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

E. Jason Baron

Online Appendix

Appendix A Referendum Mailers

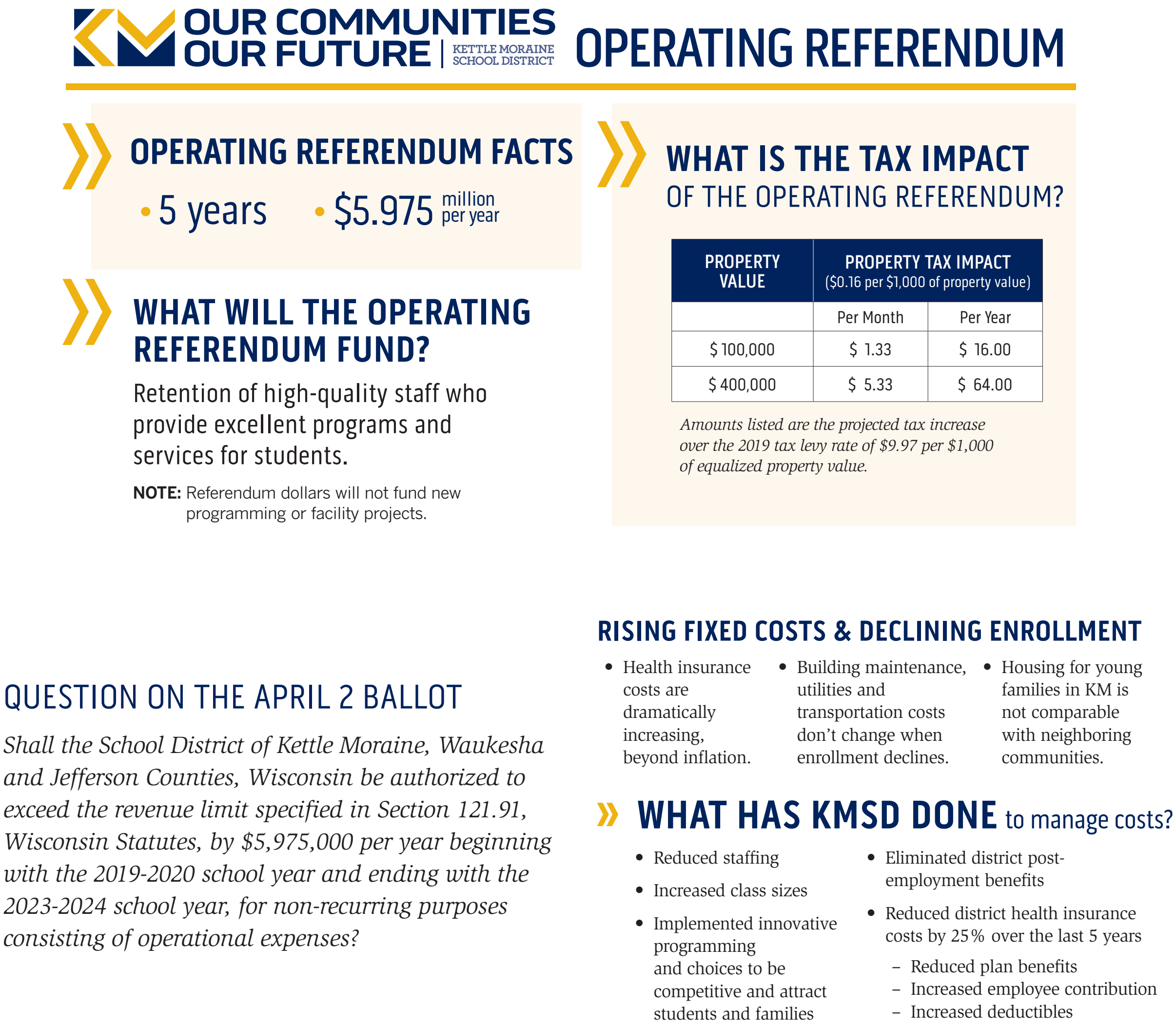
Figure A.1 shows an example of a mailer for an operational referendum. Mailers are sent to district residents with the purpose of reminding them to vote and providing them with more information about the upcoming referendum. While the figure provides the specific example of the Kettle Moraine School District, which attempted a nonrecurring referendum on April 2, 2019, the typical mailer closely resembles Kettle Moraine's.[51] 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.

Importantly, the mailer notes that, since this is an operational referendum, additional dollars will not fund facility projects. This is precisely the variation that allows me to isolate operational expenditures from school facility investments. 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.

[51] This referendum was narrowly defeated by a margin of 48% - 52%.

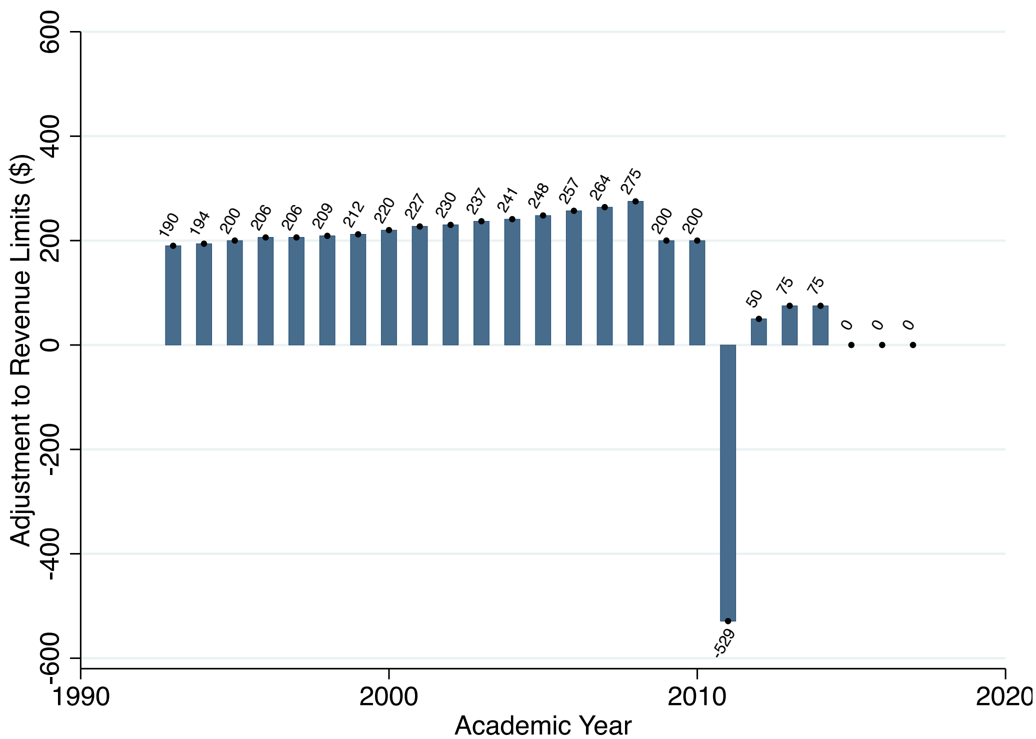
Figure A.1: Example of a Referendum Mailer



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. Source: https://www.kmsd.edu.

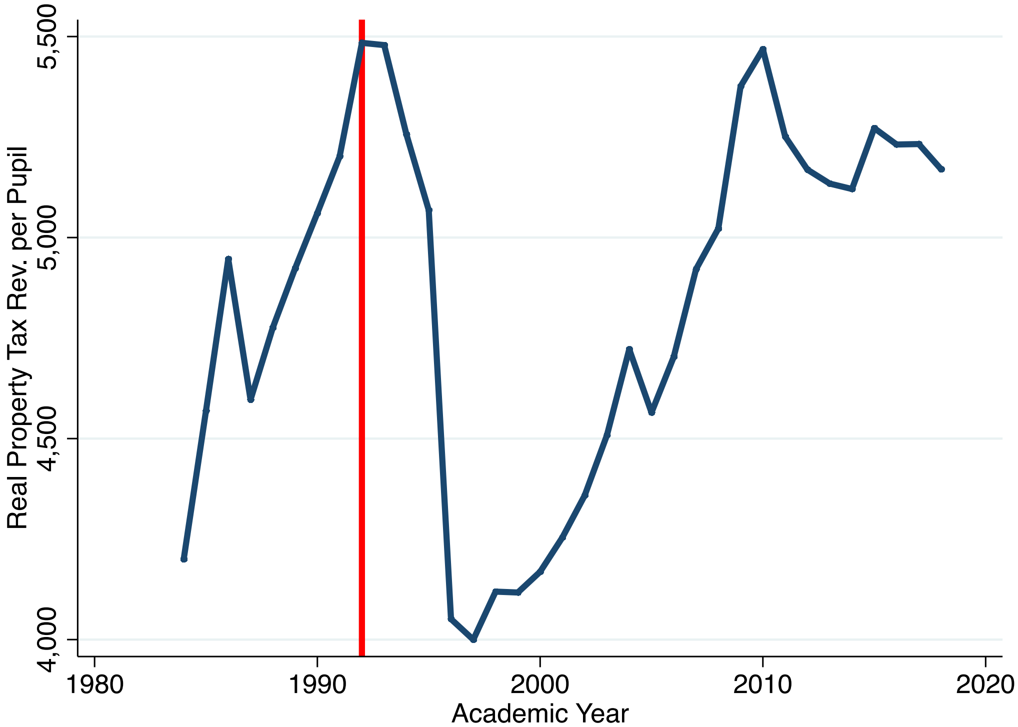
Appendix B   Additional Figures and Tables

Figure B.1: 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. Data on state-imposed revenue limit adjustments come from the Wisconsin Department of Public Instruction (WDPI).

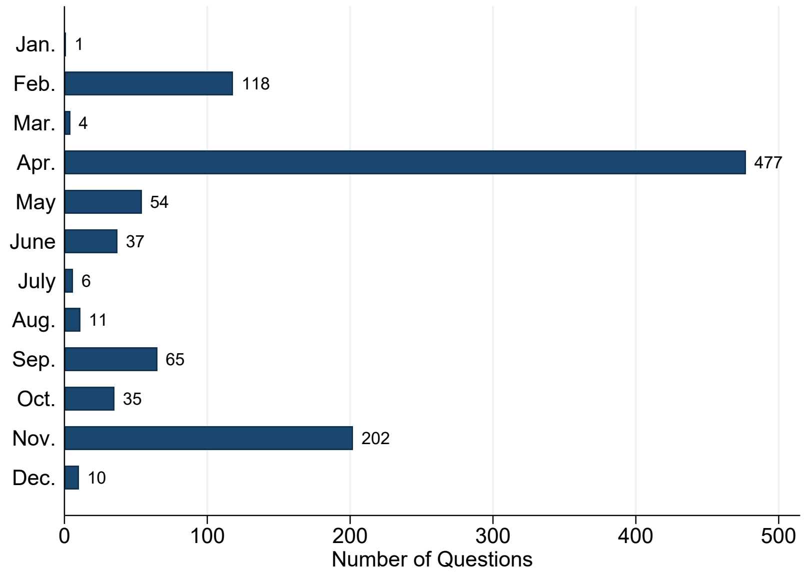
Figure B.2: Average Local Property Tax Revenue per Pupil



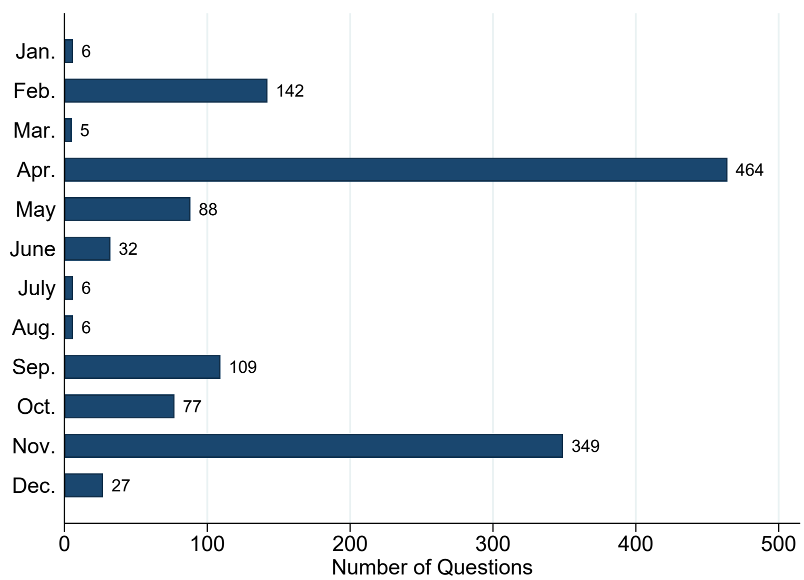
Notes: The figure plots the average local property tax revenue per pupil for Wisconsin public school districts (in 2010 dollars) before and after 1993-94, the year revenue limits were enacted. Nominal property tax revenues were converted to 2010 dollars using the Midwest Region's CPI-U. Data on Wisconsin public school districts' property tax revenue come from the Wisconsin Department of Public Instruction (WDPI).

Figure B.3: Distribution of Referenda by Month, 1996-2014

(a) Operational Referenda

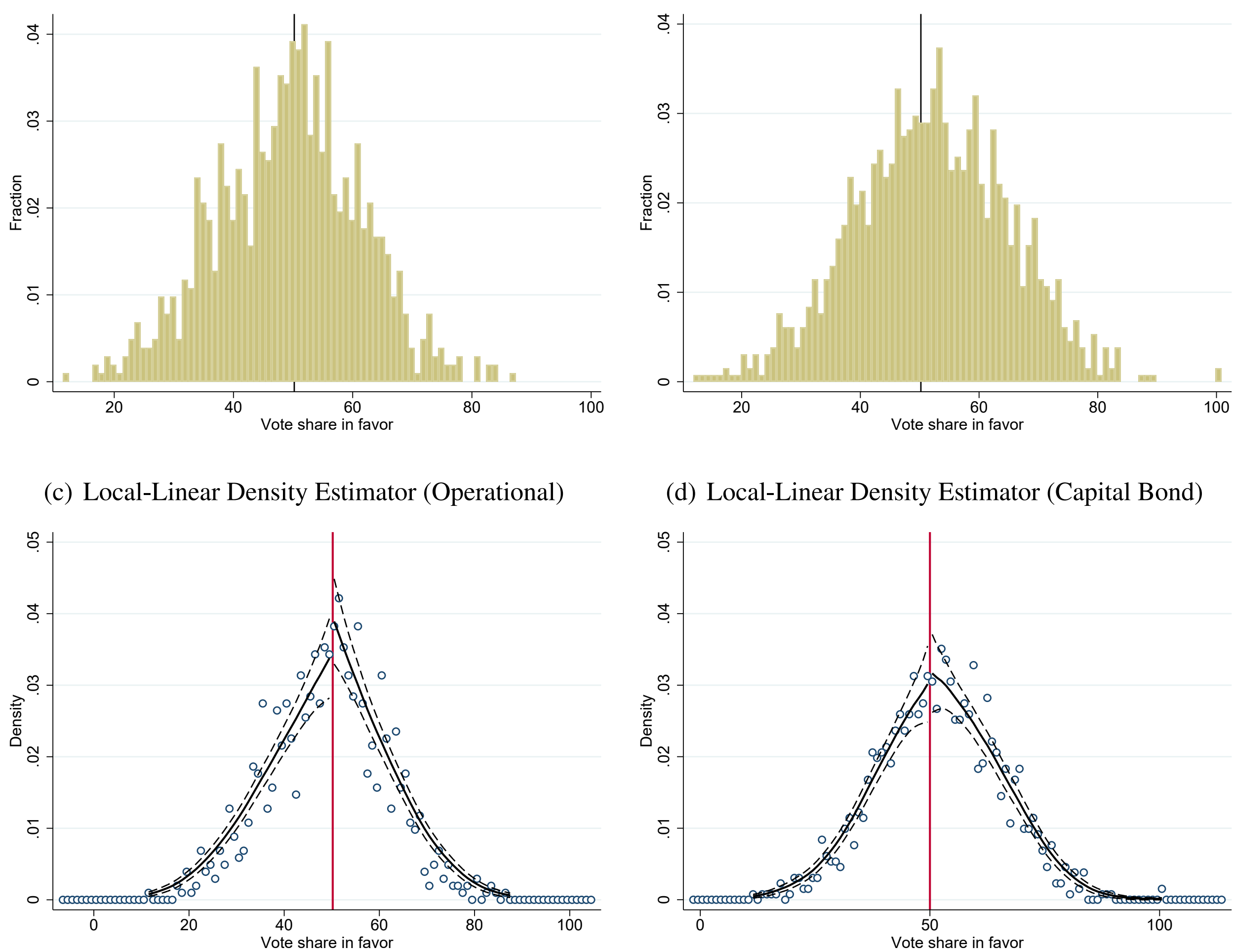


(b) Capital Bond Referenda



Notes: The figure shows the distribution of referenda by election month, separately for operational (Panel (a)) and capital bond referenda (Panel (b)). Referendum-level data come from the Wisconsin Department of Public Instruction (WDPI).

Figure B.4: Vote Share Manipulation Tests



(a) Histogram (Operational)

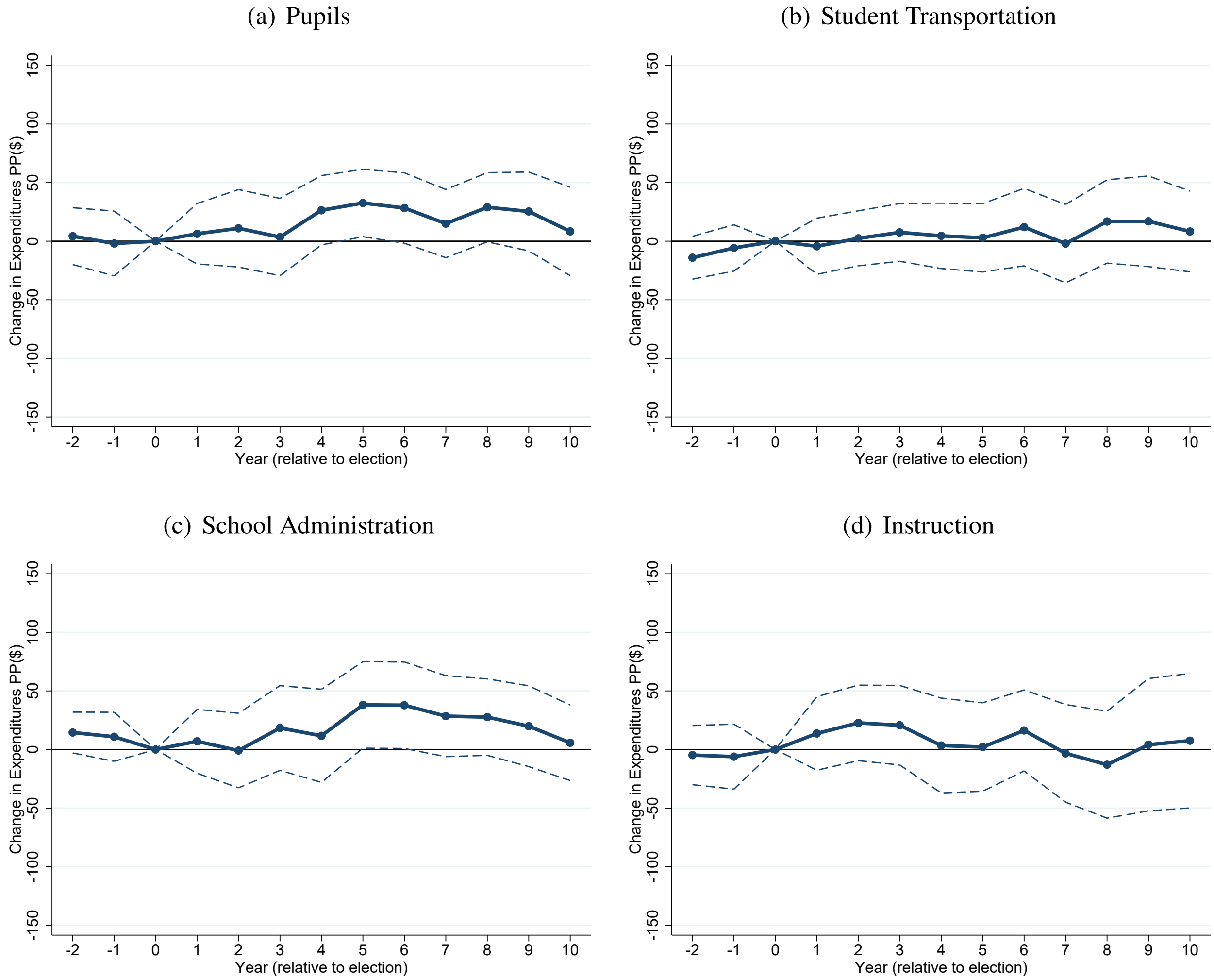
(b) Histogram (Capital Bond)

(c) Local-Linear Density Estimator (Operational)

(d) Local-Linear Density Estimator (Capital Bond)

Notes: Panels (a) and (b) show the distribution of referenda by vote share separately for operational and capital bond measures. In both panels, referenda are grouped into one percentage point bins. 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 one percentage point 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. Panels (c) and (d) show 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) separately for operational and capital bond measures.

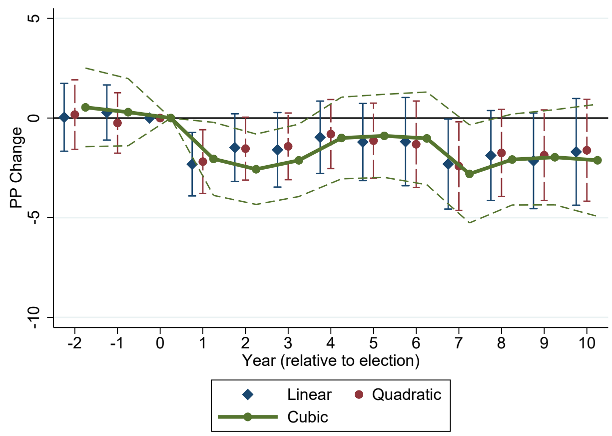
Figure B.5: Detailed Expenditures in Support Services



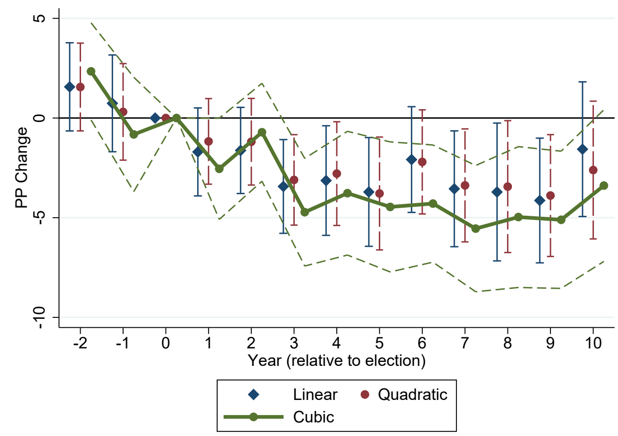
Notes: The figure presents results from the estimation of Equation 2. The solid line provides a visual representation of estimates of the β_τ^{TOT}'s while the dashed line shows the corresponding 90% confidence intervals for up to ten years after the election. Standard errors used in the construction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

Figure B.6: Additional Margins of Test Score Impacts

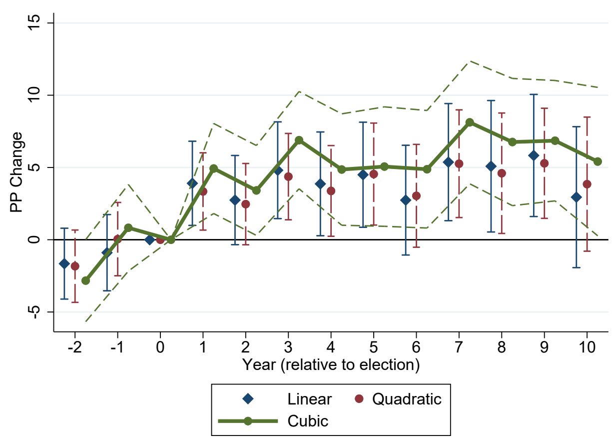
(a) % Minimal



(b) % Basic

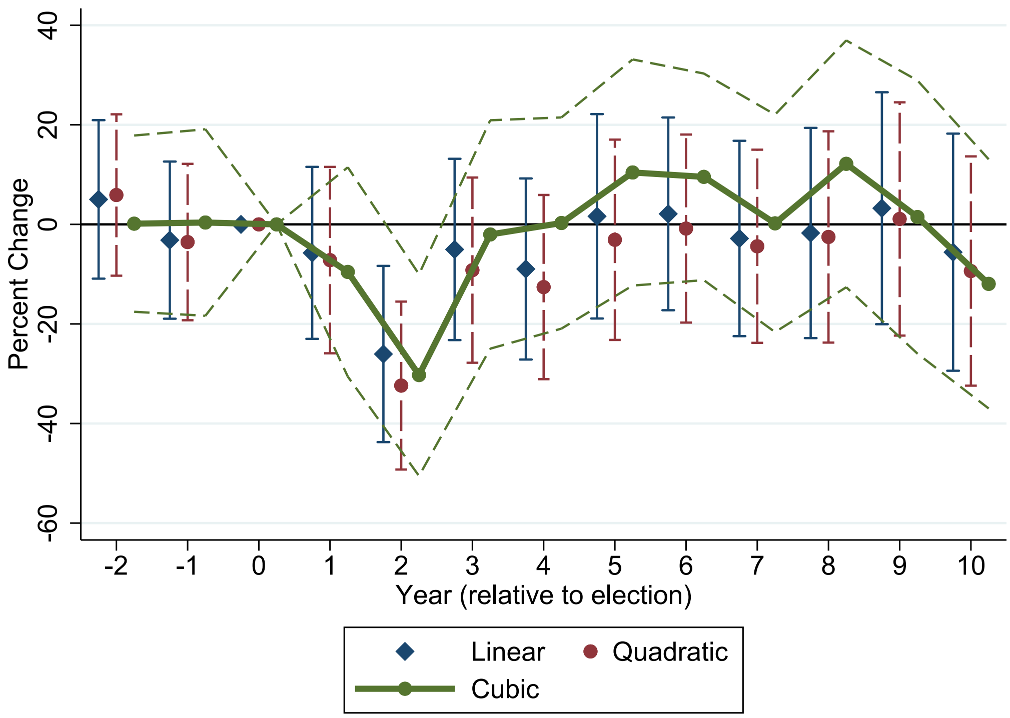


(c) % Adv. or Prof.



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

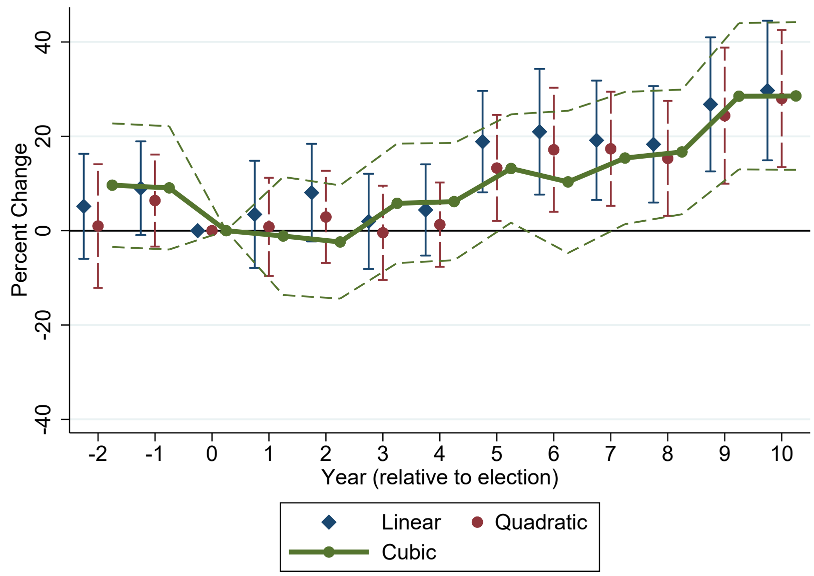
Figure B.7: Impact of an Operational Referendum Win on Out-of-State Postsecondary Enrollments



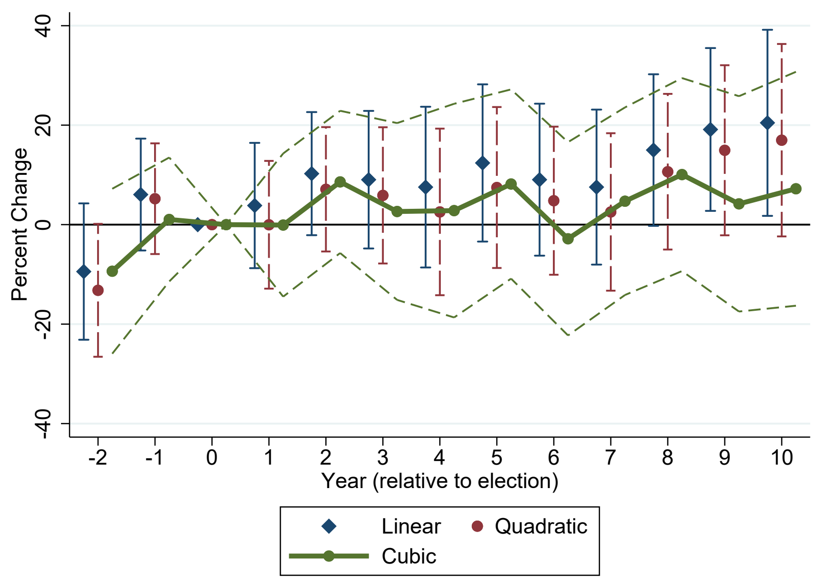
Notes: The figure presents results from the estimation of Equation 2 using a linear, quadratic, and cubic specification of the vote share. It shows estimates and 90% confidence intervals of the β_{τ}^{TOT}'s by year relative to the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held. The dependent variable in this specification is the (logged) number of high school completers in year t who enroll in either a four-year institution, a two-year technical school, or a training program outside the state in the fall of $t+1$. I control for the district's 9th grade enrollment in $t-3$ on the right-hand side of the equation.

Figure B.8: Impact of an Operational Referendum Win on Postsecondary Enrollments

(a) Four-Year Institutions Within Wisconsin



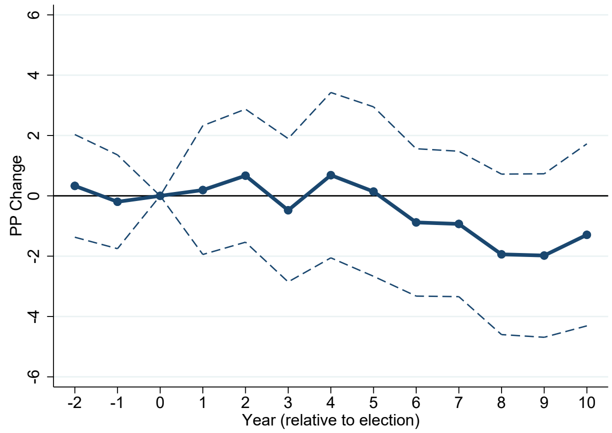
(b) Two-Year Technical Schools or Training Programs Within Wisconsin



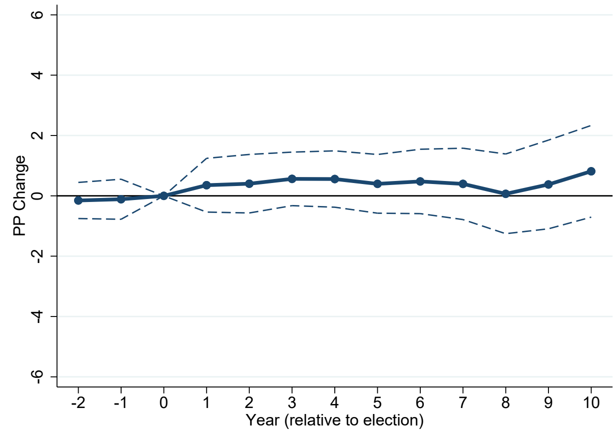
Notes: The figure presents results from the estimation of Equation 2 using a linear, quadratic, and cubic specification of the vote share. It shows estimates and 90% confidence intervals of the β_τ^{TOT}'s by year relative to the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held. The dependent variable in the specification in Panel (a) is the (logged) number of high school completers in year t who enroll in a four-year postsecondary institution within the state in the fall of $t+1$. The dependent variable in the specification in Panel (b) is the (logged) number of high school completers in year t who enroll in either a two-year technical school or a training program within the state in the fall of $t+1$. In both specifications, I control for the district's 9th grade enrollment in $t-3$ on the right-hand side of the equation.

Figure B.9: Changes in the District's Demographic Composition Following a Narrow Operational Referendum Win

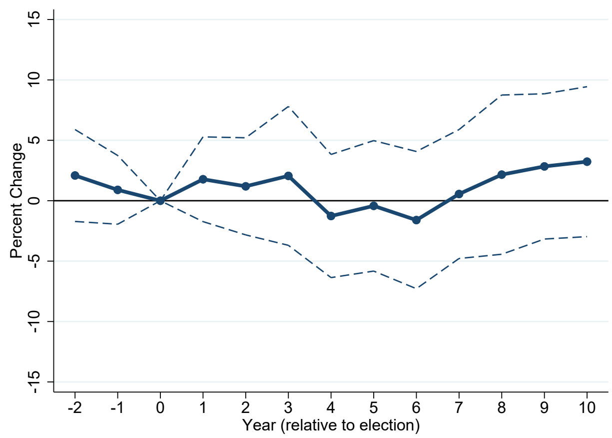
(a) Percent of Economically Disadvantaged Students



(b) Percent of Minority Students

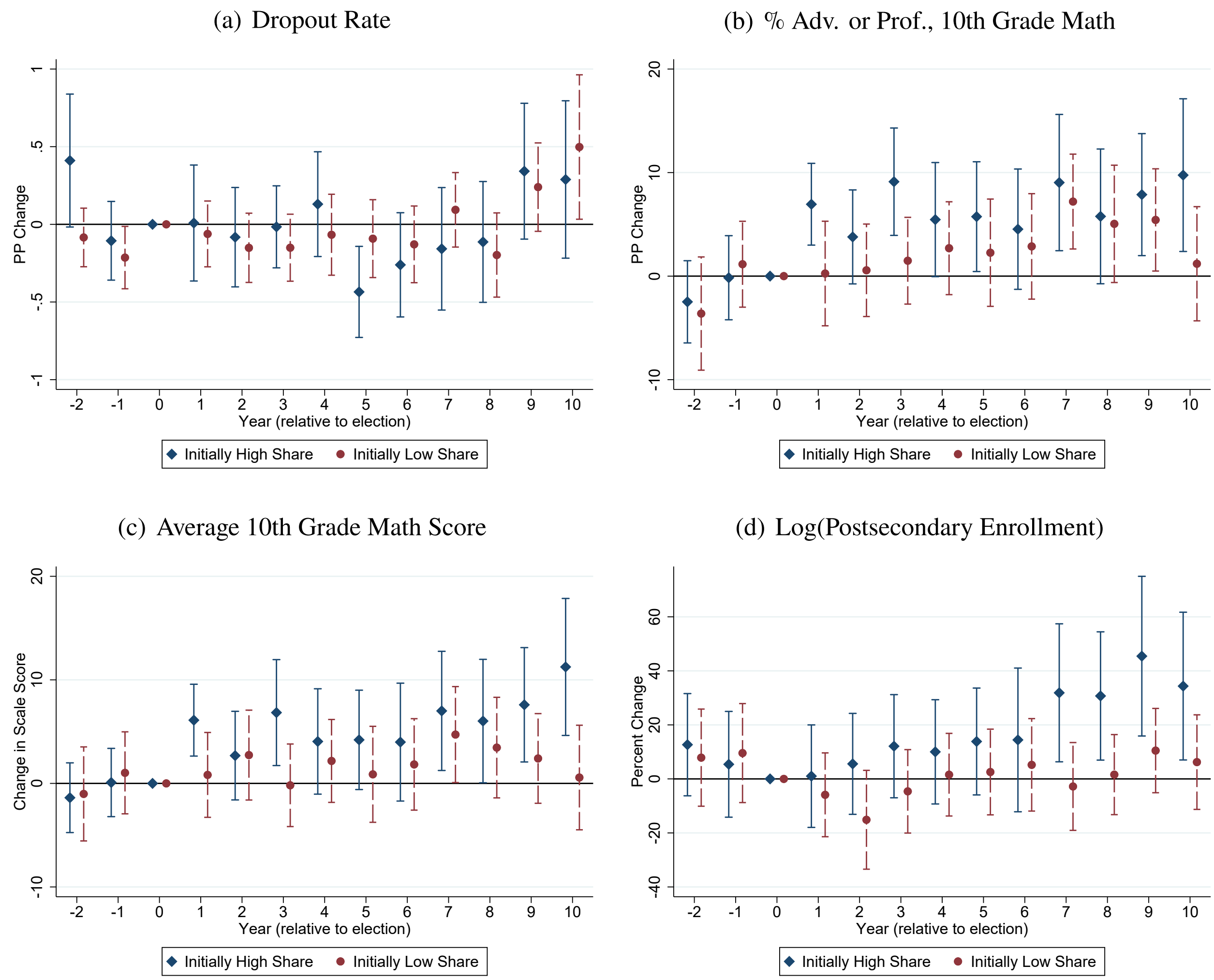


(c) Total Enrollment



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 operational measure (recurring or nonrecurring) and for the month in which the election was held.

Figure B.10: Heterogeneity of Operational Expenditure Effects by Initial Share of Economically Disadvantaged Students



(a) Dropout Rate

(b) % Adv. or Prof., 10th Grade Math

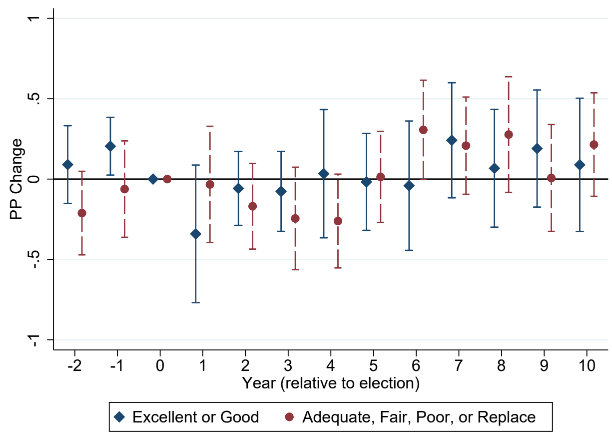
(c) Average 10th Grade Math Score

(d) Log(Postsecondary Enrollment)

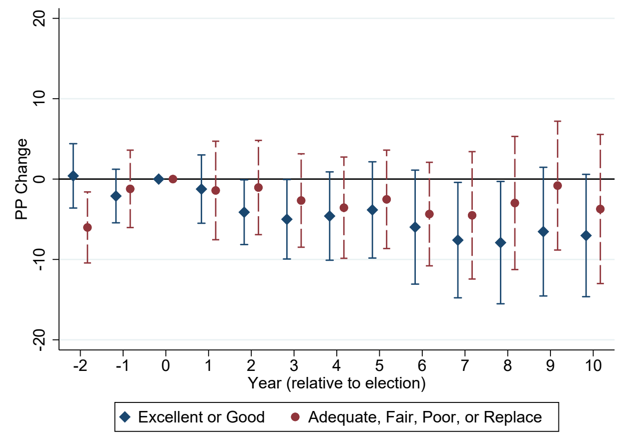
Notes: The figure explores heterogeneity in the effect of a successful operational referendum by a school district's initial share of economically disadvantaged students. I classify a school district as having an initially-high share of economically disadvantaged students if its share falls above the median of the Wisconsin 2000-01 school district distribution. The figure shows estimates of the β_{τ}^{TOT}'s and corresponding 90% confidence intervals separately for districts with an initially-high and an initially-low share of economically disadvantaged students. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held.

Figure B.11: Heterogeneity of Capital Expenditure Effects by Initial Condition of Infrastructure

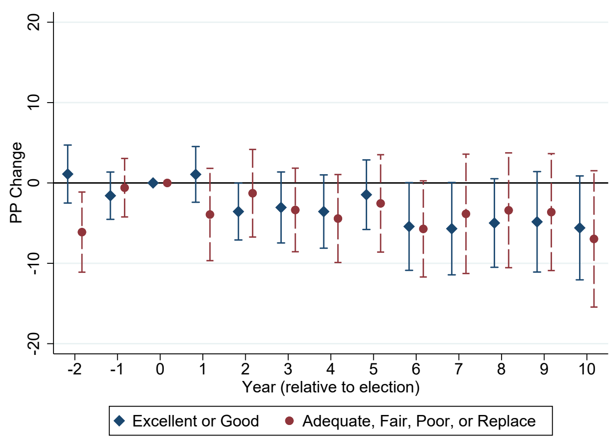
(a) Dropout Rate



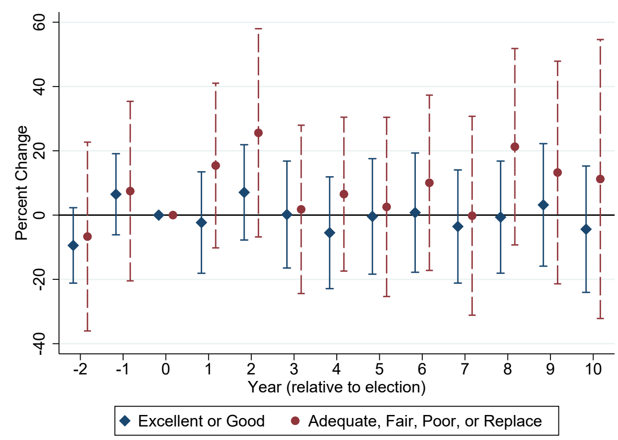
(b) % Adv. or Prof., 10th Grade Math



(c) Average 10th Grade Math Score

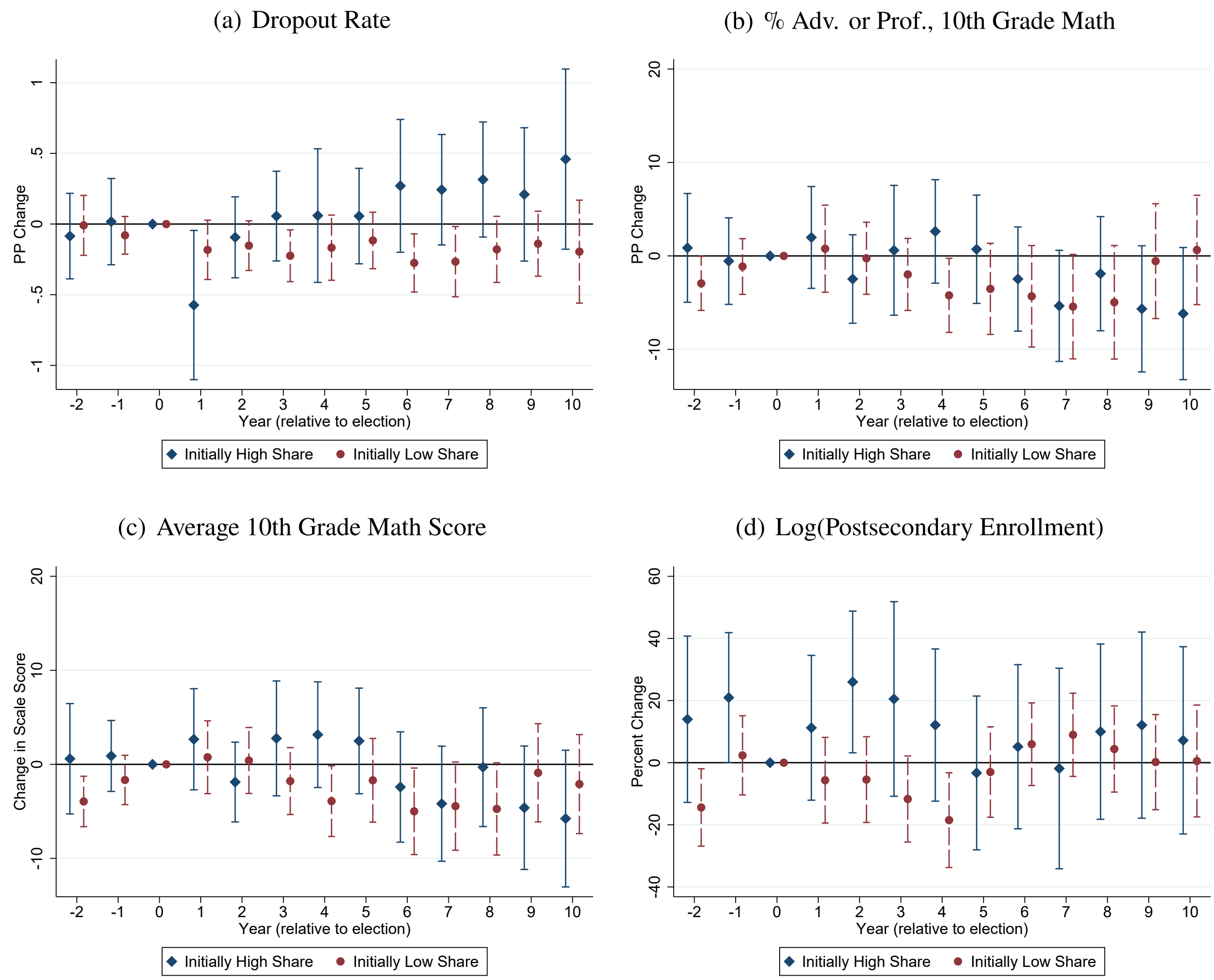


(d) Log(Postsecondary Enrollment)



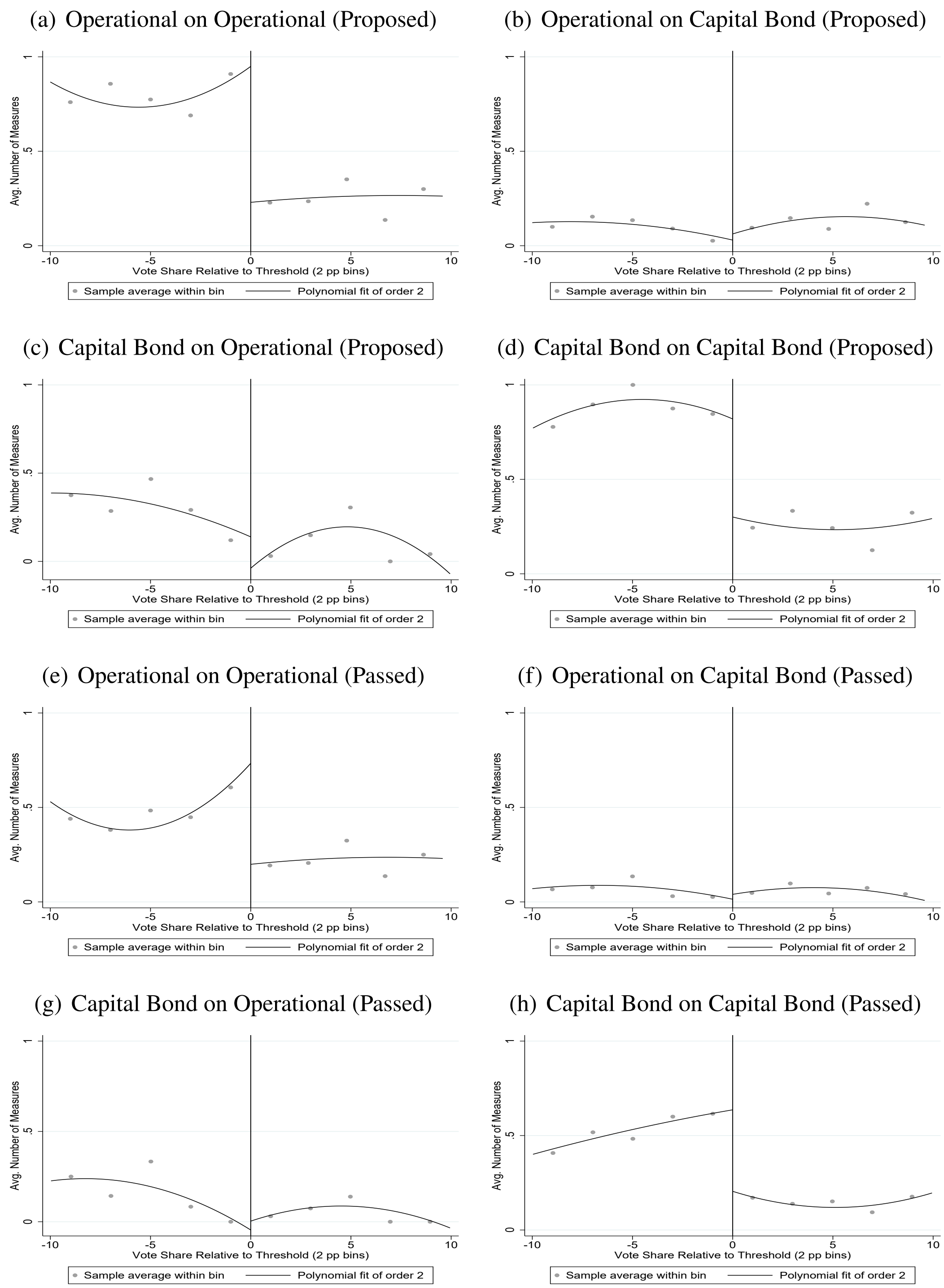
Notes: The figure explores heterogeneity in the effects of school facility investments by a school district's initial building condition. It shows estimates of the γ_τ^{TOT}'s and corresponding 90% confidence intervals separately for school districts with an initial building condition described as "excellent" or "good," and for school districts with an initial building condition described as "adequate," "fair," "poor," or "in need of replace" in Wisconsin's 1998 mandated public school facility survey. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held.

Figure B.12: Heterogeneity of Capital Expenditure Effects by Initial Share of Economically Disadvantaged Students



Notes: The figure explores heterogeneity in the effects of school facility investments by a school district's initial share of economically disadvantaged students. I classify a school district as having an initially-high share of economically disadvantaged students if its share falls above the median of the Wisconsin 2000-01 school district distribution. The figure shows estimates of the γ_{τ}^{TOT}'s and corresponding 90% confidence intervals separately for districts with an initially-high and an initially-low share of economically disadvantaged students. Standard errors used in the construction of the confidence intervals were clustered at the school district level. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held.

Figure B.13: Number of Additional Measures Proposed and Passed Within Two Years Following the Election



(a) Operational on Operational (Proposed)

(b) Operational on Capital Bond (Proposed)

(c) Capital Bond on Operational (Proposed)

(d) Capital Bond on Capital Bond (Proposed)

(e) Operational on Operational (Passed)

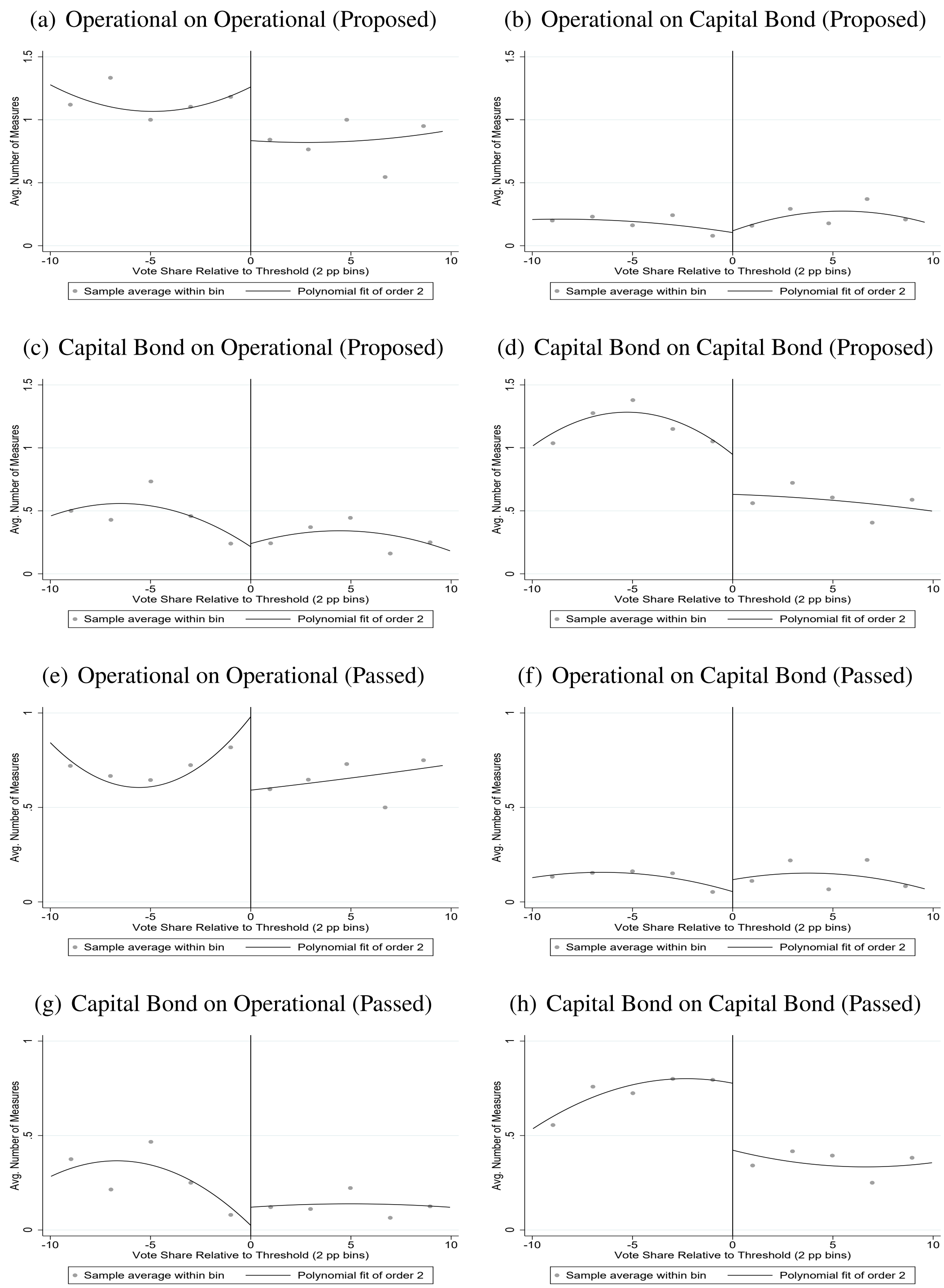
(f) Operational on Capital Bond (Passed)

(g) Capital Bond on Operational (Passed)

(h) Capital Bond on Capital Bond (Passed)

Notes: The figure shows binscatters of school districts' average number of measures passed or considered in the first two years after the focal election, along with a second-order polynomial fit, by the vote share in that focal election. Focal elections are grouped into two percentage point bins. For instance, measures that passed by a vote share in the (50,52] interval are assigned to bin 1, while those that failed by a similar margin are assigned to bin -1.

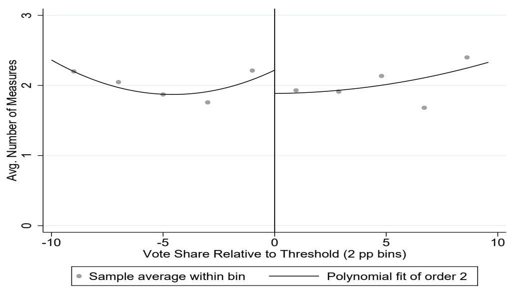
Figure B.14: Number of Additional Measures Proposed and Passed Within Four Years Following the Election



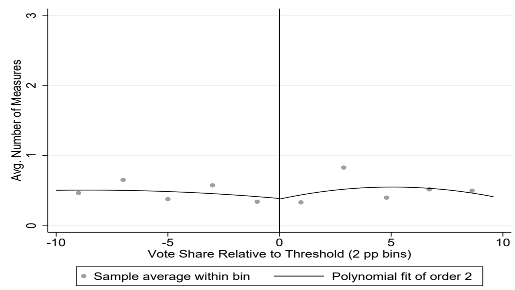
Notes: The figure shows binscatters of school districts' average number of measures passed or considered in the first four years after the focal election, along with a second-order polynomial fit, by the vote share in that focal election. Focal elections are grouped into two percentage point bins. For instance, measures that passed by a vote share in the (50,52] interval are assigned to bin 1, while those that failed by a similar margin are assigned to bin -1.

Figure B.15: Number of Additional Measures Proposed and Passed Within Ten Years Following the Election

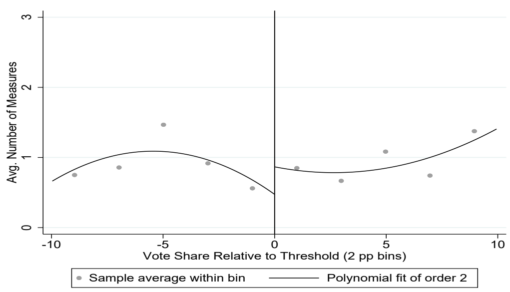
(a) Operational on Operational (Proposed)



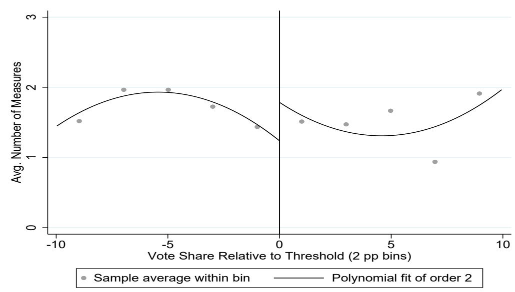
(b) Operational on Capital Bond (Proposed)



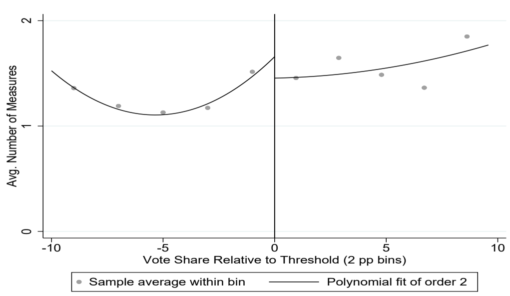
(c) Capital Bond on Operational (Proposed)



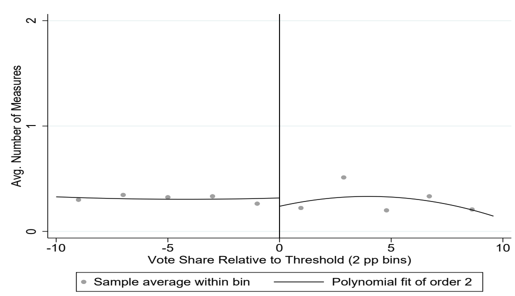
(d) Capital Bond on Capital Bond (Proposed)



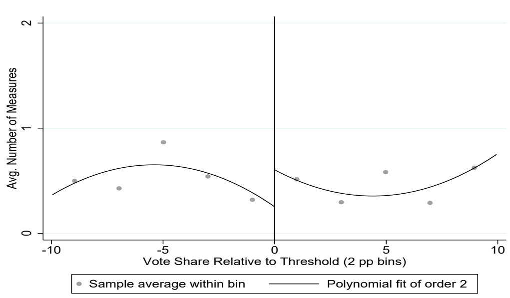
(e) Operational on Operational (Passed)



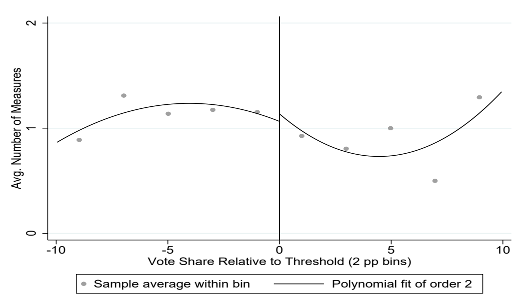
(f) Operational on Capital Bond (Passed)



(g) Capital Bond on Operational (Passed)



(h) Capital Bond on Capital Bond (Passed)



Notes: The figure shows binscatters of school districts' average number of measures passed or considered in the first ten years after the focal election, along with a second-order polynomial fit, by the vote share in that focal election. Focal elections are grouped into two percentage point bins. For instance, measures that passed by a vote share in the (50,52] interval are assigned to bin 1, while those that failed by a similar margin are assigned to bin -1.

Table B.1: The Effect of Narrow Operational Referendum Success on Test Scores

	Type of Specification		
Dependent Variable	**Cubic**	**Quadratic**	**Linear**
Panel (a): 10th Grade			
% Adv. or Prof., Math	5.89	3.91	4.40
	(1.80)	(1.67)	(2.00)
% Adv. or Prof., Reading	3.14	1.99	2.02
	(1.67)	(1.47)	(1.54)
Panel (b): 8th Grade			
% Adv. or Prof., Math	3.00	2.11	1.79
	(1.90)	(1.51)	(1.54)
% Adv. or Prof., Reading	-1.30	-0.98	-1.22
	(1.24)	(1.06)	(1.13)
Panel (c): 4th Grade			
% Adv. or Prof., Math	-0.70	-0.43	-0.47
	(1.97)	(1.60)	(1.60)
% Adv. or Prof., Reading	0.29	0.76	0.64
	(1.34)	(1.10)	(1.06)

Notes: The table presents results from the estimation of Equation 2 using a cubic, quadratic, and linear specification of the vote shares. It summarizes the average of the estimated β_{τ}^{TOT}'s across the first ten post-election years. The specification additionally controls for the type of operational measure (recurring or nonrecurring) and for the month in which the election was held.

Table B.2: Local-Linear Regressions of Successful Operational Referenda

	$t-2$	$[t, t+5]$
Panel (a): % Adv. or Prof., 10th Grade		
Conventional RD Estimate	-1.08	3.32
Conventional P-value	0.80	0.00
Bias-Corrected Estimate	-0.12	3.77
Conventional P-value	0.98	0.00
Robust P-value	0.98	0.00
CCT Bandwidth	[-6.90, 6.90]	[-8.54, 8.54]
Panel (b): % Adv. or Prof., 8th Grade		
Conventional RD Estimate	-2.59	3.24
Conventional P-value	0.48	0.01
Bias-Corrected Estimate	-2.89	3.70
Conventional P-value	0.43	0.00
Robust P-value	0.51	0.01
CCT Bandwidth	[-7.04, 7.04]	[-5.34, 5.34]
Panel (c): Dropout Rate		
Conventional RD Estimate	0.05	-0.23
Conventional P-value	0.77	0.00
Bias-Corrected Estimate	0.05	-0.26
Conventional P-value	0.77	0.00
Robust P-value	0.81	0.00
CCT Bandwidth	[-9.54, 9.54]	[-6.93, 6.93]
Panel (d): Postsec. Enrollment Share		
Conventional RD Estimate	-0.03	0.03
Conventional P-value	0.22	0.00
Bias-Corrected Estimate	-0.04	0.03
Conventional P-value	0.19	0.00
Robust P-value	0.27	0.00
CCT Bandwidth	[-11.41, 11.41]	[-5.51, 5.51]

Notes: The table shows the results of local-linear regressions of school districts' student outcomes before ($t-2$) and after the referendum ($[t, t+5]$) on the vote share in favor of the initiative and a passage indicator for each operational referendum in t. I implement the local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each table presents local-linear regression estimates without the bias-correction term removed; the second row reports the p-value corresponding to this conventional RD estimate and derived from a conventional variance estimator. The third row presents similar estimates with the bias-correction term removed; the fourth and fifth rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a robust variance estimator. The sixth row presents Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth. I use a triangular kernel function in each specification, which is standard in the literature.

Table B.3: Impacts of Operational Referenda on School Inputs

Dependent Variable	Post-Election Effect 5 Years	Post-Election Effect 10 Years	Dep. Variable Mean
Panel (a): Districtwide			
Student-Licensed Staff Ratio	-0.200	-0.141	12.41
	(0.119)	(0.144)	(1.97)
Local Teacher Experience	0.656	0.610	12.58
	(0.301)	(0.279)	(1.85)
Teacher Attrition	-0.182	-0.316	9.78
	(0.470)	(0.465)	(4.25)
Log(Teacher Compensation)	0.010	0.015	74,301
	(0.007)	(0.008)	(7,823)
Panel (b): High Schools			
Student-Teacher Ratio	-0.260	-0.004	14.74
	(0.213)	(0.225)	(3.27)
Log(Teacher Salaries)	-0.002	0.001	48,774
	(0.009)	(0.011)	(5,523)
Panel (c): Middle Schools			
Student-Teacher Ratio	0.212	0.460	15.89
	(0.251)	(0.271)	(4.98)
Log(Teacher Salaries)	0.028	0.037	49,630
	(0.013)	(0.012)	(5,240)
Panel (d): Elementary Schools			
Student-Teacher Ratio	0.041	0.172	13.64
	(0.194)	(0.195)	(2.33)
Log(Teacher Salaries)	0.007	0.010	48,271
	(0.008)	(0.009)	(4,749)

Notes: The table presents results from the estimation of Equation 2 using a cubic specification of the vote shares. The first column summarizes the average of the estimated β_{τ}^{TOT}'s across the first five post-election years, while the second column summarizes effects across the first ten post-election years. Standard errors clustered at the school district level are shown in parentheses. The third column shows the sample mean of the dependent variable, along with its standard deviation in parentheses. I show the sample mean of teacher compensation and salaries in levels, rather than in logs, in order to help the reader better understand the magnitude of the effect. The first panel shows effects for district-level variables, while Panels (b), (c), and (d) show effects separately for the district's high schools, middle schools, and elementary schools, respectively. The school-level student-teacher ratio and teacher salary variables were computed using an individual-level teacher dataset, while the variables in Panel (a) are publicly reported by the WDPI. Due to data constraints in the individual-level dataset, variables measuring class sizes and teacher compensation are slightly different in the school- and district-level measures. District-level measures of class sizes are student-licensed staff ratios, while school-level measures are student-teacher ratios instead. Similarly, district-level teacher compensation figures include salaries and fringe benefits, while school-level measures include only salaries. Also due to data constraints, I was unable to calculate school-level measures of teacher attrition and local teacher experience.

Table B.4: Impacts of Capital Bond Referenda on School Inputs

Dependent Variable	Post-Election Effect 5 Years	Post-Election Effect 10 Years	Dep. Variable Mean
Student-Licensed Staff Ratio	-0.027	0.091	12.41
	(0.134)	(0.151)	(1.97)
Local Teacher Experience	-0.211	-0.307	12.58
	(0.299)	(0.341)	(1.85)
Teacher Attrition	-0.493	-0.684	9.78
	(0.455)	(0.489)	(4.25)
Log(Teacher Compensation)	0.000	-0.002	74,301
	(0.008)	(0.009)	(7,823)

Notes: The table presents results from the estimation of Equation 2 using a cubic specification of the vote shares. The first column summarizes the average of the estimated γ_{τ}^{TOT}'s across the first five post-election years, while the second column summarizes effects across the first ten post-election years. Standard errors clustered at the school district level are shown in parentheses. The third column shows the sample mean of the dependent variable, along with its standard deviation in parentheses. I show the sample mean of teacher compensation and salaries in levels, rather than in logs, in order to help the reader better understand the magnitude of the effect.

Table B.5: Statistical Tests of the Null that Operational and Capital Expenditure Effects Are Equal

Dependent Variable	P-Value
Panel (a): Linear Specification	
Dropout Rate	0.589
% Adv. or Prof., 10th Grade Math	0.035
Avg. 10th Grade Math Score	0.052
Log(Postsecondary Enrollment)	0.015
Panel (b): Quadratic Specification	
Dropout Rate	0.877
% Adv. or Prof., 10th Grade Math	0.062
Avg. 10th Grade Math Score	0.066
Log(Postsecondary Enrollment)	0.035
Panel (c): Cubic Specification	
Dropout Rate	0.595
% Adv. or Prof., 10th Grade Math	0.005
Avg. 10th Grade Math Score	0.024
Log(Postsecondary Enrollment)	0.220

Notes: The table presents p-values from statistical tests of the null hypothesis that the average of the estimated β_{τ}^{TOT}'s across the first ten post-election years is equal to the average of the estimated γ_{τ}^{TOT}'s from Equation 2.

Table B.6: Differences Between School Districts That Proposed Each Type of Referendum

Dependent Variable (*t-1*)	Proposed Op. Referendum (*t*)	Proposed Bond Referendum (*t*)	Diff (1)-(2)
Panel (a): Student Outcomes			
Dropout Rate (%)	1.17	1.06	0.11
	(1.23)	(1.09)	(0.15)
% Adv. or Prof., 10th Grade	45.81	48.49	-2.68
	(12.90)	(12.47)	(1.78)
Avg. Scale Score, 10th Grade	565.95	569.11	-3.16
	(13.93)	(12.82)	(2.12)
Postsecondary Enrollment Share	0.41	0.41	-0.01
	(0.11)	(0.10)	(0.01)
Panel (b): District Characteristics			
Student-Licensed Staff Ratio	12.92	13.14	-0.22
	(4.09)	(3.65)	(0.09)
Teacher Experience	12.96	12.58	0.38
	(1.98)	(1.80)	(0.09)
Teacher Compensation	74,417	74,335	82
	(7,531)	(7,603)	(491)
Teacher Attrition (%)	9.40	9.64	-0.23
	(3.40)	(4.55)	(0.18)
Property Values PP	523,898	495,559	28,339
	(447,124)	(287,843)	(20,851)
Urban Centric Locale	2.41	2.29	0.13
	(1.13)	(1.04)	(0.12)
Fall Enrollment	2,002	2,484	-482
	(3,358)	(3,506)	(180)
Number of School Districts	314	376	404

Notes: The table shows differences in observables between school districts that proposed an operational referendum and those that proposed a capital bond referendum. Columns (1) and (2) show the means and standard deviations (in parentheses) of district-level outcomes in $t-1$ separately for districts that proposed an operational referendum in t and for those that proposed a capital bond referendum in t—regardless of whether the referendum passed or not. Column (3) reports the point estimates and standard errors clustered at the district level of tests for equality of means. Panel (a) shows student outcomes, while Panel (b) presents variables measuring district characteristics. 314 (376) unique school districts proposed an operational (capital bond) referendum at some point from 1996 to 2014. 404 school districts proposed either an operational or a capital bond referendum at some point during this sample period.

Appendix C 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 of the RD estimator used to identify the ITT effects. While this estimator has several limitations relative to the estimator used to identify TOT effects, ITT effects are estimable using standard RD approaches. Since the estimator used in Equation 2 embeds a variety of additional assumptions related to the separability of effects over time as well as the homogeneity of effects across the distribution of vote shares, it is important to understand whether the main results of the paper are driven by the additional structure imposed by the dynamic RD framework. I conclude by presenting results from ITT estimates. ITT effects are strikingly similar to estimates of the TOT effects presented in the main body of the paper: they both indicate that increases in operational spending have substantial positive effects on test scores, dropout rates, and postsecondary enrollment, but that additional capital expenditures have little impacts.

C.1 RD in a Cross-Sectional Framework

Suppose that school district d holds an operational and a capital bond referendum. The operational referendum receives vote share v_d^o while the capital bond referendum receives vote share v_d^b. Let $P_d^o = 1(v_d^o > 50)$ and $P_d^b = 1(v_d^b > 50)$ be indicators for the passage of an operational and a capital bond referendum, respectively. We can write some district-level outcome y_d (e.g., revenue limits, expenditures, or test scores) as:

$$y_d = \alpha + P_d^o \beta + P_d^b \gamma + \epsilon_d \tag{C.1}$$

where β is the causal effect of operational referendum passage on y_d (holding constant whether the district also passes a capital bond referendum); γ is the causal effect of capital bond referendum passage on y_d (holding constant whether the district also passes an operational referendum); and ϵ_d represents all additional determinants of y_d, with $E[\epsilon_d] = 0$.

In general, we would expect that both $E[\epsilon_d P_d^o] \neq 0$ and $E[\epsilon_d P_d^b] \neq 0$. For instance, districts where an operational referendum passes are likely to differ from school districts where the operational referendum is defeated along both observable and unobservable characteristics. Relative to residents in districts in which an operational 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 C.1 is likely to yield a biased estimate of both β and γ. However, provided there is no manipulation of the vote share near the 50% threshold, the correlation between P_d^o and ϵ_d, and between P_d^b and ϵ_d, can be kept close to zero by focusing only on close operational and capital bond referenda. To estimate the causal impact of additional school spending induced by each type of referendum, one can use an RD design that compares outcomes in school districts that narrowly pass the particular referendum (the "treatment group") to those where the same type of initiative is narrowly defeated (the "control group").

As in previous papers that have implemented the dynamic RD strategy (e.g., Hong and Zimmer (2016), Martorell, Stange and McFarlin Jr (2016), and Cellini, Ferreira and Rothstein (2010)), I use 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, Ferreira and Rothstein (2010) show, the following regression of district outcomes on referenda passage, controlling for flexible polynomials of degree g in v_d^o and v_d^b, will provide consistent estimates of β and γ:

$$y_d = \alpha + P_d^o\beta + f_g(v_d^o) + P_d^b\gamma + f_g(v_d^b) + \varepsilon_d \tag{C.2}$$

C.2 RD with Panel Data and Multiple Treatments

The cross-sectional framework can be extended to allow for multiple referenda of each type in the same school district throughout the sample period. I redefine P_{dt}^o to be equal to one if district d passes an operational referendum in school year t and zero otherwise (either if there was no operational referendum held in year t or if a proposed referendum was rejected). Similarly, I define P_{dt}^b to be equal to one if district d passes a capital bond referendum in school year t and zero otherwise. Assuming that the partial effect of each type 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 of each type:

$$y_{dt} = \sum_{\tau=0}^{\bar{\tau}} [P_{d,t-\tau}^o\beta_\tau + P_{d,t-\tau}^b\gamma_\tau] + \epsilon_{dt} \tag{C.3}$$

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 C.3, these effects are captured by β_τ and γ_τ, since the equation holds constant all other referendum wins. These effects are known as the "treatment on the treated" (TOT)—β_τ^{TOT} and γ_τ^{TOT}. Therefore, a consistent estimate of β_τ^{TOT} will isolate the impact of an operational referendum passage (with no intermediate referendum-approved changes to the district's resources) in $t-\tau$ on a district's outcome in t. Similarly, a consistent estimate of γ_τ^{TOT} will isolate the impact of a successful capital bond referendum in 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 and the γ_τ^{TOT}'s.

An alternative to examining TOT effects is to focus on the impact of passing an operational or a capital bond referendum in $t-\tau$ and "allowing" the school district to make decisions regarding subsequent referenda as its residents wish. This effect, known as the "intent-to-treat" (ITT), incorporates effects of $P^o_{d,t-\tau}$ and $P^b_{d,t-\tau}$ on y_{dt} operating through additional operational and capital bond referendum wins in intermediate years $\{P^o_{d,t-\tau+1}, P^o_{d,t-\tau+2}, \ldots, P^o_{dt}\}$ and $\{P^b_{d,t-\tau+1}, P^b_{d,t-\tau+2}, \ldots, P^b_{dt}\}$. Thus, ITT estimates do not necessarily reflect the impact of additional expenditures solely associated with the passage of a particular referendum.

C.3 Estimating ITT Effects

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 an operational and a capital bond referendum in school year t. One can write the district's outcome τ years later as:

$$y_{d,t+\tau} = P^o_{dt}\beta_\tau^{ITT} + f_g(v^o_{dt}) + P^b_{dt}\gamma_\tau^{ITT} + f_g(v^b_{dt}) + \varepsilon_{d,t+\tau} \tag{C.4}$$

While Equation C.4 ensures that $\varepsilon_{d,t+\tau}$ is uncorrelated with P^o_{dt} and P^b_{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 and the γ_τ^{ITT}'s I follow Cellini, Ferreira and Rothstein (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) from 1996-97 through 2014-15. I then map these elections to outcomes in district d in years $t-2$ through $t+5$. 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 $[t-2, t+5]$ windows are $[1999, 2006]$ and $[2001, 2008]$, and the 2001-2006 observations are included in each. Observations in the final stacked proposal-level panel are thus uniquely identified by the district d, the school year of the specific referendum t, the type of referendum (operational or capital bond), 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:

$$y_{d,t+\tau} = P^o_{dt}\beta^{ITT}_\tau + f_g(v^o_{dt}) + P^b_{dt}\gamma^{ITT}_\tau + f_g(v^b_{dt}) + \mu_{dt} + \theta_t + \lambda_\tau + \varepsilon_{d,t+\tau} \quad \text{(C.5)}$$

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, Ferreira and Rothstein (2010) both β^{ITT}_τ and γ^{ITT}_τ, as well as the coefficients on the polynomials, are allowed to vary flexibly for $\tau > 0$ but are constrained to be zero for $\tau \leq 0$.[52] Standard errors are clustered at the district level to account for the serial correlation induced by multiple proposals in some school districts, and for within-district correlation over time.

C.4 ITT Effects of Successful Operational Referenda

Table C.1 presents ITT effects of operational referendum passage on district-level fiscal and student outcomes. It shows estimates of the β^{ITT}_τ's from Equation C.5, specifying $f_g(v^o_{dt})$ and $f_g(v^b_{dt})$ as third-order polynomials, along with standard errors for up to five years after the election.

ITT estimates are remarkably similar to those shown in the main body of the paper and tell a similar story. The estimates indicate that narrowly approving an operational referendum increases revenue limits per pupil by roughly \$300 in the year following the election. This effect is relatively constant and is only statistically significant at the 5% level for the first three years after the election. Increases in revenue limits translate into similar increases in spending. Narrowly approving an operational referendum leads to an increase in operational expenditures per pupil of roughly \$250 in the year after the election.

Increases in spending translate into substantial improvements in student outcomes. Referendum approval in a narrow election leads to a sharp increase in the percent 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 students who subsequently enroll in a postsecondary education program

[52]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.

within the state. Five years after the election, treated school districts have a relative decline in the dropout rate of roughly 0.20 percentage points, and a relative increase in postsecondary enrollment of roughly 11%. The robustness of the main results to the choice of estimator provides strong evidence that additional operational spending is associated with large improvements in student outcomes.

An advantage of estimating ITT effects is that I am able to use the more common RD techniques associated with cross-sectional RD designs. Because the panel used to estimate ITT effects is at the proposal level and each observation is uniquely identified by the district d, the school year of the specific referendum t, the type of referendum (operational or capital bond), and the year relative to the election, I am able to analyze the data one year at a time in a cross-sectional framework. For instance, I can implement a standard cross-sectional RD design where I focus only on operational elections and I set the outcome in a local-linear regression to be the district's expenditures or student test scores four years after the election.

Thus, as a comparison to the panel approach that employs proposal and year fixed effects, I next estimate the effect of operational referendum passage on fiscal and student outcomes using a more common cross-sectional RD design. While these estimates will inherently capture both the direct and indirect effects of a successful operational referendum since intermediate elections are not held constant, this approach allows me to (1) ensure that the main results of the paper are not driven by the panel structure of the dynamic RD design, (2) present standard RD plots for key outcome variables, and (3) implement more common non-parametric, local-linear regressions.

Figures C.1, C.2, and C.3 present typical RD plots for all operational referendum attempts from 1996 to 2014. The figures show binscatters of school districts' fiscal and student outcomes against the running variable (re-centered vote share) along with a second-order polynomial fit. Panels (a) and (c) of Figure C.1 compare average revenue limits and total expenditures per pupil in $t-2$ between districts in which the focal operational referendum eventually passed in t and those in which it eventually failed. Both panels show little evidence of a discontinuity near the cutoff two years before the election. However, Panels (b) and (d) show clear evidence that districts that narrowly passed an operational referendum in t spent roughly \$500-\$600 more per pupil in $t+1$, relative to districts in which the referendum was narrowly defeated.

Figure C.2 tells a similar story for student test scores. Panels (a), (c), and (e) compare the percent of students who score in the advanced or proficient levels on the math portion of the WKCE in 10th, 8th, and 4th grade in $t-2$ between districts in which the focal operational referendum eventually passed in t and those in which it eventually failed. All three of these panels show little evidence of a discontinuity at the 50% threshold two years before the election. However, Panels (b), (d), and (f) provide evidence that, relative to districts in which the referendum was narrowly defeated, districts that barely passed an operational referendum in t had a significantly larger percent of students who score in the advanced or proficient levels by $t+4$. Lastly, Figure C.3 presents RD plots for the district's dropout rate and for the percent of district students who subsequently enroll in a postsecondary education program within the state. Similar to Figures C.1 and C.2, the figure shows little evidence of a discontinuity near the threshold in either of these outcomes in $t-2$, but reveals large improvements in these outcomes in the years following the election.

To more formally quantify the magnitude of these effects, I implement local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). Tables C.2, C.3, and C.4 show the results of local-linear regressions of school districts' outcomes before ($t-2$) and after the referendum ($[t, t+5]$) on the vote share in favor of the initiative and a passage indicator for each operational referendum in t.

The first row of each table presents local-linear regression estimates without the bias-correction term removed; the second row reports the p-value corresponding to this conventional RD estimate and derived from a conventional variance estimator. The third row presents similar estimates with the bias-correction term removed; the fourth and fifth rows report two p-values corresponding to the bias-corrected RD estimate: one derived from a conventional variance estimator and one derived from a robust variance estimator. The sixth row presents Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth. Finally, the last row of each panel presents the kernel function used to construct the local polynomial estimator. I use a triangular kernel function in each specification, which is standard in the literature.

Table C.2 shows local-linear regressions of revenue limits and total expenditures per pupil. The estimates show little evidence of differences between eventual narrow winners and losers along both of these outcomes in $t-2$. However, there is clear evidence that in the five years following the operational referendum, narrow winners have higher revenue limits and spend roughly \$500 more per year relative to narrow losers. Tables C.3 and C.4 show similar patterns for the district's test scores, dropout rate, and postsecondary enrollment. The first column in each table shows little evidence that academic outcomes differed in districts that eventually passed and lost a close operational referendum. However, the estimates in the second column show clear evidence that academic outcomes improved substantially in the years following the election in districts that narrowly approved a referendum. On average, in the five years after the election, these districts experienced higher shares of students scoring advanced or proficient in 10th, 8th, and 4th grade math test scores, a lower dropout rate, and a larger share of students who subsequently enrolled in a postsecondary education program within the state.

C.5 ITT Effects of Successful Capital Bond Referenda

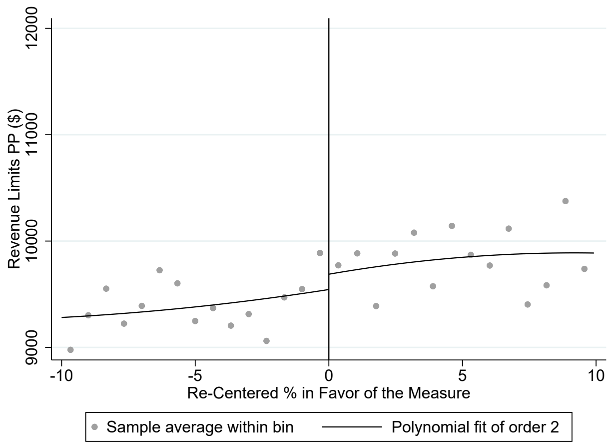
Table C.5 presents estimates of the ITT effects of capital bond referendum passage on district-level fiscal and student outcomes. It shows estimates of the γ_{τ}^{ITT}'s from the estimation of Equation C.5, specifying $f_g(v_{dt}^{o})$ and $f_g(v_{dt}^{b})$ as third-order polynomials, along with standard errors clustered at the school district level in parentheses.

ITT estimates are remarkably similar to the TOT estimates shown in the main body of the paper and tell a similar story. ITT estimates indicate that narrowly approving a capital bond referendum results in large and immediate increases in outstanding long-term debt. Narrowly passing a capital bond referendum also results in sharp increases in capital spending that are concentrated in the first two years after the election.

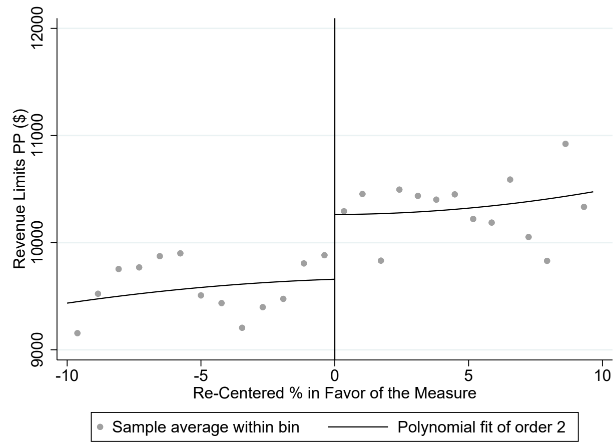
The last five rows of the table examine the impact of narrowly passing a capital bond referendum on the three academic outcomes examined throughout the study: test scores, the dropout rate, and postsecondary enrollment. ITT estimates of the impact of capital bond passage on student outcomes are close to zero and mostly statistically significant. The robustness of the main results of the paper to the choice of estimator reinforces the finding that, while additional operational expenditures are associated with large improvements in student outcomes, there is little evidence that additional capital expenditures have persistent meaningful effects.

Figure C.1: RD Plots for Fiscal Outcomes (Operational Referenda)

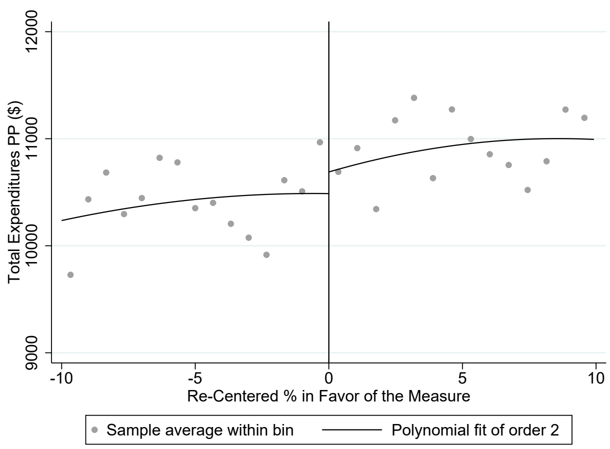
(a) Revenue Limits PP ($t-2$)



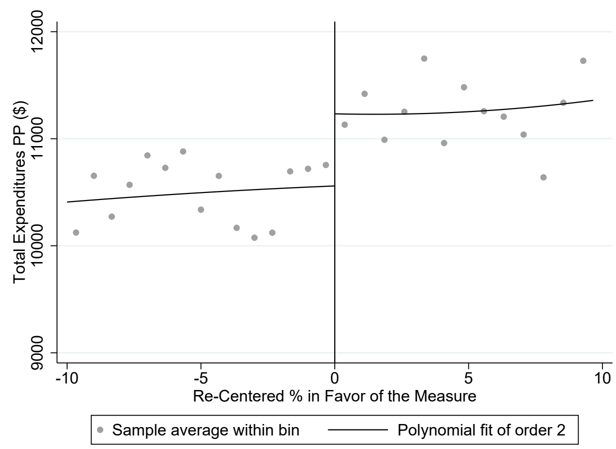
(b) Revenue Limits PP ($t+1$)



(c) Operational Exp. PP ($t-2$)



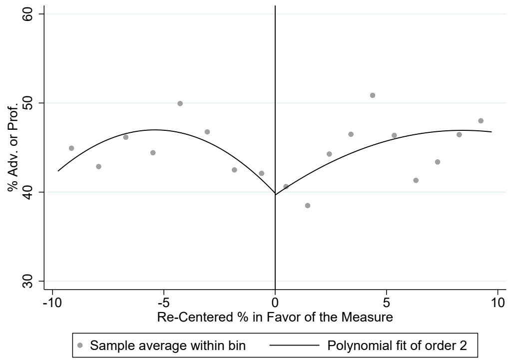
(d) Operational Exp. PP ($t+1$)



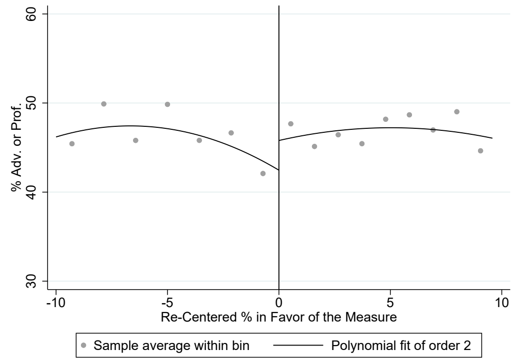
Notes: The figure shows binscatters of school districts' average fiscal outcomes along with a second-order polynomial fit. The x-axis is the percent of district residents in favor of passage re-centered at the 50% vote threshold. Panels (a) and (c) show outcomes in $t-2$ while Panels (b) and (d) present outcomes in $t+1$; t represents the year of the focal operational referendum. The figure was generated using the Stata package "rdplot," which implements data-driven RD plots. The number of bins was selected via evenly-spaced mimicking variance using spacing estimators. The local polynomial estimator was constructed with a uniform kernel function. These are both the default options.

Figure C.2: RD Plots for Test Scores (Operational Referenda)

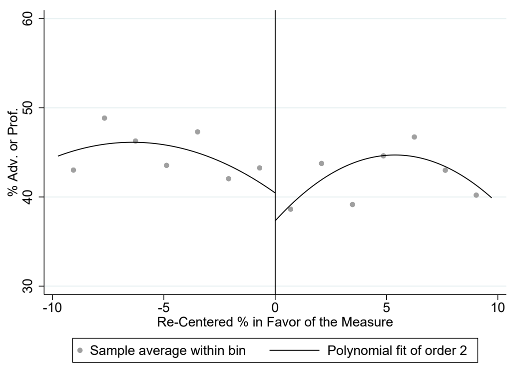
(a) % Adv. or Prof. 10th Grade ($t-2$)



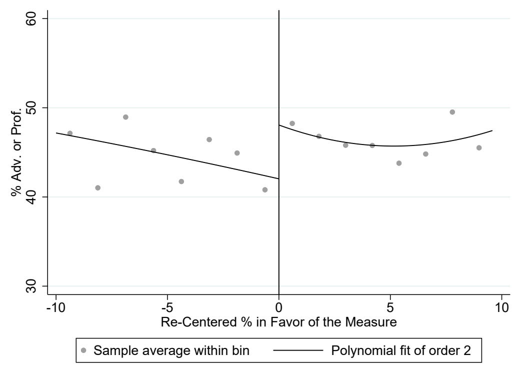
(b) % Adv. or Prof. 10th Grade ($t+4$)



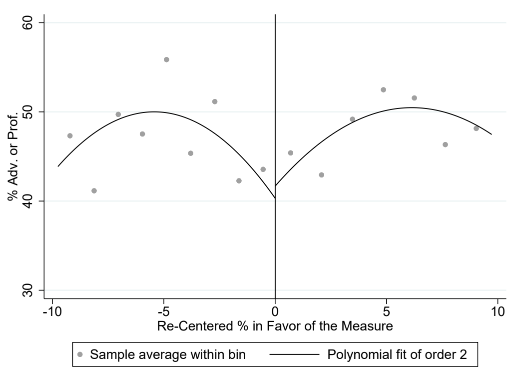
(c) % Adv. or Prof. 8th Grade ($t-2$)



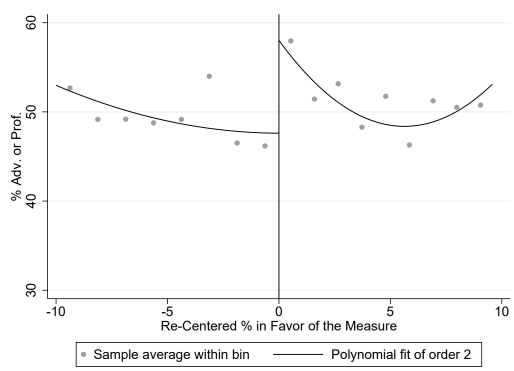
(d) % Adv. or Prof. 8th Grade ($t+4$)



(e) % Adv. or Prof. 4th Grade ($t-2$)



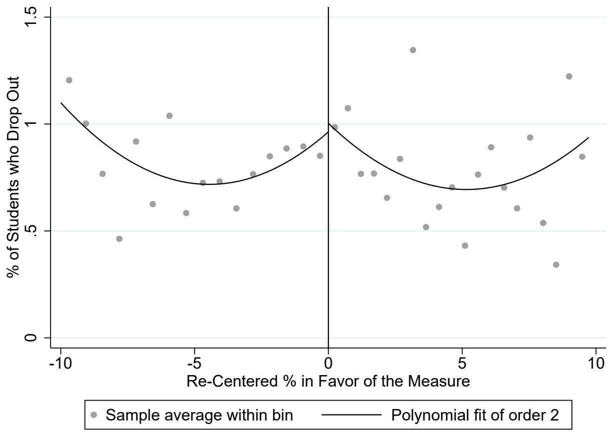
(f) % Adv. or Prof. 4th Grade ($t+4$)



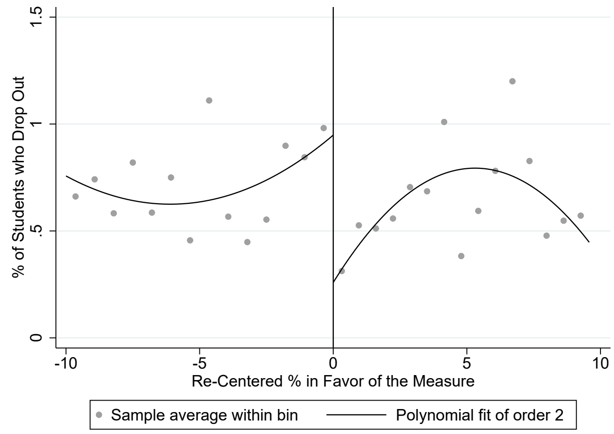
Notes: The figure shows binscatters of school districts' student outcomes along with a second-order polynomial fit. The x-axis is the percent of district residents in favor of passage re-centered at the 50% vote threshold. Panels (a), (c), and (e) show outcomes in $t-2$ while Panels (b), (d), and (f) present outcomes in $t+4$; t represents the year of the focal operational referendum. The figure was generated using the Stata package "rdplot," which implements data-driven RD plots. The number of bins was selected via evenly-spaced mimicking variance using spacing estimators. The local polynomial estimator was constructed with a uniform kernel function. These are both the default options.

Figure C.3: RD Plots for Dropout Rate and Postsecondary Enrollment (Operational Referenda)

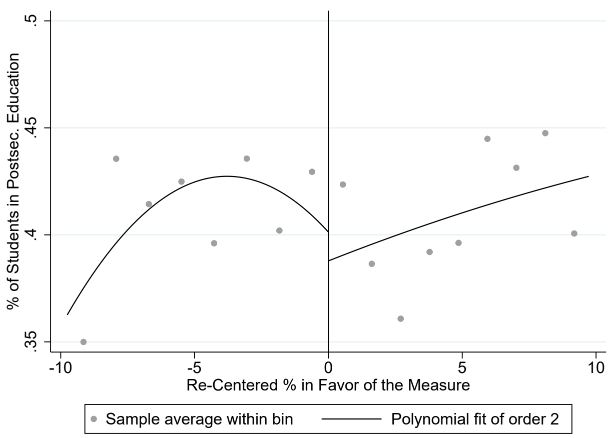
(a) Dropout Rate ($t-2$)



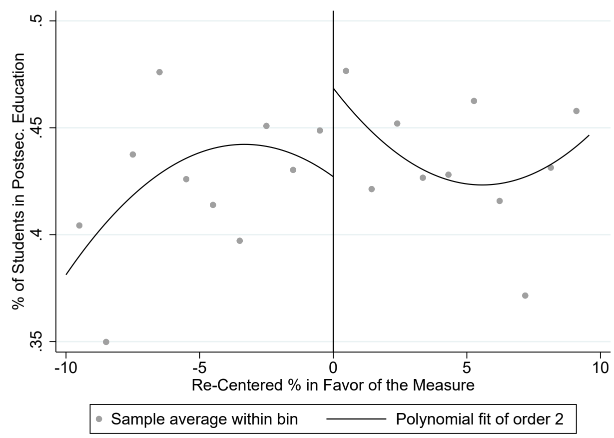
(b) Dropout Rate ($t+5$)



(c) % Enrolled in Postsecondary Education ($t-2$)



(d) % Enrolled in Postsecondary Education ($t+5$)



Notes: The figure shows binscatters of school districts' student outcomes along with a second-order polynomial fit. The x-axis is the percent of district residents in favor of passage re-centered at the 50% vote threshold. Panels (a) and (c) show outcomes in $t-2$ while Panels (b) and (d) present outcomes in $t+5$; t represents the year of the focal operational referendum. The figure was generated using the Stata package "rdplot," which implements data-driven RD plots. The number of bins was selected via evenly-spaced mimicking variance using spacing estimators. The local polynomial estimator was constructed with a uniform kernel function. These are both the default options.

Table C.1: ITT Effects of Narrow Operational Referendum Win on Fiscal and Student Outcomes

	Year Relative to the Election				
Dependent Variable	**1 yr**	**2 yrs**	**3 yrs**	**4 yrs**	**5 yrs**
Rrevenue Limits PP	295	254	278	218	221
	(68)	(90)	(100)	(140)	(172)
Operational Exp. PP	238	209	261	167	216
	(88)	(105)	(114)	(172)	(166)
% Adv. or Prof., 10th Grade	3.64	4.82	4.71	5.71	3.95
	(1.90)	(2.18)	(2.03)	(2.42)	(2.37)
% Adv. or Prof., 8th Grade	3.10	5.46	3.81	6.24	6.23
	(2.10)	(2.63)	(2.63)	(2.99)	(3.04)
% Adv. or Prof., 4th Grade	1.34	0.11	-1.36	0.92	-1.63
	(2.31)	(2.58)	(2.69)	(2.77)	(3.10)
Dropout Rate	-0.06	-0.17	-0.10	-0.05	-0.20
	(0.09)	(0.09)	(0.10)	(0.09)	(0.11)
Log(Postsecondary Enrollment)	0.03	0.05	0.05	0.03	0.11
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)

Notes: The table presents results from the estimation of Equation C.5, specifying $f_g(v^o_{dt})$ and $f_g(v^b_{dt})$ as third-order polynomials. Estimates of the β^{ITT}_τ's along with standard errors clustered at the district level in parentheses are shown for up to five years after the election.

Table C.2: Local-Linear Regressions of Fiscal Outcomes (Operational Referenda)

	$t-2$	$[t, t+5]$
Panel (a): Revenue Limits PP		
Conventional RD Estimate	214.35	337.38
Conventional P-value	0.33	0.00
Bias-Corrected Estimate	205.20	290.28
Conventional P-value	0.36	0.00
Robust P-value	0.44	0.00
CCT Bandwidth	[-11.27, 11.27]	[-2.58, 2.58]
Kernel Type	Triangular	Triangular
Panel (b): Operational Exp. PP		
Conventional RD Estimate	244.09	486.50
Conventional P-value	0.37	0.00
Bias-Corrected Estimate	253.02	433.02
Conventional P-value	0.35	0.00
Robust P-value	0.43	0.00
CCT Bandwidth	[-11.31, 11.31]	[-2.92, 2.92]
Kernel Type	Triangular	Triangular

Notes: The table shows the results of local-linear regressions of school districts' fiscal outcomes before ($t-2$) and after the referendum ($[t, t+5]$) on the vote share in favor of the initiative and a passage indicator for each operational referendum in t. I implement the local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each table presents local-linear regression estimates without the bias-correction term removed; the second row reports the p-value corresponding to this conventional RD estimate and derived from a conventional variance estimator. The third row presents similar estimates with the bias-correction term removed; the fourth and fifth rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a robust variance estimator. The sixth row presents Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth. Finally, the last row of each panel presents the kernel function used to construct the local polynomial estimator. I use a triangular kernel function in each specification, which is standard in the literature.

Table C.3: Local-Linear Regressions of Mathematics Test Scores (Operational Referenda)

	$t-2$	$[t,t+5]$
Panel (a): % Adv. or Prof., 10th Grade		
Conventional RD Estimate	-1.08	3.32
Conventional P-value	0.80	0.00
Bias-Corrected Estimate	-0.12	3.77
Conventional P-value	0.98	0.00
Robust P-value	0.98	0.00
CCT Bandwidth	[-6.90, 6.90]	[-8.54, 8.54]
Kernel Type	Triangular	Triangular
Panel (b): % Adv. or Prof., 8th Grade		
Conventional RD Estimate	-2.59	3.24
Conventional P-value	0.48	0.01
Bias-Corrected Estimate	-2.89	3.70
Conventional P-value	0.43	0.00
Robust P-value	0.51	0.01
CCT Bandwidth	[-7.04, 7.04]	[-5.34, 5.34]
Kernel Type	Triangular	Triangular
Panel (c): % Adv. or Prof., 4th Grade		
Conventional RD Estimate	1.00	6.37
Conventional P-value	0.83	0.00
Bias-Corrected Estimate	1.88	7.17
Conventional P-value	0.68	0.00
Robust P-value	0.73	0.00
CCT Bandwidth	[-7.58, 7.58]	[-4.18, 4.18]
Kernel Type	Triangular	Triangular

Notes: The table shows the results of local-linear regressions of school districts' test scores before ($t-2$) and after the referendum ($[t,t+5]$) on the vote share in favor of the initiative and a passage indicator for each operational referendum in t. I implement the local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each table presents local-linear regression estimates without the bias-correction term removed; the second row reports the p-value corresponding to this conventional RD estimate and derived from a conventional variance estimator. The third row presents similar estimates with the bias-correction term removed; the fourth and fifth rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a robust variance estimator. The sixth row presents Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth. Finally, the last row of each panel presents the kernel function used to construct the local polynomial estimator. I use a triangular kernel function in each specification, which is standard in the literature.

Table C.4: Local-Linear Regressions of the District's Dropout Rate and Postsecondary Enrollment (Operational Referenda)

	$t-2$	$[t, t+5]$
Panel (a): Dropout Rate		
Conventional RD Estimate	0.05	-0.23
Conventional P-value	0.77	0.00
Bias-Corrected Estimate	0.05	-0.26
Conventional P-value	0.77	0.00
Robust P-value	0.81	0.00
CCT Bandwidth	[-9.54, 9.54]	[-6.93, 6.93]
Kernel Type	Triangular	Triangular
Panel (b): Postsec. Enrollment Share		
Conventional RD Estimate	-0.03	0.03
Conventional P-value	0.22	0.00
Bias-Corrected Estimate	-0.04	0.03
Conventional P-value	0.19	0.00
Robust P-value	0.27	0.00
CCT Bandwidth	[-11.41, 11.41]	[-5.51, 5.51]
Kernel Type	Triangular	Triangular

Notes: The table shows the results of local-linear regressions of school districts' dropout rates and share of students who subsequently enroll in a postsecondary education program within the state before ($t-2$) and after the referendum ($[t, t+5]$) on the vote share in favor of the initiative and a passage indicator for each operational referendum in t. I implement the local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each table presents local-linear regression estimates without the bias-correction term removed; the second row reports the p-value corresponding to this conventional RD estimate and derived from a conventional variance estimator. The third row presents similar estimates with the bias-correction term removed; the fourth and fifth rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a robust variance estimator. The sixth row presents Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth. Finally, the last row of each panel presents the kernel function used to construct the local polynomial estimator. I use a triangular kernel function in each specification, which is standard in the literature.

Table C.5: ITT Effects of Narrow Capital Bond Referendum Win on Fiscal and Student Outcomes

	Year Relative to the Election				
Dependent Variable	**1 yr**	**2 yrs**	**3 yrs**	**4 yrs**	**5 yrs**
Total Capital Outlays PP	4,313	916	-1,379	-942	-852
	(420)	(514)	(361)	(314)	(305)
Long-Term Debt PP	6,541	5,825	3,530	3,217	2,528
	(869)	(755)	(813)	(852)	(915)
% Adv. or Prof., 10th Grade	1.54	-1.05	0.02	0.84	1.99
	(1.90)	(1.81)	(2.05)	(2.34)	(2.62)
% Adv. or Prof., 8th Grade	0.50	2.07	1.98	4.47	0.23
	(1.91)	(2.46)	(2.67)	(2.63)	(2.96)
% Adv. or Prof., 4th Grade	-2.10	-4.33	-2.97	-3.78	-5.59
	(2.22)	(2.55)	(3.10)	(3.14)	(3.08)
Dropout Rate	-0.08	-0.03	-0.04	0.10	-0.03
	(0.07)	(0.10)	(0.09)	(0.13)	(0.10)
Log(Postsecondary Enrollment)	0.05	0.09	0.02	0.01	0.00
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)

Notes: The table presents results from the estimation of Equation C.5, specifying $f_g(v^o_{dt})$ and $f_g(v^b_{dt})$ as third-order polynomials. Estimates of the γ^{ITT}_τ's along with standard errors clustered at the district level in parentheses are shown for up to five years after the election.

Appendix D Benchmarking Postsecondary Enrollment Effects

This section explores how the magnitudes of the postsecondary enrollment effects documented in this study compare to those in Jackson, Johnson and Persico (2016)'s seminal study and those of other educational interventions.

D.1 Comparison to Other Studies

Jackson, Johnson and Persico (2016) study the effects of school finance reforms that began in the early 1970s and find that a 10% increase in spending across all 12 grades increased average years of completed schooling by 0.31 years. In contrast, my most conservative estimate shows that narrowly passing an operational referendum increases the percent of 9th grade students in the district who subsequently enroll in a postsecondary education program within the state by three percentage points (from roughly 42% to 45%).[53] If one assumes, as a lower bound, that all students induced into college completed only one more year of schooling—and that spending effects are linear—then my estimates indicate that increasing operational spending by $1,000 (10%) in all 12 grades leads to approximately 0.14 additional years of schooling. In contrast, if one assumes as an upper bound that all students induced into college completed four more years of schooling, then my estimates indicate that increasing operational spending in all 12 grades by $1,000 (10%) leads to approximately 0.58 additional years of schooling.

As in Hyman (2017), the lower bound is calculated as follows: I first multiply the 3 percentage point increase in postsecondary enrollment by the 1 additional year of schooling ($0.03 \times 1 = 0.03$). Now, Table C.2 shows that a successful operational referendum increases spending by roughly $500 per pupil each year for five years. Thus, I then multiply 0.03 by 2 (to convert from $500 to $1,000—assuming spending effects are linear), and then by (12/5) to get the spending effect of 12 years rather than 5 ($0.03 \times 2 \times (12/5) \approx 0.14$). The upper bound is calculated as follows: I first multiply the 3 percentage point increase in postsecondary enrollment by the 4 additional years of schooling ($0.03 \times 4 = 0.12$). I then multiply 0.12 by 2 (to convert from $500 to $1,000), and then by (12/5) ($0.12 \times 2 \times (12/5) \approx 0.58$). The midpoint of these two extremes is roughly 0.36 additional years of schooling. Thus, while these back-of-the-envelope calculations are certainly imperfect, they suggest that the educational attainment effects documented in this study are similar to those in Jackson, Johnson and Persico (2016).

[53] Local-linear regression estimates in Table C.4 show that the percent of students who subsequently enroll in a postsecondary education program within the state increases by roughly three percentage points in the five years after a successful operational referendum. The average percent of students who enroll in a postsecondary education program within the state in my sample is roughly 42% (see Table 2).

D.2 Comparison to Other Educational Interventions

To explore how the magnitude of this effect compares to other educational interventions, I create an index of cost-effectiveness as in Hyman (2017) by dividing the policy's cost by the proportion of new students it induces into postsecondary education. For instance, Hyman (2017) examines the effect of increased primary school spending as a result of Michigan's 1994 school finance reform (Proposal A) on college enrollment. He finds that $1,000 of additional spending per pupil during each of grades four through seven led to a three percentage point increase in the probability that a student enrolled in postsecondary education. Therefore, the amount of money spent to induce one additional student into postsecondary school in the Michigan intervention is roughly $133,333 (=$4,000/0.03). In contrast, Table C.2 shows that a successful operational referendum in Wisconsin costs roughly $2,500 per pupil ($500 per pupil for five years), and it increases the probability that a student enrolls in postsecondary education by roughly three percentage points (see Table C.4). This implies that a successful operational referendum has a cost per student induced into college of roughly $83,333 (=$2,500/.03). As Hyman (2017) points out, this estimated cost is much lower than the amount spent to induce one additional student into college from class size reductions in the Tennessee STAR experiment ($400,000) (Dynarski, Hyman and Schanzenbach, 2013).

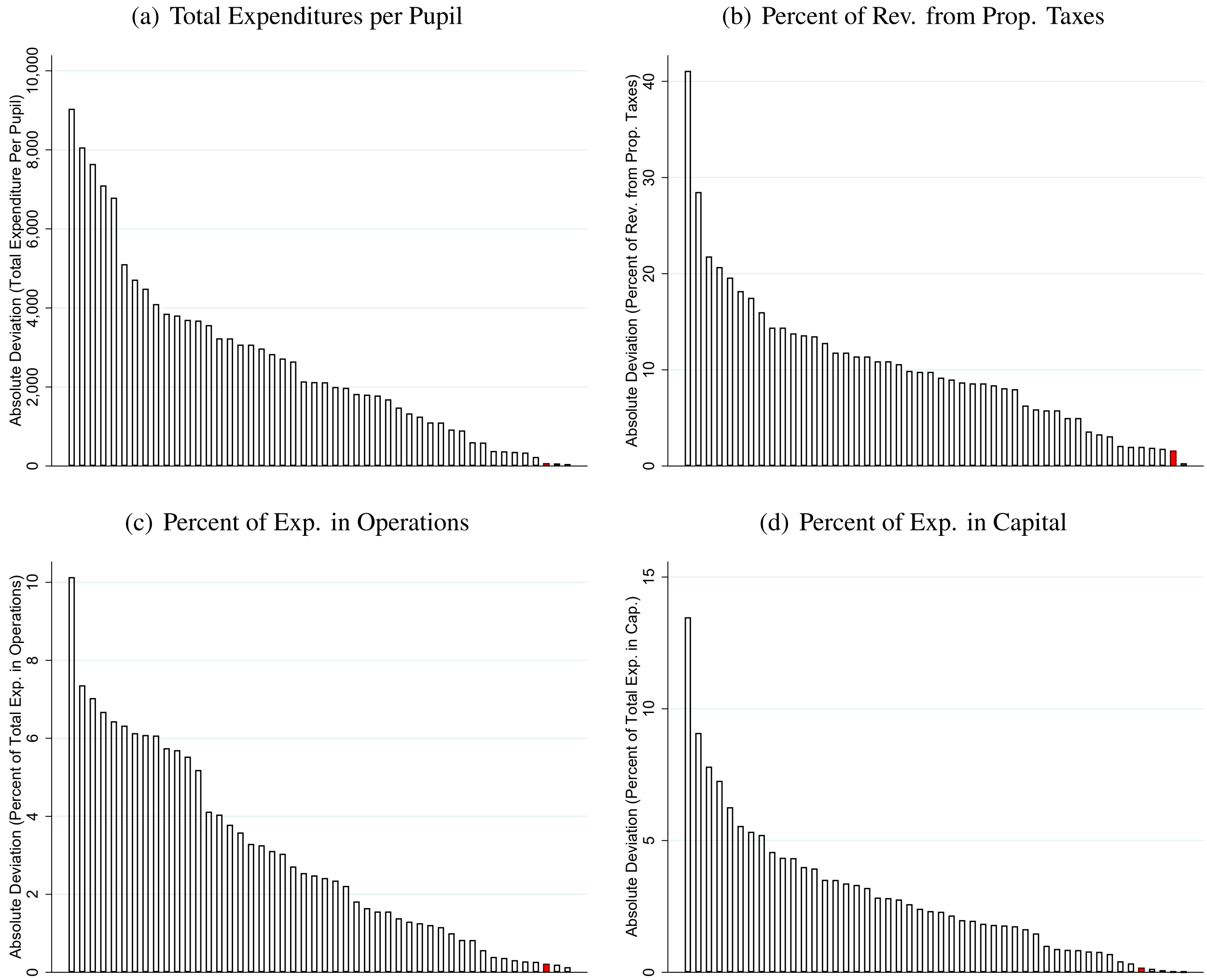
Appendix E Wisconsin's School Finance Generalizability

One concern with using data from only one state is whether the results are externally valid. In other words, how similar is Wisconsin's school finance system relative to other states in the U.S.? Could we apply the insights gained from this study to states in other parts of the country? Figure E.1 investigates how similar Wisconsin's school finance was to other states during the 2014-15 academic year.

Each panel shows absolute deviations from the national average along four dimensions related to school finance. Panel (a) shows absolute deviations in total expenditures per pupil. Panel (b) shows deviations in the percent of total revenue that school districts receive from local property taxes. Finally, Panels (c) and (d) show deviations in the percent of total expenditures devoted to operations and capital, respectively. The solid red bar represents the state of Wisconsin, while unfilled bars represent remaining states. The states more similar to the national average are on the rightmost end of the figure.

In all panels, Wisconsin has one of the smallest absolute deviations from the national average. For instance, in 2014-15, Wisconsin's public schools spent roughly $12,726 per pupil on average. This expenditure was only $70 lower than the national average (the third closest absolute deviation). Similarly, Wisconsin's percent of total expenditures that were allocated to operations (88%) and to capital (7.6%) ranked fifth and third closest to the national average, respectively. These findings suggest that Wisconsin is not an outlier along these dimensions, and that its school finance system is quite similar to that of the average U.S. state. Policymakers may therefore have some confidence that this study's main findings are not unique to Wisconsin's institutional context and may apply broadly across the country.

Figure E.1: State Deviations from the National Average



Notes: Figure shows state absolute deviations from the national average in four key school finance metrics during the 2014-15 academic year. Panel (a) shows deviations in total expenditures per pupil. Panel (b) shows deviations in the percent of total revenue derived from local property taxes. Panels (c) and (d) show deviations in the percent of total expenditures allocated to operations and capital, respectively. The solid red bar represents the state of Wisconsin. Unfilled bars represent the remaining states in the contiguous U.S. Data on each state's school finance system come from the National Center for Education Statistics' Digest of Education Statistics.