zero and mostly statistically insignificant.

Taken together, these findings indicate that increases in discretionary operational funds can significantly improve educational outcomes and may be a more productive use of resources than school facility investments. This finding is consistent with studies showing that teacher quality is one of the most important school-related inputs in the education production function (Chetty, Friedman, and Rockoff 2014; Rivkin, Hanushek, and Kain 2005; Rockoff 2004) and with those showing that smaller class sizes can improve student outcomes (Angrist and Lavy 1999; Finn and Achilles 1999; Krueger and Whitmore 2001). It is also consistent with studies in Texas (Martorell, Stange, and McFarlin Jr. 2016) and California (Cellini, Ferreira, and Rothstein 2010) that show that bond-financed school facility investments do not generate appreciable improvements in student achievement.[4]

This study contributes to the quasi-experimental literature examining the effect of school spending on student outcomes. By separately examining the impacts of both operational and capital expenditures, this study is the first to simultaneously compare the effectiveness of two distinct types of school spending within the same institutional setting. Previous studies either estimate the joint impact of increases in operational and capital expenditures (Brunner, Hyman, and Ju 2018; Candelaria and Shores 2019; Jackson, Wigger, and Xiong 2021; Jackson, Johnson, and Persico 2016; Johnson and Jackson 2019; Lafortune, Rothstein, and Schanzenbach 2018), estimate only capital expenditures (Cellini, Ferreira, and Rothstein 2010; Lafortune and Schönholzer 2019; Martorell, Stange, and McFarlin Jr. 2016; Neilson and Zimmerman 2014; Rauscher 2019), or focus exclusively on operational expenditures (Abott et al. 2020).

[4] In contrast, recent studies on the impacts of large-scale school construction projects in urban school districts generally document large improvements in student outcomes (Lafortune and Schönholzer 2019; Neilson and Zimmerman 2014). In Section VID, I discuss plausible explanations for these differing effects.

While one could simply compare operational and capital spending estimates across institutional contexts to examine this question, this approach would likely conflate institutional differences with differences in treatment effects. Existing studies in the SFR literature have documented substantial heterogeneity in spending effects across states (Jackson 2018). Furthermore, recent studies examining the effects of capital and operational expenditures have focused on individual states with large institutional differences. For instance, studies examining the effects of locally financed capital expenditures have primarily focused on the two states with the largest number of public school districts—California (Cellini, Ferreira, and Rothstein 2010) and Texas (Martorell, Stange, and McFarlin Jr. 2016)—while those examining operational expenditure effects have focused mostly on Midwestern and Northeastern states (Abott et al. 2020).

Estimating the individual effects of operational and capital expenditures in the same context offers three additional advantages over simply comparing estimates across institutional settings. First, it allows me to show that school districts that pass operational referenda tend to be similar to districts that pass capital bond referenda, so that differences in the returns to each spending type are not driven by systematic differences between the two types of districts. Furthermore, I show that the main results hold even when relying solely on the staggered timing of operational and capital bond referenda and restricting my sample only to school districts that passed both types of referenda. This second finding reinforces the observation that differences in the marginal returns to each type of expenditure do not simply reveal systematic differences between the two types of districts. Finally, I find little evidence that districts pass operational and capital bond referenda in a systematic sequence, so that differences in the returns to each type of expenditure do not simply reflect districts' choices to pass a certain type of referendum first.

Overall, this study advances the literature primarily by showing that expenditures targeted to operational functions may be more effective than those targeted to school facilities. However, this study additionally contributes to the literature in two ways. First, it is one of few papers to estimate the impacts of spending on student outcomes past high school completion (other papers include Biasi 2019; Hyman 2017; Jackson, Wigger, and Xiong 2021; and Jackson, Johnson, and Persico 2016). Second, it employs a research design that relies on relatively mild assumptions compared to those needed for other nonexperimental approaches typically used in the school spending literature. As Lee and Lemieux (2010) show, the identification assumptions in RD designs tend to be relatively weaker and more easily testable than other popular program evaluation methods such as difference-in-differences and instrumental variables.

I. Background

A. *Wisconsin's School Finance System*

There are 421 school districts in Wisconsin. Each school district derives its revenue from four major sources: state aid, local property taxes, federal aid, and other local (nonproperty tax) revenues such as donations and student fees. Districts derive most of their revenue from a combination of state aid and local property taxes. For instance, in the 2014–2015 academic year, Wisconsin school districts received roughly 90 percent of total revenue through a combination of these two sources. While local school districts have always had the ability to collect property taxes in order to raise revenue, the degree of school districts' reliance on local property taxes has been a source of debate in Wisconsin for many decades.

Prior to the 1993–1994 academic year, Wisconsin local school boards generally had the ability to decide how much revenue to raise via property taxes. Accelerating property taxes during the late 1980s and early 1990s, however, led to the enactment of a state law in 1993 that caps the annual increase in a school district's per-pupil revenue derived from general state aid and local property taxes.[5] These caps, known as revenue limits, control roughly 90 percent of the average Wisconsin school district's resources and are thus one of the most important aspects of the state's school finance system.[6]

Revenue limits allow the legislature to control the amount of property tax revenue that a school district can raise. To see this, note that the limits are binding on the combined state aid and local property tax revenue. As a result, if the legislature increases the amount of aid to a particular school district, then the district is required to lower the local property tax in order to stay within the revenue limits. Indeed, while the state's share of K–12 funding was roughly 49 percent in 1993–1994, the legislature committed in 1993 to increase state aid and fund two-thirds of total K–12 education revenues by the 1996–1997 school year.

Revenue limits and the concurrent increase in the state's share of K–12 education funding were implemented with the goal of reducing school districts' reliance on the local property tax. Online Appendix Figure B.2 shows that revenue limits were largely successful in reducing the school portion of the property tax. The figure plots the average local property tax revenue per pupil for Wisconsin public school districts (in 2010 dollars) before and after 1993–1994, the year that revenue limits were enacted. It shows that property tax revenue was accelerating in the years prior to the enactment of revenue limits but decreased dramatically shortly thereafter. It took roughly 20 years for local property tax revenues to reach their pre-revenue limit levels.

[5] Other forms of aid such as federal grants, state categorical aid, and other nonproperty tax local revenues are exempt from revenue limits.

[6] The revenue limits that each school district faces in a given year largely reflect that district's per-pupil spending in 1992–1993, the year before the enactment of the limits. Revenue limits were initially set based on each district's actual amount of spending per pupil in 1992–1993 and are adjusted each year primarily by the actions of the state legislature and long-term changes in the district's student enrollment. The formula for the allowable revenue limit growth is designed so that, all else equal, districts with declining enrollment face a tighter revenue limit. Each year, the state legislature determines the allowable per-pupil increase in revenue limits. This adjustment is primarily based on the rate of inflation but may also reflect the health of the state's economy. The adjustment is reported as a dollar amount that applies uniformly to all school districts. Online Appendix Figure B.1 shows the allowable annual adjustments to revenue limits set by the state legislature since 1993–1994.

B. *Referenda to Exceed Revenue Limits*

The only means of bypassing revenue limits is through the passage of a local referendum in which districts ask for voter approval to increase local property taxes. Wisconsin's revenue limit law requires school districts to hold separate referenda for operational purposes (e.g., instruction and support services) and for bond issues targeted to fund major capital projects (e.g., new buildings or renovations).

In theory, it should not matter whether additional referendum-approved resources are legally restricted to certain purposes—such as capital or operational expenditures—as districts could simply divert other unrestricted funds away from these targeted categories with no impact on the total allocation of funds. Despite this standard public finance theory, a large empirical literature has documented that restricted grants tend to stick in the targeted accounts. This empirical anomaly was coined the "flypaper effect" by Arthur Okun since the money appears to "stick where it hits." This effect has been documented in various settings;[7] most relevant to this study, however, is the fact that previous studies of school districts' bond referenda tied to capital expenditures find strong evidence of flypaper effects. For instance, Cellini, Ferreira, and Rothstein (2010) and Martorell, Stange, and McFarlin Jr. (2016) find no evidence that additional bond-approved resources earmarked for capital have any effect on operational expenditures. In Sections VA and VIA, I show strong evidence of flypaper effects for both types of referenda.[8] In other words, I find no evidence that passing an operational referendum has any impact on capital expenditures or that passing a capital bond referendum has any effect on operational expenditures—even ten years after the initial election.[9] These results imply that my analysis of the effects of successful operational and capital bond referenda will identify the impacts of additional operational and capital expenditures, respectively.

In an operational referendum, school districts can either ask voters to override revenue limits for a given time period (nonrecurring) or indefinitely (recurring). In a nonrecurring referendum, a school district asks its voters for permission to override revenue limits for a predetermined period of time.[10] In contrast, a recurring referendum authorizes a permanent addition to the district's revenue limit.

For major capital projects, districts may issue up to $1 million in debt without a referendum. Debt issued without a referendum must be paid off using funds within the revenue limit. All other debt must be approved through a local bond referendum.

[7] Hines and Thaler (1995) provide a review of earlier evidence for the flypaper effect, while Inman (2008) covers more recent studies. See also Fisher and Papke (2000) for a review of studies examining education-specific effects.

[8] This finding directly contributes to the flypaper effect literature. The evidence for flypaper effects has been challenged on the grounds that most papers cannot adequately control for the endogeneity of specific grants (e.g., see Knight 2002). However, the RD approach in this study allows me to overcome this criticism, as it identifies the causal effect of additional restricted funds on resource allocation.

[9] Examining the dynamics of the flypaper effect is crucial, as previous studies have shown that it could disappear over time as school districts have time to reoptimize (Gordon 2004).

[10] For instance, a district may ask its voters to exceed the revenue cap by $1 million each year for the subsequent four years. At the end of the four years, however, exceeding the state-imposed revenue limit is no longer authorized, and the limit returns to its original amount. The median operational nonrecurring measure in my sample asks voters for permission to exceed revenue limits for four years.

If a bond referendum is approved by voters, then the annual debt service payments are exempt from the state-imposed revenue limits and the incurred debt is paid off via the increase in local property taxes. Thus, in either referendum, district residents who vote in favor of the measure are agreeing to a predetermined increase in their property taxes.

Prior to the election, mailers are sent to district residents with the purpose of reminding them to vote and providing them with more information about the upcoming referendum.[11] There are few restrictions on the dates that school districts can place a referendum on the ballot. A local school board can either call a special election or hold the referendum at a regular primary or general election date.[12]

Table 1 shows summary statistics for all referenda held by Wisconsin school districts from 1996–1997 to 2014–2015, the sample period of this study. From 1996 to 2014, there were 2,331 individual questions on the ballot to override state-imposed revenue limits. Roughly 45 percent of these questions were for operational purposes. Virtually every school district in Wisconsin held at least 1 type of referendum (operational *or* capital bond) during this time period (404 out of 421). Seventy-one percent of school districts held both an operational *and* a capital referendum. In total, voters approved 53 percent of all referenda. Elections were relatively close: on average, the percent of votes in favor of approving a given initiative was slightly below 51 percent.

II. Data

A. *Referendum-Level Dataset*

To estimate the effect of narrowly approving a referendum on student outcomes, I combine three primary datasets. First, I obtain a referendum-level dataset from the Wisconsin Department of Public Instruction (WDPI). This dataset reports, for each referendum attempt, the school district's unique identifier, the date of the referendum, the type, the amount of proposed additional revenue, voter turnout and votes in favor, a brief description of the intended purpose of the referendum, and the actual wording of the question that voters see at the ballot (WDPI 2020a).

B. *Administrative Dataset*

Information on each school district's referendum history is matched to NCES district-level K–12 revenue and expenditure data and to WDPI data containing each district's revenue limits, property values, student-staff ratios, total enrollment, urban-centric locale code, share of economically disadvantaged students, and

[11] Online Appendix A discusses in much more detail a specific example of a mailer.

[12] Online Appendix Figure B.3 shows the distribution of referenda by election month separately for operational and capital bond referenda. The figure shows that most elections, roughly 65 percent, were held in April and November, the months during which spring and fall general elections are held. Another 20 percent of referenda were placed in February, August, and September, months during which spring and fall primary elections take place. The remaining referenda were placed on the ballot as special elections (in months without other elections).

Table 1—Referenda Summary Statistics (1996–2014)

Variable	Observations	Mean	Median	SD	Min	Max
Panel A. All referenda						
Referendum passed	2,331	0.53	1	0.50	0	1
Percent in favor	2,331	50.81	50.94	12.71	11	100
Amount approved PP	1,238	4,863	2,625	5,960	7	61,808
Number of questions per district	404	5.77	5	4.07	1	27
Panel B. Recurring referenda						
Referendum passed	427	0.36	0	0.48	0	1
Percent in favor	427	45.63	46.08	11.85	11	81
Amount approved PP	154	738	402	921	7	5,208
Panel C. Nonrecurring referenda						
Referendum passed	593	0.59	1	0.49	0	1
Percent in favor	593	51.86	52.09	11.52	17	87
Amount approved PP	348	3,389	2,364	4,221	30	45,771
Panel D. Capital bond referenda						
Referendum passed	1,311	0.56	1	0.50	0	1
Percent in favor	1,311	52.02	52.16	13.08	12	100
Amount approved PP	736	6,422	4,365	6,646	35	61,808

Notes: The table shows summary statistics for all referenda held by Wisconsin school districts between 1996–1997 and 2014–2015, the sample period of this study. Data on individual referenda are collected and made publicly available by the WDPI. Panel A provides summary statistics for all referenda (pooling operational and capital bond measures). Panels B and C report summary statistics separately for operational recurring and nonrecurring measures, respectively. Panel D reports statistics for capital bond referenda. The amount approved per pupil was converted to 2010 dollars using the Midwest Region's CPI-U. For nonrecurring referenda, the total amount approved is simply the sum of the approved annual increase over the time period of the referendum. For instance, if a school district passes a referendum to exceed revenue limits by $3,000 per pupil each year for 5 years, then the total amount approved per pupil would be reported as $15,000. For recurring referenda, a school district may either ask its voters for a given increase in revenue limits beginning in the following year or it may phase in the increase over several years. In either case, the WDPI reports the amount of the permanent increase. As an example, a school district may pass a referendum to exceed its revenue limits by $1,000 per pupil in the following year and by an additional $500 in the second year and thereafter. In this case, the increase in revenue limits would be reported as $1,500 per pupil (the amount of the permanent increase).

share of minority students.[13] This dataset also contains information on teacher characteristics, including each district's average teacher compensation, local teacher experience, and attrition.[14, 15]

[13] NCES (2017), complemented with WDPI (2020b), WDPI (2019c), and WDPI (2016b). The urban-centric locale takes the value of 1 if the school district is located in a city, 2 if it is located in a suburb, 3 if it is located in a town, and 4 if it is located in a rural area. An economically disadvantaged student is one who is either participating in the National School Lunch Program or a member of a household that meets the income eligibility guidelines for free or reduced-price meals.

[14] All revenue, expenditure, and compensation figures are converted to 2010 dollars using the Midwest Region's CPI-U (BLS 2018). A school district's average local teacher experience is defined as the average number of years of experience its teachers have *within* the district. As in Ronfeldt, Loeb, and Wyckoff (2013), teacher attrition in year t is defined as the proportion of teachers in a given school district in year $t-1$ who left the district by year t.

[15] I also use an individual-level dataset published annually by the WDPI containing detailed information on the universe of Wisconsin public school teachers (WDPI 2016a). This dataset reports each teacher's first and last name, district and school of employment, birth year, and total salary. This information allows me to construct variables measuring student-teacher ratios and teacher salaries at the school level to explore how additional resources are distributed *within* the school district (to high schools, middle schools, or elementary schools).

C. *Student Outcomes*

District-Level Test Scores.—I also collect three measures of student outcomes from the WDPI. First, I collect data on each district's share of students who score in one of four proficiency levels (advanced, proficient, basic, or minimal performance) on the math and reading portions of the state's standardized test, the Wisconsin Knowledge and Concepts Examination (WKCE) (WDPI 2015). Federal law requires an annual review of student academic progress. In Wisconsin, students demonstrate their progress through their participation in the WKCE. The test is administered each November to students in fourth, eighth, and tenth grade.[16] The share of students who perform at the advanced or proficient levels is usually the focus of school district administrators in Wisconsin when analyzing the WKCE proficiency summary for district improvement purposes. However, I also collect the district's average scale score in order to calculate effect sizes in terms of standard deviations—a more common way to interpret effect sizes in the economics of education literature.

The WKCE is used as one of the measures of student outcomes for two main reasons. First, the WKCE is a "high-stakes" examination; test scores are used as one of several criteria for advancing students from fourth to fifth grade and from eighth to ninth grade. Second, Chetty, Friedman, and Rockoff (2014) show that impacts on student test scores are correlated with students' long-term outcomes, such as teenage pregnancy, college attendance, and earnings. Nevertheless, test scores are imperfect measures of learning and may not always reflect changes in human capital accumulation.[17] Therefore, I collect two additional district-level measures of student outcomes: the district's dropout rate and the share of students who enroll in postsecondary education.

Dropout Rate.—District-level dropout rates are reported as annual events for grades 7 through 12 (WDPI 2019a). Annual event dropout rates are used to track annual changes in the district's dropout behavior. The dropout rate for school district d in year t is calculated as the total number of students in grades 7–12 in district d who dropped out during year t divided by the total number of students in grades 7–12 who were expected to complete the school term in school district d in year t (the number of students who completed the school term plus the number of dropouts).[18]

[16] With the exception of students with severe cognitive disabilities, every public school student is required to participate in the WKCE.

[17] For instance, previous studies have found that test-based school accountability may incentivize educators to cheat by changing students' answers (Jacob and Levitt 2003) or to "teach to the test" (Neal 2012). The WKCE was designed to meet the accountability requirements of the No Child Left Behind Act and may therefore be particularly susceptible to these weaknesses.

[18] Dropouts are counted at most once in a given school year. A dropout in a given school year may, in a subsequent school year, reenroll in school, drop out again, or complete high school. The annual event dropout rate reported by the WDPI has several advantages over the rates reported by other states due to Wisconsin's implementation of the Individual Student Enrollment System (ISES). ISES was designed to document student movements into and out of the K–12 educational system. Data on student movements are used by ISES to determine whether a student is actually a dropout or not. Thus, every school district in the state follows a unified criteria for who qualifies as a dropout.

A dropout in t is defined as a student who was enrolled in the district at some point during t, was not enrolled at the beginning of $t + 1$ (by the third Friday in September), has not completed high school, and does not meet any of the following exclusionary conditions: (i) transfer to another public school district or private school (either inside or outside the state), (ii) transfer to a state-approved educational program, or (iii) temporary absence due to expulsion, suspension, a school-approved illness, or death.[19]

A key advantage of this definition is that any time a student exits the school district, the district must report to the WDPI the reason for the exit. This forces school districts to understand whether the student transferred to another public or private school inside or outside the state or whether the student exited due to any of the remaining exclusionary conditions described above. Therefore, the dropout rate used throughout this study will only classify a student as a dropout if either (i) the student actually dropped out or (ii) the school district was unable to obtain official written documentation that the student is either continuing in an educational program elsewhere or exited due to another exclusionary condition.

Postsecondary Enrollment.—The WDPI also reports the number of each school district's high school completers in year t who enroll in a postsecondary education program during the fall of $t + 1$ (WDPI 2019b).[20] For each school district, I obtain the number of high school completers who enroll (i) in a four-year institution within the state, (ii) a four-year institution outside the state, (iii) a two-year technical college or training program within the state, and (iv) a two-year technical college or training program outside the state.[21] I then divide each of these measures by the school district's ninth grade enrollment in $t - 3$ to obtain the *fraction* of district students that subsequently enroll in each type of postsecondary education program (e.g., four-year versus two-year).[22] I also show specifications in which the dependent variable is the logged number of high school completers who enroll in postsecondary education. In these specifications, I control for ninth grade enrollment on the right-hand side.

[19] Students who completed t but did not return as expected for $t + 1$ are counted as dropouts for $t + 1$.

[20] The WDPI merges individual high school completer data from Wisconsin school districts to postsecondary enrollment data from the National Student Clearinghouse (NSC). The NSC collects enrollment data from over 3,000 postsecondary institutions enrolling over 95 percent of all postsecondary students in the United States. These institutions include public and private universities, two-year technical colleges, and training programs.

[21] A limitation of measuring postsecondary enrollment as the school district's number of high school completers who subsequently enroll in a postsecondary institution is that students who complete high school in a given district did not necessarily spend the majority of their high school career in that district. For instance, if a student spends the majority of her high school career in a particular school district but transfers to a new school district as a senior, completes high school, and enrolls in a postsecondary institution, then she will not be counted as a postsecondary enrollee for the district in which she spent the majority of her career.

[22] I divide by the district's ninth grade enrollment in $t - 3$ rather than by the number of high school completers in t because the number of high school completers is clearly endogenous. As I show in Section VB, operational referendum approval leads to a large decline in the district's dropout rate. Using ninth grade enrollment in $t - 3$ in the denominator instead is likely to mitigate the endogeneity concern because the number of dropouts is much smaller in ninth grade.

Table 2—Summary Statistics for Fiscal, Academic, and Demographic Variables

Dependent variable	All districts	Never proposed	Proposed at least one	Difference (2) − (3)
Panel A. Fiscal outcomes				
Revenue limits PP	9,520	9,485	9,525	−40
	(954)	(816)	(972)	(95)
Total expenditures PP	10,555	11,826	10,375	1,450
	(1,384)	(1,391)	(1,286)	(298)
Instructional expenditures PP	6,432	6,991	6,353	637
	(780)	(698)	(758)	(145)
Support services PP	3,739	4,401	3,645	756
	(702)	(716)	(648)	(141)
Other expenditures PP	384	434	377	57
	(100)	(96)	(99)	(18)
Panel B. Student outcomes				
Dropout rate (percent)	1.59	5.76	0.99	4.77
	(2.08)	(2.67)	(1.04)	(0.88)
Percent adv. or prof., tenth grade	43.22	15.89	46.86	−30.98
	(15.96)	(14.87)	(12.10)	(6.01)
Average scale score, tenth grade	562.73	523.98	567.90	−43.91
	(19.62)	(20.74)	(12.35)	(8.39)
Postsecondary enrollment share	0.39	0.18	0.42	−0.24
	(0.13)	(0.12)	(0.10)	(0.05)
Panel C. District characteristics				
Student-staff ratio	8.51	8.48	8.51	−0.03
	(1.32)	(0.94)	(1.37)	(0.10)
Teacher experience	12.40	11.09	12.58	−1.49
	(1.87)	(1.41)	(1.85)	(0.16)
Teacher compensation	74,664	77,267	74,299	2,968
	(8,167)	(9,915)	(7,824)	(1,220)
Teacher attrition (percent)	10.01	11.68	9.78	1.90
	(4.20)	(3.37)	(4.25)	(0.15)
Property values PP	516,895	337,130	542,250	−205,120
	(430,530)	(263,487)	(443,324)	(68,660)
Urban centric locale	2.31	1.32	2.45	−1.13
	(1.13)	(0.82)	(1.09)	(0.30)
Fall enrollment	2,067	6,329	1,888	4,442
	(5,149)	(21,199)	(2,823)	(5,117)
Number of school districts	421	17	404	421

Notes: Column 1 shows means and standard deviations of outcomes computed over all district-year observations in the panel. Columns 2 and 3 show summary statistics separately for districts that proposed at least one referendum during the sample period and those that did not. Column 4 reports the point estimates and clustered standard errors at the district level of tests for equality of means. All variables except for WKCE scores and postsecondary enrollment are available from 1996–1997 to 2014–2015. Due to administrative changes to the WKCE, year-to-year comparisons are only valid from 2005 to 2013. Similarly, postsecondary enrollment data are only available from 2005 to 2014.

D. *Final Sample*

The final sample contains a balanced panel from 1996–1997 to 2014–2015 of the 404 Wisconsin school districts that attempted at least 1 measure during the sample period. Table 2 presents summary statistics. The table shows that the 17 school districts that never proposed a referendum during the sample period are vastly different from those that proposed at least one. Districts that proposed at least one referendum tend to have much better student outcomes: a lower dropout rate, a higher share of tenth grade students who score in the advanced or proficient levels on the math portion of the WKCE, a higher tenth grade math scale score, and a larger share of ninth grade students who subsequently enroll in a postsecondary institution within the state. Furthermore, these districts tend to have fewer students enrolled, are less likely to be in an urban area, have higher property values and levels of teacher experience, and have lower rates of teacher attrition.

The large differences between these two groups are likely driven by Milwaukee Public Schools (MPS), which did not attempt a referendum from 1996 to 2014. MPS enrolls a significantly larger share of students than the rest of school districts in Wisconsin and differs substantially along observables from other districts in the state, which are largely located in rural or suburban areas. Furthermore, it is important to note that while these two groups of districts differ substantially along observables, the vast majority of school districts ($\approx$ 96 percent) attempted at least one referendum during the sample period. The fact that these school districts enroll roughly 90 percent of all students in Wisconsin suggests that this study's findings are likely not driven by a selected sample of school districts.

III. Validity of the RD Design

The RD research design uses close elections to approximate a randomized experiment. This requires that, conditional on having a very close election, referendum success (or failure) is as good as random. In this section, I examine two diagnostics needed for the validity of the RD design in the Wisconsin setting.

I first demonstrate the need to focus on narrow elections if one wishes to obtain causal estimates of school spending. The first column of Table 3 reveals large preelection differences between winning and losing districts along several outcomes. School districts in which the referendum is eventually approved have significantly higher revenue limits, expenditures per pupil, and test scores in the year prior to the election. The second column restricts the sample to narrow elections. Focusing only on close elections eliminates all statistically significant differences between winning and losing districts and substantially shrinks the point estimates. These results indicate that observables are "locally" balanced in the year before the election, which should be the case if treatment assignment is indeed locally randomized. Accordingly, the last two columns indicate that narrowly winning and losing districts followed similar trajectories in the years prior to the election in the main outcomes that I examine throughout the study.

Second, a key assumption underlying the RD design is that school districts cannot precisely control voting results around the 50 percent vote share (Lee and Lemieux 2010). If treatment is indeed as good as random, then it should be equally likely that voters either just pass or just reject the referendum. Panels A and B of online Appendix Figure B.4 show simple histograms of the vote shares separately for operational and capital bond referenda, while panels C and D show the results of McCrary's (2008) test. All four panels show little evidence of discontinuities at the

Table 3—Local Balance of Treatment and Control Groups

	$(t-1)$		$(t-1)$ to (t)	
Dependent variable	(1)	(2)	(3)	(4)
Panel A. Fiscal outcomes				
Revenue limits PP	243	160	13	47
	(91)	(128)	(16)	(27)
Total expenditures PP	283	196	0	4
	(113)	(163)	(31)	(43)
Instructional expenditures PP	153	154	−22	−21
	(70)	(108)	(20)	(27)
Support services PP	133	56	20	23
	(50)	(68)	(18)	(25)
Other expenditures PP	−4	−14	1	2
	(8)	(11)	(2)	(3)
Panel B. Student outcomes				
Dropout rate	0.02	0.08	−0.05	−0.10
	(0.05)	(0.07)	(0.05)	(0.07)
Percent adv. or prof., tenth grade	2.29	−0.23	−0.15	0.21
	(1.03)	(1.58)	(0.79)	(1.19)
Average scale score, tenth grade	1.61	−0.80	−0.01	0.73
	(0.93)	(1.42)	(0.72)	(1.02)
Postsecondary enrollment share	0.01	−0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Only narrow elections	N	Y	N	Y

Notes: The table presents regressions of fiscal and student outcomes in the year before the election $(t-1)$ on an indicator of whether or not the referendum was eventually approved in time t. The first column shows differences in outcomes for all elections (operational and capital) from 1996 to 2014. The second column restricts the sample to elections that were decided by less than 6 percentage points of the vote share—the smallest bandwidth used throughout the main body of the paper. 302 unique school districts held 696 referenda with a vote share in this interval from 1996 to 2014. The last two columns repeat the first two specifications, but they take as the dependent variable the change in the specific outcome between $t-1$ and t. Data on individual referenda and district-level student outcomes come from the WDPI. District-level total current expenditures and current expenditures by source were collected from the NCES. Fiscal variables were converted to 2010 dollars using the Midwest Region's CPI-U.

50 percent cutoff. Altogether, there appears to be little cause for concern regarding the "as good as random" assumption of treatment assignment in close elections.

IV. Empirical Strategy

It is an open question how (or whether) the effects of capital improvements on student outcomes will differ from those of additional operational expenditures. A recent detailed literature review of the effects of school spending on student outcomes indicates that while studies examining additional operational expenditures generally find large improvements in student outcomes, evidence on the effectiveness of capital expenditure increases is less clear (Jackson 2018).

However, as previously mentioned, it is difficult to compare estimates across individual studies examining capital and operational spending independently since they each focus on states with large institutional differences. Therefore, simply comparing operational and capital spending estimates across contexts would likely conflate institutional differences with differences in the true marginal returns of each spending type. These limitations highlight the need for an empirical design that isolates the marginal effects of each type of expenditure within the same general setting.

A. *Dynamic RD Approach*

Suppose that district d holds an operational and a capital bond referendum in year t. The operational referendum receives vote share v_{dt}^{o}, while the capital bond referendum receives vote share v_{dt}^{b}. Let $P_{dt}^{o} = \mathbf{1}(v_{dt}^{o} > 50)$ and $P_{dt}^{b} = \mathbf{1}(v_{dt}^{b} > 50)$ be indicators for the passage of an operational and a capital bond referendum, respectively: equal to one if district d passes the specific type of referendum in school year t and zero otherwise (either if there was no referendum of that type held in year t or if a proposed referendum was rejected). Then, a district-level outcome y_{dt} (e.g., revenue limits, expenditures, or test scores) can be specified as a function of the full history of successful operational and capital bond referenda:

$$y_{dt} = \sum_{\tau=\underline{\tau}}^{\bar{\tau}} P_{d,t-\tau}^{o} \beta_{\tau}^{TOT} + P_{d,t-\tau}^{b} \gamma_{\tau}^{TOT} + \epsilon_{dt}. \tag{1}$$

The parameters of interest, β_{τ}^{TOT} and γ_{τ}^{TOT}, represent the TOT effect of an operational and a capital bond referendum approval, respectively. For instance, β_{τ}^{TOT} provides the causal effect on y_{dt} of exogenously passing an operational referendum in district d in year $t - \tau$ and "prohibiting" the district from passing any subsequent operational or capital bond referenda (since all intermediate referendum approvals are held constant). Therefore, a consistent estimate of β_{τ}^{TOT} will isolate the impact of an operational referendum passage in $t - \tau$ (with no intermediate referendum-approved changes to the district's resources) on a district's outcome in t. Similarly, a consistent estimate of γ_{τ}^{TOT} will isolate the impact of a successful capital bond referendum in $t - \tau$ (with no intermediate referendum-approved changes to the district's resources) on a district's outcome in t.

An alternative to examining TOT effects is to focus on the impact of passing an operational or capital bond referendum in $t - \tau$ and "allowing" the school district to make decisions regarding subsequent referenda as its residents wish. This effect, known as the "intent to treat" (ITT), incorporates effects of $P_{d,t-\tau}^{o}$ and $P_{d,t-\tau}^{b}$ on y_{dt} operating through additional operational and capital bond referendum wins in intermediate years. As a result, ITT estimates do not necessarily reflect the impact of additional expenditures solely associated with the passage of a particular referendum. In the main body of the paper, I focus only on estimates of TOT effects. Estimates of ITT effects are discussed in Section VC and online Appendix C and yield remarkably similar results.

A simple regression like equation (1) would likely yield biased estimates of both the β_{τ}^{TOT} and the γ_{τ}^{TOT} terms, as factors in ϵ_{dt} are likely to be correlated with both concurrent and past successful referenda of either type. However, since there is no evidence of manipulation of the vote share near the 50 percent threshold for either type of referendum (see online Appendix Figure B.4), the correlation between P_{dt}^{o} and ϵ_{dt} and between P_{dt}^{b} and ϵ_{dt} can be kept close to zero by focusing only on close elections. Therefore, to estimate the causal impact of additional operational and capital spending, one can use an RD design that compares outcomes in school districts that narrowly pass each type of referendum to those where the initiative is narrowly defeated. I follow Cellini, Ferreira, and Rothstein (2010) and implement the main design using a parametric framework that retains all observations in the sample but absorbs variation from nonclose elections with flexible controls for the vote share. However, I also show robustness checks using nonparametric strategies, including local linear regressions.

As Cellini, Ferreira, and Rothstein (2010) show, if the standard assumption that passing a referendum is "as good as random" in narrow elections holds (an assumption tested in Section III), the endogeneity described above can be mitigated by augmenting equation (1) with flexible polynomials of degree g in the vote shares, $f_g(v^o_{d,t-\tau})$ and $f_g(v^b_{d,t-\tau})$, and with indicators for the presence of an operational and a capital bond referendum on the ballot in year $t-\tau$, $m^o_{d,t-\tau}$ and $m^b_{d,t-\tau}$.[23] After adding school year (θ_t) and district-level (μ_d) fixed effects, the estimating equation becomes

$$y_{dt} = \sum_{\tau=\underline{\tau}}^{\bar{\tau}} \Big[P^o_{d,t-\tau}\beta^{TOT}_\tau + m^o_{d,t-\tau}\,\kappa_\tau + f_g\big(v^o_{d,t-\tau}\big) + P^b_{d,t-\tau}\gamma^{TOT}_\tau + m^b_{d,t-\tau}\pi_\tau + f_g\big(v^b_{d,t-\tau}\big)\Big] + \mu_d + \theta_t + \varepsilon_{dt}. \tag{2}$$

This equation is estimated on a school district–year panel from 1996–1997 to 2014–2015 where each district-year observation is used exactly once for the 404 school districts that attempted at least 1 operational or capital bond referendum during the sample period.[24] Standard errors are clustered at the district level. For the main results of the paper, I specify $f_g(\cdot)$ as a third-order polynomial. However, I also show robustness checks with linear and quadratic specifications instead.

Intuitively, equation (2) identifies the β^{TOT}_τ (γ^{TOT}_τ) coefficients by contrasting between school districts where an operational (capital) referendum in year $t-\tau$ narrowly passed and those where the election was narrowly rejected, but the sequence of previous and subsequent operational and capital bond proposals, vote shares, and successful referenda is similar. Therefore, this strategy allows me to fully exploit the joint distribution of operational and capital bond referenda by holding constant intermediate election outcomes and isolating the independent causal effects of each type of referendum.

[23] $v^o_{d,t-\tau} = 0$ if district d did not hold an operational referendum in year $t-\tau$. Similarly, $v^b_{d,t-\tau} = 0$ if district d did not hold a capital bond referendum in year $t-\tau$.

[24] In cases where a school district holds multiple elections of the same type (operational or capital bond) in the same year, I keep only the initiative with the lowest margin of victory (or defeat). However, the results are robust to alternative criteria such as keeping the initiative with the largest vote share in favor (as in Cellini, Ferreira, and Rothstein 2010) or the first initiative in each year (as in Hong and Zimmer 2016).

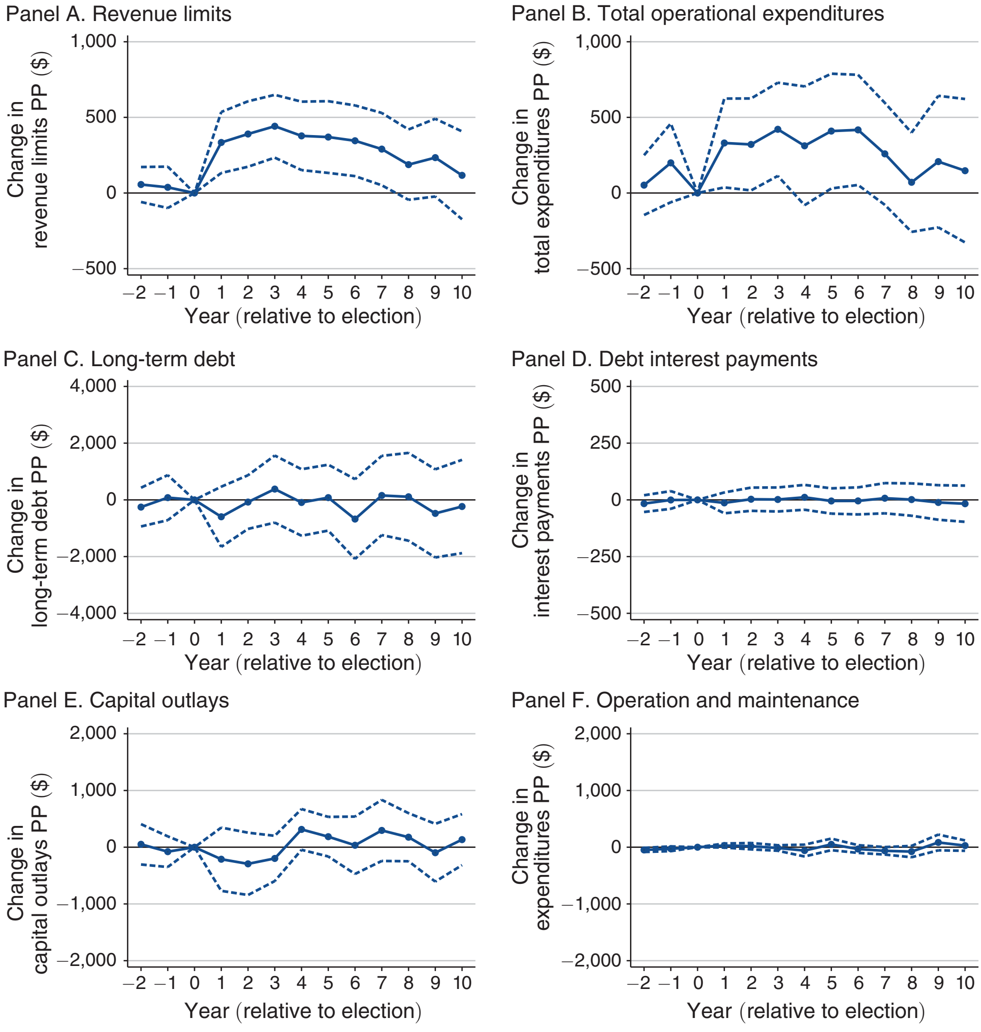

Figure 1. TOT Estimates of Successful Operational Referenda ("First-Stage" Evidence)

Notes: The figure presents results from the estimation of equation (2). The solid line provides a visual representation of estimates of the β_{τ}^{TOT} terms, while the dashed line shows the corresponding 90 percent confidence intervals for up to 10 years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

V. The Effect of Passing an Operational Referendum

A. *"First-Stage" Evidence: Impacts on Operational Spending*

Results from the estimation of equation (2) are shown in Figure 1. The figure presents estimates of the dynamic treatment effects of operational referendum approval on district-level fiscal outcomes by year relative to the election. It provides a visual representation of estimates of the β_{τ}^{TOT} terms along with 90 percent

Table 4—TOT Estimates of Narrow Operational Referendum Success on Fiscal Outcomes

Dependent variable	Postelection effect
Revenue limits PP	316
	(116)
Op. expenditures PP	298
	(160)
Inst. expenditures PP	198
	(95)
Support services PP	111
	(86)
Other expenditures PP	−11
	(9)

Notes: The table presents results from the estimation of equation (2). It shows the average of the estimated β_τ^{TOT} terms across the first ten postelection years along with standard errors clustered at the district level in parentheses. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

confidence intervals 2 years before and up to 10 years after the election. Table 4 summarizes the average effects across the first ten postelection years.

Panel A of Figure 1 shows that districts that narrowly approve an operational referendum have similar revenue limits to districts in which the initiative is narrowly defeated in the two years leading up to the election. However, narrowly passing an operational referendum increases revenue limits per pupil by roughly $300 in the year following the election. This magnitude increases slightly and persists only for the first seven years after the election, plausibly due to the pooling of recurring and nonrecurring initiatives. Panel B shows that increases in revenue limits translate into higher levels of per-pupil operational spending. The estimates in Table 4 show that narrowly approving an operational referendum increases operational expenditures by roughly $300 per pupil each year during the 10 years following the initial election. This effect corresponds to a 3 percent increase relative to the average operational expenditure per pupil in my sample (see Table 2). Table 4 shows that most of the additional resources are spent in the instructional account.[25] The remainder of the additional resources are spent in the account for support services (e.g., expenditures to hire additional guidance counselors, social workers, and school psychologists).[26]

Even though the β_τ^{TOT} terms in equation (2) represent the TOT effects of approving an *operational* referendum, one may still be worried about conflating the effects of approving an operational referendum with those of approving a capital bond referendum. First, school districts could place both types of questions on the ballot during the same academic year. Second, although operational referenda are earmarked for

[25] Expenditures in this account include any activity dealing directly with the interaction between teachers and students (e.g., expenditures to hire additional teachers, aides, and classroom assistants and/or to increase the salaries of these workers).

[26] Online Appendix Figure B.5 examines changes in detailed expenditures within the account for support services. The estimates indicate that the increase in the support services account is largely driven by pupil expenditures. Examples of such expenditures include attendance and social work services, guidance services, and health services. There is little evidence that expenditures on administrators or student transportation increase following a successful referendum.

operational expenditures, one may be concerned that districts will find a way to divert resources toward capital inputs given the fungibility of expenditures.

The estimates shown in panels C, D, E, and F of Figure 1 provide no evidence that narrowly approving an operational referendum leads to changes in variables that would change as a result of a successful capital bond referendum. Specifically, there is no evidence of changes in outstanding long-term debt or debt interest payments per pupil. Similarly, there is no evidence of changes in district-level capital outlays or in expenditures for the operation and maintenance of grounds, buildings, and equipment. These estimates indicate that there is enough variation in the timing of operational and capital questions to separately identify changes in spending induced by each type of referendum, and that all of the additional funds from a successful operational referendum stick in operational accounts and are not reallocated to capital outlays.

B. *"Second-Stage" Evidence: Impacts on Student Outcomes*

This section explores the effects of narrowly passing an operational referendum on three district-level measures of student outcomes: the district's dropout rate, test scores, and postsecondary enrollment. Figure 2 presents estimates of the dynamic treatment effects of operational referendum approval on district-level student outcomes by year relative to the election. Table 5 summarizes average effects across the first ten postelection years. To explore the sensitivity of the main results to alternative orders of the polynomial, both Figure 2 and Table 5 present results separately for linear, quadratic, and cubic specifications of the vote shares.

Dropout Rate.—Panel A of Figure 2 shows that barely passing an operational referendum leads to a significant decline in the district's dropout rate. This effect persists for the first eight years following the election. The estimates across all three specifications in Table 5 show that narrowly passing an operational referendum decreases the district's dropout rate by an average of 0.07 to 0.11 percentage points across all 10 postelection years. This effect corresponds to roughly a 7 percent to 11 percent decline relative to the average rate in the sample (see Table 2).

Test Scores.—Panel B of Figure 2 shows that narrowly passing an operational referendum sharply increases the percent of students who score in the advanced or proficient levels on the math portion of the tenth grade WKCE. The estimates across all three specifications in Table 5 show that across all postelection years, the percent of students who score advanced or proficient is roughly 4 to 6 percentage points higher in districts that narrowly passed an operational referendum.[27]

[27] Online Appendix Figure B.6 investigates additional margins of the test score impacts. It shows the effect of narrowly passing an operational referendum on the percent of students in the school district who score in the minimal (panel A) and basic (panel B) proficiency levels on the math portion of the tenth grade WKCE. The figure shows that both the percent of students who score in the minimal proficiency level and the percent who score in the basic level sharply decline following a narrow operational referendum win. These estimates indicate that test score improvements due to additional operational spending are dispersed throughout the score distribution and are not concentrated in the middle-to-upper end of the distribution.

Table 5—TOT Estimates of Narrow Operational Referendum Success on Student Outcomes

Dependent variable	Postelection effect
Panel A. Cubic specification	
Dropout rate	−0.11
	(0.07)
Percent adv. or prof., tenth grade math	5.89
	(1.80)
Average tenth grade math score	4.53
	(1.81)
log(postsecondary enrollment)	0.07
	(0.05)
Panel B. Quadratic specification	
Dropout rate	−0.08
	(0.07)
Percent adv. or prof., tenth grade math	3.91
	(1.67)
Average tenth grade math score	3.29
	(1.62)
log(postsecondary enrollment)	0.12
	(0.04)
Panel C. Linear specification	
Dropout rate	−0.07
	(0.07)
Percent adv. or prof., tenth grade math	4.40
	(2.00)
Average tenth grade math score	3.56
	(1.88)
log(postsecondary enrollment)	0.14
	(0.04)

Notes: The table presents results from the estimation of equation (2) using a cubic, quadratic, and linear specification of the vote shares. It summarizes the average of the estimated β_{τ}^{TOT} terms across the first ten postelection years. Standard errors clustered at the school district level are shown in parentheses. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

To better understand the economic significance of these effects, the third row of each panel of Table 5 presents the estimates obtained when using the district's average scale score on the math portion of the tenth grade WKCE as the dependent variable. These estimates show that narrowly approving a referendum leads to an average increase in test scores of roughly 3 to 4 points across all postelection years. These improvements correspond to an increase in test scores of roughly 7 percent to 9 percent of a student-level standard deviation.[28,29]

[28] These estimates are obtained by dividing the test score improvements in WKCE points by 43.2—the standard deviation of the 2002–2003 mathematics test score distribution for tenth grade. This is the earliest year for which the WDPI publishes standard deviations of student-level test score distributions.

[29] Online Appendix Table B.1 investigates test score outcomes in much more detail. It shows test score effects for tenth, eighth, and fourth grade and for both reading and math. The estimates show that narrowly approving an operational referendum generally has a larger impact on math test scores than on reading test scores. For instance, narrowly passing an operational referendum increases the percent of students who score advanced or proficient on the math and reading portions of the tenth grade WKCE by roughly 6 and 3 percentage points, respectively. This finding

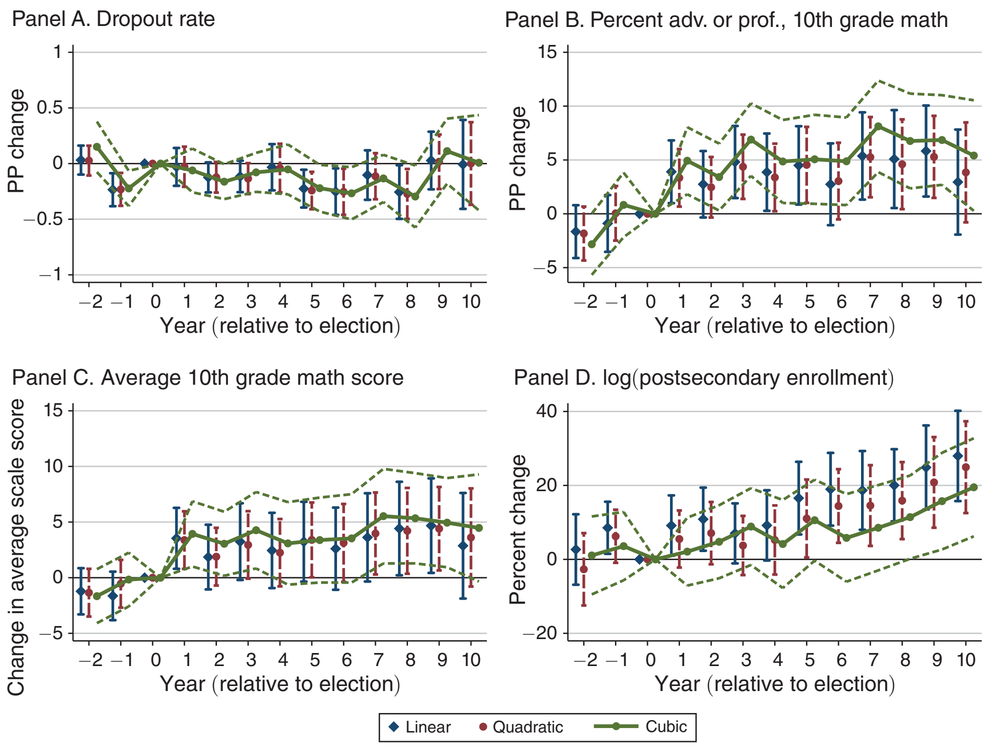

Figure 2. TOT Estimates of Successful Operational Referenda ("Second-Stage" Evidence)

Notes: The figure presents results from the estimation of equation (2) using a linear, quadratic, and cubic specification of the vote share. It shows estimates and 90 percent confidence intervals of the β_{τ}^{TOT} terms by year relative to the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

It is difficult to benchmark the magnitude of these effects against other studies in the school spending literature due to differences in institutional contexts and in the amount of additional spending. For instance, Abott et al. (2020) focus on grades 3 through 8 and find that increasing school spending by $1,000 per pupil raises test scores by approximately 15 percent of a standard deviation. Similarly, Lafortune, Rothstein, and Schanzenbach (2018) focus on grades 4 and 8 and find that an increase in spending of $1,000 per pupil raises test scores by roughly 24 percent of a standard deviation. If one assumes that spending effects are linear, then my most conservative estimate indicates that allocating an additional $1,000 to per-pupil operational expenditures increases tenth grade math test scores by roughly 23 percent of a standard deviation. Therefore, even though all three of these studies

is not particularly surprising, as reading skills may be less malleable than math skills in later grades. The table also shows that test score effects are much larger for tenth and eighth graders than for fourth graders. In fact, I find no evidence that additional operational expenditures have any impact on fourth grade math and reading test scores. In Section VE, I show that this is likely the result of districts disproportionately allocating the additional operational funds toward high schools and junior/middle schools and not toward elementary schools.

focus on different school grades and institutional contexts, the estimated spending effects are similar.

Postsecondary Enrollment.—Finally, panel D of Figure 2 shows estimates of the effects of operational referendum approval on postsecondary enrollment. The dependent variable in this specification is the (logged) number of high school completers in year t who enroll in a postsecondary institution within the state in the fall of $t + 1$. I control for the district's ninth grade enrollment in $t - 3$ on the right-hand side of the equation.[30] The estimates show that five years after the election, the number of high school completers who subsequently enroll in postsecondary education within the state is roughly 10 percent higher in treated districts. The treatment effect is increasing in the number of years since the election: ten years after referendum approval, postsecondary enrollment is over 20 percent higher in treated school districts. Across all three specifications, the estimates in Table 5 indicate that narrowly passing an operational referendum increases postsecondary enrollment within Wisconsin by 7 percent to 14 percent across all postelection years.[31]

A limitation of the data is that I do not observe the quality of the postsecondary education programs that students enroll in. While a comparison between four- and two-year institutions is certainly an imperfect proxy for differences in college quality, recent work has shown that the private returns of four-year postsecondary institutions are larger than those of two-year colleges.[32] Therefore, in the absence of better college quality data, online Appendix Figure B.8 shows the impact of passing an operational referendum separately for the number of high school completers who subsequently enroll in a four-year program within Wisconsin (panel A) and those who subsequently enroll in either a two-year technical school or a training program within the state (panel B). The figure shows clear evidence that the increase in postsecondary enrollment within the state is largely driven by enrollments at four-year institutions.[33]

[30] As described in Section IIC, I control for ninth grade enrollment in $t - 3$—rather than for the number of high school completers in t—because the number of high school completers is clearly endogenous.

[31] While only 13 percent of students in Wisconsin public schools attend postsecondary education outside the state, one may still be concerned that increases in postsecondary enrollments within Wisconsin do not represent an increase in overall college going but rather a substitution away from out-of-state to in-state enrollments. Online Appendix Figure B.7 examines the impact of narrowly passing an operational referendum on the number of high school completers who subsequently enroll in a postsecondary education program (either a four-year program, a two-year technical school, or a training program) outside the state. The figure shows no evidence that additional operational expenditures lead to declines in postsecondary enrollments outside the state at the same time that enrollments within the state increase. Therefore, I interpret the rise in postsecondary education enrollments within Wisconsin as evidence that additional operational expenditures lead to increases in overall college going.

[32] Specifically, Smith, Goodman, and Hurwitz (2020) show that enrolling in a public 4-year institution (relative to a 2-year institution) raises a student's household income around age 30 by 20 percent and has even larger impacts for students from low-income high schools.

[33] Online Appendix D explores how the estimates of additional spending on postsecondary enrollment in this study compare to those in Jackson, Johnson, and Persico (2016) and to those of other educational interventions. The calculations in the online Appendix show that my estimates are remarkably similar to those in Jackson, Johnson, and Persico (2016) and suggest that providing school districts with additional discretionary operational funds is much more cost effective than simply reducing elementary school class sizes.

C. *Robustness Checks*

The results presented so far indicate that additional school spending induced by operational referendum approval in narrow elections translates into substantially better student outcomes. This section presents a variety of alternative specifications that are meant to probe the robustness of the main results of the paper.

RD Estimator and Bandwidth Selection.—As in Hong and Zimmer (2016); Martorell, Stange, and McFarlin (2016); and Cellini, Ferreira, and Rothstein (2010), the main results in this paper implement the dynamic RD strategy using a parametric framework that retains all elections in the sample but absorbs variation from nonclose elections with flexible controls for the vote share. A global parametric framework is necessary in dynamic RD because the strategy is explicitly designed to hold constant the sequence of future referenda, and this sequence may include both marginal and inframarginal measures. Therefore, if one implements a restricted-bandwidth framework, as in nonparametric local linear regressions, then future inframarginal referenda will be excluded from the analysis and the RD design will not yield the desired TOT estimate.

To examine the robustness of the main findings to bandwidth selection, this section instead presents estimates of ITT effects. While ITT effects have several limitations over TOT effects, they are estimable using standard RD techniques associated with cross-sectional RD designs.[34] Figures 3 and 4 present typical RD plots for all operational referendum attempts from 1996 to 2014. The figures show little evidence of a discontinuity near the threshold two years prior to the election in any of the main fiscal and student outcomes examined throughout the study but reveal improvements in these outcomes in the five years following the election.

Online Appendix Table B.2 shows the results of local linear regressions. The first column shows little evidence that academic outcomes differed in districts that eventually passed and lost a close operational referendum. However, the second column shows clear evidence that academic outcomes improved substantially in the years following the election in districts that narrowly approved a referendum. The robustness of the main results of the paper to the choice of estimator and bandwidth reinforces the finding that additional operational expenditures lead to large improvements in student outcomes.

Demographic Changes.—Given that I use aggregate district-level data, one may be concerned with changes in school districts' student composition as a result

[34] Estimating ITT effects corresponds to examining the impact of an operational referendum passage in some year on a district's outcomes in a later year without controlling the district's behavior in intermediate years. Thus, to estimate ITT effects one can simply examine outcomes in subsequent years for school districts that pass or fail a given close operational referendum. While these estimates will inherently capture both the direct and indirect effects of a successful operational referendum (through its effects on the probability of holding—and passing—subsequent operational and capital bond referenda), this approach allows me to (i) ensure the main outcomes of the paper are not driven by the panel structure of the dynamic RD design, (ii) present standard RD plots for key outcome variables, and (iii) implement more common nonparametric, local linear regressions. Online Appendix C describes the ITT estimator in much more detail.

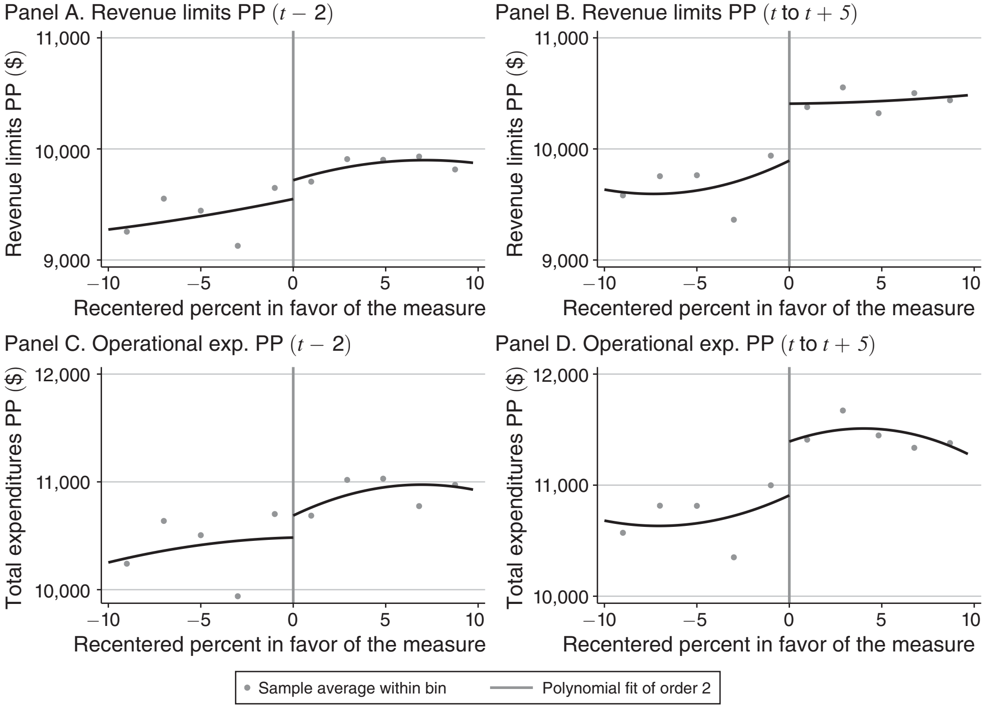

Figure 3. RD Plots for Fiscal Outcomes (Operational Referenda)

Notes: The figure shows average school district fiscal outcomes in 2 percentage point bins along with a second-order polynomial fit. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the referendum was approved with a vote share in the (50%–52%] interval. Panels A and C show outcomes in $t-2$, while panels B and D present outcomes in t through $t+5$; t represents the year of the focal operational referendum. The local polynomial estimator was constructed with a uniform kernel function, which is standard in the literature.

of referendum approval.[35] To test this, I estimate equation (2) with each of the following district demographic variables as the outcome of interest: the percent of economically disadvantaged students, the percent of minority students, and total enrollment. The results from this estimation are shown in online Appendix Figure B.9 and provide little evidence of changes in district composition due to operational referendum approval.

For instance, the average of the estimated effects on the district's percent of economically disadvantaged students across the first 10 postelection years is a decline of roughly 0.61 percentage points, with a standard error of 1.16. Thus, I can rule out even a 3 percentage point decline in this share (for reference, 34 percent of students are classified as economically disadvantaged in the average district in my sample). A 1 percentage point decline in the percent of economically disadvantaged students

[35] For instance, if affluent parents of students in districts where a referendum barely failed perceive the loss may be disruptive to instruction, they may choose to remove their children from districts in the control group and enroll them in either private or treated schools. If this were the case, my estimates could be driven by the change in student composition, rather than the direct effects of additional school spending.

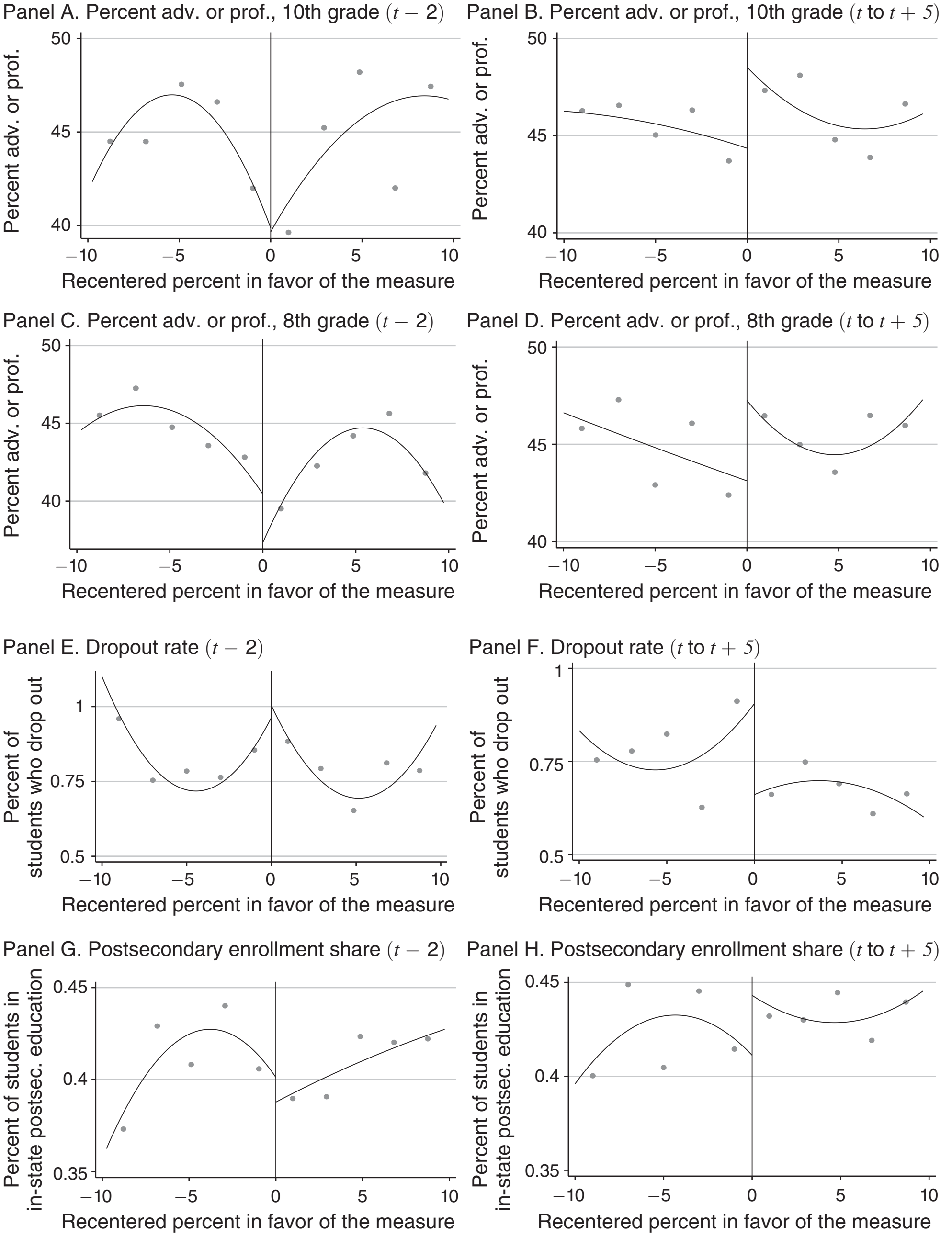

Figure 4. RD Plots for Student Outcomes (Operational Referenda)

Notes: The figure shows average school district student outcomes in 2 percentage point bins along with a second-order polynomial fit. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the referendum was approved with a vote share in the (50%–52%] interval. The local polynomial estimator was constructed with a uniform kernel function, which is standard in the literature.

in my sample is associated with an increase of 1 percent of a standard deviation in tenth grade math test scores.[36] Therefore, I can rule out effects on test scores as large as 3 percent of a standard deviation from this channel—much smaller than the 7–9 percent increase documented in Section VB. Furthermore, the dynamics of the impacts shown in online Appendix Figure B.9 indicate that the effect of referendum approval on the percent of economically disadvantaged students does not become negative until five years after the election—yet test score improvements shown in Figure 2 occur much sooner. Therefore, it is unlikely that changes in student composition are driving the main results of the paper.

D. *Heterogeneity by Initial Share of Economically Disadvantaged Students*

Online Appendix Figure B.10 examines heterogeneity in the effect of a successful operational referendum by a school district's initial share of economically disadvantaged students. It presents estimates of equation (2) separately for districts with an initially high share of economically disadvantaged students and those with an initially low share.[37] Although the confidence intervals become wider as a result of a smaller sample size, the figure shows that improvements in student outcomes are largely driven by districts with an initially high share of economically disadvantaged students. This finding is consistent with previous studies showing that improvements in student outcomes from additional school spending tend to be concentrated among low-income students (Candelaria and Shores 2019; Jackson, Johnson, and Persico 2016; Rauscher 2019).

E. *Exploring Mechanisms*

This section examines whether changes to specific observable school inputs can be (at least partially) credited as likely mechanisms for the observed improvements in student outcomes. I focus on four key inputs employed in the school quality literature: a school district's student–licensed staff ratio, teacher compensation, teacher experience, and teacher attrition.

Smaller class sizes have been shown to increase standardized test scores, the likelihood that students take college-entrance exams, and high school graduation rates (Bloom and Unterman 2014; Krueger and Whitmore 2001). Furthermore, additional counselor appointments have been shown to increase student achievement and reduce the frequency of disciplinary incidents and other behavioral problems (Carrell and Hoekstra 2014; Reback 2010). Panel A of online Appendix Table B.3 shows that narrowly passing an operational referendum led to a decline of roughly 0.2 students ($\approx$2 percent) in the student–licensed staff ratio. Staff in this category

[36] Of course, this association is not causal and is likely an overestimate. In fact, even conditioning on district fixed effects substantially reduces this estimate. Moreover, credible estimates of the impacts of increases in the share of economically disadvantaged students on test scores are typically much smaller than this association (e.g., Hoxby and Weingarth 2005). Nevertheless, using an upper-bound estimate of this relationship is a useful exercise to understand the extent to which impacts on student composition could be driving the results.

[37] I classify a school district as having an initially high share of economically disadvantaged students if its share falls above the median of the Wisconsin 2000–2001 school district distribution (the earliest year this variable is made publicly available).

include all licensed school staff such as teachers, guidance counselors, and school psychologists. This effect is consistent with the observed increases in expenditures in the instruction and support services accounts.

Referendum approval also led to increases of roughly half a year in average local teacher experience. Increases in teacher experience have been shown to improve student test scores directly (Papay and Kraft 2015; Rockoff 2004). Furthermore, the increase in teacher experience could reflect the decline in teacher attrition shown in the third row of the table, though this estimate is imprecise. Holding compositional effects constant, teacher attrition has been shown to disrupt instruction (Baron 2018; Ronfeldt, Loeb, and Wyckoff 2013). Thus, the observed increase in teacher experience may have both direct and indirect positive effects on student outcomes.

Narrowly passing an operational referendum also led to a small increase in teacher compensation of roughly 2 percent. Increases in teacher compensation may help school districts attract and retain a more highly qualified teaching workforce. While there may be other mechanisms through which additional operational spending improves student outcomes, the results in this section suggest that the results are driven, at least partially, by a combination of reductions in class sizes and teacher attrition, additional licensed staff, and increases in teacher experience and compensation.[38]

VI. The Effect of Passing a Capital Bond Referendum

A. *"First-Stage" Evidence: Bond Approval and Capital Expenditures*

Figure 5 shows estimates of the dynamic treatment effects of capital bond referendum approval on district-level fiscal outcomes by year relative to the election. The solid line provides a visual representation of estimates of the γ_τ^{TOT} terms, while the dashed line shows the corresponding 90 percent confidence intervals for 2 years before and up to 10 years after the election.

Panels A and B show that capital bond referendum approval in a narrow election results in large and immediate increases in both outstanding long-term debt and debt interest payments per pupil. Narrowly passing a capital bond referendum also results in sharp increases in capital spending that are concentrated in the first two years after the election (panel C). In the year following the election, capital spending increases by roughly \$4,000 (200 percent) per pupil. This effect begins to decline two years after the election and completely dissipates by the third year. This pattern is remarkably similar to the one documented by studies in California

[38] Finally, while school-level expenditure data are not available in Wisconsin, examining how inputs are distributed within the district and across schools may shed some light on why improvements in these mechanisms have large impacts on tenth and eighth grade test scores but not on fourth grade. Panels B, C, and D of online Appendix Table B.3 show the impact of passing an operational referendum on student-teacher ratios and teacher salaries separately for the district's high schools, junior/middle schools, and elementary schools. The table provides evidence that the decline in the student–licensed staff ratio documented above is primarily driven by high schools in the district. Similarly, increases in teacher compensation appear to be driven entirely by the district's junior/middle schools. There is no evidence of improvements in these inputs in elementary schools. Thus, it is likely that the lack of observed impact on fourth grade test scores is the result of districts disproportionately allocating additional operational funds toward high schools and junior/middle schools and not toward elementary schools.

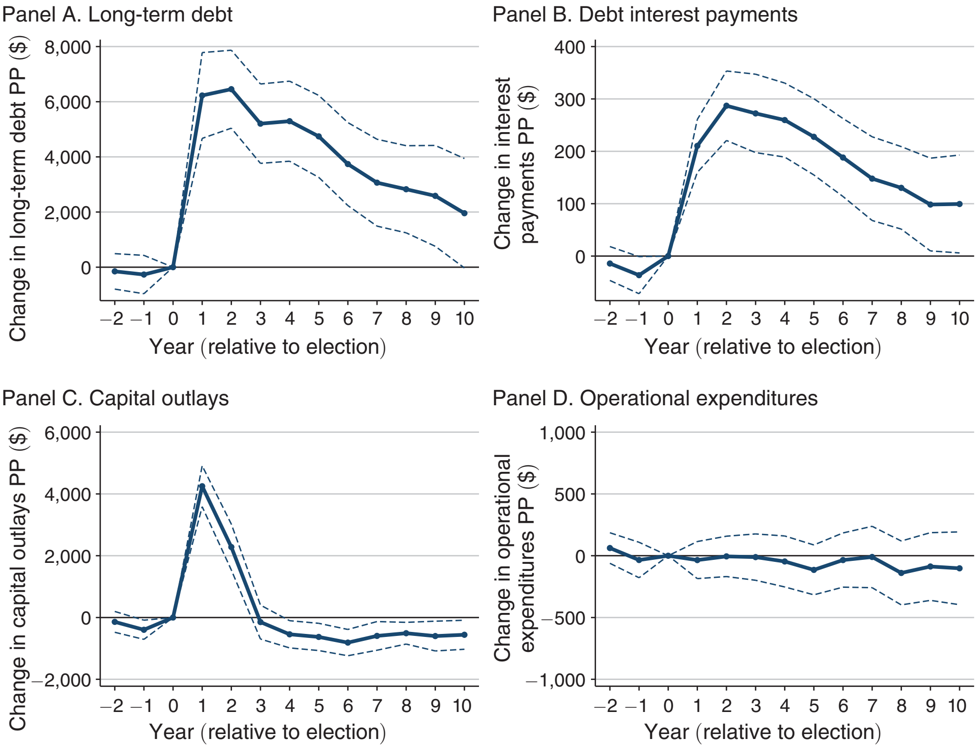

Figure 5. TOT Estimates of Successful Capital Bond Referenda ("First-Stage" Evidence)

Notes: The figure presents results from the estimation of equation (2). It shows estimates of the dynamic treatment effects of capital bond referendum approval on district-level fiscal outcomes by year relative to the election. The solid line provides a visual representation of estimates of the γ_{τ}^{TOT} terms, while the dashed line shows the corresponding 90 percent confidence intervals for up to 10 years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

(Cellini, Ferreira, and Rothstein 2010), Texas (Martorell, Stange, and McFarlin 2016), and Michigan (Hong and Zimmer 2016).[39]

Even though these expenditures are earmarked for local capital improvements, one may be concerned that districts will find a way to divert resources toward noncapital inputs given the fungibility of expenditures. I find strong evidence against this prediction. As with operational referenda, all of the additional resources induced by a successful capital bond referendum stick in the capital outlay account and are not reallocated to operating expenditures (Figure 5, panel D), which allows me to

[39] Importantly, the average annual increase in capital expenditures across the first ten postelection years is roughly $300 per pupil—which is identical to the operational expenditure dollar increase stemming from successful operational referenda. However, the average school district in my sample spends only approximately $1,000 each year in capital outlays. Thus, the annual increase in capital expenditures documented in this section is roughly 30 percent—a much larger increase relative to the 3 percent increase in operational expenditures documented in Section VA.

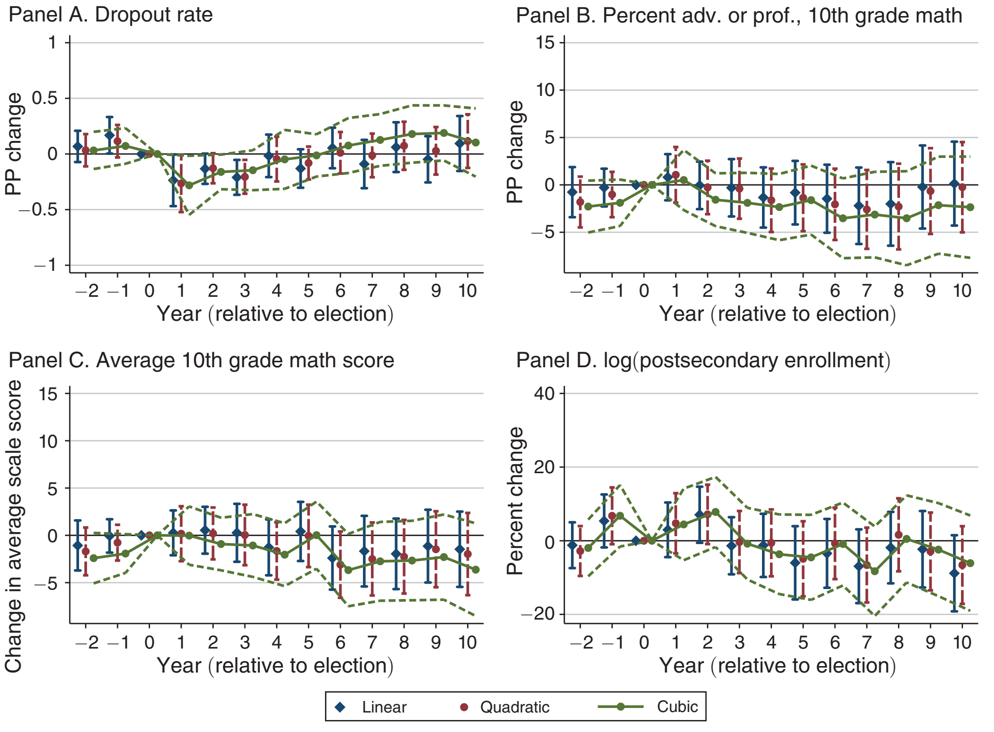

Figure 6. TOT Estimates of Successful Capital Bond Referenda ("Second-Stage" Evidence)

Notes: The figure presents results from the estimation of equation (2) using a linear, quadratic, and cubic specification of the vote share. It shows estimates of the γ_τ^{TOT} terms and corresponding 90 percent confidence intervals for up to 10 years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

isolate capital expenditure effects. Thus, estimates of the impact of capital bond passage can be interpreted as the effects of school facility investments.

B. *"Second-Stage" Evidence: Capital Expenditures and Student Outcomes*

Figure 6 examines the impact of narrowly passing a capital bond referendum on the four academic outcomes examined throughout the study: the district's dropout rate, the percent of students who score advanced or proficient on the math portion of the tenth grade WKCE, the average tenth grade WKCE math score, and the number of high school completers who subsequently enroll in a postsecondary education program within the state. Consistent with studies in California (Cellini, Ferreira, and Rothstein 2010) and Texas (Martorell, Stange, and McFarlin 2016), the results provide little evidence that school capital campaigns improve student outcomes, even ten years after the initial election. TOT estimates of the impact of capital bond passage on test scores, dropout rates, and postsecondary enrollment are close to zero and mostly statistically insignificant. For instance, across most specifications and postelection years, I can rule out effects as large as 5 percent of a standard deviation on test scores.

Online Appendix Table B.4 provides a plausible explanation for these results. The table examines the impact of a successful capital bond referendum on the four key district inputs described above: the district's student–licensed staff ratio, teacher experience, teacher compensation, and teacher attrition. In contrast to the large documented effects of successful operational referenda on these variables, there is little indication that additional capital expenditures affect these inputs. These results are consistent with textual analyses of the intended purpose of bond-approved resources, which reveal that school districts often use additional capital expenditures to repair, maintain, and upgrade existing structures rather than to build new ones.

C. *Heterogeneity by Initial Condition of School Infrastructure*

Theoretically, it is possible that the average null impact documented above masks substantial heterogeneity by the initial condition of a school district's infrastructure. In other words, there may be diminishing returns to school facility investments: school districts with buildings in poor condition may experience substantial benefits from additional capital outlays, whereas school districts with buildings in excellent condition may not. This section empirically tests these predictions.

Online Appendix Figure B.11 shows estimates of the impact of school facility investments separately for school districts with an initial (at the beginning of the sample) building condition described as "excellent" or "good" and school districts with an initial building condition described as "adequate," "fair," "poor," or "in need of replace."[40] Altogether, the figure provides little evidence that school facility investments improve academic outcomes, even for districts where expenditures may have a higher marginal rate of return.[41]

D. *Comparison to Previous Studies of Capital Spending*

These findings differ from those of recent studies showing that large-scale school construction projects in contexts where school facilities were initially in poor condition or nonexistent generally improve student outcomes. For instance, Neilson and Zimmerman (2014) study a comprehensive school construction project in New Haven, Connecticut. The project, financed mainly through state and federal sources, cost roughly $70,000 per student and involved rebuilding virtually every school

[40] In 1998, the Wisconsin legislature passed a law requiring the WDPI to survey the condition of each public school building in the state. The survey was conducted during the 1998–1999 academic year and contains detailed information on the condition of each school building at that time. The average school district in Wisconsin has four buildings. I take a weighted average of the initial condition of each building within the district, where the weights are each building's total square feet. Roughly 60 percent of school districts in my sample have an initial building condition described as "excellent" or "good," while the remaining 40 percent are described as "adequate," "fair," "poor," or "in need of replace."

[41] Online Appendix Figure B.12 also examines heterogeneity in the effect of a successful capital bond referendum, but by a school district's initial share of economically disadvantaged students. Similar to the findings in online Appendix Figure B.11, the figure shows no consistent evidence that additional capital expenditures impact student outcomes—even in school districts with an initially high share of economically disadvantaged students.

building in the district. In contrast, the median per-pupil bond campaign approved in Wisconsin is only approximately $4,400 per pupil, and bond funds are frequently used to repair, maintain, and modernize existing structures rather than to build new schools.[42] Thus, the stark contrast in both the magnitude and use of the additional funds across these two contexts provides a plausible explanation for their differing effects.

It is important to note, however, that large-scale construction projects are atypical in the United States, where school facility investments are generally financed locally through the issuance of local general obligation bonds. Echoing the main findings of this paper, studies of locally financed school facility investments have generally found little or no improvements in student outcomes (Cellini, Ferreira, and Rothstein 2010; Hong and Zimmer 2016; Martorell, Stange, and McFarlin 2016). Thus, the existing capital spending literature points to substantial heterogeneity in the effects of different school facility investments (e.g., school construction versus building renovations).

VII. Discussion of Differences in the Marginal Effects of Operational and Capital Expenditures

A. *Additional Robustness Checks*

The main contribution of this paper is to shed light on the marginal effects of two distinct types of expenditures: operational and capital. So far, the results suggest that operational expenditures have large marginal effects on student outcomes, whereas capital expenditures do not.[43] However, there remain several threats to identification that one must address before fully reaching this conclusion. This section describes and addresses these concerns and demonstrates the importance of estimating the returns to each type of spending within the same general context in dealing with these threats.

First, to conclude that additional operational expenditures have larger marginal impacts than additional capital expenditures, one must show that school districts that pass each type of spending are similar.[44] Table 6 shows differences in observables between school districts that passed an operational referendum and those that passed a capital bond referendum. Online Appendix Table B.6 repeats this same

[42] Furthermore, in contrast to contexts where school facilities are initially in extremely poor conditions, only 5 percent of school districts in Wisconsin at the beginning of my sample were described as having "poor" or "in need of replace" infrastructure.

[43] Online Appendix Table B.5 presents formal statistical tests of the null hypothesis that operational and capital expenditure effects in equation (2) are equal. In most specifications, I am able to reject the null for test scores and postsecondary enrollment.

[44] Suppose, for example, that school districts that pass capital bond referenda are those in relatively wealthier areas, while those that pass operational referenda tend to be in poorer areas. This hypothesis would generate the same patterns that I have documented so far. However, these results would simply reflect the fact that operational expenditures are increasing in areas with a relatively larger marginal value of spending rather than actual differences in the marginal returns of each type of expenditure. This example shows why simply comparing operational and capital spending estimates across institutional contexts would likely conflate institutional differences with differences in treatment effects. However, the fact that this paper estimates the effects of each type of spending in the same context allows me to directly test this hypothesis.

Table 6—Differences between School Districts That Passed Each Type of Referendum

Dependent variable ($t-1$)	Passed op. referendum (t)	Passed bond referendum (t)	Difference (1) − (2)
Panel A. Student outcomes			
Dropout rate (percent)	1.36	1.11	0.25
	(1.38)	(1.15)	(0.24)
Percent adv. or prof., tenth grade	46.08	48.98	−2.91
	(12.97)	(12.29)	(2.61)
Average scale score, tenth grade	565.83	569.70	−3.87
	(14.10)	(12.11)	(3.16)
Postsecondary enrollment share	0.41	0.41	−0.01
	(0.10)	(0.10)	(0.01)
Panel B. District characteristics			
Student–licensed staff ratio	12.56	13.06	−0.50
	(1.38)	(1.23)	(0.18)
Teacher experience	13.01	12.57	0.43
	(2.11)	(1.86)	(0.13)
Teacher compensation	74,347	74,389	−42
	(7,673)	(7,517)	(595)
Teacher attrition (percent)	9.46	9.60	−0.14
	(3.31)	(4.94)	(0.25)
Property values PP	545,635	487,115	58,520
	(526,820)	(278,358)	(36,766)
Urban centric locale	2.24	2.21	0.03
	(1.15)	(1.01)	(0.19)
Fall enrollment	2,037	2,642	−605
	(3,882)	(3,739)	(301)
Number of school districts	236	365	394

Notes: The table shows differences in observables between school districts that passed an operational referendum and those that passed a capital bond referendum. Columns 1 and 2 show the means and standard deviations (in parentheses) of district-level outcomes in $t-1$ separately for districts that passed an operational referendum in t and those that passed a capital bond referendum in t. Column 3 reports the point estimates and standard errors clustered at the district level of tests for equality of means. Panel A shows student outcomes, while panel B presents variables measuring district characteristics. 236 (365) unique school districts passed an operational (capital bond) referendum at some point from 1996 to 2014. 394 school districts passed either an operational or a capital bond referendum at some point during this sample period.

exercise, but for districts that proposed—rather than strictly passed—a referendum of each type. Both tables show little evidence that the two types of districts are significantly different from each other in the year prior to the election.

As mentioned in Section IV, the main results of the paper come from the estimation of equation (2) on the sample of 404 school districts that proposed *either* a capital bond or an operational referendum at some point between 1996 and 2014. To further investigate whether cross-sectional differences between school districts that pass (or propose) each type of spending are driving the main results of the paper, I reestimate equation (2), but only on the sample of 286 school districts that proposed both an operational and a capital bond referendum at some point between 1996 and 2014 and on the sample of 207 school districts that passed both types of referenda during the sample period. These specifications rely solely on the staggered timing

Table 7—Comparison of TOT Estimates in the Restricted and Unrestricted Panels

Dependent variable	Postelection effect unrestricted	Postelection effect proposed both	Postelection effect passed both
Panel A. Operational referenda			
Dropout rate	−0.11	−0.15	−0.21
	(0.07)	(0.08)	(0.10)
Percent adv. or prof., tenth grade math	5.89	4.11	4.86
	(1.80)	(1.69)	(2.38)
Average tenth grade math score	4.53	2.93	3.26
	(1.81)	(1.79)	(2.19)
log(postsecondary enrollment)	0.07	0.06	0.07
	(0.05)	(0.05)	(0.06)
Panel B. Capital bond referenda			
Dropout rate	−0.04	−0.01	0.03
	(0.08)	(0.10)	(0.13)
Percent adv. or prof., tenth grade math	−1.03	−0.49	0.33
	(1.80)	(1.96)	(1.98)
Average tenth grade math score	−1.13	−0.19	0.22
	(1.69)	(1.93)	(2.05)
log(postsecondary enrollment)	−0.02	−0.02	−0.04
	(0.05)	(0.04)	(0.05)
Number of school districts	404	286	207

Notes: The table presents results from the estimation of equation (2) using a cubic specification of the vote shares. Panel A summarizes the average of the estimated β_τ^{TOT} values across the first ten postelection years, while panel B summarizes the average of the estimated γ_τ^{TOT} values. Standard errors clustered at the school district level are shown in parentheses. The first column shows estimates obtained when estimating equation (2) on the 404 schools districts that proposed at least one operational or capital bond referendum from 1996 to 2014. The second column shows estimates obtained when restricting the sample to only the 286 school districts that proposed both types of referenda from 1996 to 2014. Finally, the third column shows estimates obtained when restricting the sample to only the 207 school districts that passed both types of referenda during the sample period. The specifications additionally control for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

of operational and capital bond referenda rather than on cross-sectional differences between districts that passed (or proposed) each type of referendum.

Table 7 compares the main estimates documented in Sections V and VI to estimates from the restricted samples. The estimates across all three specifications continue to indicate that additional operational expenditures have large positive impacts on student outcomes, while additional capital expenditures do not. These results suggest that the main findings of the paper are not driven by systematic differences between the groups of school districts that pass (or propose) each type of referendum.

Finally, a remaining concern is whether there is a systematic sequence in which school districts attempt (and pass) each type of referendum.[45] Online Appendix Figure B.13 highlights two important aspects of the joint distribution of the choice

[45] For instance, suppose that school districts in Wisconsin usually pass operational referenda before passing capital bond referenda. This hypothesis could also generate the patterns in spending returns documented in this paper. However, these results would simply reflect the fact that operational expenditures increase first—and along the steeper part of the spending returns curve. A key advantage of this paper is that I am able to directly test this hypothesis because I observe the full joint distribution of operational and capital bond referenda. Thus, I can examine whether the probability of passing a capital bond referendum increases after narrowly passing an operational referendum, and vice versa.

to raise funds for operational and capital bond referenda: First, districts in which an initial measure is narrowly defeated are more likely to propose and pass a measure of the same type in the two years following the election, relative to districts that initially pass the measure narrowly.[46]

Second, there is no evidence that districts pass operational and capital bond referenda in a systematic sequence. Panels B and F show no evidence that narrowly passing an operational referendum leads to an increase in the number of proposed (or passed) capital bond referenda in the years following the election. Panels C and G show that the opposite is also true. Taken together, these results yield little evidence of a systematic ordering of operational and capital bond referenda.

B. *Joint Distribution of Returns across Expenditure Types*

Altogether, there is little evidence that alternative explanations—such as systematic differences between the types of districts that pass each type of referendum or a systematic ordering of referenda—are driving the main results of the paper. Importantly, these findings do not suggest that operational expenditures "matter" and capital expenditures do not. Instead, the results indicate that marginal expenditures in Wisconsin are more effective at impacting student outcomes when targeted to operational functions rather than to improving existing facilities and that districts could be leaving achievement gains on the table by passing capital bonds rather than operational referenda. To further examine this question, I estimate the full joint distribution of returns across operational and capital bond referenda using the following equation:

$$(3) \quad y_{dt} = \sum_{\tau=\underline{\tau}}^{\bar{\tau}} \Big[P^o_{d,t-\tau}\beta^{TOT}_\tau + m^o_{d,t-\tau}\kappa_\tau + f_g\big(v^o_{d,t-\tau}\big) + P^b_{d,t-\tau}\gamma^{TOT}_\tau + m^b_{d,t-\tau}\pi_\tau + f_g\big(v^b_{d,t-\tau}\big) + \big(P^o_{d,t-\tau} \times P^b_{d,t-\tau}\big)\delta^{TOT}_\tau \Big] + \mu_d + \theta_t + \varepsilon_{dt}.$$

This equation is identical to equation (2) but includes an interaction term between $P^o_{d,t-\tau}$ and $P^b_{d,t-\tau}$. Thus, in this specification, $\beta^{TOT}_\tau(\gamma^{TOT}_\tau)$ captures the effect of narrowly passing only an operational (capital bond) referendum in $t-\tau$ on outcomes in year t; δ^{TOT}_τ captures the differential effect of narrowly passing an operational referendum when the district also narrowly passes a capital bond referendum concurrently in $t-\tau$.[47]

Results from the estimation of equation (3) are shown in Table 8. The first two columns of the table show averages of the estimated β^{TOT}_τ and γ^{TOT}_τ terms, respectively, across the first five postelection years. These estimates continue to indicate that narrowly passing an operational referendum alone leads to increases in student test scores of 4.36 WKCE points, or roughly 10 percent of a standard deviation, while

[46] Online Appendix Figures B.14 and B.15 repeat this exercise but for the four and ten years following the election, respectively.

[47] 14 percent of all operational referenda passed in a year where a capital bond referendum was also passed.

Table 8—Joint Distribution of Returns across Expenditure Types

Dependent variable	Postelection effect operational referenda	Postelection effect capital bond referenda	Postelection effect interaction term
Percent adv. or prof., tenth grade math	5.86	0.16	−3.44
	(1.57)	(1.32)	(1.37)
Average tenth grade math score	4.36	0.54	−2.84
	(1.60)	(1.30)	(1.28)
Revenue limits PP	444	14	−217
	(116)	(59)	(74)

Notes: The table presents results from the estimation of equation (3) on three different district-level outcomes using a cubic specification of the vote shares. The first column summarizes the average of the β_τ^{TOT} values across the first five postelection years, while the second column summarizes the average of the γ_τ^{TOT} values. Finally, the third column shows averages of the δ_τ^{TOT} values. Standard errors clustered at the school district level are shown in parentheses. The specifications additionally control for the type of operational measure (recurring or nonrecurring) and the month in which the election was held.

narrowly passing a capital bond referendum alone has no impact on student test scores (row 2).[48]

The third column of the table shows averages of the δ_τ^{TOT} values. These estimates indicate that the marginal impact of a narrow operational referendum win is roughly 2.84 WKCE points smaller when the school district narrowly passes a capital bond referendum concurrently (row 2). As the third row of the table shows, the attenuated test score effect is likely the result of districts asking for a \$217 relatively lower per-pupil revenue limit increase in the operational referendum—presumably so that districts can bundle and pass both the operational and the capital bond referendum. Since successful capital bond referenda have little impacts on test scores, these estimates suggest that districts presumably could have instead asked for an additional \$217 in the operational referendum, not passed a capital bond referendum concurrently, and gotten a 4.36 point (10 percent of a standard deviation) increase in test scores—as opposed to a 1.52 point (3.5 percent of a standard deviation) increase.

These findings do not necessarily imply that districts are allocating resources suboptimally. Investments in school facilities may generate other nonacademic benefits such as improvements in student health and morale, and increases in property values (Cellini, Ferreira, and Rothstein 2010).[49] However, to justify the choice to pass a capital bond referendum concurrently, it must be the case that districts

[48] As described in Section VB, estimates in standard deviations are obtained by dividing the test score improvements in WKCE points by 43.2—the standard deviation of the 2002–2003 mathematics test score distribution for tenth grade.

[49] It is also important to point out an additional caveat when comparing operational and capital expenditure effects, which is that operational and capital expenditures may pay off over different timelines. In other words, while operational expenditures may generate immediate benefits, capital investments made today will retain their value over several years, even after accounting for depreciation. This paper is designed to understand the short-run tradeoff between these two expenditures, but it may be hard to detect even large per-dollar capital investment effects if they depreciate slowly, since the effect in any given year could be small.

value these other nonacademic benefits at least as much as a 6.5 percentage point increase of a standard deviation in test scores.[50]

VIII. Conclusion

This study leverages detailed administrative data along with a credible research design and a novel source of quasi-experimental variation in Wisconsin to estimate the effect of two distinct types of school spending. I identify spending effects using an RD design that compares school districts in which referenda to exceed revenue limits pass or fail by narrow margins. That Wisconsin law requires districts to hold separate referenda for operational purposes and for bond issues targeted to fund major capital projects allows me to separately identify the effects of increases in operational and capital expenditures, which differentiates this study from those in the existing literaturure.

In general, I find that Wisconsin school districts allocate roughly two-thirds of the additional resources from a successful operational referendum to instruction in the form of higher teacher compensation and experience, lower student-teacher ratios, and lower teacher attrition. Improvements in these inputs result in substantial improvements in student outcomes: an 8 percent increase of a standard deviation in test scores, a 9 percent decrease in the district's dropout rate, and a 10 percent increase in postsecondary enrollment. Districts that narrowly pass a capital bond referendum allocate all of the additional resources to capital outlays. In contrast to increases in operational expenditures, I find no evidence that increases in capital investments improve student outcomes. Overall, these findings indicate that increases in discretionary operational funds can significantly improve educational outcomes and may be a more productive use of marginal resources than school facility investments.

When generalizing this study's findings, however, one should keep in mind their external validity. The estimates presented in this study are most generalizable to states with a similar school finance system to Wisconsin. Furthermore, the RD research design identifies only local average treatment effects since it exploits variation stemming from relatively close elections. As a result, it remains unclear whether these findings would generalize to inframarginal elections. Nevertheless, in online Appendix E, I show that Wisconsin's school finance system is quite similar to that of the average US state. Specifically, Wisconsin's average public school expenditures per pupil—as well as the shares of total resources that are devoted to operations and capital—are nearly identical to US averages. Moreover, the fact that estimates of school facility investments in Wisconsin closely resemble those in other states suggests that estimates of operational expenditure effects could also be externally valid.

[50] Of course, this discussion assumes that the amount of political capital required to pass either type of measure depends solely on the expected academic and nonacademic returns of the referendum rather than on residents' preferences for particular spending types. It also assumes that districts have perfect information regarding the returns to each type of referendum (or at least that they have learned over time).

Relying on a novel source of variation and employing a different identification strategy, the results in this paper are consistent with those of other recent quasi-experimental studies in the school spending literature showing that additional school resources can significantly improve student outcomes. While most of these recent studies show that money matters in public education, the optimal allocation of resources across expenditure types remains an open empirical question. This paper advances the literature by showing substantial heterogeneity in the effectiveness of two distinct expenditure types. Continuing to examine which types of school spending are most effective and under which institutional contexts and incentives additional spending is most likely to improve student outcomes represents an important topic for future research.

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