

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

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Abstract

This study examines the causal impact of additional school spending on student outcomes. State-imposed revenue limits cap the total amount of revenue that a school district in Wisconsin can raise. If a district wishes to exceed this cap, it must hold a local referendum. I leverage close elections in a dynamic regression discontinuity framework to identify the impact of additional spending on educational outcomes. Importantly, Wisconsin law requires school districts to hold separate referenda for operational purposes (e.g., instruction and support services) and for bond issues targeted to fund school facility investments. This allows me to estimate the independent effects of additional operational and capital expenditures. I find that narrowly passing an operational referendum leads to a 5% increase in per-pupil spending. Districts allocate most of these additional resources to instruction, yielding increases in teacher experience and compensation, and reductions in class sizes and teacher turnover. Increases in operational funds result in a 25% reduction in the dropout rate, an increase in test scores of approximately 30% of a standard deviation, and a 15% increase in postsecondary enrollment. In contrast, narrowly approving a bond referendum leads to a sharp and immediate increase in capital outlays. These additional funds are primarily used to repair, maintain, or upgrade existing structures and are not associated with improvements in student outcomes.

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JEL Classification: H0, H41, H75, I20, I22, I24, I28, J24

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“Everybody’s going to tell you how much they value education. I’ve got an expression I use: Don’t tell me what you value. Show me your budget and I will tell you what you value.”

— Joe Biden, Former U.S. Vice President

“The notion that spending more money is going to bring about different results is ill-placed and ill-advised.”

— Betsy DeVos, U.S. Secretary of Education

1 Introduction

In an effort to improve the quality of public schools, the U.S. 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 Policy interest in improving public schools is largely driven by the recognition of the increasing importance of human capital accumulation to both individuals and society at large.² Numerous studies have documented a causal private return to education (Oreopoulos and Petronijevic, 2013; Ashenfelter and Krueger, 1994; Angrist and Krueger, 1991). Similarly, locations with a more educated population tend to enjoy higher wages and employment growth, lower crime rates, and better amenities (Shapiro, 2006; Glaeser, 2005; Lochner and Moretti, 2004).

As highlighted in the above quotes, despite the increased investment in public schools whether additional school spending improves academic outcomes remains a topic of controversy. Perhaps due to early observational studies that showed small effects of additional spending on student outcomes, economists have long questioned the effectiveness of resource-based school policies (Hanushek, 2003; Coleman et al., 1966). Many of these early studies, however, may be limited in their ability to make causal claims due to the endogenous relationship between these two variables.³

¹Author’s calculations from National Center for Education Statistics (NCES) data. These expenditures are reported in constant 2017-18 dollars based on the CPI-U adjusted to a school-year basis. Total expenditures include current operational expenditures, capital outlays, and interest on school debt.

²For instance, the “college wage premium” – the earnings gap between those with a high school and a college education – was relatively small in 1980 (30%), but has since continued to increase each year and is now more than double its 1980 level (70%). These figures are based on calculations from the Census of Population and the American Community Survey, and reflect the differences in the hourly average wages of full-time male workers aged 25-60 in 2010 dollars.

³Ineffective allocation of school resources due to teachers’ unions, declining teacher quality, bureaucratization of public schools, a lack of competition in public education, and diminished returns to school spending are among the theories that have been proposed to explain why these early observational studies found no link between school spending and educational outcomes (Hoxby and Leigh, 2004; Betts, 1996; Hoxby, 1996).

This study examines the causal impact of additional school spending on student outcomes by leveraging detailed administrative data 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 1993-94 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 to increase operational expenditures (e.g., teacher compensation and class sizes), it must ask for voter approval to increase property taxes in a local “operational referendum.” A simple majority vote of district residents is required for the initiative to pass.

Since 1993, Wisconsin school districts have held roughly 1,200 operational referenda. While districts that pass a referendum are likely to differ both in terms of observable and unobservable characteristics from districts where the initiative is defeated, these differences can be minimized by focusing on narrow elections. A district that passes a referendum by a small margin (e.g., 50.1% 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 these close elections in a regression discontinuity (RD) framework to identify the causal impact of additional school spending on student outcomes.⁴ Importantly, I am able to examine in detail the ways in which narrow winners allocate the additional resources as well as the mechanisms through which additional school spending impacts student outcomes.

The standard RD design, however, is complicated by the dynamic nature of treatment in this setting. First, a school district may attempt (and pass) multiple referenda throughout the sample period. Second, many “control” districts (those where the initial referendum narrowly fails) are eventually “treated,” which generates imperfect compliance in treatment assignment. In other words, districts that narrowly reject an initial proposal are likely to consider and pass a new measure in subsequent years. These features complicate identification of dynamic treatment effects.

I deal with these econometric challenges in two ways. First, as in previous studies in which imperfect compliance is present, I estimate “intent-to-treat” (ITT) effects of initial treatment assignment. A weakness of ITT effects is that they combine both the direct effects of a successful referendum as well as the indirect effects via its influence on subsequent district decisions to hold (or not hold) additional elections. The ITT effect can be interpreted as the impact of passing an initial referendum and then allowing district residents to consider (and potentially pass) new measures in subsequent years. Thus, ITT estimates do not necessarily reflect the impact of additional

⁴Several studies use close elections as sources of identification in other contexts. For instance, [Whalley \(2013\)](#) examines the causal impact of appointed treasurers on a city’s borrowing costs by leveraging close elections in cities seeking to switch from elected to appointed treasurers. [DiNardo and Lee \(2004\)](#) use narrow unionization elections to estimate the economic impact of unionization on outcomes such as business survival and wages. [Lee \(2001\)](#) examines close elections to the U.S. House of Representatives to assess the magnitude of the incumbency advantage.

operational expenditures solely associated with the passage of a single referendum, as there could be additional expenditure increases approved through a later referendum.

To isolate only the direct effects of successful referenda, I adapt the “one-step” dynamic RD estimator developed by [Cellini et al. \(2010\)](#) and estimate “treatment-on-the-treated” (TOT) effects.⁵ Estimates of TOT effects yield the causal impacts of successful referenda, holding subsequent outcomes constant. Intuitively, the dynamic RD approach compares the outcomes of school districts where a similar initiative at some point in time was narrowly successful to those where the initiative was narrowly defeated, but the sequence of prior and subsequent initiatives, votes, and successful referenda is similar.

I apply these estimators to a rich administrative dataset combining information on two decades of Wisconsin operational 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. Wisconsin presents a particularly interesting context because state law requires school districts to hold separate referenda for operational purposes and for bond issues targeted to fund capital projects (e.g., new buildings or renovations). This allows me to isolate discretionary operational expenditure effects from investments in school facilities, which differentiates this study from others in the existing school spending literature. Previous studies either estimate the joint impact of increases in operational and capital expenditures ([Candelaria and Shores, 2019](#); [Jackson et al., 2018](#); [Lafortune et al., 2018](#); [Kogan et al., 2017](#); [Jackson et al., 2015](#)), or focus exclusively on capital expenditure effects ([Rauscher, 2019](#); [Hong and Zimmer, 2016](#); [Martorell et al., 2016](#); [Cellini et al., 2010](#)).

I first show that winning and losing school districts have significantly different observable characteristics in the year prior to the election. In general, eventual winners have relatively higher levels of spending as well as higher test scores, presumably reflecting residents’ preferences for higher levels of educational spending. However, differences are mitigated when I focus only on narrow elections, which demonstrates the strength of the RD design in this setting. Specifically, winning and losing districts in elections that were decided by less than one percentage point of the vote share have no significant differences in the year prior to the election across any of the outcomes that I examine throughout the study.

Estimates of both ITT and TOT effects indicate that referendum approval in a narrow election leads to a sharp and immediate increase in operational expenditures of roughly \$500 (5%) per pupil. This effect persists for at least seven years after the election. Using detailed administrative

⁵[Cellini et al. \(2010\)](#) examine the effects of a successful bond referendum (targeted to fund school facility investments) on local housing prices in California. Other studies that have employed the one-step estimator developed by [Cellini et al. \(2010\)](#) include [Rauscher \(2019\)](#), [Hong and Zimmer \(2016\)](#), and [Martorell et al. \(2016\)](#) who examine the impacts of bond referenda on student outcomes, and [Pérez Pérez and Suher \(2019\)](#) who study the impact of hiring tax credits in distressed labor markets.

data, I then examine the ways in which treated school districts allocate these additional resources. Doing so provides a thorough understanding of the first-stage relationship between the referendum and spending, and can shed light on the mechanisms through which a successful election may influence student outcomes.

Treated districts allocate most of the additional resources, roughly \$400 per pupil, to instructional expenditures in the form of additional teachers and teacher aides, increases in average teacher compensation and experience, and reductions in teacher attrition. 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, transportation, or the operation and maintenance of schools. I show that all of the additional spending from operational referenda sticks in operational expenditure accounts and is not associated with increases in capital outlays.

I show that the chosen resource allocation results in substantial improvements in student outcomes. Specifically, I find that narrowly passing a referendum leads to an increase in average district test scores of approximately 30% of a (district-level) standard deviation on the state's standardized exam, a 25% reduction in the district's dropout rate, and a 15% increase in the number of high school completers in the district who subsequently enroll in postsecondary education.⁶ To understand the economic significance of the test score effects, one can compare their magnitude with that from the effect of a reduction in class size of eight students, which has been shown to increase test scores by up to 60% of a standard deviation (Angrist and Lavy, 1999; Finn and Achilles, 1999). I conduct a number of robustness checks to ensure that the results are not sensitive to the choice of estimator and specification, endogenous sorting of school districts just above the 50% threshold, or changes in the income and racial composition of local residents following a successful referendum.

As mentioned above, Wisconsin's school finance system requires school districts to hold separate referenda for operational purposes and for bond issues targeted to fund school facility investments. This allows me to additionally estimate effects from increases in capital expenditures within a similar institutional context. Since the enactment of revenue limits, Wisconsin school districts have held roughly 1,300 bond referenda. I isolate exogenous variation in school facility investments by comparing school districts where bond referenda pass and fail by narrow margins in a dynamic RD framework.

I find that narrowly approving a bond referendum leads to a sharp and immediate increase of roughly \$4,000 per pupil in capital outlays. As with operational referenda, I show that all of the additional resources induced by a successful bond referendum stick in the capital outlay account

⁶These findings stand in contrast to early observational studies that find little effect of school resources on student outcomes. I reconcile my results with those from this early literature by showing that more traditional OLS regressions of student outcomes on school spending show zero or no effects of additional resources in my data. These results highlight the need to rely on plausibly exogenous variation if one wishes to obtain causal estimates of school spending.

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, and teacher attrition. Furthermore, I find little evidence that school facility investments impact student outcomes. TOT estimates of the impact of 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 is consistent with studies showing that teacher quality is one of the most important school-related inputs in the education production function (Chetty et al., 2014; Rivkin et al., 2005; Rockoff, 2004), and with those showing that smaller class sizes can improve student outcomes (Krueger and Whitmore, 2001; Angrist and Lavy, 1999; Finn and Achilles, 1999). It is also consistent with studies in Texas (Martorell et al., 2016) and California (Cellini et al., 2010) which show that school facility investments do not generate appreciable improvements in student achievement.

This study contributes to the literature examining the impact of school spending on student outcomes. After numerous early studies using observational variation failed to find evidence of an impact of school spending on academic outcomes (Hanushek, 2003; Coleman et al., 1966), an emerging quasi-experimental literature has re-examined this question. This literature primarily relies on variation from court-ordered school finance reforms (SFRs) and generally finds more positive impacts of additional resources on short- and medium-term outcomes such as test scores and educational attainment (Candelaria and Shores, 2019; Lafortune et al., 2018; Hyman, 2017), and longer-term outcomes such as wages, employment, and income mobility (Biasi, 2019; Jackson et al., 2015).⁷

In contrast to studies exploiting SFR-induced variation, this paper examines whether additional school spending improves student outcomes at more modern levels of public school spending. Recent levels of spending are significantly higher than they were during the period when the first SFRs took place (1970s). Due to potentially diminishing marginal returns of school spending, it is unclear *ex ante* whether additional spending continues to matter. My paper provides evidence that additional resources to public schools continue to have significant impacts on educational outcomes, even after large increases in K-12 spending throughout the last few decades.

⁷See Jackson (2018) for a detailed literature review on this subject. Other studies in the recent school spending literature that do not rely on SFR-induced variation examine the impacts of additional federal Title I funds and find mixed results. While two studies focused on New York City find no discernible impacts on student outcomes (Matsudaira et al., 2012; Van der Klaauw, 2008), two multi-state studies show increases in educational attainment (Johnson, 2015; Cascio et al., 2013).

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. While most recent studies in the school spending literature find that money matters in public education, the optimal allocation of resources across expenditure types is an open empirical question. Most studies in the existing literature rely on research designs that cannot separately identify spending effects across expenditure categories. Exploiting a novel source of plausibly exogenous variation with a credible research design and detailed administrative data, this study advances the literature by showing that expenditures targeted to operational functions may be more efficient than those targeted to school facilities.

2 Background

2.1 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 (non-property tax) revenues such as donations and student fees. Figure 1 shows that Wisconsin school districts derive most of their revenue from a combination of state aid and local property taxes. For instance, in the 2014-15 academic year, Wisconsin school districts received roughly 90% 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 (relative to the level of state aid) has been a source of debate in Wisconsin for many decades.

Prior to the 1993-94 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.⁸ These caps, known as revenue limits, control roughly 90% of the average Wisconsin school district's resources, and are thus one of the most important aspects of the state's school finance system.⁹

The revenue limits that each school district faces in a given year largely reflect that district's per-pupil spending in 1992-93, the year before the enactment of the limits. Revenue limits were

⁸Other forms of aid such as federal grants, state categorical aid, and other non-property tax local revenues are exempt from revenue limits.

⁹Revenue limits were imposed through the passage of Wisconsin's Act 16 in 1993. The Act imposed revenue limits on school districts for the 1993-94 through 1997-98 time period. However, revenue limits became permanent through the passage of Act 27 in 1995. See the Wisconsin Legislative Bureau's *Local Government Expenditure and Revenue Limits, Informational Paper 12* available at <https://dpi.wi.gov/> for a detailed explanation of Wisconsin's revenue limit law.

initially set based on each district's actual amount of spending per pupil in 1992-93 and are adjusted each year primarily by the actions of the state legislature and long-term changes in the district's student enrollment. The annual growth in revenue limits for each school district is a deterministic function of three factors: the district's prior year controlled revenue (general state aid and property tax revenue), the state legislature's adjustment, and a three-year average of full-time equivalent (FTE) students. 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. Figure 2 shows the allowable annual adjustments to revenue limits set by the state legislature since 1993-94. Prior to 2010, the allowable per-pupil adjustments were intended to serve as inflationary adjustments to the limits. Annual adjustments ranged from \$190 in 1993-94 to \$275 in 2008-09.¹⁰ However, following the Great Recession and the looming state budget deficit that followed, the legislature reduced the allowable annual revenue limit adjustment from \$200 per pupil in 2010-11 to -\$529 in 2011-12. As a result of an improving economy, modest increases to revenue limits were approved by the legislature in 2012-13, but were reduced again to \$0 in 2015-16.

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% in 1993-94, the legislature committed in 1993 to increase state aid and fund two thirds of total K-12 education revenues by the 1996-97 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. Figure 3 shows that revenue limits were largely successful in reducing the school portion of the property tax. The figure plots the average mill rate in Wisconsin before and after 1993-94, the year revenue limits were enacted.¹¹ It shows that the mill rate was accelerating in the years prior to the enactment of revenue limits, but has since decreased dramatically.

¹⁰During the first two years of the revenue limit law, school districts were allowed to choose between the state-imposed dollar amount for the adjustment or the rate of inflation. Following the enactment of Wisconsin's Act 27 in 1995, the state stopped offering this choice and instead decided that every school district would adjust their revenue limits based on a flat, per-pupil dollar amount.

¹¹The mill rate is defined as the total property tax dollars levied by public school districts per \$1,000 of equalized property value.

2.2 Referenda to Exceed Revenue Limits

The only means of bypassing revenue limits is through the passage of a local referendum.¹² Wisconsin's revenue limit law allows school districts to exceed the caps through voter approval in an "operational referendum." In this type of referendum, a school district asks its voters for permission to exceed revenue limits for operational expenses (e.g., instruction and support services).

In addition to asking for voter approval to exceed revenue limits for operational purposes, a school district can also attempt a referendum if it wishes to borrow for major capital projects (e.g., new buildings or renovations) by issuing bonds. 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. If a bond referendum is approved by voters, then the annual debt service payments are exempt from the state-imposed revenue limits. In the main body of the paper, I focus only on operational referenda. However, I separately examine the impacts of successful bond referenda on student outcomes in Section 7.

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. 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. In contrast, a recurring referendum authorizes a permanent addition to the district's revenue limit. In either referendum, district residents who vote in favor of the measure are agreeing to a predetermined increase in their property taxes.

The school district of Germantown was the first to attempt a referendum during the fall of 1994. The school district asked its voters for approval to exceed revenue limits in a recurring referendum. The referendum was easily defeated with only 37% of voters casting a ballot in favor of the measure. Since then, roughly 75% of Wisconsin's 421 school districts have held at least one referendum. There have been 1,212 individual questions on the ballot to override state-imposed revenue limits for operational purposes since Germantown's first attempt in 1994.

There are few restrictions on the dates 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.¹³ Figure 4 shows the distribution of referenda by election month. The

¹²If a school district exceeds its maximum allowable revenue in the absence of referendum approval, the Wisconsin Department of Public Instruction reduces the district's state aid by the amount of the excess revenue. If the aid reduction is insufficient to cover the excess revenue, the school district could be ordered by the State Superintendent to either decrease the property tax levy or to refund the amount with interest.

¹³As of January 1, 2018, due to the passage of Act 59, a school district can only ask two referendum questions in one calendar year. Furthermore, the referendum must be held on regularly scheduled spring and fall election days. Districts

figure shows that most elections, roughly 70%, were held in April and November, the months during which spring and fall general elections are held. Another 18% of referenda were placed in February, August, and September, months during which spring and fall primary elections take place.¹⁴ The remaining referenda were placed on the ballot as special elections (in months without other elections).

Table 1 shows summary statistics for operational referenda held by Wisconsin school districts since 1993-94, the first academic year under the revenue limit system. Since the enactment of revenue limits, there have been 1,213 individual referenda on the ballot. Panel (a) provides summary statistics for all referenda (pooling recurring and nonrecurring measures). Panels (b) and (c) report summary statistics separately for recurring and nonrecurring measures respectively. The table shows that the majority of referenda during this time have been for nonrecurring purposes (roughly 60%). In total, voters have approved 54% of all proposed initiatives. Elections appear to be relatively close: on average, the vote share in favor of approving a given initiative has been slightly above 50%. Voters have been much more likely to approve nonrecurring measures than recurring ones (63% versus 41%). The average nonrecurring measure asks voters for permission to exceed revenue limits by four years. Finally, the median number of questions asked by an individual school district (conditional on proposing at least one referendum) during this time period was three.

Figure 5 plots the number and pass rates of referenda from 1993-2018. The figure shows that both of these variables are highly cyclical. More than 80 questions were posed during the 2000-01 academic year, a period with a strong economy and high consumer confidence. However, in the years after the burst of the dot-com bubble and the recession that followed, the number of referenda declined by roughly 50%. The Great Recession brought a similar decline to the number of initiatives proposed by Wisconsin school districts. While the share of approved referenda also tends to be cyclical, it has been steadily increasing since the end of the Great Recession amid a recovering economy and decreases in state aid to school districts during the economic downturn.

Figure 6 shows an example of a referendum mailer. Mailers are sent to district residents with the purpose of reminding them to vote and providing them with more information about the upcoming referendum. While the figure provides the specific example of the Kettle Moraine School District, which attempted a nonrecurring referendum on April 2, 2019, the typical mailer closely resembles Kettle Moraine's.¹⁵ A mailer will typically list the actual question voters will see at the ballot. For instance, in this example the school district of Kettle Moraine asks voters for permission to exceed state-imposed revenue limits by \$5,975,000 per year for five years.

who have faced a natural disaster are exempt from this requirement and are allowed to have a special referendum within six months of the date of the disaster.

¹⁴Beginning in 2012, the fall primary election was moved to August.

¹⁵This referendum was narrowly defeated by a margin of 48% - 52%.

While the actual question usually offers little detail as to how the increased revenue will be used, other parts of the mailer address this question. As an example, Kettle Moraine plans to use the additional revenue to retain high-quality staff. The mailer also addresses why there is a need for additional revenue. Most districts cite declining enrollment and rising costs, as well as declines in state appropriations for K-12 education, as the main reasons why the district must seek voter support. Finally, mailers provide an estimate of the property tax impact that the referendum will have if approved. For instance, if Kettle Moraine's measure is approved, taxes are projected to increase 16 cents per \$1,000 of property value over the current tax levy rate. Although a lot of cross-district variation exists in the specific purpose cited for the referendum, textual analysis tools applied to individual-referendum data from the Wisconsin Department of Public Instruction (WDPI) reveal that school districts often ask voters for additional resources to maintain existing educational programs, maintain low class sizes, retain and recruit high-quality staff, and invest in classroom technology.

3 Data

3.1 Referendum-Level Dataset

To estimate the effect of narrowly approving a referendum on student outcomes, I combine four primary datasets. First, I obtain a referendum-level dataset from the WDPI from 1996-97 through 2014-15. This dataset reports, for each referendum attempt, the school district's unique identifier, the date of the referendum, the type (recurring or nonrecurring), 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. This information allows me to construct additional variables such as the vote share in favor of the measure, and whether or not the referendum was approved by voters.

3.2 Administrative Dataset

Information on each school district's referendum history is matched to detailed district-level K-12 revenue and expenditure data from the National Center for Education Statistics (NCES). This dataset reports total revenues by source (e.g., local, state, or federal) and function-specific expenditures (e.g., operational or capital) for every school district in Wisconsin. For each function-specific expenditure, the dataset further details specific expenditures in each account. For instance, the total current operation expenditure is the sum of expenditures in the instruction account and the various support services accounts. The dataset specifies the total expenditure in each of these accounts.

I merge district-level revenue and expenditure data from the NCES to a district-level dataset from the WDPI containing each district's revenue limits per pupil and student-staff ratio. Furthermore, I use an individual-level dataset published annually by the WDPI containing detailed information on the universe of Wisconsin public school teachers. This dataset includes covariates such as each teacher's first and last name, district and school of employment, birth year, total salary and fringe benefits, and years of teaching experience. This information allows me to construct additional variables such as each district's average local teacher experience, teacher attrition, and teacher salaries and benefits.¹⁶

3.3 Student Outcomes

I match each district's referendum history and fiscal variables to 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 portion of the state's standardized test, the Wisconsin Knowledge and Concepts Examination (WKCE).¹⁷ 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.¹⁸

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 et al. (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

¹⁶The student-staff ratio is equal to the district's student enrollment count (in all grade levels) divided by the district's number of FTE staff. The WDPI also reports student-staff ratios by specific staff categories. Each staff assignment is grouped into one of three categories: administrative positions (e.g., district administrator, principal, and director of special education), licensed positions (e.g., teachers, guidance counselors, and school psychologists), and aides/support/other. The value of fringe benefits incorporates the district's contribution to the pension system, as well as other benefits such as health, life, and disability insurance. All revenue, expenditure, and compensation figures are converted to 2010 dollars using the Midwest Region's CPI-U. 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 et al. (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 .

¹⁷The 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 school and district improvement purposes. However, I also collect the district's average scale score in order to calculate effect sizes in terms of district standard deviations - a more common way to interpret effect sizes in the economics of education literature.

¹⁸The WKCE is a criterion-referenced assessment. It is designed to compare a student's strengths and weaknesses to standards set by the WDPI (the Wisconsin Model Academic Standards). With the exception of students with severe cognitive disabilities, every public school student is required to participate in the WKCE. See the *WSAS Administrator's Interpretive Guide* available at <https://dpi.wi.gov/> for a more thorough description of the exam.

measures of learning and may not always reflect changes in human capital accumulation.¹⁹ Therefore, I collect two additional district-level measures of student outcomes: the district’s dropout rate and the share of each district’s high school completers who subsequently enroll in postsecondary education.

District-level dropout rates are reported as annual events for grades 7 through 12. 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 amount of students in grades 7-12 who were expected to complete the school term in school district d in year t . A “dropout” in year t is defined as any student who exits during school year t without completing the school term and does not re-enroll by the 3rd Friday of September of $t + 1$. The total amount of students expected to complete the school term is the sum of the number of students who completed the school term plus the number of dropouts. This figure could be higher or lower than the fall enrollment figure since it adjusts for transfers in and out of the school district after the date of the fall enrollment count.²⁰

The share of each district’s high school completers who subsequently enroll in postsecondary education is also reported annually by the WDPI. The WDPI merges individual high school completer data in Wisconsin to postsecondary enrollment data from the National Student Clearinghouse (NSC).²¹ The WDPI then reports each school district’s share of high school completers in year t who enroll in a postsecondary education program in the state during the fall of year $t + 1$.²²

3.4 Final Sample

The final sample contains a balanced panel from 1996-97 to 2014-15 of the 314 Wisconsin school districts that attempted at least one measure during the sample period. Table 2 presents summary

¹⁹For 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.

²⁰Dropouts are counted at most once in a given school year. A dropout in a given school year may, in a subsequent school year, re-enroll in school, dropout again, or complete high school.

²¹The NSC collects enrollment data from over 3,000 postsecondary institutions enrolling over 95% of all postsecondary students in the United States. These institutions include public and private universities, two-year technical colleges, and training programs. NSC data include all enrolled students in these institutions. The dataset includes variables such as each high school completer’s name as well as the name and the type of postsecondary institution that the student is enrolled in.

²²The numerator in the share is the number of high school completers in year t who enroll in a postsecondary education program in the state during the fall of year $t + 1$, while the denominator is simply the number of high school completers in year t . Since the number of high school completers may be endogenous to the approval of a referendum, as a robustness check I replace the number of high school completers with the total fall senior enrollment in year t , as well as with the number of students expected to complete the term in year t . Estimates using alternative definitions are both qualitatively and quantitatively similar to the results presented in the main body of the paper. These results are available upon request.

statistics. Panel (a) shows summary statistics for district-level fiscal outcomes, while Panel (b) and Panel (c) present summary statistics of variables measuring student outcomes and district characteristics respectively. Column (1) shows the means and standard deviations (in parentheses) of outcomes computed over all district-year observations in the panel. Columns (2) and (3) show summary statistics separately for school districts that proposed at least one referendum during the sample period and those that did not. Finally, Column (4) reports the point estimates and robust standard errors (in parentheses) of tests for equality of means.

The table shows that districts that proposed at least one referendum have similar levels of per-pupil revenue limits and total current expenditures to districts that did not propose a measure during the sample period. However, districts that proposed a measure tend to have better student outcomes: a lower dropout rate, a higher share of students who score in the advanced or proficient levels on the WKCE, and a larger share of high school completers who subsequently enroll in postsecondary education. Furthermore, these districts have lower student-staff ratios, higher levels of teacher experience, and lower rates of teacher attrition.

4 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 three diagnostics needed for the validity of the RD design in the Wisconsin setting.

4.1 Local Balance of the Treatment and Control Groups

I first demonstrate the need to focus on narrow elections if one wishes to obtain causal estimates of school spending. Specifically, I show that while winning and losing school districts have significantly different observable characteristics in the year prior to the election, differences are mitigated when the sample is restricted to narrow elections.

The first two columns of Table 3 present 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 controls only for school year fixed effects. It reveals large pre-election 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, presumably reflecting residents' preferences for higher levels of educational spending.

The second column restricts the sample to narrow elections. It presents pre-election differences

between winning and losing districts in elections that were decided by less than one percentage point of the vote share. 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.

The last two columns in Table 3 repeat the first two specifications, but they take as the dependent variable the change in the specific outcome between $t - 2$ and $t - 1$. The estimates indicate that winning and losing districts followed similar trajectories in the years prior to the election in the main outcomes that I examine throughout the study.

4.1.1 Distribution of the Vote Share

A key assumption underlying the RD design is that school districts cannot precisely control voting results around the 50% vote share (Lee and Lemieux, 2010). Consider as an example a school board that has a strong preference for additional revenue because it believes the outcomes of its students will improve if it can achieve higher levels of per-pupil spending. If school boards with these preferences are able to manipulate the vote share to be just above 50% so that they can override state-imposed revenue limits, then treatment assignment would no longer approximate a randomized experiment.

If treatment is indeed as good as random, then it should be equally likely that voters either just pass or just reject the referendum. On the other hand, if districts can influence the election to pass the referendum, more school districts will pass the referendum than reject it near the 50% threshold. As a result, one can infer whether there is manipulation of the vote share by examining the continuity of the vote share distribution around the threshold. Panel (a) of Figure 7 shows a histogram of the vote shares for operational referenda. The figure shows no evidence of a discontinuity around the 50% vote share.

McCrary (2008) proposes a two-step test for the presence of a discontinuity in the density function of the forcing variable at the 50% threshold. In the first step, the forcing variable is partitioned into equally spaced bins and frequency counts are computed within those bins. In the second step, the frequency counts are taken as the dependent variable in a local linear regression. Local linear smoothing is conducted separately on each side of the 50% cutoff to allow for a potential discontinuity in the density function. The log difference of the coefficients on the intercepts of the two separate local regressions provides an estimate of the discontinuity in the density at the threshold. Panel (b) of Figure 7 shows the densities estimated in the first step (open circles) as well as the second-step smoothing (solid lines) and corresponding 95% confidence intervals (dashed lines). There is no statistically significant discontinuity in the density at the 50% cutoff.

Altogether, the evidence presented in this section provides little cause for concern regarding

the “as good as random” assumption of treatment assignment in close elections. These results demonstrate the strength of the RD design in the Wisconsin setting.

5 Empirical Strategy

Simply regressing a district’s change in average student outcomes on the district’s change in per-pupil spending is unlikely to yield causal estimates. Federal- and state-level changes to funding formulas since the 1960s brought about by school finance reforms are likely to weaken the observed relationship between changes in district resources and student outcomes (Jackson et al., 2015). For instance, the Elementary and Secondary Education Act of 1965 provides additional funding to school districts that enroll increasing shares of low-income pupils. In Wisconsin, specifically, there are a number of programs that support the costs of enrolling additional economically disadvantaged students, as well as students eligible for special education.

Ideally, to address the endogeneity problem, one would randomly assign additional spending to a group of school districts and measure subsequent differences in student outcomes relative to school districts that were not assigned treatment. In the absence of such randomized controlled experiments, the literature has largely relied on quasi-experimental shocks to school spending induced by school finance reforms (Biasi, 2019; Candelaria and Shores, 2019; Lafortune et al., 2018; Hyman, 2017; Jackson et al., 2015). In contrast to these studies, this paper employs a within-state analysis. Specifically, it leverages quasi-experimental variation stemming from the design of Wisconsin’s school finance system. Since the 1993-94 academic year, Wisconsin law caps the total amount of revenue that a school district can raise for operating expenses. The only way a school district can exceed this limit is through the passage of a local referendum. I leverage close elections in an RD framework to estimate the causal impact of additional school spending on student outcomes.

The standard RD design is complicated by the dynamic nature of referenda in Wisconsin. First, a school district may attempt (and pass) multiple referenda throughout the sample period. Second, a district in which a first initiative is narrowly defeated may consider and pass a new proposal in a subsequent year, which generates imperfect compliance in treatment assignment. Previous studies dealing with imperfect compliance usually focus on ITT effects of initial treatment assignment. A weakness of ITT effects is that they combine both the direct effects of a successful referendum as well as the indirect effects via its influence on subsequent district decisions to hold (or not to hold) additional elections. Thus, ITT estimates do not necessarily reflect the impact of additional operational expenditures solely associated with the passage of a single referendum.

To isolate only the direct effects of successful referenda, I adapt the “one-step” dynamic RD estimator developed by Cellini et al. (2010) and estimate TOT effects. Estimates of TOT effects

yield the causal impacts of successful referenda, holding subsequent outcomes constant. In the main body of the paper, I focus only on estimates of TOT effects. Estimates of ITT effects are discussed in detail in Appendix A and yield remarkably similar results.

5.1 Dynamic RD Estimator

Suppose that district d holds a referendum in year t to override state-imposed revenue limits and that the referendum receives vote share v_{dt} . Let $P_{dt} = 1(v_{dt} > 50)$ be an indicator for passage of the referendum: equal to one if district d passes a referendum in school year t and zero otherwise (either if there was no referendum held in year t or if a proposed referendum was rejected). Assuming that the partial effect of referendum approval in one year on outcomes in some subsequent year (holding all intermediate referendum approvals constant) depends only on the elapsed time between the passage of the referendum and the year the outcome is observed, 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 referenda:

$$y_{dt} = \sum_{\tau=0}^{\bar{\tau}} P_{d,t-\tau} \beta_{\tau}^{TOT} + \varepsilon_{dt} \quad (1)$$

The coefficient of interest β_{τ}^{TOT} represents the TOT effect of referendum approval. It provides the causal effect, on y_{dt} , of exogenously passing a referendum in district d in year $t - \tau$ and “prohibiting” the district from passing any subsequent referenda (since all intermediate referendum approvals are held constant). Therefore, a consistent estimate of β_{τ}^{TOT} will isolate the impact of referendum passage in $t - \tau$ (with no subsequent referendum-approved changes to the district’s revenue limits) on a district’s outcome in t .

A simple regression like Equation 1, however, would yield biased estimates of the β_{τ}^{TOT} ’s as factors in ε_{dt} are likely to be correlated both with concurrent and past successful referenda. As shown in the previous section, districts where a referendum passes differ from school districts where the referendum is defeated along both observable and unobservable characteristics. Relative to residents in districts in which a referendum fails, residents in winning districts prefer higher levels of educational spending that correlate with higher average levels of income and education and in turn better student outcomes. However, since there is no evidence of manipulation of the vote share near the 50% threshold (see Figure 7), the correlation between P_{dt} and ε_{dt} can be kept close to zero by focusing only on close elections.

Therefore, to estimate the causal impact of additional school spending, one can use an RD design that compares outcomes in school districts that narrowly pass a referendum (the “treatment group”) to those where the initiative is narrowly defeated (the “control group”). I follow [Hong](#)

and Zimmer (2016), Martorell et al. (2016), and Cellini et al. (2010), and implement the dynamic RD strategy using a parametric framework that retains all observations in the sample, but absorbs variation from non-close elections with flexible controls for the vote share. As Cellini et al. (2010) show, under the assumption that $E[\varepsilon_{dt}|v_{dt}]$ is continuous, it can be approximated by a g th order polynomial with coefficients γ_τ , $f_g(v_{d,t-\tau}, \gamma_\tau)$, which becomes arbitrarily accurate as $g \rightarrow \infty$. As a result, a regression of district outcomes on referendum approvals, controlling for concurrent polynomials in v_{dt} , will provide consistent estimates of the β_τ^{TOT} 's.

If the standard RD assumption that passing a referendum is “as good as random” when focusing only on narrow elections holds (an assumption tested in Section 4), the endogeneity described above can be mitigated by augmenting Equation 1 with $f_g(v_{d,t-\tau}, \gamma_\tau)$ and with an indicator for the presence of a referendum on the ballot in year $t - \tau$, $m_{d,t-\tau}$.²³ Following Hong and Zimmer (2016), Martorell et al. (2016), and Cellini et al. (2010), the coefficients on $P_{d,t-\tau}$, $m_{d,t-\tau}$, and $f_g(v_{d,t-\tau})$ are allowed to vary freely with τ (for $\tau > 0$) but are constrained to be zero for $\tau < 0$.²⁴ After adding school year (θ_t) and district-level (μ_d) fixed effects, the estimating equation becomes:

$$y_{dt} = \sum_{\tau=0}^{\bar{\tau}} (P_{d,t-\tau} \beta_\tau^{TOT} + m_{d,t-\tau} \kappa_\tau + f_g(v_{d,t-\tau}, \gamma_\tau)) + \mu_d + \theta_t + \varepsilon_{dt} \quad (2)$$

This equation is estimated on a school district-year panel from 1996-97 to 2014-15 where each district-year observation is used exactly once for the 314 school districts that attempted at least one measure during the sample period.²⁵ Standard errors are clustered at the district level. For the main results of the paper, I specify $f_g(v_{d,t-\tau})$ as a third-order polynomial. However, I show that the results are robust to linear and quadratic specifications of the vote share instead. Intuitively, Equation 2 identifies the β_τ^{TOT} coefficients by contrasting between school districts where a referendum in year $t - \tau$ narrowly passed and those where the election was narrowly rejected, but the sequence of previous and subsequent proposals, vote shares, and successful referenda is similar.

²³ $v_{d,t-\tau} = 0$ if district d did not hold an election in year $t - \tau$.

²⁴The results are robust to relaxing this assumption, excluding the year prior to the election, and estimating leads. The $\tau = 0$ coefficient is constrained to zero as it is not plausible that referendum approval can have an effect on the district's budget that year. Revenue limit increases resulting from approved referenda occur no sooner than the academic year following the election.

²⁵In cases where a school district holds multiple elections 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 et al. (2010)) or the first initiative in each year (as in Hong and Zimmer (2016)).

6 Results

6.1 First Stage: Referendum Approval and Operational Spending

6.1.1 Descriptive Analysis

This section begins by showing that referendum approval leads to an increase in revenue limits and total spending in the years following the election. It also investigates the ways in which treated school districts allocate these additional resources. I first present descriptive graphical analyses of average district revenue limits and total spending by the margin of victory (or defeat) in the year preceding the election and three years after it.

Figure 8 shows average school district outcomes (conditional on school year fixed effects) in two-percentage point bins 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. Bin -1, which corresponds to those measures that failed by less than two percentage points (with a vote share in the (48% - 50%) interval), is excluded from the regression used to control for school year effects so that all estimates can be interpreted as differences relative to this bin.²⁶ The dashed line traces out district outcomes in the year preceding the election, while the solid line depicts outcomes three years after the election.

A number of important patterns emerge from the figure. Panel (a) shows that, in the year prior to the election, school districts that narrowly passed a referendum had nearly identical revenue limits to school districts in which the initiative was narrowly defeated. However, three years after the election, school districts that narrowly approved the referendum had substantially higher revenue limits per pupil (roughly \$500 more). Panel (b) shows that the increase in revenue limits translated into large increases in total current expenditures per pupil. Three years after the election, school districts that barely passed a measure spent roughly \$600 more per pupil than school districts in which the initiative was narrowly defeated.²⁷

Panel (b) also demonstrates the importance of focusing only on narrow elections when comparing winning and losing school districts. In general, school districts that were already spending higher amounts per pupil in the year prior to the election are more likely to approve the referendum, presumably reflecting residents' preferences for higher levels of education spending. For instance, in the year prior to the election, school districts that approved the measure with a vote share between 52% to 54% spent roughly \$500 more per pupil than school districts in which the initiative

²⁶This analysis is performed at the proposal level and for all proposals in the panel. Therefore, if a school district had multiple elections throughout the sample period, the school district is used more than once.

²⁷Total expenditures may increase more than revenue limits as a result of Wisconsin's formula for distributing state aid. The level of equalization aid that each school district receives is a function of the district's property values, enrollment, and expenditures. An increase in expenditures is rewarded by additional state aid as long as the school district does not have property values and spending levels that are already in the upper tail of the distribution.

narrowly failed. However, these differences are mitigated when focusing only on school districts with close elections. Lastly, Panel (b) shows that in school districts where the referendum was approved total expenditures per pupil increase significantly (relative to pre-election levels) three years after the election. There is no change between pre- and post-election total expenditures in districts where the initiative was defeated.

Panel (c) shows that essentially all of the additional resources are spent in the instructional account (roughly \$500). 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 increase the salaries of these workers). The remainder of the additional resources (\$100) are spent in the account for support services (e.g., expenditures to hire additional guidance counselors, social workers, and school psychologists).

6.1.2 TOT Estimates

Results from the estimation of Equation 2 are shown in Figure 9. The figure presents estimates of the dynamic treatment effects of 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} 's along with 90% confidence intervals for up to ten years after the election.²⁸ The estimates shown in the figure tell a remarkably similar story to the one told by the simple descriptive analysis shown in Figure 8. Panel (a) shows that narrowly approving a referendum increases revenue limits per pupil by roughly \$500 in the year following the election. This effect is relatively constant and persists only for the first eight 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 spending. Narrowly approving a referendum leads to an increase in total current expenditures per pupil of roughly \$500. This effect corresponds to a 5% increase relative to the average current expenditure per pupil in my sample. As in Figure 8, Panels (c) and (d) show that most of the additional resources are spent in the instructional and support services accounts.

Figure 10 examines changes in detailed expenditures within the account for support services. The estimates indicate that the increase in the support services account shown in Figure 9 is entirely driven by pupil expenditures. These expenditures are designed to improve the well-being of students. Examples of such expenditures include attendance and social work services (e.g., activities designed to improve student attendance at school and help with student problems at home), guidance services (e.g., counseling with students and parents and assisting students with educational and career plans), and health services (e.g., providing students with appropriate medical, dental, and nursing services). There is no evidence that expenditures in the school administration, general

²⁸The results are robust to alternative lag specifications.

administration, and student transportation accounts increase following a successful referendum.

As described in Section 2.2, in addition to asking for voter approval to exceed revenue limits, a school district can also attempt a referendum if it wishes to borrow for major capital projects (e.g., new buildings or renovations) by issuing bonds. If school districts place both types of questions on the ballot concurrently, one may be worried about conflating the effects of approving a referendum to exceed revenue limits with those of approving a bond referendum.

The estimates shown in Figure 11 provide no evidence that narrowly approving a referendum to exceed revenue limits leads to changes in capital outlays. Similarly, there is no evidence of changes in district-level 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 bond questions to separately identify changes in spending induced by the former type of referendum.

6.2 Second Stage: Operational Expenditures and Student Outcomes

Figure 12 shows estimates of the TOT effects of referendum approval on three district-level measures of student outcomes: the district's dropout rate, the share of students who score in the advanced or proficient levels on the math portion of the WKCE, and the number of high school completers who subsequently enroll in postsecondary education.

Panel (a) shows that barely passing a referendum leads to a significant decline in the district's dropout rate. This effect persists for the first eight years following the election, and is strongest five years after the approval of the referendum. The estimates indicate that school districts that narrowly approve a referendum experience a 0.25 percentage point decline in the dropout rate relative to districts where the initiative narrowly fails. This effect corresponds to roughly a 25% decline in the dropout rate relative to the average rate for the group of school districts that proposed at least one referendum during the sample period (see Table 2).

Panel (b) shows that referendum approval in a narrow election leads to a sharp and immediate increase in the share of students in the school district who score in the advanced or proficient levels on the math portion of the 10th grade WKCE.²⁹ In the school year following the election, the share of students who perform at the advanced or proficient levels is six percentage points higher in school districts that barely passed a measure relative to school districts where the initiative narrowly failed. Furthermore, the dynamics of the treatment effects suggest that improvements in test scores are increasing over time. Seven years after the election, the share of students who perform at advanced or proficient levels is ten percentage points higher in treated districts.

²⁹The 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 school and district improvement purposes. Results for eighth grade students are similar to those for tenth grade. However, there is no evidence that referendum approval impacts test scores in fourth grade. These results are discussed in more detail in Appendix A.

To better understand the economic significance of these effects, Panel (c) shows the estimates obtained when using the district’s average score on the math portion of the 10th grade WKCE as the outcome of interest. District-level test scores are standardized using the annual statewide test score distribution. The estimates indicate that narrowly approving a referendum leads to an increase in test scores of roughly 30% of a standard deviation. One can understand the size of these effects by comparing their magnitude with that from the effect of a reduction in class size of eight students, which has been shown to increase student achievement by up to 60% of a standard deviation (Angrist and Lavy, 1999; Finn and Achilles, 1999).

Finally, Panel (d) shows estimates of the effects of referendum approval on postsecondary enrollment.³⁰ Similar to the effects on the district’s dropout rate, there appears to be a lag in the TOT effects of referendum approval on the district’s number of high school completers who subsequently enroll in postsecondary education. Five years after the election, postsecondary enrollment among high school completers is roughly 10% higher in treated school districts. The treatment effect appears to be increasing in the number of years since the election; ten years after referendum approval, postsecondary enrollment is 20% higher in treated school districts relative to districts in the control group. Figure C.1 shows that this effect is primarily driven by increases in enrollment at four-year institutions, not two-year colleges or training programs.

6.3 Robustness Checks

The results presented in this section indicate that additional school spending induced by 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.

6.3.1 Vote Share Specification

If an incorrect parametric functional form for $f_g(v_{d,t-\tau}, \gamma_\tau)$ is used in Equation 2, then estimates of the β_τ^{TOT} ’s will generally be biased (Lee and Lemieux, 2010). Furthermore, as Gelman and Imbens (2019) show, controlling for high-order polynomials in RD designs can lead to noisy estimates and high sensitivity to the degree of the polynomial used. To explore the sensitivity of the main results to alternative orders of the polynomial, Figure 13 presents results from the estimation of Equation

³⁰The dependent variable in this specification is the (logged) number of high school completers in year t who enroll in a postsecondary education program in the state in the fall of year $t + 1$. I control for the total number of high school completers in year t on the right-hand side of the equation. Since the number of high school completers may be endogenous to the approval of a referendum, as a robustness check I replace the number of high school completers with the total fall senior enrollment in year t , as well as with the number of students expected to complete the term in year t . Estimates using these alternative controls are both qualitatively and quantitatively similar to the results presented in this section and are available upon request.

2 using a linear and a quadratic specification of the vote share instead. The figure shows estimates of the β_{τ}^{TOT} 's and corresponding 90% confidence intervals separately for specifications including a first- and a second-order polynomial in the vote share. Estimates from these specifications are similar both in magnitude and statistical significance to those including a third-order polynomial.

6.3.2 Demographic Changes

Given that I use aggregate district-level data one may be concerned with changes in school districts' student composition as a result of referendum approval. 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 school districts in the control group and enroll them in either private or treated schools.³¹ If this were the case, my estimates may be driven by the change in the composition of students in treated and control schools, rather than the direct effects of school spending induced by referendum approval.

To test whether referendum approval led to changes in district demographics, I estimate Equation 2 with each of the following district demographic variables as the outcome of interest: the share of minority students, total enrollment, and the share of economically disadvantaged students. The results from this estimation are shown in Figure 14 and provide no evidence of changes in district composition due to referendum approval.

6.4 Exploring Mechanisms

This section presents a plausible explanation for the observed improvement in student outcomes. Although many factors including individual characteristics and family environment can impact academic outcomes, school-related inputs such as class sizes and teacher quality have been shown to be important determinants of student success. Therefore, I first examine whether changes to specific observable school inputs can be (at least partially) credited as likely mechanisms for the observed effects. I focus on four key inputs employed in the school quality literature: a school district's student-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).

³¹Due to Wisconsin's "open enrollment" policy, parents can apply for their children to attend a public school in a district other than the one in which they reside.

Panel (a) of Figure 15 shows that narrowly passing a referendum leads to a decline of roughly 0.25 percentage points in the student-staff ratio. This effect persists for up to five years after the election, and corresponds to a decline of roughly 3% relative to the average student-staff ratio for the group of school districts that proposed at least one referendum during the sample period. Table C.1 shows that the decline in the student-staff ratio is primarily driven by an increase in the number of licensed and support staff (e.g., teachers, teacher aides, guidance counselors, social workers, and school psychologists), and not by additional administrative positions. These effects are consistent with the observed increases in expenditures in the instruction and support services accounts.

Referendum approval also leads to increases of roughly half a year in the average local teacher experience (Panel (b)). This effect appears to be relatively constant and persists for eight years after the election. Increases in teacher experience have been shown to improve student test scores directly (Papay and Kraft, 2015; Rockoff, 2004). The increase in teacher experience could reflect the relative sharp decline in teacher attrition in narrowly winning districts following the election (Panel (c)). Holding compositional effects constant, teacher attrition has been shown to disrupt instruction (Baron, 2018; Ronfeldt et al., 2013). Thus, the observed increase in teacher experience may have both direct and indirect positive effects on student outcomes.

Panel (d) shows an increase in teacher compensation of roughly 3% five years after the election. The lag in these effects could be due to collectively bargained agreements that do not allow teacher compensation to immediately adjust.³² Increases in teacher compensation may help school districts attract and retain a more highly-qualified teaching workforce. Given the importance of teachers to the production of student achievement, increases in compensation could be at least partially responsible for the observed improvement in student outcomes (Chetty et al., 2014; Rivkin et al., 2005; Rockoff, 2004).

While there may be other mechanisms through which additional spending from referendum approval 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 student counselors, social workers, and school psychologists, and increases in teacher experience and compensation. It is important to note that using an entirely different source of variation and identification strategy, the results in this paper (both in terms of improvements in student outcomes and mechanisms) are consistent with those of other recent studies in the school spending literature (Jackson, 2018; Lafortune et al., 2018; Hyman, 2017; Jackson et al., 2015).

Importantly, in Appendix B I reconcile my results with those of earlier observational studies in the literature by showing that more traditional OLS regressions of student outcomes on school

³²Prior to the passage of Wisconsin's Act 10 in 2011, teachers' unions in the state had the ability to collectively bargain with local school boards over all aspects of teacher compensation. The lag in the estimated effects could reflect stickiness in compensation levels as a result of union contracts.

spending show little to no impacts of additional spending in my data. These results highlight the need to rely on plausibly exogenous variation if one wishes to obtain causal estimates of school spending.

7 Impacts of School Facility Investments in Wisconsin

While most recent studies in the school spending literature find that money matters in public education, the optimal allocation of resources across expenditure types remains an open empirical question. Public expenditures on school facilities in the U.S. totalled roughly \$80 billion in 2015.³³ In an era of lean public budgets, understanding which types of spending are most efficient has considerable significance for economic policy. This section attempts to shed light on this question by separately examining the impacts of narrowly approving a bond referendum tied to school facility investments.

Wisconsin presents a particularly interesting context to investigate optimal resource allocation because state law requires school districts to hold separate referenda for operational purposes and for bond issues targeted to fund capital expenditures (e.g., capital improvements and maintenance projects). This allows me to separately estimate operational expenditure effects from investments in school facilities within a similar institutional context, which differentiates this study from others in the existing school spending literature.³⁴

Poor physical environments, overcrowded classrooms, inadequate ventilation and air quality may impede student learning. Thus, capital expenditures could result in substantial improvements in academic outcomes if they mitigate such environmental conditions, reduce student distractions, and improve teacher morale and turnover. This section isolates exogenous variation in school facility investments by comparing school districts where bond referenda pass and fail by narrow margins.

Wisconsin school districts may issue up to \$1 million in debt without a referendum. This debt must be paid off using funds within the revenue limit. If a district wishes to issue additional debt to fund capital improvements, it must ask for voter approval to increase property taxes in a local bond referendum. A simple majority vote of district residents is required for the initiative to pass. Once a bond referendum is approved by voters, the annual debt service payments are exempt from state-imposed revenue limits and debt is paid off with the additional property tax revenue.

³³ Author's calculation from NCES data. These expenditures are reported in constant 2017-18 dollars and reflect the sum of capital outlays and interest on school debt.

³⁴ Previous studies either estimate the joint impact of increases in operational and capital expenditures (Candelaria and Shores, 2019; Jackson et al., 2018; Lafortune et al., 2018; Kogan et al., 2017; Jackson et al., 2015), or focus exclusively on capital expenditure effects (Rauscher, 2019; Hong and Zimmer, 2016; Martorell et al., 2016; Cellini et al., 2010).

From 1996-97 to 2014-15, 376 Wisconsin school districts held roughly 1,300 bond referenda. Panel (a) of Figure 16 shows a histogram of the vote shares for bond referenda. Similar to the distribution of vote shares for operational referenda, the histogram shows no evidence of a discontinuity around the 50% threshold. The test proposed by McCrary (2008) is shown in Panel (b) and shows no statistically significant discontinuity in the density function of the vote share at the 50% cutoff. These results provide little evidence of endogenous sorting just above the 50% threshold.

7.1 First Stage: Bond Approval and Capital Expenditures

To examine the dynamic treatment effects of bond referendum approval on capital outlays per pupil, I estimate Equation 2 on the sample of school districts that attempted at least one bond referendum throughout the sample period. Figure 17 provides a visual representation of estimates of the β_{τ}^{TOT} 's along with 90% confidence intervals for up to ten years after the election.

Panel (a) shows that bond referendum approval in a narrow election results in large and immediate increases in capital spending that are concentrated in the first two years after the election. In the year following the election, capital spending increases by roughly \$4,000 per pupil. This effect begins to decline two years after the election, and completely dissipates by the third year. The magnitude and pattern of the capital outlay increase is remarkably similar to the one documented by studies in California (Cellini et al., 2010), Texas (Martorell et al., 2016), and Michigan (Hong and Zimmer, 2016). As with operational referenda, all of the additional resources induced by a successful bond referendum stick in the capital outlay account and are not reallocated to operating expenditures (Figure 17 Panel (b)), which allows me to isolate capital expenditure effects.

Figure 18 provides little indication that bond referendum approval affects district-level inputs such as student-staff ratios, teacher experience, teacher compensation, or teacher attrition. These results are consistent with textual analyses of the intended purpose of bond-approved resources, which reveal that bonds are frequently used for the repair, maintenance, and modernization of existing structures.

7.2 Second Stage: Capital Expenditures and Student Outcomes

Figure 19 examines the impact of bond referendum approval in a narrow election on the three academic outcomes examined throughout the study. Consistent with studies in California (Cellini et al., 2010) and Texas (Martorell et al., 2016), the results provide little evidence that school capital campaigns improve student outcomes. TOT estimates of the impact of bond passage on test scores, dropout rates, and postsecondary enrollment are close to zero and mostly statistically insignificant.

It is important to note that investments in school facilities may generate other nonacademic benefits. For instance, renovations could lead to improvements in student health and morale. Fur-

thermore, new facilities could be aesthetically appealing which may lead to increases in property values (Cellini et al., 2010). Nevertheless, the results in this section indicate that expenditures targeted to instruction and other operational functions may be more efficient at impacting student outcomes than investments targeted to improving existing facilities.

8 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 causal impact of additional school spending on educational outcomes. 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. Unlike school districts with overwhelming voter support or opposition, the group of districts with close elections have no significant differences in observables in the years prior to the election.

The standard RD design is complicated by the dynamic nature of referenda in Wisconsin. First, a school district may attempt (and pass) multiple referenda throughout the sample period. Second, districts that narrowly reject an initial proposal are likely to consider and pass a new measure in subsequent years, which generates imperfect compliance in treatment assignment. These features complicate identification of dynamic treatment effects. To deal with these econometric challenges, I first estimate the ITT effects of a successful referendum in a narrow election. A weakness of ITT effects is that they combine both the direct effects of a successful referendum as well as the indirect effects via its influence on subsequent district decisions to hold (or not hold) additional elections. To uncover only the direct effects of a successful referendum, I adapt the dynamic RD approach developed by Cellini et al. (2010) and estimate TOT effects.

I apply these estimators to a rich administrative dataset combining information on two decades of Wisconsin operational 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. The fact that Wisconsin law requires school districts to hold separate referenda for operational purposes (e.g., teacher compensation and class sizes) and for bond issues targeted to fund major capital projects (e.g., new buildings or renovations) allows me to separately identify the effects of increases in operational and capital expenditures, which differentiates this study from those in the existing school spending literature.

In general, I find that Wisconsin school districts allocate roughly 80% 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: a 30% increase of a (district-level) standard deviation in test scores, a 25% decrease in the district's dropout rate, and a

15% increase in postsecondary enrollment at four-year institutions. Districts that narrowly pass a 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 result in better student outcomes.

This finding is consistent with studies showing that improving teacher quality and reducing class sizes can have large positive impacts on student outcomes (Chetty et al., 2014; Krueger and Whitmore, 2001; Angrist and Lavy, 1999). It is also consistent with studies in California (Cellini et al., 2010) and Texas (Martorell et al., 2016) which find little evidence that school capital campaigns improve student outcomes. Overall, the findings in this study 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.

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 local average treatment effects since it exploits variation stemming from relatively close elections. Thus, it is unclear whether these estimates generalize to school districts with elections further away from the 50% threshold. Still, the fact that estimates of school facility investments in Wisconsin closely resemble those in other states suggests the remaining results could also be externally valid.

Relying on a novel source of variation and employing a different identification strategy, the results in this paper are consistent with those of recent studies in the school spending literature (Jackson, 2018; Lafortune et al., 2018; Hyman, 2017; Jackson et al., 2015). While most of these recent studies find that money matters in public education, the optimal allocation of resources across expenditure types remains an open empirical question. This paper advances this literature by showing that expenditures targeted to operational functions may be more efficient than those targeted to school facilities. Continuing to examine which types of school spending are most efficient 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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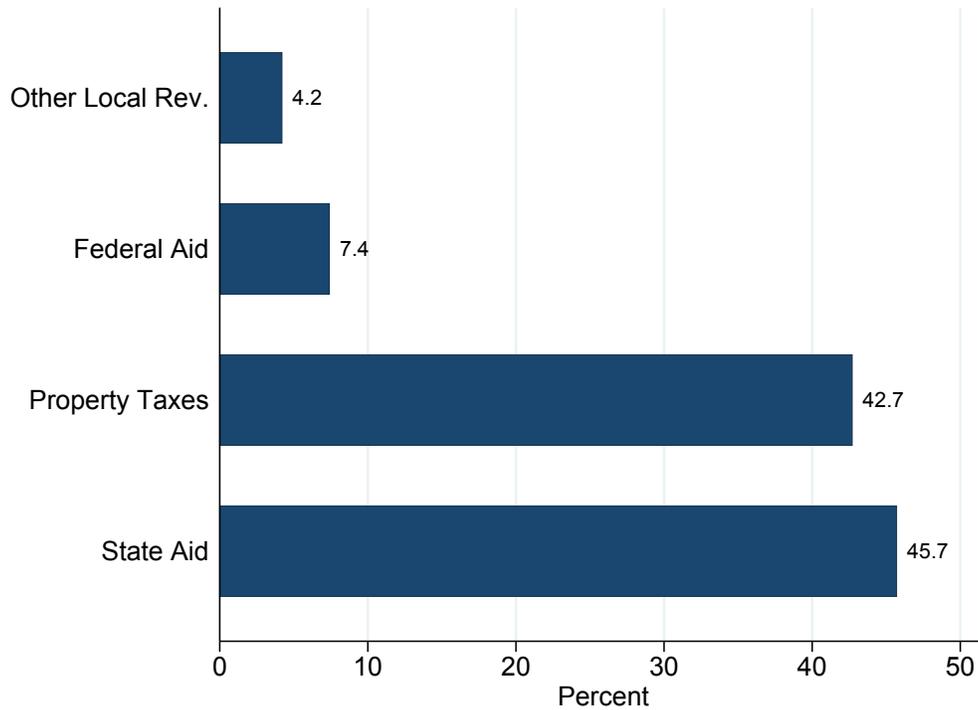
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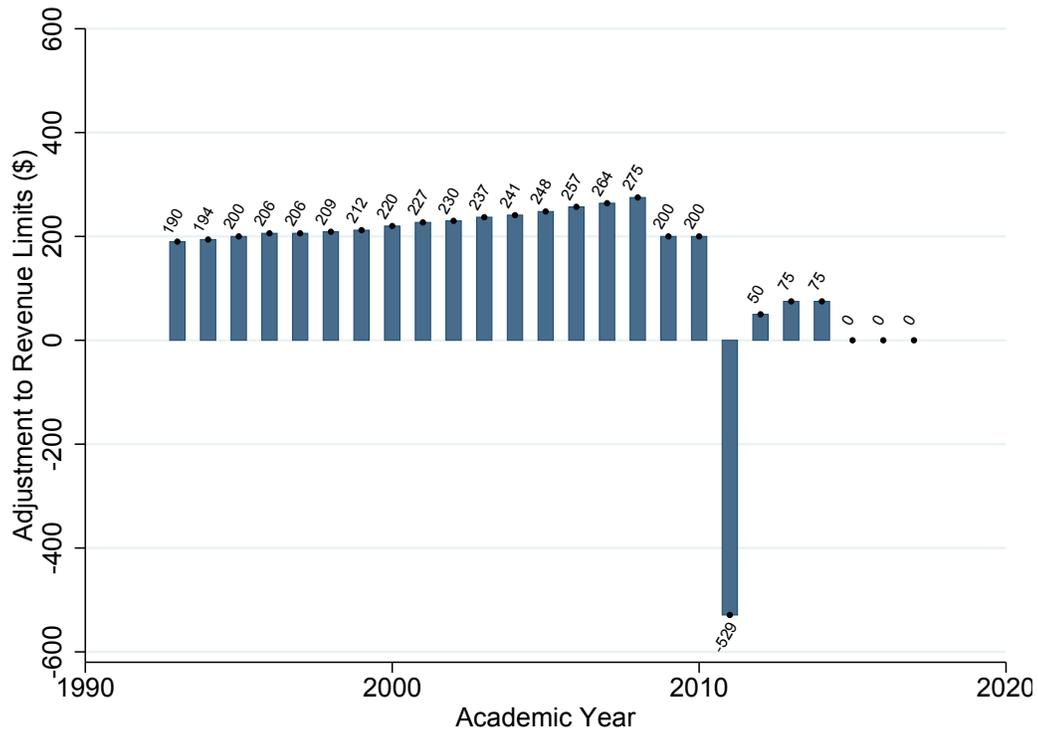
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Figure 1: School District Revenue Sources by Share of Total Revenue (2014-15)



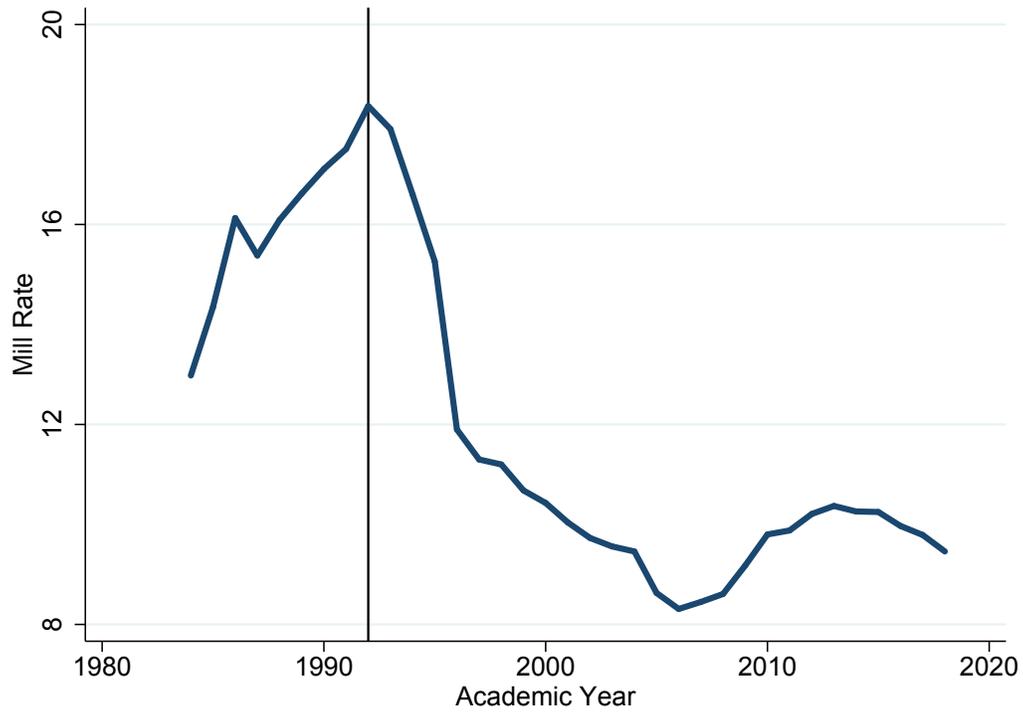
Notes: The figure shows Wisconsin school districts' revenue sources (by share of total revenue) in the 2014-15 academic year. Data come from the Wisconsin Legislative Bureau's *State Aid to School Districts, Informational Paper 24* available at <https://dpi.wi.gov/>. The figure shows that Wisconsin school districts derive most of their revenue from a combination of state aid and local property taxes (roughly 90%). The remaining revenue comes from a combination of federal aid (7.4%) and other local (non-property tax) revenues such as donations and student fees (4.2%).

Figure 2: State-Imposed Annual Adjustments to Per-Pupil Revenue Limits



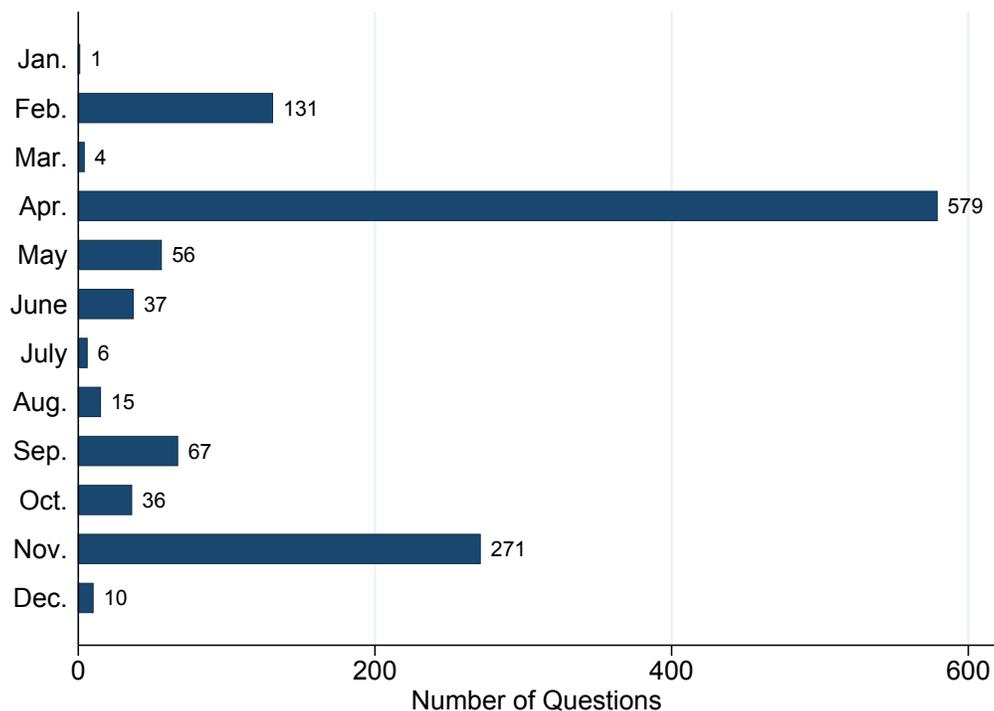
Notes: The figure presents the allowable annual adjustments to per-pupil revenue limits set by the state legislature since 1993-94, the first year under the revenue limit system. Each year, the state legislature determines the allowable per-pupil adjustment to revenue limits as a specific dollar amount. For instance, during the first year of the limits school districts were able to increase per-pupil revenue limits by up to \$190. Prior to 2010, the allowable per-pupil adjustments were intended to serve as inflationary adjustments to the limits. However, following the Great Recession and the looming state budget deficit that followed, the legislature reduced the allowable annual revenue adjustment from \$200 per pupil in 2010-11 to -\$529 in 2011-12. As a result of an improving economy, modest increases to revenue limits were approved by the legislature in 2012-13, but were reduced again to \$0 in 2015-16. Data on state-imposed revenue limit adjustments come from the Wisconsin Department of Public Instruction (WDPI).

Figure 3: Time Series of Wisconsin's School Mill Rate



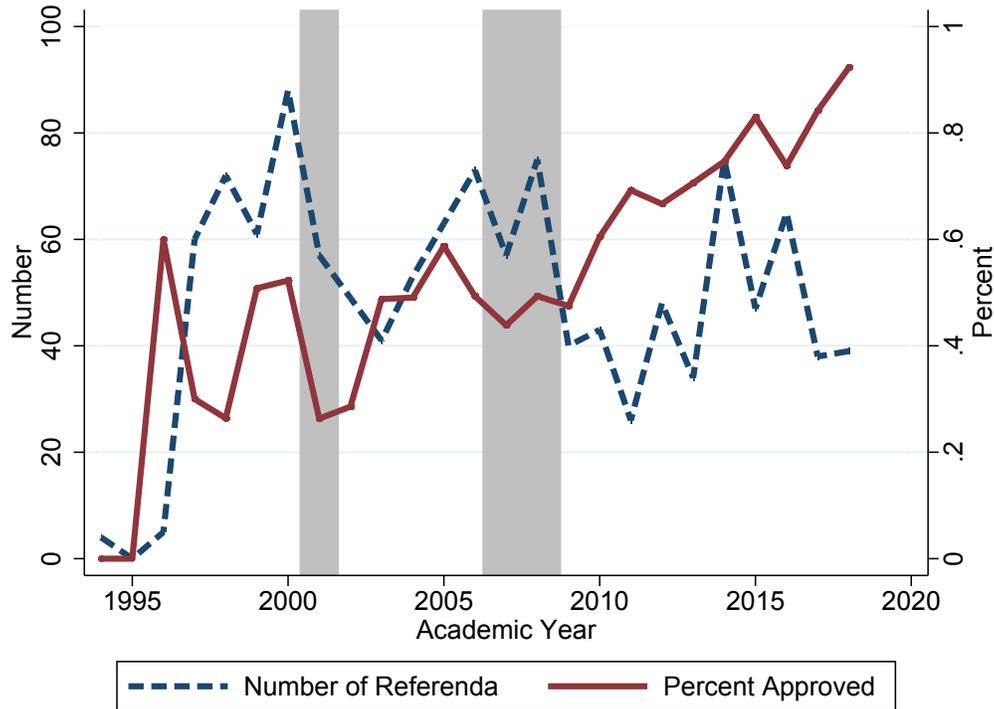
Notes: The figure plots Wisconsin's average mill rate before and after 1993-94, the year revenue limits were first enacted. The mill rate is a measure of the school portion of the property tax. It is defined as the total amount of property tax dollars levied by public school districts per \$1,000 of equalized property value. The figure shows that revenue limits were largely successful in reducing the school portion of the property tax. The mill rate was accelerating in the years prior to the enactment of revenue limits, but has since decreased dramatically. Data on Wisconsin's annual average mill rate come from the Wisconsin Department of Public Instruction (WDPI).

Figure 4: Distribution of Referenda by Month, 1993-2018



Notes: The figure shows the distribution of referenda by election month. The figure shows that most elections, roughly 70%, were held in April and November, the months during which spring and fall general elections are held. Another 18% 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 statewide elections). Referendum-level data come from the Wisconsin Department of Public Instruction (WDPI).

Figure 5: Percent of Approved Referenda Over Time



Notes: The figure plots the number and pass rates of referenda from 1993-2018. The solid line plots the percent of all referenda that were approved, while the dotted line traces out the total number of referenda proposed each year. Recessionary periods, as defined by the National Bureau of Economic Research (NBER) are depicted with gray vertical bars. The figure shows that both of these variables are highly cyclical. More than 80 questions were posed during the 2000-01 academic year, a period with a strong economy and high consumer confidence. However, in the years after the burst of the dot-com bubbles, the number of referenda declined by roughly 50%. The Great Recession brought a similar decline to the number of initiatives proposed by Wisconsin school districts. While the share of approved referenda also tends to be cyclical, it has been steadily increasing since the end of the Great Recession. Referendum-level data come from the Wisconsin Department of Public Instruction (WDPI).

Figure 6: Example of a Referendum Mailer

OUR COMMUNITIES OUR FUTURE | KETTLE MORAINE SCHOOL DISTRICT **OPERATING REFERENDUM**

» **OPERATING REFERENDUM FACTS**

- 5 years
- \$5.975 million per year

» **WHAT WILL THE OPERATING REFERENDUM FUND?**

Retention of high-quality staff who provide excellent programs and services for students.

NOTE: Referendum dollars will not fund new programming or facility projects.

» **WHAT IS THE TAX IMPACT OF THE OPERATING REFERENDUM?**

PROPERTY VALUE	PROPERTY TAX IMPACT (\$0.16 per \$1,000 of property value)	
	Per Month	Per Year
\$ 100,000	\$ 1.33	\$ 16.00
\$ 400,000	\$ 5.33	\$ 64.00

Amounts listed are the projected tax increase over the 2019 tax levy rate of \$9.97 per \$1,000 of equalized property value.

QUESTION ON THE APRIL 2 BALLOT

Shall the School District of Kettle Moraine, Waukesha and Jefferson Counties, Wisconsin be authorized to exceed the revenue limit specified in Section 121.91, Wisconsin Statutes, by \$5,975,000 per year beginning with the 2019-2020 school year and ending with the 2023-2024 school year, for non-recurring purposes consisting of operational expenses?

RIISING FIXED COSTS & DECLINING ENROLLMENT

- Health insurance costs are dramatically increasing, beyond inflation.
- Building maintenance, utilities and transportation costs don't change when enrollment declines.
- Housing for young families in KM is not comparable with neighboring communities.

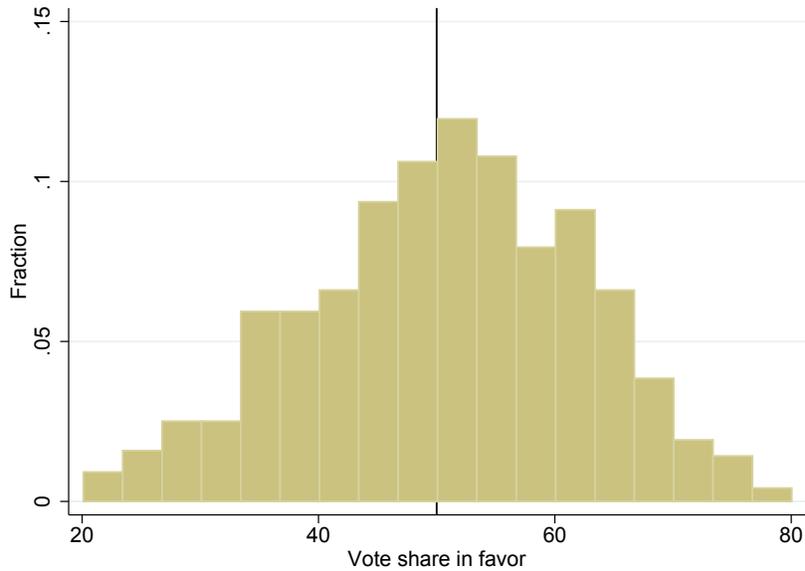
» **WHAT HAS KMSD DONE to manage costs?**

- Reduced staffing
- Increased class sizes
- Implemented innovative programming and choices to be competitive and attract students and families
- Eliminated district post-employment benefits
- Reduced district health insurance costs by 25% over the last 5 years
 - Reduced plan benefits
 - Increased employee contribution
 - Increased deductibles

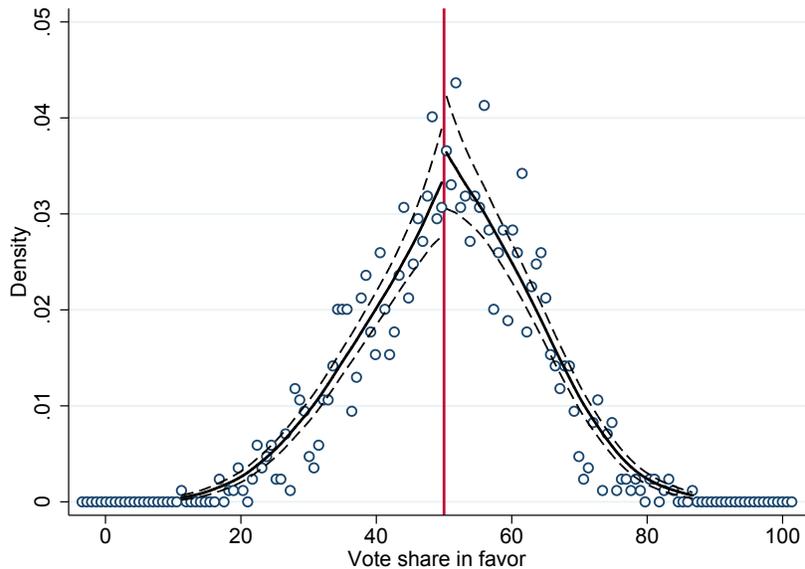
Notes: The figure shows an example of a referendum mailer. Mailers are sent to district residents with the purpose of reminding them to vote and providing them with more information about the upcoming referendum. While the figure provides the specific example of Kettle Moraine School District, which attempted a nonrecurring referendum on April 2, 2019, the typical mailer closely resembles Kettle Moraine's. A mailer will typically list the actual question voters will see at the ballot. For instance, in this example the school district of Kettle Moraine asks voters for permission to exceed state-imposed revenue limits by \$5,975,000 per year for five years. While the actual question usually offers little detail as to how the increased revenue will be used, other parts of the mailer address this question. As an example, Kettle Moraine plans to use the additional revenue to retain high-quality staff. The mailer also addresses why there is a need for additional revenue. Most districts cite declining enrollment and rising costs, as well as declines in state appropriations for K-12 education, as the main reasons why the district must seek voter support. Finally, mailers provide an estimate of the property tax impact that the referendum will have if approved. For instance, if Kettle Moraine's measure is approved, taxes are projected to increase 16 cents per \$1,000 of property value over the current tax levy rate. Source: <https://www.kmsd.edu>.

Figure 7: Vote Share Manipulation Tests

(a) Vote Share Distribution

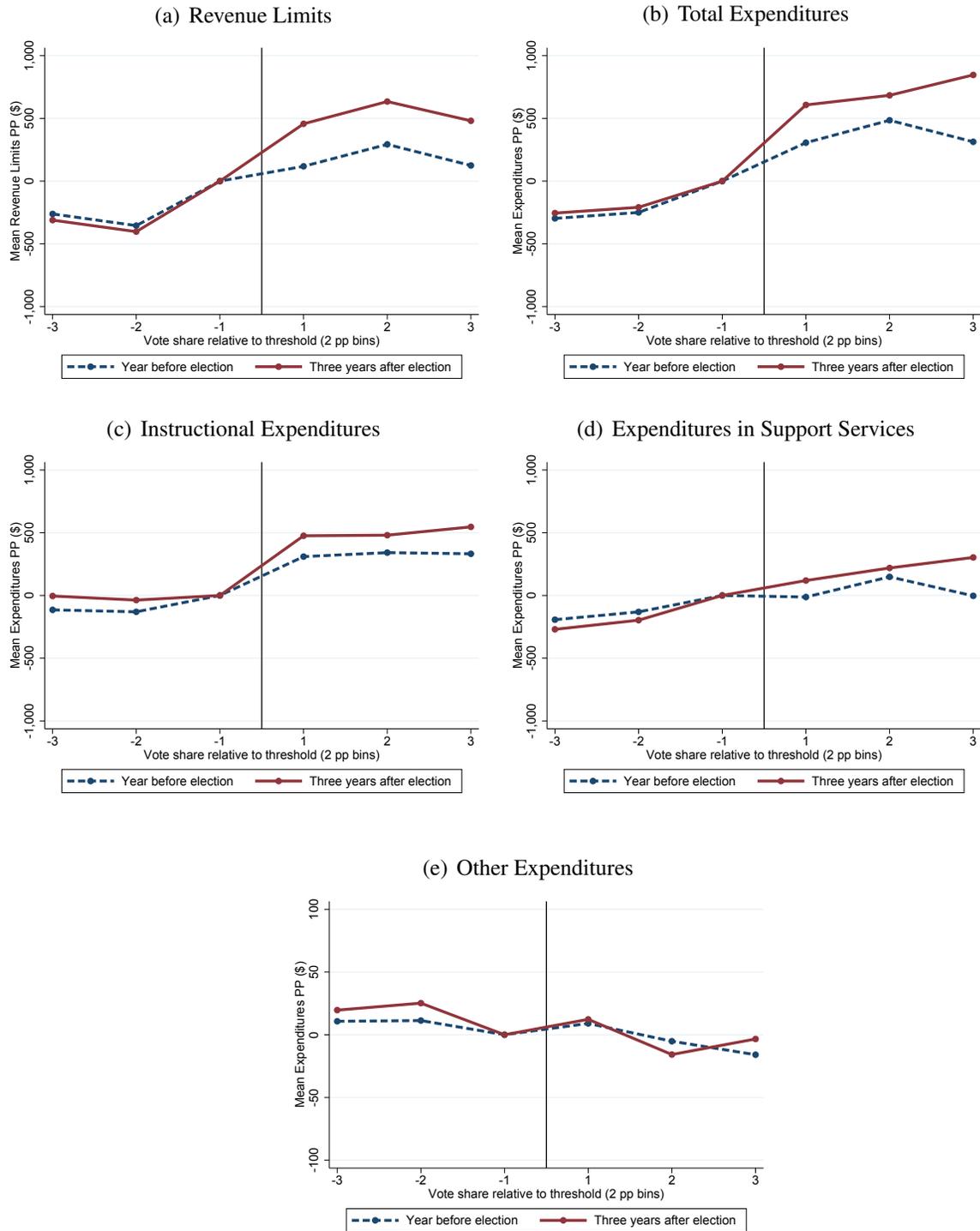


(b) Local Linear Density Estimator



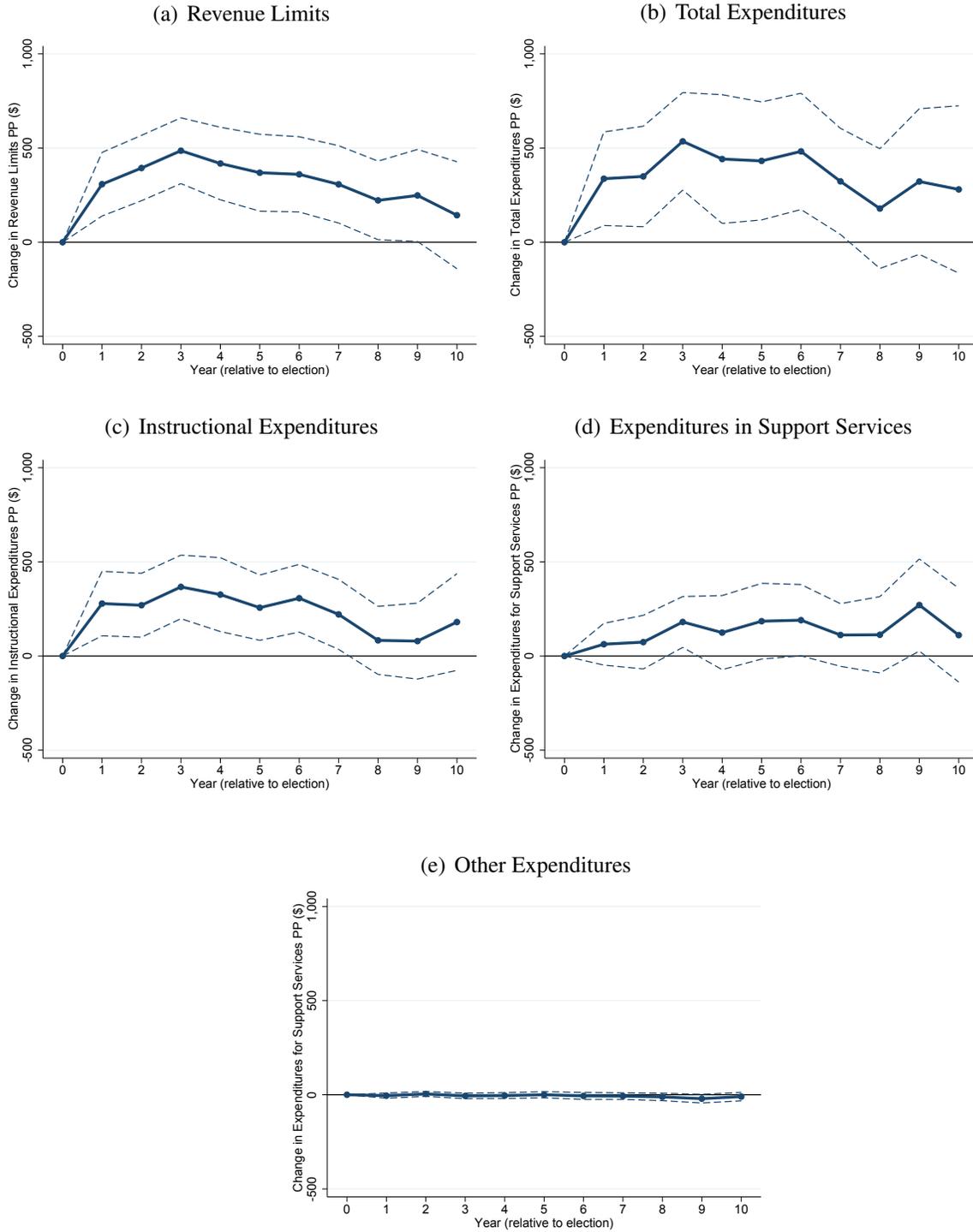
Notes: Panel (a) shows the distribution of referenda by vote share. Vote shares are censored at 20% and 80%. [McCrary \(2008\)](#) proposes a two-step test for the presence of a discontinuity in the density function of the forcing variable at the 50% threshold. In the first step, the forcing variable is partitioned into equally spaced bins and frequency counts are computed within those bins. In the second step, the frequency counts are taken as the dependent variable in a local linear regression. Local linear smoothing is conducted separately on each side of the 50% cutoff to allow for a potential discontinuity in the density function. The log difference of the coefficients on the intercepts of the two separate local regressions provides an estimate of the discontinuity in the density at the threshold. Panel (b) shows the densities estimated in the first step (open circles) as well as the second-step smoothing (solid lines) and corresponding 95% confidence intervals (dashed lines).

Figure 8: Graphical Analysis of the First Stage



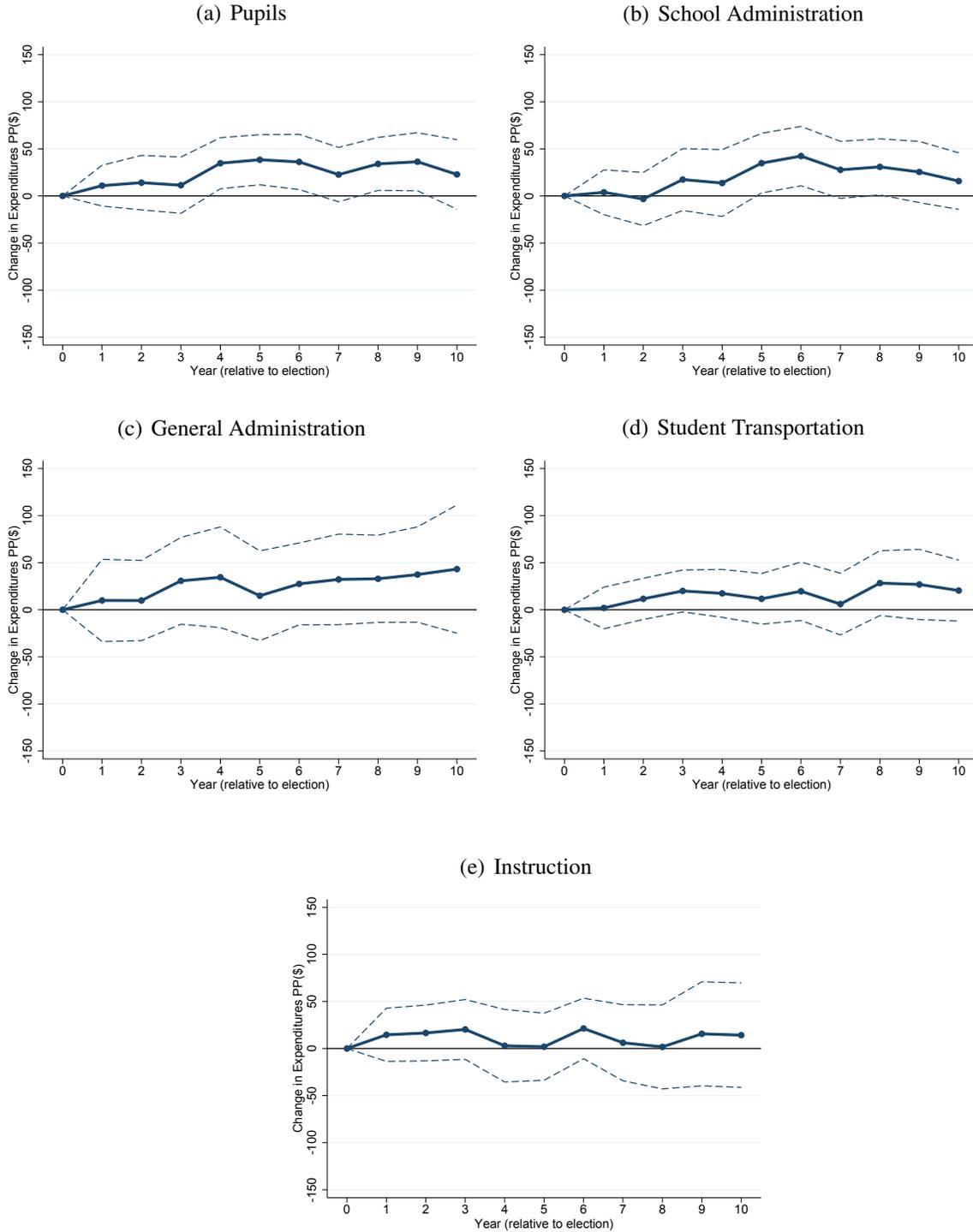
Notes: The figure presents average district revenue limits and expenditures per pupil by margin of victory or defeat in the year prior to the election and three years after it. It shows average outcomes, conditional on school year effects, in two-percentage point bins defined by the vote share relative to 50%. Bin -1, which corresponds to those measures that failed by less than two percentage points, is excluded from the regression used to control for school year effects so that all estimates can be interpreted as differences relative to this bin.

Figure 9: TOT Estimates (First Stage)



Notes: The figure presents results from the estimation of Equation 2. The solid line provides a visual representation of estimates of the β_t^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

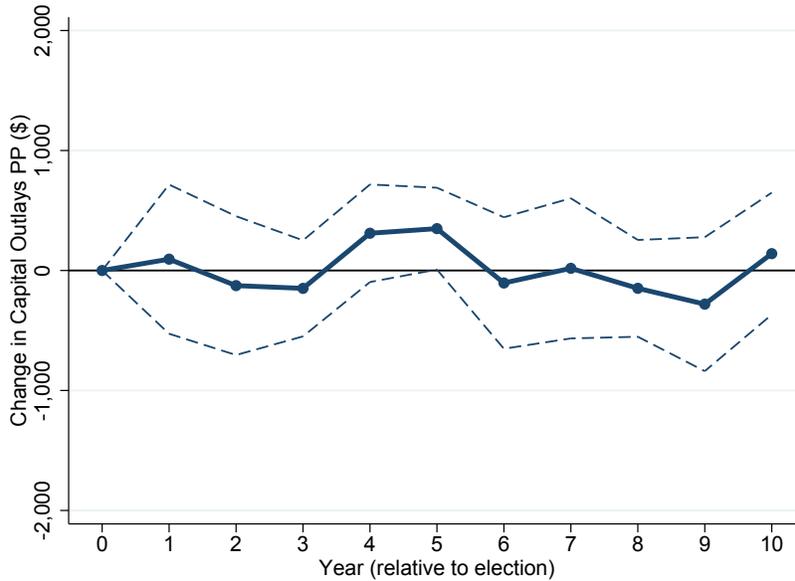
Figure 10: Detailed Expenditures in Support Services



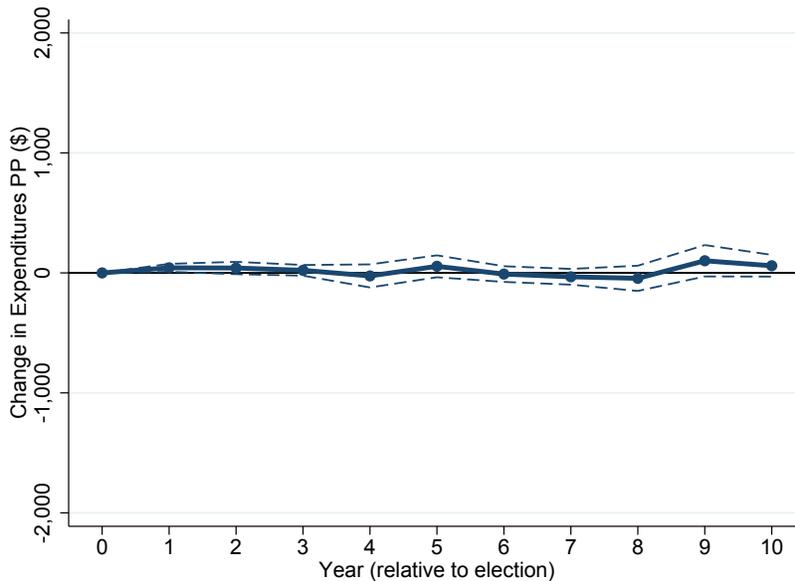
Notes: The figure presents results from the estimation of Equation 2. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

Figure 11: Placebo for Bond Measures

(a) Capital Outlays

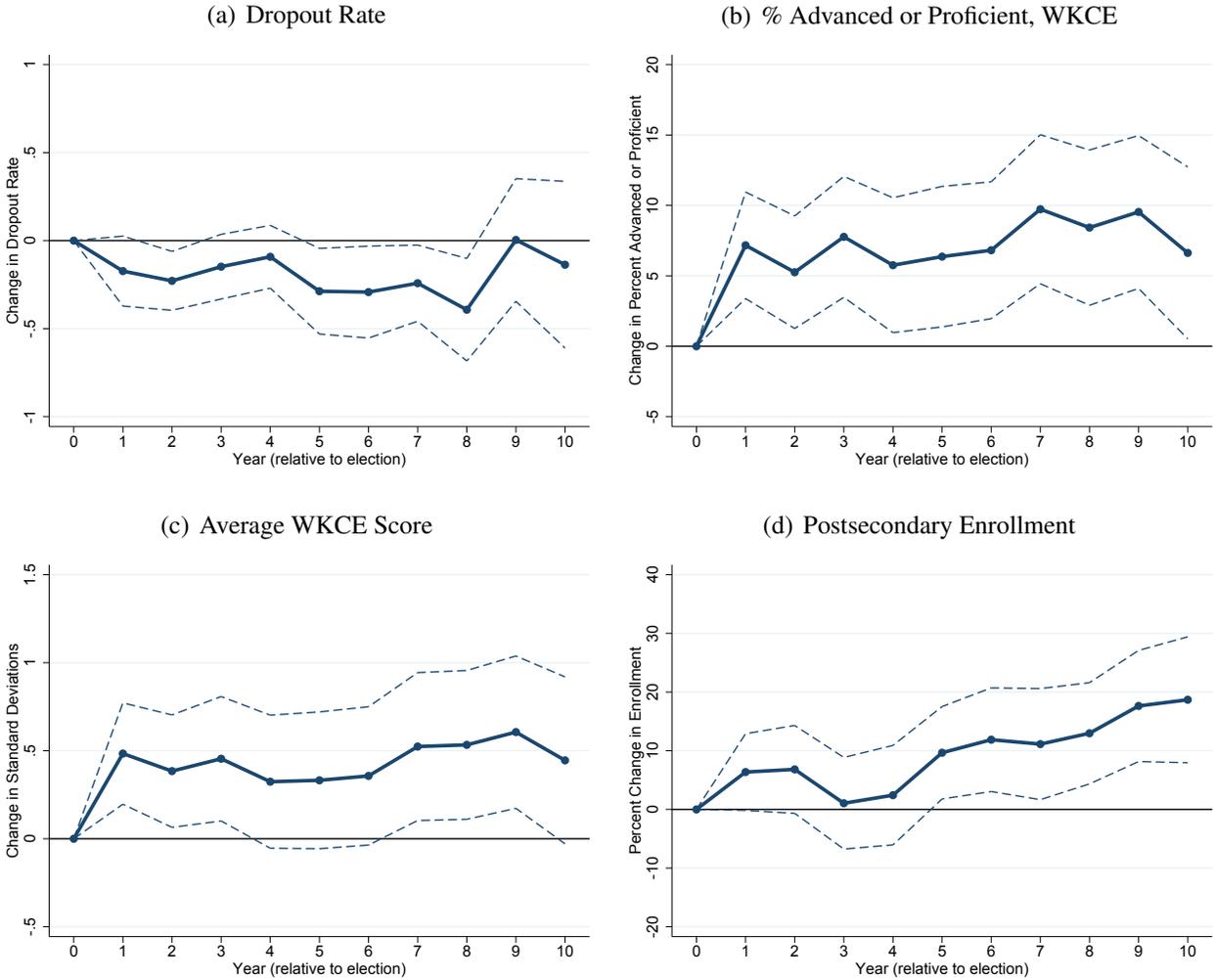


(b) Operation and Maintenance



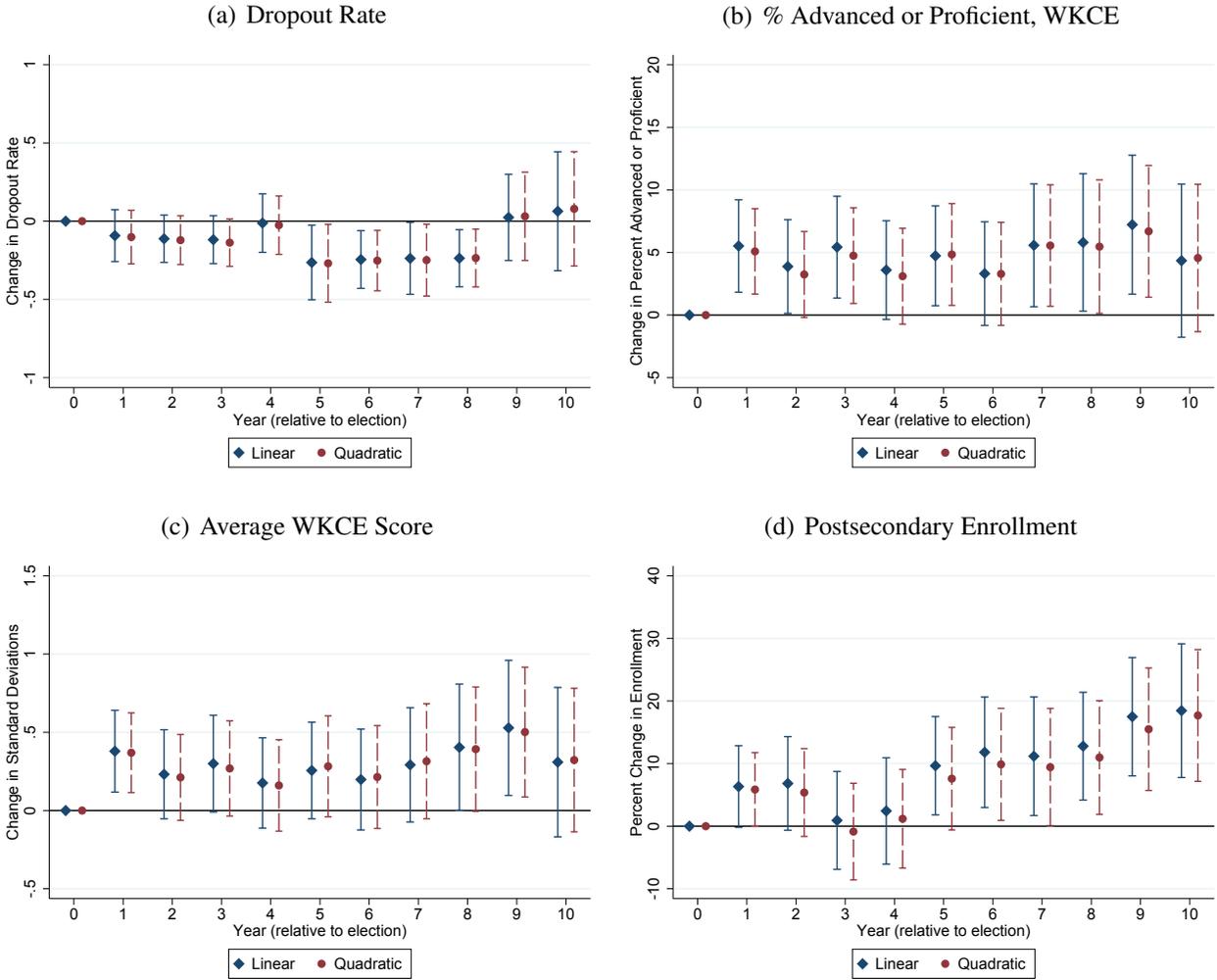
Notes: The figure presents results from the estimation of Equation 2. It shows estimates of the dynamic treatment effects of referendum approval on district-level fiscal outcomes by year relative to the election. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

Figure 12: TOT Estimates (Second Stage)



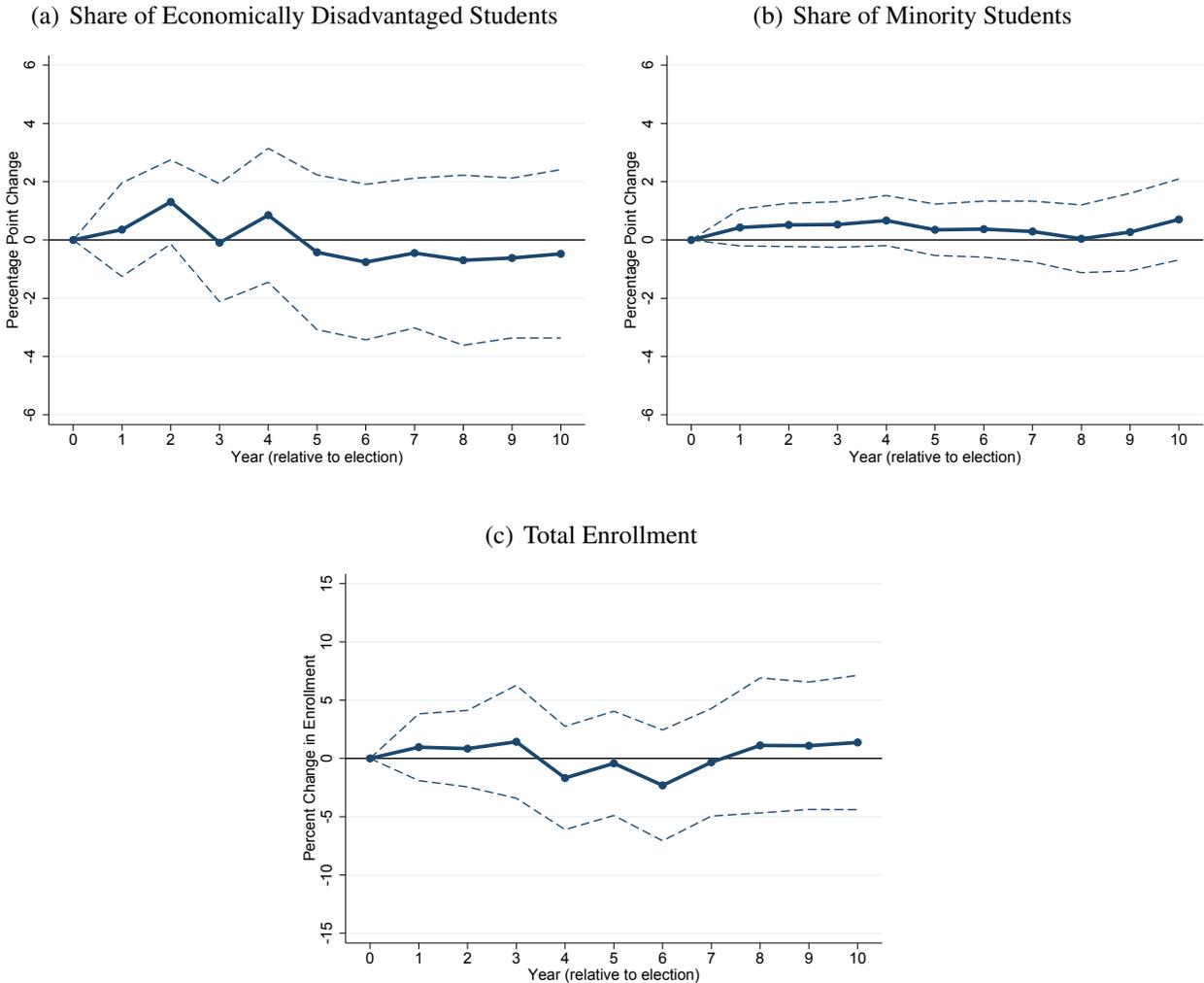
Notes: The figure presents results from the estimation of Equation 2. It shows estimates of the dynamic treatment effects of referendum approval on district-level student outcomes by year relative to the election. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

Figure 13: Linear and Quadratic Specifications of the Vote Share



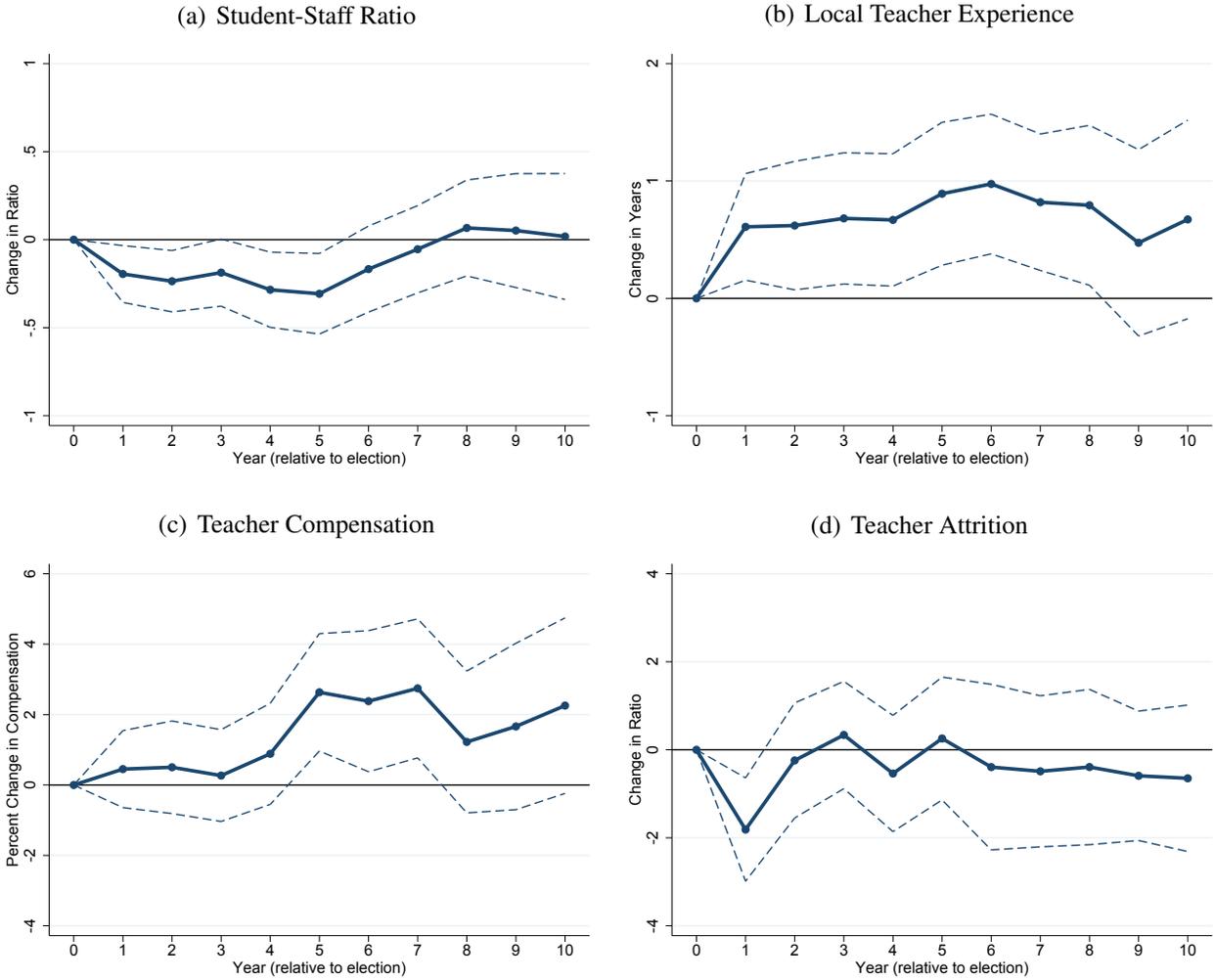
Notes: The figure explores the sensitivity of the main results to alternative orders of the polynomial vote share specification. It presents results from the estimation of Equation 2 using a linear and a quadratic specification of the vote share. The figure shows estimates of the β_{τ}^{TOT} 's and corresponding 90% confidence intervals separately for specifications including a first- and a second-order polynomial in the vote share. 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

Figure 14: Changes in the District's Demographic Composition



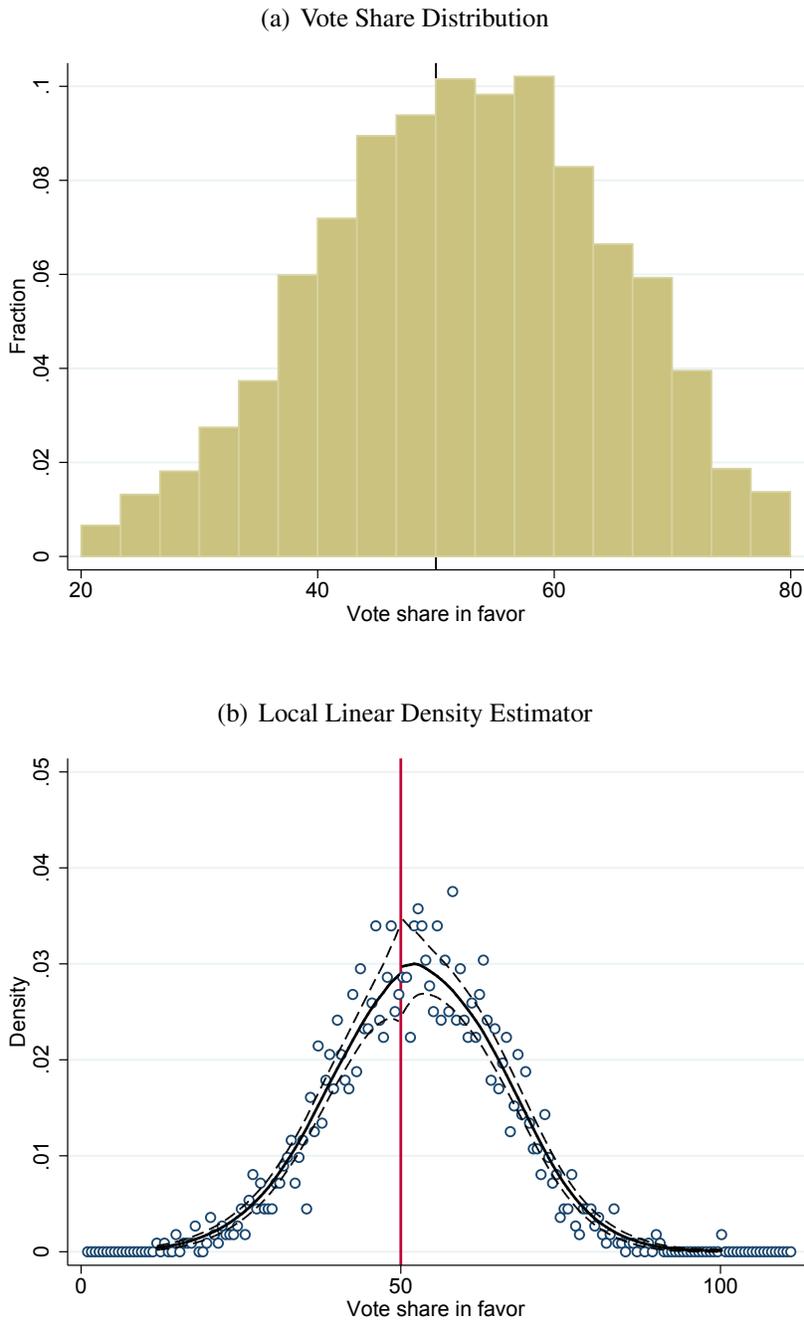
Notes: The figure presents results from the estimation of Equation 2. It shows estimates of the dynamic treatment effects of referendum approval by year relative to the election on district-level demographic variables. The solid line provides a visual representation of estimates of the β_t^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

Figure 15: Plausible Mechanisms



Notes: The figure presents results from the estimation of Equation 2. It shows estimates of the dynamic treatment effects of referendum approval by year relative to the election on district-level variables that have been shown by previous literature to influence student outcomes. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year.

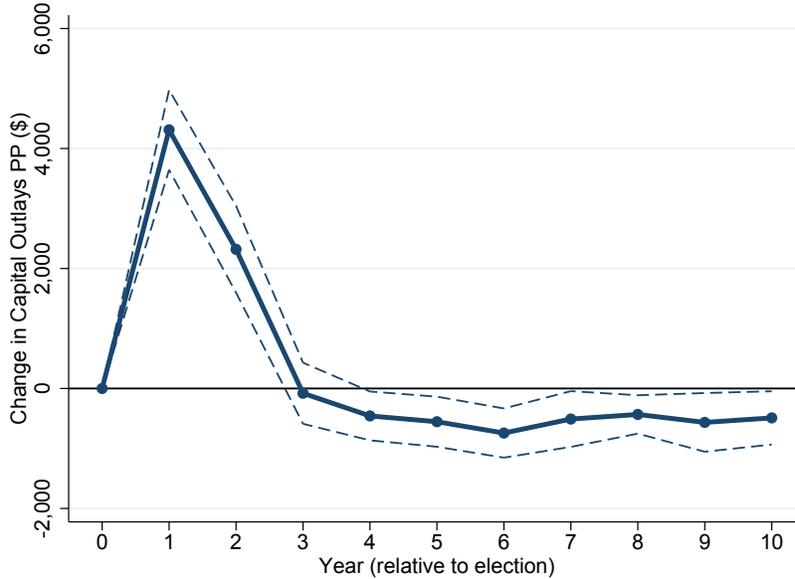
Figure 16: Vote Share Manipulation Tests for Bond Referenda



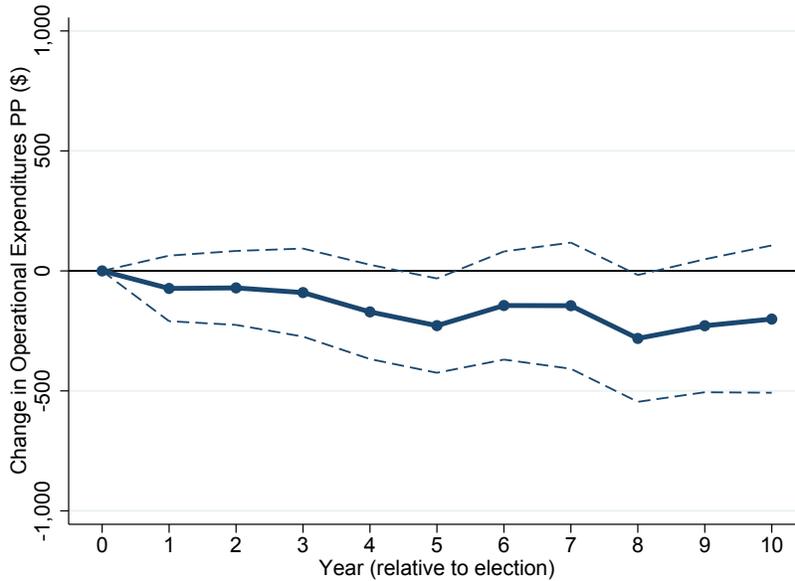
Notes: Panel (a) shows the distribution of bond referenda by vote share. Vote shares are censored at 20% and 80%. [McCrary \(2008\)](#) proposes a two-step test for the presence of a discontinuity in the density function of the forcing variable at the 50% threshold. In the first step, the forcing variable is partitioned into equally spaced bins and frequency counts are computed within those bins. In the second step, the frequency counts are taken as the dependent variable in a local linear regression. Local linear smoothing is conducted separately on each side of the 50% cutoff to allow for a potential discontinuity in the density function. The log difference of the coefficients on the intercepts of the two separate local regressions provides an estimate of the discontinuity in the density at the threshold. Panel (b) shows the densities estimated in the first step (open circles) as well as the second-step smoothing (solid lines) and corresponding 95% confidence intervals (dashed lines).

Figure 17: First Stage Impact of Successful Bond Referenda

(a) Capital Outlays

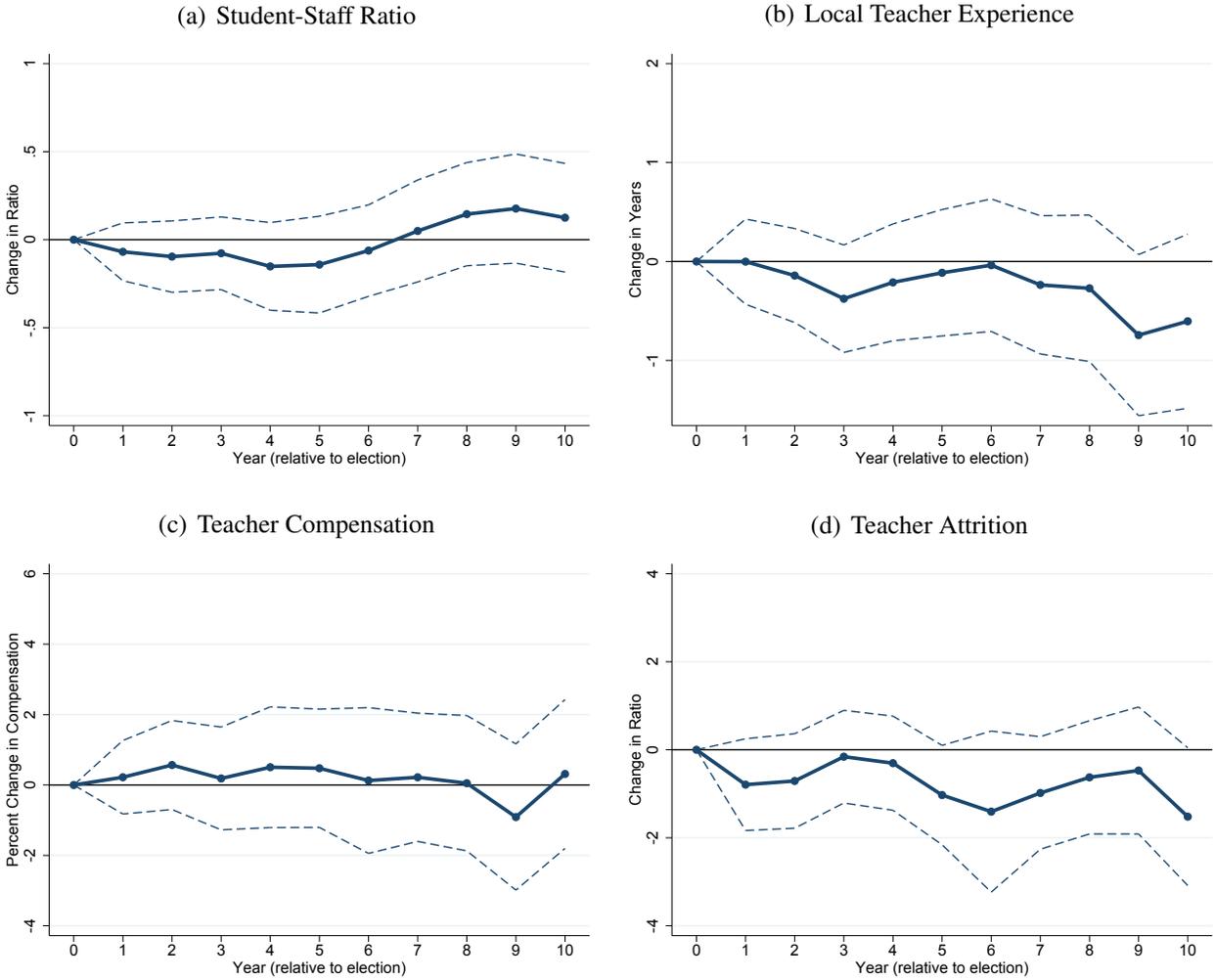


(b) Operational Expenditures



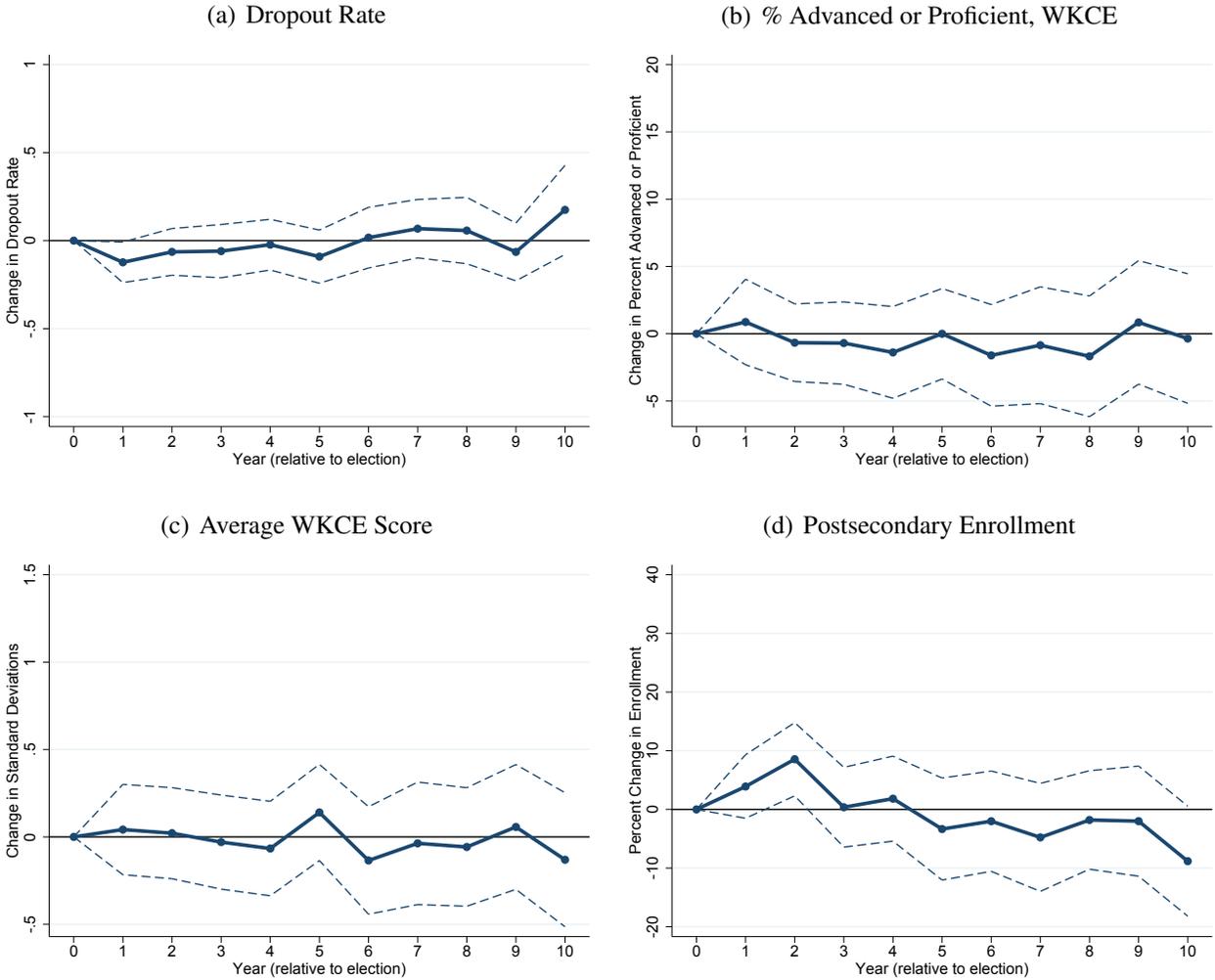
Notes: The figure presents results from the estimation of Equation 2 on the sample of school districts that held at least one bond referendum throughout the sample period. It shows estimates of the dynamic treatment effects of bond referendum approval on district-level fiscal outcomes by year relative to the election. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 voter turnout, whether or not an operational referendum was concurrently placed on the ballot, and the number of bond referenda that the school district placed on the ballot that school year.

Figure 18: Impact of Bond Referenda on School Inputs



Notes: The figure presents results from the estimation of Equation 2 on the sample of school districts that held at least one bond referendum throughout the sample period. It shows estimates of the dynamic treatment effects of bond referendum approval by year relative to the election on district-level variables that have been shown by previous literature to influence student outcomes. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 voter turnout, whether or not an operational referendum was concurrently placed on the ballot, and the number of bond referenda that the school district placed on the ballot that school year.

Figure 19: Impact of Capital Expenditures on Student Outcomes



Notes: The figure presents results from the estimation of Equation 2 on the sample of school districts that held at least one bond referendum throughout the sample period. It shows estimates of the dynamic treatment effects of bond referendum approval on district-level student outcomes by year relative to the election. The solid line provides a visual representation of estimates of the β_{τ}^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 voter turnout, whether or not an operational referendum was concurrently placed on the ballot, and the number of bond referenda that the school district placed on the ballot that school year.

Table 1: Operational Referendum Summary Statistics (1993-2018)

Variable	N	Mean	Median	Std Dev	Min	Max
Panel (a): All Referenda						
Referendum Passed	1,213	0.54	1	0.50	0	1
Vote Share in Favor	1,213	50.67	51.15	12.17	11.39	86.53
Amount Approved PP	657	2,859	1,675	3,899	7	45,771
# of Questions per District	314	3.69	3	2.69	1	19
Panel (b): Recurring Referenda						
Referendum Passed	490	0.41	0	0.49	0	1
Vote Share in Favor	490	47.10	47.32	12.47	11.39	80.60
Amount Approved PP	201	789	482	976	7	7,356
Panel (c): Nonrecurring Referenda						
Referendum Passed	723	0.63	1	0.48	0	1
Vote Share in Favor	723	53.10	53.51	11.34	17.03	86.53
Number of Years	723	3.99	4	2.04	1	20
Amount Approved PP	456	3,771	2,635	4,332	30	45,771

Notes: The table shows summary statistics for operational referenda held by Wisconsin school districts since 1993-94, the first year under the revenue limit system. Data on individual referenda are collected and made publicly available by the Wisconsin Department of Public Instruction (WDPI). Panel (a) provides summary statistics for all referenda (pooling recurring and nonrecurring together). Panels (b) and (c) report summary statistics separately for recurring and nonrecurring measures respectively. 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 five 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).

Table 2: Summary Statistics for Fiscal, Academic, and Teacher Variables

Dependent Variable	All Districts	Never Proposed	Proposed At Least One	Diff (2)-(3)
Panel (a): Fiscal Outcomes				
Revenue Limits PP	9,767 (1,800)	9,853 (2,726)	9,738 (1,346)	115 (63)
Total Expenditures PP	10,598 (1,992)	10,528 (2,847)	10,622 (1,599)	-94 (66)
Instructional Expenditures PP	6,373 (1,042)	6,340 (1,430)	6,384 (871)	-45 (34)
Support Services PP	3,817 (1,060)	3,806 (1,508)	3,821 (856)	-15 (35)
Other Expenditures PP	408 (125)	383 (146)	417 (116)	-34 (4)
Panel (b): Student Outcomes				
Dropout Rate	1.51 (1.97)	2.68 (2.91)	1.01 (1.03)	1.67 (0.31)
% Adv or Prof, 10th Grade	45.67 (12.81)	43.94 (13.48)	46.16 (12.57)	-2.22 (0.55)
Postsecondary Enrollment (Share)	43.13 (11.08)	41.49 (11.94)	43.59 (10.79)	-2.10 (0.46)
Panel (c): District Characteristics				
Student-Staff Ratio	8.05 (1.41)	8.19 (1.33)	8.01 (1.44)	0.18 (0.04)
Teacher Experience	12.83 (2.30)	12.17 (2.30)	13.06 (2.26)	-0.89 (0.06)
Teacher Compensation	71,098 (7,556)	71,107 (7,439)	71,095 (7,597)	12 (197)
Teacher Attrition	10.34 (5.64)	10.81 (5.96)	10.18 (5.52)	0.63 (0.15)
Number of School Districts	421	107	314	421

Notes: The table presents summary statistics. Panel (a) shows summary statistics for district-level fiscal outcomes, while Panel (b) and Panel (c) present summary statistics of variables measuring student outcomes and district characteristics respectively. Column (1) shows the means and standard deviations (in parentheses) of outcomes computed over all district-year observations in the panel. Columns (2) and (3) show summary statistics separately for school districts that proposed at least one referendum during the sample period and those that did not. Finally, Column (4) reports the point estimates and robust standard errors (in parentheses) of tests for equality of means. Bold coefficients are statistically significant at the 5% level. All variables except for the share of students performing at advanced or proficient levels on the math portion of the WKCE and the share of high school completers who subsequently enroll in postsecondary education are available from 1996-97 to 2014-15. Postsecondary enrollment data are only available from 2005-06 on. Similarly, due to administrative changes to the WKCE, year-to-year comparisons are only valid from 2005-06 through 2014-15. Data on individual referenda, district-level student outcomes, and district-level teacher variables come from the WDPI. District-level total current expenditures and current expenditures by source were collected from the NCES. Fiscal variables and average teacher compensation were converted to 2010 dollars using the Midwest Region's CPI-U.

Table 3: Local Balance of Treatment and Control Groups

Dependent Variable	$(t - 1)$		$(t - 2)$ to $(t - 1)$	
	(1)	(2)	(3)	(4)
Panel (a): Fiscal Outcomes				
Revenue Limits PP	281.63	-69.52	6.42	-32.85
	(135.19)	(183.72)	(10.66)	(42.47)
Total Expenditures PP	461.69	-57.06	53.80	55.73
	(225.31)	(269.59)	(27.36)	(90.34)
Instructional Expenditures PP	303.31	-1.54	23.61	21.50
	(128.10)	(209.82)	(15.83)	(57.43)
Support Services PP	168.07	-102.65	26.95	36.84
	(103.25)	(120.17)	(19.16)	(76.12)
Other Expenditures PP	-9.69	47.13	3.25	-2.61
	(8.68)	(22.81)	(3.09)	(8.37)
Panel (b): Student Outcomes				
Dropout Rate	0.00	0.00	-0.03	-0.31
	(0.10)	(0.28)	(0.10)	(0.19)
% Adv or Prof, 10th Grade	2.95	-0.35	2.08	0.44
	(1.33)	(5.41)	(1.27)	(4.34)
Postsecondary Enrollment (Share)	0.83	-1.97	0.35	-5.18
	(1.18)	(0.65)	(1.11)	(3.83)
School Year FE	Y	Y	Y	Y
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 . Bold coefficients are statistically significant at the 5% level. The first column controls only for school year fixed effects. The second column restricts the sample to elections that were decided by less than one percentage point of the vote share. The last two columns repeat the first two specifications, but they take as the dependent variable the change in the specific outcome between $t - 2$ and $t - 1$. 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.

Appendix A ITT and TOT Estimates in Dynamic RD Designs

In this section, I describe the dynamic RD estimators in more detail. First, I show how the RD design approximates a randomized experiment in a cross-sectional framework. This analysis is complicated by the dynamic nature of referenda in Wisconsin: a school district in which an initial proposal is narrowly defeated could propose and pass a new measure in subsequent years. Thus, I extend the cross-sectional analysis to account for the presence of multiple referenda in the same district. Here I also discuss two possible interpretations of the causal effect of referendum passage on school districts' outcomes: the ITT and the TOT effects. Third, I describe the implementation and limitations of the RD estimator used to identify the ITT effect. I conclude this section by showing that the ITT estimates are similar to estimates of the TOT effects presented in the main body of the paper.

A.1 RD in a Cross-Sectional Framework

Suppose that school district d holds a referendum to override state-imposed revenue limits and that the referendum receives vote share v_d . Let $P_d = 1(v_d > 50)$ be an indicator for passage of the referendum. We can write some district-level outcome y_d (e.g., revenue limits, expenditures, or test scores) as:

$$y_d = \alpha + P_d\beta + \varepsilon_d \quad (3)$$

where β is the causal effect of referendum passage on y_d and ε_d represents all additional determinants of y_d , with $E[\varepsilon_d] = 0$.

In general, we might expect $E[\varepsilon_d P_d] \neq 0$. In other words, districts where a referendum passes are likely to differ from school districts where the referendum is defeated along both observable and unobservable characteristics. For instance, relative to residents in districts in which a referendum fails, residents in winning districts may prefer higher levels of education spending that might correlate with higher average levels of income and education and in turn better student outcomes. Therefore, a simple regression like Equation 3 is likely to yield a biased estimate of β . However, provided there is no manipulation of the vote share near the 50% threshold, the correlation between P_d and ε_d can be kept close to zero by focusing only on close elections. To estimate the causal impact of additional school spending, one can use an RD design that compares outcomes in school districts that narrowly pass a referendum (the “treatment group”) to those where the initiative is narrowly defeated (the “control group”).

As in [Hong and Zimmer \(2016\)](#), [Martorell et al. \(2016\)](#), and [Cellini et al. \(2010\)](#), I implement the dynamic RD strategy using a parametric framework that retains all observations in the sample,

but absorbs variation from non-close elections with flexible controls for the vote share. Under the assumption that $E[\varepsilon_d|v_d]$ is continuous, an approximation by a g th order polynomial with coefficients γ_ε , $f_g(v_d, \gamma_\varepsilon)$ will become arbitrarily accurate as $g \rightarrow \infty$. Thus, the following regression of district outcomes on referendum passage controlling for a flexible polynomial in v_d will provide consistent estimates of β :

$$y_d = \alpha + P_d\beta + f_g(v_d, \gamma_\varepsilon) + \varepsilon'_d \quad (4)$$

where $\varepsilon'_d \equiv \varepsilon_d - f_g(v_d, \gamma_\varepsilon)$ is asymptotically uncorrelated with P_d .

A.2 RD with Panel Data and Multiple Treatments

The cross-sectional framework can be extended to allow for multiple referenda throughout the sample period in the same school district. I redefine P_{dt} to be equal to one if district d passes a referendum in school year t and zero otherwise (either if there was no referendum held in year t or if a proposed referendum was rejected). Assuming that the partial effect of referendum passage in one year on outcomes in some subsequent year (holding all intermediate referenda constant) depends only on the elapsed time between the passage of the referendum and the year the outcome is observed, a district outcome in year t can be specified as a function of the full history of successful referenda:

$$y_{dt} = \sum_{\tau=0}^{\bar{\tau}} P_{d,t-\tau}\beta_\tau + \varepsilon_{dt} \quad (5)$$

There are two possible definitions of the causal effect of referendum passage in $t - \tau$ on an outcome in year t . First, one can examine the effect of exogenously passing a referendum in district d in year $t - \tau$ and “prohibiting” the district from passing any subsequent referenda. From Equation 5, this is β_τ , since it holds constant all other referendum wins. This effect is known as the “treatment on the treated” (TOT), β_τ^{TOT} . Therefore, an estimate of β_τ^{TOT} will isolate the impact of referendum passage (with no subsequent changes in the district’s revenue limits) in $t - \tau$ on a district’s outcome in t . The main body of the paper has focused on estimates of the β_τ^{TOT} ’s.

An alternative to examining the TOT effect is to focus on the impact of passing a 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 $P_{d,t-\tau}$ on y_{dt} operating through intermediate referendum wins in subsequent years $\{P_{d,t-\tau+1}, P_{d,t-\tau+2}, \dots, P_{dt}\}$. Thus, the ITT effect of $P_{d,t-\tau}$ on y_{dt} is:

$$\beta_{\tau}^{ITT} = \frac{dy_{dt}}{dP_{d,t-\tau}} = \frac{\partial y_{dt}}{\partial P_{d,t-\tau}} + \sum_{h=1}^{\tau} \left(\frac{\partial y_{dt}}{\partial P_{d,t-\tau+h}} \times \frac{dP_{d,t-\tau+h}}{dP_{d,t-\tau}} \right) = \beta_{\tau}^{TOT} + \sum_{h=1}^{\tau} \beta_{t-h}^{TOT} \pi_h \quad (6)$$

where $\pi_h \equiv dP_{d,t-\tau+h}/dP_{d,t-\tau}$ represents the effect of passing a referendum in year $t - \tau$ on the probability of passing a referendum h years later. As Figure A.1 shows, on average, districts that pass a referendum are less likely to pass other referenda in the subsequent year ($\pi_h < 0$ for $h \leq 1$ and $\pi_h = 0$ for $h > 1$). Under the assumption that $\beta_{\tau-h}^{TOT} \geq 0$ for all h , this implies that, relative to β_{τ}^{TOT} , β_{τ}^{ITT} will be downward biased.

A.3 Estimating ITT Effects

Recall that estimating ITT effects corresponds to examining the impact of referendum passage in some year on a district's outcomes in a later year without controlling the district's behavior in the intermediate years. Thus, to estimate ITT effects one can simply examine outcomes in subsequent years for school districts that pass or fail a given referendum, controlling flexibly for the vote share in that specific election but *not* for any subsequent elections or referendum outcomes. Consider a district d that held a referendum election in school year t . One can write the district's outcome τ years later as:

$$y_{d,t+\tau} = P_{dt} \beta_{\tau}^{ITT} + f_g(v_{dt}, \gamma_{\tau}) + \varepsilon'_{d,t+\tau} \quad (7)$$

where $f_g(v_{dt}, \gamma_{\tau})$ is a (third-order) polynomial in v_{dt} with coefficients γ_{τ} and $\varepsilon'_{d,t+\tau} \equiv \varepsilon_{d,t+\tau} - f_g(v_{dt}, \gamma_{\tau})$, which asymptotically is uncorrelated with P_{dt} .

While Equation 7 ensures that $\varepsilon'_{d,t+\tau}$ is uncorrelated with P_{dt} , the error term has a component that varies across districts but is fixed over time within districts. Therefore, to obtain more precise estimates of the β_{τ}^{ITT} 's I follow Cellini et al. (2010) and pool data from multiple τ , including periods preceding the election ($\tau < 0$), as well as controls that absorb district-level heterogeneity.

To implement this strategy, I identify all (d, t) combinations with an election (e.g., Green Bay Area Public School District in 2001). I then map these elections to outcomes in district d in years $t - 2$ through $t + 6$. If a district has multiple elections and the school years for outcomes overlap, the same district-year observation is used more than once. As an example, if Green Bay Area Public School District held a referendum in 2001 and in 2003, the $[t - 2, t + 6]$ windows are $[1999, 2007]$ and $[2001, 2009]$, and the 2001-2007 observations are included in each. Observations in the final dataset are thus uniquely identified by the district d , the school year of the specific referendum t , and the year relative to the election (the number of years elapsed between the referendum and the time at which the outcome is measured) τ . I use this sample to estimate the following equation:

$$y_{d,t+\tau} = P_{dt}\beta_{\tau}^{ITT} + f_g(v_{dt}, \gamma_{\tau}) + \mu_{dt} + \theta_t + \lambda_{\tau} + \varepsilon_{d,t+\tau} \quad (8)$$

where $\mu_{dt}, \theta_t, \lambda_{\tau}$ represent fixed effects for specific elections (which absorb district-level unobserved heterogeneity), school years, and years relative to the election, respectively. As in [Cellini et al. \(2010\)](#) both γ_{τ} and β_{τ}^{ITT} are allowed to vary flexibly for $\tau > 0$ but are constrained to be zero for $\tau \leq 0$.³⁵ Standard errors are clustered at the district level to account for the serial correlation induced by multiple proposals in some school districts, as well as within-district error correlation over time.

A.4 Comparing ITT and TOT Estimates

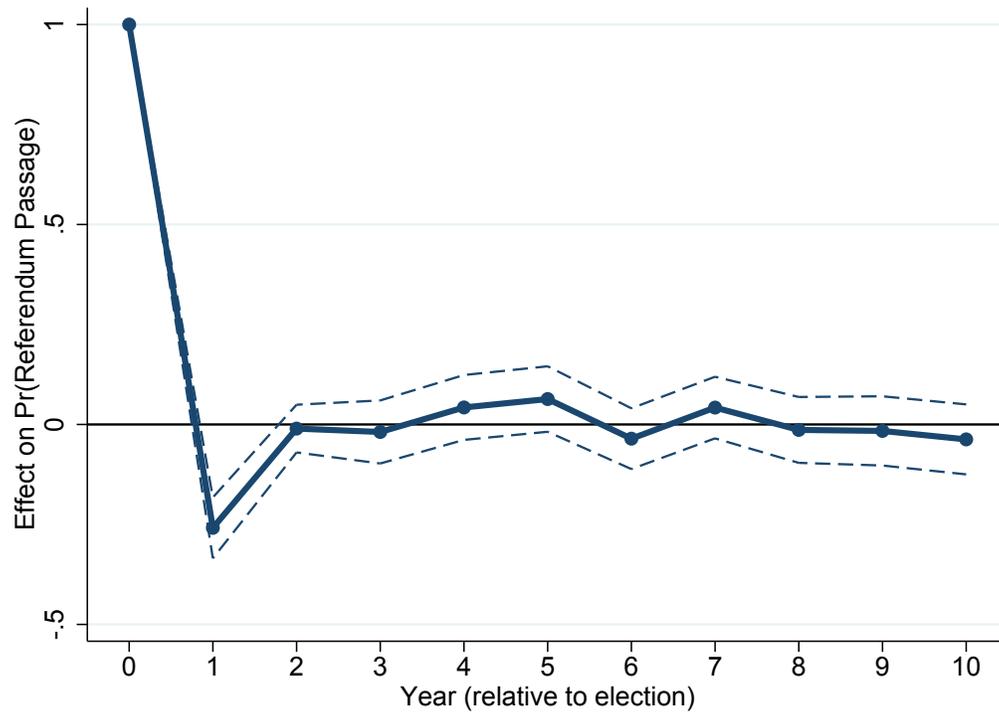
Table [A.1](#) compares the TOT estimates presented in the main body of the paper to estimates of the ITT effects for district-level fiscal and student outcomes. As previously mentioned, the dynamics in treatment assignment imply that the ITT effects of referendum passage on expenditures and student outcomes are downward biased. The estimates presented in the table confirm this pattern. The TOT estimates shown in Panel (a) are larger and more precise than the ITT estimates shown in Panel (b). Nevertheless, ITT estimates are remarkably similar to those shown in the main body of the paper and tell a qualitatively similar story.

ITT estimates indicate that narrowly approving a referendum increases revenue limits per pupil by roughly \$300 in the year following the election. This effect is relatively constant and persists only for the first three years after the election. Increases in revenue limits translate into similar increases in spending. Narrowly approving a referendum leads to an increase in current expenditures per pupil of roughly \$300, or 3% relative to the average current expenditure per pupil in my sample.

Increases in spending translate into substantial improvements in student outcomes. Referendum approval in a narrow election leads to a sharp increase in the share of students in the district who score in the advanced or proficient levels on the math portion of the 8th and 10th grade WKCE. Furthermore, barely passing a referendum leads to a decline in the district's dropout rate and an increase in the number of high school seniors who subsequently enroll in postsecondary education. Five years after the election, treated school districts have a relative decline in the dropout rate of roughly 0.25 percentage points, and a relative increase in postsecondary enrollment of roughly 10%. The robustness of the main results to the choice of estimator provides strong evidence that additional school spending is associated with large improvements in student outcomes.

³⁵The $\tau = 0$ coefficient is constrained to zero as it is not plausible that referendum approval can have an effect on the district's budget that year. Revenue limit increases resulting from approved referenda occur no sooner than the academic year following the election.

Figure A.1: Effect of Referendum Passage on the Probability of Passing a Later Referendum



Notes: The figure presents results from the estimation of Equation 8 with $P_{j,t+\tau}$ as the dependent variable. The solid line provides a visual representation of estimates of the β_t^{ITT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level.

Table A.1: ITT Estimates of Referendum Success on Fiscal and Student Outcomes

Dependent Variable	Year Relative to the Election					
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs
Panel (a): TOT Estimates						
Revenue Limits PP	307.82 (102.42)	393.03 (105.56)	486.62 (105.52)	417.17 (116.98)	367.96 (123.82)	358.11 (120.76)
Total Expenditures PP	337.56 (150.36)	350.60 (161.36)	537.70 (156.28)	443.15 (207.08)	430.79 (188.74)	482.71 (187.02)
% Adv or Prof, 10th Grade	7.09 (150.36)	5.28 (161.36)	7.78 (156.28)	5.82 (207.08)	6.42 (188.74)	6.80 (187.02)
% Adv or Prof, 8th Grade	2.66 (1.84)	4.65 (2.15)	4.65 (2.24)	4.62 (2.23)	6.99 (2.74)	0.34 (2.82)
% Adv or Prof, 4th Grade	1.13 (2.06)	1.61 (1.99)	1.03 (2.27)	2.57 (2.29)	-1.15 (2.25)	-2.17 (2.40)
Dropout Rate	-0.17 (0.12)	-0.23 (0.11)	-0.14 (0.11)	-0.09 (0.11)	-0.30 (0.15)	-0.30 (0.16)
Postsecondary Enrollment	0.06 (0.04)	0.07 (0.05)	0.01 (0.05)	0.02 (0.05)	0.10 (0.05)	0.12 (0.05)
Panel (b): ITT Estimates						
Revenue Limits PP	303.32 (79.96)	271.40 (102.23)	309.19 (110.39)	223.07 (153.61)	226.79 (193.74)	268.59 (196.54)
Total Expenditures PP	291.65 (101.83)	226.14 (115.81)	331.51 (121.64)	217.34 (184.02)	237.02 (176.59)	308.99 (170.93)
% Adv or Prof, 10th Grade	4.84 (1.98)	5.77 (2.38)	5.70 (2.13)	6.74 (2.53)	4.70 (2.57)	5.52 (2.71)
% Adv or Prof, 8th Grade	4.79 (2.26)	6.32 (2.76)	3.49 (2.79)	7.31 (3.14)	7.10 (3.30)	3.44 (3.33)
% Adv or Prof, 4th Grade	0.82 (2.55)	0.92 (2.79)	-2.21 (2.98)	0.82 (3.04)	-1.60 (3.27)	-1.91 (3.40)
Dropout Rate	-0.08 (0.09)	-0.17 (0.10)	-0.14 (0.11)	-0.08 (0.10)	-0.26 (0.11)	-0.08 (0.11)
Postsecondary Enrollment	0.02 (0.05)	0.01 (0.05)	0.02 (0.05)	0.00 (0.06)	0.10 (0.06)	0.02 (0.07)

Notes: The table compares estimates of the ITT and TOT effects of referendum approval on fiscal and student outcomes. Panel (a) presents results from the estimation of Equation 2. It presents estimates of the β_{τ}^{TOT} 's along with standard errors clustered at the district level in parentheses for up to six years after the election. Panel (b) presents estimates of the β_{τ}^{ITT} 's from the estimation of Equation 8. Bold coefficients are statistically significant at the 5% level. Data on individual referenda and district-level student outcomes come from the WDPI. District-level revenues and total current expenditures were collected from the NCES. Fiscal variables were converted to 2010 dollars using the Midwest Region's CPI-U.

Appendix B Comparison to Naive Regressions

As mentioned previously, numerous early studies in the economics of education literature failed to find evidence that increases in school spending were associated with improvements in student outcomes. Many of these studies, however, either used cross-sectional data to relate variation in student outcomes to variation in school resources, or panel data to correlate changes in these two variables. As a result, these studies likely suffer from omitted variable bias; federal- and state-level changes to K-12 education funding formulas since the 1960s brought about by school finance reforms likely weaken the observed relationship between district resources and student outcomes.

In this section, I show that I am able to replicate the null findings of early studies by estimating similar specifications using my data. Specifically, I estimate regressions of the following form:

$$y_{dt} = \beta_0 + \beta_1 \log(\text{Spending}_{dt}) + \mathbf{X}_{dt}\Theta + \mu_d + \theta_t + \varepsilon_{dt} \quad (9)$$

where μ_d and θ_t are defined as in Equation 2; y_{dt} is one of four district-level measures of student outcomes: the share of students who score at advanced or proficient levels on the math portion of the WKCE, the average math WKCE score, the dropout rate, and the number of high school completers who subsequently enroll in postsecondary education; $\log(\text{Spending}_{dt})$ is the log of the district's per-pupil total current expenditures; \mathbf{X}_{dt} is a vector of time-varying district-level demographics and teacher characteristics. The parameter of interest, β_1 , measures the change in the dependent variable as a result of a 1% increase in total current expenditures per pupil.

The baseline results from the estimation of Equation 9 are shown in Table B.1. Each row reports estimates of β_1 for a particular outcome of interest and across four different specifications. Column (1) reports the results of a simple cross-sectional regression for the 2005-06 school year.³⁶ This regression includes controls for the district's total enrollment, as well as for the share of minority and economically disadvantaged students. In Column (2), I add to the cross-sectional regression controls for the district's average teacher salary and local experience. Heteroskedasticity-robust standard errors are reported in parentheses in the first two columns. Column (3) pools all available years of data in my sample and controls for district demographics and teacher characteristics. Lastly, in Column (4) I add district and year fixed effects. Standard errors are clustered at the district level and are shown in parentheses in the last two columns. Bold coefficients are statistically significant at the 5% level.

In general, estimates across all student outcomes and specifications indicate that the impact of increases in per-pupil spending on student outcomes is either zero or economically small. Most estimates are not statistically significant at the 5% level and those that are significant are small

³⁶This is the first school year for which postsecondary enrollment data are available.

in magnitude. As an example, models with year and district fixed effects indicate that a 10% increase in per-pupil spending is associated with an increase in test scores of 4.5% of a standard deviation. However, there is no evidence that changes in spending are related to changes in the district's dropout rate or in the share of high school seniors who subsequently enroll in postsecondary education.

These findings suggest that simple regressions that relate actual variation in student outcomes to variation in school resources may suffer from attenuation bias. The magnitude of the estimates demonstrates the importance of using quasi-experimental variation in school spending to identify causal relationships.

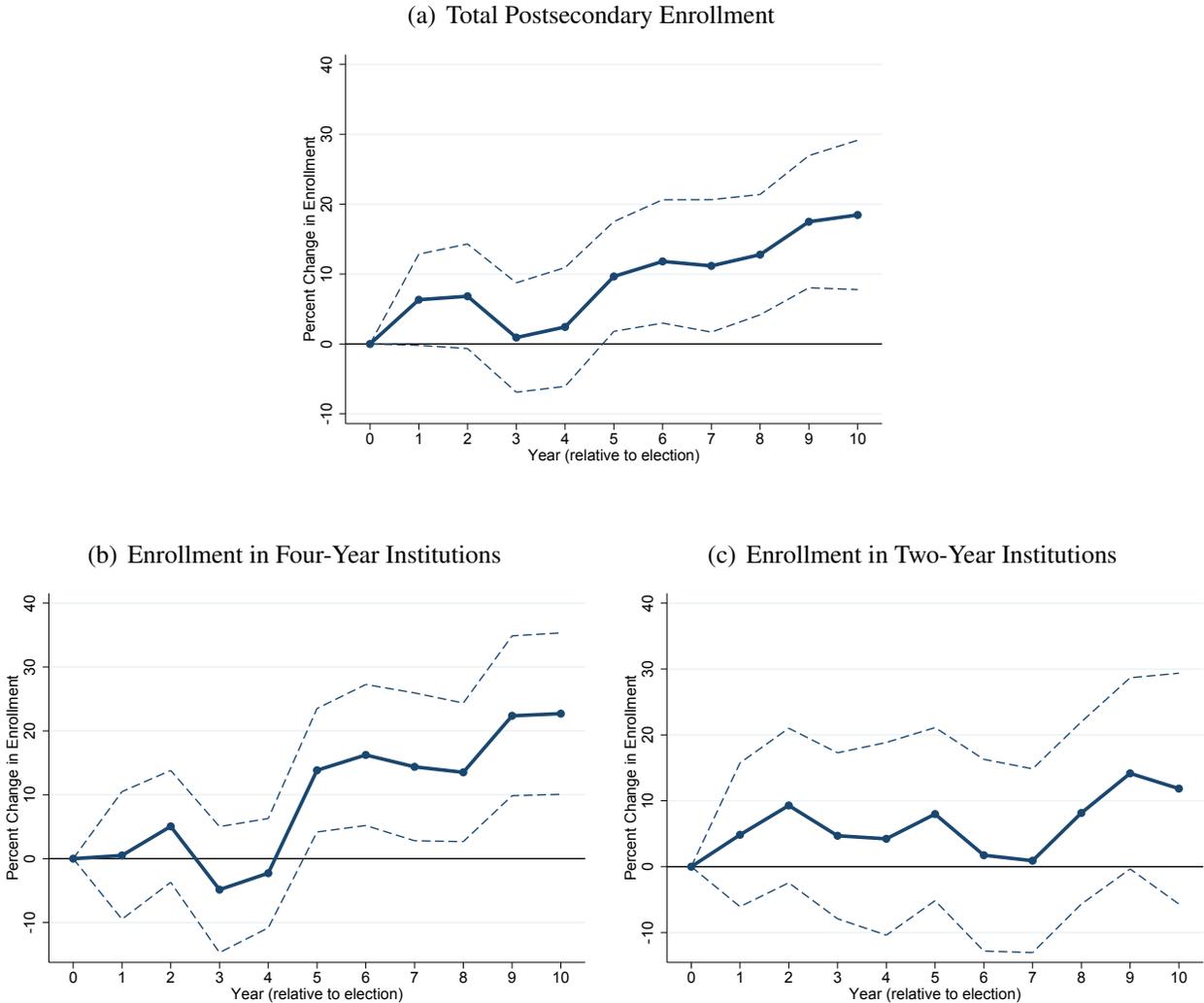
Table B.1: Naive Regressions of Student Outcomes on School Spending

Dependent Variable	(1)	(2)	(3)	(4)
% Adv of Prof, WKCE	-12.56 (8.40)	9.14 (6.63)	-0.16 (3.14)	-4.85 (3.12)
Average WKCE	-0.94 (0.75)	1.06 (0.49)	0.23 (0.24)	0.45 (0.23)
Dropout Rate	0.46 (0.31)	-0.33 (0.26)	-0.25 (0.34)	-0.15 (0.53)
Postsecondary Enrollment	-0.64 (0.18)	-0.19 (0.13)	-0.24 (0.08)	0.10 (0.08)
District Demographics	X	X	X	X
Teacher Variables		X	X	X
Pooled OLS			X	X
District and Year FE				X

Notes: The table presents baseline results from the estimation of Equation 9. Bold coefficients are statistically significant at the 5% level. Each row reports estimates of β_1 for a particular outcome of interest and across four different specifications. Column (1) reports the results of a simple cross-sectional regression for the 2005-06 school year. This regression includes controls for the district's total enrollment, as well as for the share of minority and economically disadvantaged students. In Column (2), I add to the cross-sectional regression controls for the district's average teacher salary and local experience. Heteroskedasticity-robust standard errors are reported in parentheses in the first two columns. Column (3) pools all available years of data in my sample and controls for district demographics and teacher characteristics. Lastly, in Column (4) I add district and year fixed effects. The dependent variable in the last row is the (logged) number of high school completers in year t who enroll in a postsecondary education program in the state in the fall of year $t + 1$. This specification adds a control for the district's (logged) total number of high school completers in year t . Standard errors are clustered at the district level and are shown in parentheses in the last two columns. Data on district-level student outcomes, demographics, and teacher variables come from the WDPI. District-level data on total current expenditures were collected from the NCES. Fiscal variables were converted to 2010 dollars using the Midwest Region's CPI-U.

Appendix C Additional Figures and Tables

Figure C.1: Postsecondary Enrollment by Institution Level



Notes: The figure presents results from the estimation of Equation 2. It shows estimates of the dynamic treatment effects of referendum approval on district-level postsecondary enrollment by institution level. The dependent variable is the (logged) number of high school completers in year t who enroll in a postsecondary education program in the state during the fall of year $t + 1$. The solid line provides a visual representation of estimates of the β_t^{TOT} 's while the dashed line shows the corresponding 90% confidence intervals for up to ten 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 total number of high school completers in year t , the type of measure (recurring or nonrecurring), voter turnout, whether or not a bond referendum was concurrently placed on the ballot, and the number of referenda the school district placed on the ballot that school year. Data on district-level postsecondary enrollment (by institution level) come from the WDPI and the NSC. Two-year institutions include two-year technical colleges and training programs.

Table C.1: Effects on Student-Staff Ratios by Staff Category

Dependent Variable	Year Relative to the Election						\bar{Y}
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs	
Student-Total Staff Ratio	-0.19 (0.10)	-0.24 (0.11)	-0.19 (0.12)	-0.28 (0.13)	-0.31 (0.14)	-0.17 (0.15)	8.01 (1.44)
Student-Licensed Staff Ratio	-0.28 (0.13)	-0.28 (0.14)	-0.24 (0.17)	-0.38 (0.19)	-0.24 (0.17)	-0.23 (0.19)	12.35 (2.04)
Student-Support Staff Ratio	-1.15 (0.75)	-1.79 (0.79)	-1.63 (0.85)	-1.78 (0.89)	-6.09 (3.92)	-1.05 (1.20)	26.36 (8.52)
Student-Administrative Staff Ratio	11.72 (13.44)	2.67 (10.80)	0.01 (11.60)	7.23 (11.46)	8.41 (11.61)	1.56 (11.88)	230.22 (130.10)

Notes: The table presents results from the estimation of Equation 2 with each type of student-staff ratio as the dependent variable of interest. It shows estimates of the β_{τ}^{TOT} 's along with standard errors clustered at the district level in parentheses for up to six years after the election. To understand the magnitude of the estimates, the last column shows the sample mean of the dependent variable along with its standard deviation in parentheses. Bold coefficients are statistically significant at the 5% level. Data on district-level student-staff ratios (by staff category) come from the WDPI.