Crowd-out of Private Contributions to Local Public Goods: Evidence from School Tax Referenda

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Abstract

While primarily publicly funded government entities, most public school districts receive private donations. I estimate how local school taxes crowd out private, voluntary contributions to public education. To do this, I exploit quasi-experimental variation in tax revenue stemming from local elections. I collect data from a large set of referenda in which local taxes face voter approval in four Midwestern states, combined with administrative records of the sources of school district revenues. Using a regression discontinuity design around voting thresholds that determine passage of local referenda, I show that private contributions to public school systems are not crowded out by local taxes. My preferred specification allows rejecting that a one dollar increase in taxes causes more than approximately a 1.25 cent decrease in private contributions.

1 Introduction

Local residents fund public school districts through taxes whether they favor it or not, but parents and community members provide additional, voluntary contributions in the form of cash gifts, fund-raising, and volunteering. As local preferences and economic conditions change, local governments frequently decide whether and how to adjust the level of spending

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on schools, and hence taxation. If these decisions influence the level of donations the schools receive, the change in total revenue will not be equal to the change in tax revenue. A tax increase may result in a loss in donations, or conversely, a loss in tax revenue may be made up by increased donations. In this paper, I present empirical evidence on the extent to which taxes crowd out private contributions to public school districts.

Public schools are a major category of government spending, totaling $592 billion in 2012, 46% of which came from local sources. In addition, education is the second most prevalent category of charitable contributions in the United States following religious organizations, although most goes to higher education institutions.

Classic economic models of voluntary contribution predict total crowd-out: each dollar of additional government spending will reduce private spending by a dollar (Bergstrom, Blume, and Varian 1986). However, across a variety of sectors, most empirical studies have shown evidence of incomplete crowd-out. This paper provides the first estimates of crowd-out in public K-12 education and shows there is no economically meaningful level of crowd out and at most minimal crowd in.

My crowd-out estimates rely on minimal empirical assumptions. I exploit quasi-experimental variation in tax revenues that arise from local elections to increase taxes in four Midwestern states: Michigan, Minnesota, Ohio, and Wisconsin. Following the work of Cellini, Ferreira, and Rothstein (2010) in California, I use a regression discontinuity design around the vote threshold that results in the higher taxes prevailing. Under the assumption that unobservable determinants of contributions are continuous at the vote threshold, this isolates variation in tax revenues that is unrelated to other factors by which school districts differ.

I use two sources of data from each state in my sample. I collect data on a large set of local elections to approve tax increases, or 'tax referenda'. Because referenda with just under a majority fail while those with a majority prevail, these elections create source of plausibly exogenous variation in local tax revenues. I combine these with school district financial reports that detail the sources of all revenues received by each school district, whether from taxes, private contributions, or other sources. Across the four states, my data include 9,810 referenda in 1,640 school districts.

I estimate the effect of local tax revenue on voluntary contributions to schools by instrumenting for it with the passage of a tax increase. A successful referenda increases local tax revenue by approximately $350. However, local tax revenues do not crowd out private contributions to local public school districts. Specifically, I can reject that a one-dollar increase in local taxes causes more than a 1.22-cent decrease in contributions. In fact, they point towards a statistically insignificant and economically very small increase in contributions.

This paper has three main contributions. First, I provide the most credible estimate of crowd-out of private contributions in the education sector. Most existing estimates of crowd-out study non-profits that provide social services (Hungerman 2005; Gruber and Hungerman 2007; Andreoni and Payne 2011a; Boberg-Fazlic and Sharp 2015[1]), and the limited evidence from other sectors suggests that crowd-out may differ across varying types of charities. There is very little evidence for or against crowd out in the education sector[2] and this is the first paper to address the question in the realm of primary and secondary education.

Second, this is a setting in which we might expect the crowd out model to hold. These donations end up in the school district budget and hence, could have been for substituted with tax dollars. Since both the taxes and public donations come primarily from local residents, when taxes increase, donors must cut back somewhere. Donations to the public schools which directly benefited from the tax increase would be a logical place to do so. The potential donors are also relatively likely to be aware of the increased taxes. The tax increase

[1] Andreoni and Payne (2013) and Andreoni (2006) review earlier papers in this literature.

[2] A few studies exist in the higher education sector: Payne (2001) finds that public and private research fundings for universities are positively correlated, Connolly (1997) and Ehrenberg, Rees, and Brewer (1993) study whether external grants crowd out internal funding at universities, and Jones (2015) finds that the introduction of state lotteries that fund education crowd out donations to higher education.

required a public vote, and since the regression discontinuity design uses close elections, it may have been contentious. In nearly all cases, the election approves an increase in the property tax which are thought to be highly salient (Cabral and Hoxby, 2012). In contrast, the existing literature typically uses federal funding which may come from federal taxes or deficit spending.

Finally, this setting provides estimates with policy relevance. Perhaps most convincing empirical evidence of crowd-out in the literature come from unusual circumstances. Gruber and Hungerman (2007) shows that church charitable activity decreased in response to the New Deal. However, the New Deal is unlikely to happen again and likely produced a fundamentally different response than smaller changes. This paper estimates the crowd-out that results from decisions on the level of public funding that governments make annually.

In addition to the literature on crowd-out, I contribute to a smaller literature on private contributions to public schools. This literature shows that school contributions respond to the perceived quality of schools (Figlio and Kenny 2009) and the size of the school district (Brunner and Sonstelie 2003; Nelson and Gazley 2014). This paper contributes a new source of data on contributions to public schools, administrative records of school finances, and tests whether they respond to fiscal policy.

In the next section, I describe the setting of contributions to public school districts. In Section 3, I lay out the empirical regression discontinuity strategy. In Section 4, I summarize the data. In Section 5, I provide evidence of the validity of the instrument and the effect of passing a referendum on revenues. In Section 6, I present the main estimates of crowd-out, and in Section 7, I conclude.

2 Contributions, School Finance, and Crowd-Out

This paper studies how taxes crowd out contributions to public school districts. When a household contemplates contributing to their school, they know that it will benefit all students, not just their own. Without any government funding, families would need to contribute if they wish to keep the school operating. However, schools receive large amounts of government funding, predominantly from state and local governments.

In order to rationalize private donations to public schools, there must be demand for higher funding that goes unmet by taxation. If all households in a district had identical preferences and local governments set funding levels to the voter's optimum then there would be no reason to donate additional funds. This is not likely to be the case. First, without perfect Tiebout sorting heterogeneity in the preferred level of public spending within school districts remains. Second, in most states school districts face constraints in their ability to set funding levels due to state laws intended to decrease funding inequities across districts.

If the public good funding level motivates donors, classic economic models suggest we should observe dollar for dollar crowd-out. From the perspective of a donor, a dollar taken through taxes is identical to a dollar donated so donors will decrease donations by the amount taxed (Bergstrom, Blume, and Varian 1986). In this setting, most people taxed are likely not donors. Nonetheless, standard models result in near total crowd out as long as the number of donors is large. Taxes paid by non-donors appear to donors as increases in spending power. They will desire to spend some, but not all, of this increase on the public good and hence will decrease donations by less than the amount of the non-donor's taxes. With a large enough number of donors decreasing their donations, the crowd-out will approach total crowd-out (Andreoni 1989; Andreoni 2006). While my data do not contain the number of donors to schools, it is likely that there are many. In 2012, 58 percent of parents reported being involved in school fundraising in some capacity (NCES 2015).

However, if donors give for reasons other than funding a public good, taxation may not be a perfect substitute for donations and crowd out will be less than total. Andreoni (1990) suggests that donations cause a “warm glow” that taxes do not. Donations may also have a social value in signaling a desirable quality (Glazer and Konrad 1996). In the case of giving to schools, both are possible. Parents may feel good about supporting their child’s school and local business owners may seek to increase their stature among customers through donations.

The analysis presented in this paper uses data from four Midwestern states: Michigan, Minnesota, Ohio, and Wisconsin. In all four states, most school districts receive donations. However, as in all states, the vast majority of funding comes from a combination of state, local, and to a lesser extent federal taxes. The relative shares of these sources varies.

In Michigan, Minnesota, Ohio, and Wisconsin, the state government sets a base funding level per pupil. A combination of local taxes and transfers from the state fund this base level. Districts then have some abilities to raise revenues beyond this level. What these abilities allow them to do varies by state. In Michigan, local districts only have the authority to increase revenues for capital expenses. In Michigan, Minnesota, and Ohio, districts can increase revenues for both capital and operational expenses. In most cases, to increase taxes the school board must receive approval from the voters.[3]

If districts increase revenues, they face differing ‘tax prices’ between and within the four states. A district’s tax price is additional revenue that the district will have to fund via local taxes in order to increase spending by a dollar. If the tax price is less than one, the state pays the difference. In most states, these formulas resulted from school finance reforms since the 1970’s (Hoxby 2001).[4]

[3]For operational expenses, the school board may alter taxes without voter approval if they remain below a state set level. To exceed this cap, they must ask the voters. In Michigan, Minnesota, and Ohio all capital expenses requiring issuing debt require voter approval. In Wisconsin, only indebtedness exceeding a debt cap requires voter approval.

[4]These tax prices differ for capital and operational spending. Michigan has a tax cost of one for capital

Table I: Comparison of school finances in 4 Midwestern states

	Mich	Minn	Ohio	Wisc	US
# Districts	549	346	663	425	13,569
Avg # of students	2,525	2,326	2,433	2,037	3,560
Avg expenditures per student	$11,616	$13,983	$12,937	$13,343	$14,226
Local tax revenue per student	$3,673	$2,213	$5,694	$6,210	$6,739
% revenue from local tax	27.4	17.2	39.3	44.3	43.6
Private contributions per student	41.7	61.7	43.0	77.7	31.21
Able to increase operational budget?	No	Yes	Yes	Yes	
Able to increase capital budget?	Yes	Yes	Yes	Yes	
State median household income	48,273	60,702	48,081	51,467	52,250

Note: All school figures from the 2012-13 fiscal year. All dollar figures are in 2014 dollars. Median household income from 2013 Census American Community Survey estimates.

Table I summarizes major revenue and policy features of the four states and compares them to United States averages. The table only partially reflects the differences in their ability to increase revenues. Districts in Michigan and Minnesota get a smaller portion of their revenues from local sources than those in Ohio and Wisconsin. However, because local revenues fund some of the revenue base that the state sets, local taxes partly fund districts in Michigan, even though the districts have no ability to increase revenues.

Whether caused by heterogeneous preferences or limits on taxation, private contributions to public schools, as reported to the federal government, increased from $789 million in 2005-2006 to $1.03 billion in 2012-2013.[5] 64% of school districts reported receiving some private contributions.[6] Figure I displays county level averages of school contributions per student across the four states.

In most cases in all four states, districts that want to increase taxes must send their residents to the polls. In all these cases, the school board proposes a new tax level. Then

spending while Ohio's is roughly .50 (Duncombe and Wang 2009). In Minnesota and Wisconsin tax prices differ by the size of the districts tax base and by their level of spending.

[5]Author's calculation in 2014 dollars from Census/NCES Annual Survey of School System Finances. Includes only donations from those states reporting to the federal government which in 2012-13 was all but eight states.

[6]The 64% of school districts receiving contributions enroll 78% of public school students.

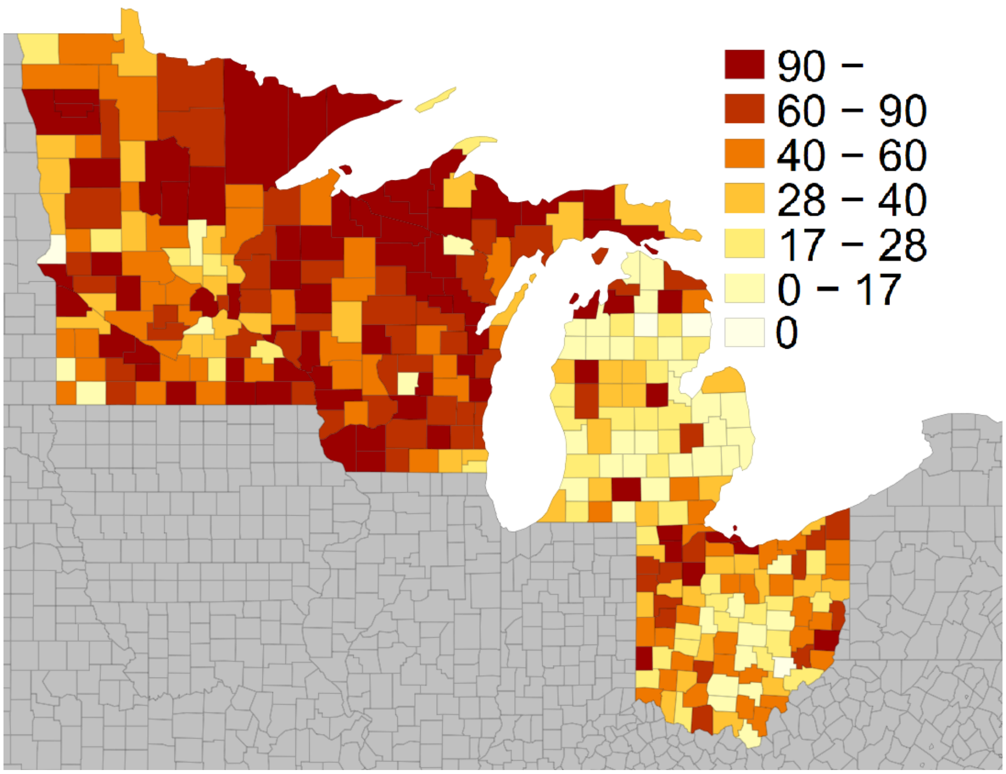


Figure I: Maps of average contributions to schools per student, by county

Note: For each county, the map shows the average level of contributions per student across all districts. Data described in Section 4.

the voters approve or reject the proposal. If they reject it, taxes revert to a fallback level.[7]

3 Regression Discontinuity Strategy

Estimating the extent of crowd out requires estimating the causal impact of tax revenues on private contributions to schools. A simple regression of contributions on tax revenues is unlikely to recover this parameter, although the direction of bias is unclear. The reasons that school districts have high tax revenues may also affect the level of contributions to schools. Districts whose residents prefer high levels of school spending are likely to have high tax revenues, but this might also lead them to make larger contributions to the schools. However, if districts with high levels of tax revenue tend to be more homogenous in their desired level of spending, they might be satisfied with the level of tax funding and not make additional private contributions to schools.

The requirement that voters approve tax changes through referenda creates an opportunity to estimate the causal effect of taxes on contributions. Because some elections fail while others pass, districts who proposed tax changes will vary in their tax revenues in subsequent years. While districts whose referenda pass may differ systematically from those whose fail, comparing referenda close to the election minimizes this bias. As long as there is some randomness in the portion of voters that vote in favor, elections close enough to the threshold of passing approximate a random experiment (Lee 2008). This creates quasi-random variation in the level of tax revenue between districts where the vote narrowly failed and those where it barely passed. I exploit this variation with a "fuzzy" regression discontinuity design to estimate the impact of tax revenues on private contributions to schools.

My analysis proceeds in two steps. First, I estimate the fiscal impacts of passing a

[7] For capital spending referenda, the fallback level is the prior level of taxes. For operational spending referenda, the fallback level varies across states. In Ohio, fallback levels are usually lower in real terms than the prior year, as the tax system is defined in nominal terms. In Wisconsin and Minnesota, fallback levels depend on whether a tax approved in previous years is expiring.

referendum, a sharp regression discontinuity. Second, using this impact as the first stage in an instrumental variables (or fuzzy regression discontinuity) estimation, I estimate the impact of local tax revenues on private contributions. In the remainder of this section, I describe these steps in more detail.

3.1 The effect of referenda on taxes

I use a regression discontinuity design to estimate the effect of voters approving a referendum on the level of tax revenues from local sources. Since the purpose of each referendum is explicitly to approve higher taxes than what would occur if it failed, passage should cause higher tax revenues. Aside from any effects of the passage of the referendum, districts whose referendum came just short of passing should be similar to those whose referendum barely passed.

Following the regression discontinuity literature, I do not assume that referenda close enough to the passage threshold are directly comparable. Instead, I assume that any differences between them can be accounted for by controlling for the election's vote share. By orienting the estimation around each election, rather than around the district, I treat referenda as events and estimate their effect on tax revenues. Considering, for now, only the fiscal year that follows a referendum to authorize taxes, a regression discontinuity model can estimate the impact of the referendum passing on tax revenues. This results in the estimation equation:

$$R_i = f(V_i) + \gamma P_i + \epsilon_i \tag{1}$$

in which R_i is the level of local tax revenues one year after the vote in the district in which referendum i occurred. V_i is the vote share in favor. P_i is a binary variable indicating whether referendum i passed and γ represents the effect of passing a referendum on tax revenues one year later. While this yields a consistent estimate of γ under the assumptions of the regression discontinuity design, it is inefficient.

Much of the variation in school district revenues is across districts, but does not vary over time. In Equation 1, these district specific, but time invariant characteristics end up in ϵ. However, data from prior to the referendum provide information on the levels of these characteristics. Including these data in the model can reduce the residual variation and yield a more precise estimate.

To use this information on constant characteristics, I follow Cellini, Ferreira, and Rothstein (2010) and create a panel that contains observations both before and after each referendum. In districts with referenda in multiple years, data that correspond to one district in one year can end up as multiple observations, each associated with a different referendum.[8] This design treats each referenda as entirely separate events.

By organizing the dataset in this fashion, I can estimate a separate parameter for the effect of passing a referendum on revenues in each year after the vote. By including observations from before the referenda and constraining the referendum's effect to zero in those years, I can include referendum fixed effects, which control for the time invariant characteristics of the district. I estimate the equation:

$$R_{i\tau} = f_\tau(V_i) + \gamma_\tau P_i + \zeta_i + \alpha_{s\tau} + \kappa_{s,t(i)+\tau} + u_{i\tau} \tag{2}$$

where $R_{i\tau}$ is the level of local tax revenues τ years after the vote in the district in which referendum i occurred. γ_τ now represents the effect of passing a referendum on tax revenues τ years later and is constrained to be zero for all $\tau < 0$. $f_\tau()$ controls for the relationship between the vote share on the referendum's vote and the outcome, and is also set to be zero

[8] For example if a district had referenda in both 1998 and 2000, the data corresponding to 1999 would end up in the new panel associated once with each referenda. In one case, it would represent the year after a referendum and in the other the year before.

prior to the vote[9]. ζ represents referendum fixed effects. α represents fixed effects for each state by year relative to the election. Lastly, $t(i)$ represents the fiscal year in which vote i occurred so, κ represents state by fiscal year fixed effects.

This equation allows me to estimate efficiently the impact of passing a referendum on tax revenues in subsequent years. I will use the passage of these referendum, P_i, as an instrument for tax revenues in the second stage.

3.2 The effect of taxes on contributions

As long as passing a referendum has an effect on revenues, this can act as an instrument for tax revenues, enabling me to estimate the effect of tax revenues on contributions. This requires assuming that passing a referenda only effects contributions through its effect on tax revenues. Here, I am taking tax revenues to be a proxy for the funding of the public schools. Expenditures would be another possible proxy, but expenditures are lumpy due to the nature of capital investments and so are not a good measure of the level of the good produced.

Passing a referendum may affect contributions due to a few channels. First, potential contributors (as long as they live in the school district) must now pay higher taxes, leaving them less money to spend on contributions and other goods. Second, the schools are now receiving more money, perhaps lessening the need for the contributions. Third, there may be some impact on how people feel about giving to the public schools. The first two come through the level of tax revenues. The third may or may not depending on what the source of that feeling is, but I would argue that this is largely a semantic distinction.

Using the passage of a referendum as an instrument for revenues results in a fuzzy regression discontinuity design. First, I use this to estimate the impact of taxes on contemporane-

[9] Since there is no regression discontinuity being estimated for years prior to the vote, it is not necessary to control for this relationship.

ous contributions to schools. To accomplish this, I use Equation 2 as the first stage, limited to those observations prior to the election ($\tau < 0$) and the first year following the election ($\tau = 1$).[10] The second stage estimation equation is given by:

$$C_{i\tau} = f_\tau(V_i) + \beta R_{i\tau} + \eta_j + \delta_{s\tau} + \lambda_{s,t(i)+\tau} + \varepsilon_{i\tau} \tag{3}$$

where $C_{i\tau}$ is the amount of private contributions per student τ years after the vote in the district where referendum i occurred and $R_{i\tau}$ represents tax revenues in that same district and year. δ, ζ, and η represent fixed effects as in the first stage Equation 2. Unlike in the first stage, P_i the passage of a referendum is excluded. Instead it is used to instrument for $R_{i\tau}$. β represents the impact of tax revenues on the same year's contributions. With only observations from one year following the referenda included in the estimation, there the model is just identified.

3.2.1 Dynamic effects of taxes

The models described in the previous section estimate only the effect of taxes on contemporaneous contributions. These use only data from one year after the referenda, when the first stage is likely to be strongest. If potential donors take more than one year to react to tax changes and adjust their contributions, these estimates would not show the full impact of taxes on contributions. In this section, I extend the analysis to the dynamic impact of taxes on contributions.

I allow the history of taxes to affect today's contributions. As when estimating only the contemporaneous effect, I can estimate the effect of current and lagged tax revenues on contributions in a fuzzy regression discontinuity design by instrumenting for revenues and past revenues with the passage of referenda. Since I am now estimating multiple parameters,

[10] Additional observations following the vote are used to estimate the dynamic impact of taxes in the next section.

I require multiple instruments. It is natural to allow the effect of passing a referendum to vary depending on the number of years since the referendum. This allows me to identify the dynamic impacts of taxes because passing a referendum does not have an equal impact on revenues in all subsequent years. This results in a new estimation equation:

$$C_{i\tau} = \sum_{k=0}^{T} \beta_k R_{i,\tau-k} + f_\tau(V_i) + \eta_i + \delta_{s\tau} + \lambda_{s,t(i)+\tau} + u_{i\tau} \tag{4}$$

where β_k captures the impact of tax revenues per student k years earlier on contributions per student. While in theory, the potential donors could respond to the complete history of taxes, ($T = \infty$) in practice, an assumption must be made over this timespan, T. All obervations prior to the vote and up to T years following are included in the estimation. Notably, this assumes that donations do not respond to future taxes. If donors anticipating future taxation elect not to give, this would not capture the structural parameters. However, donations primarily fund current expenditures, so future taxes are unlikely to decrease the usefulness of today's donations.

These structural parameters of the effect of current and lagged taxes on contributions to schools describe the dynamic effect of a tax change. Using them, I can describe the effect in following years of any changes in tax revenues. To illustrate crowd-out, I use these parameters to calculate the impact of a one-dollar increase in taxes on contributions τ years later, which is equal to $\sum_{k=0}^{\tau} \beta_k$.

3.3 Estimation

Estimating both the effect of referenda on revenues and the effect of revenues on contributions requires controlling for the relationship between the share of votes in favor and the outcome. In the models above, the $f()$ function represents this control. I follow the recent regression discontinuity literature in using a local linear approach where f is linear and the estimation includes only observations within some bandwidth from the threshold. Imbens and Kalyanaraman (2011) shows how to calculate the bandwidth that minimizes the asymptotic mean squared error of the estimate. I calculate the optimal bandwidth to estimate the effect of referenda on revenues using their formula with revenues residualized by the fixed effects in the model as the outcome. To calculate the optimal bandwidth for the instrumental variables model, I use the Imbens and Kalyanaraman (2011) formula with contributions residualized by the fixed effects in the model as the outcome. Imbens and Kalyanaraman (2011) argue that while this is the optimal bandwidth for the reduced form model, in practice it will differ little from the optimal bandwidth for the full instrumental variables model. In addition, I use a range of alternative bandwidths to test the robustness of the results.

This provides an unbiased estimate of γ under the now standard regression discontinuity assumption that the potential outcome functions, $E[R|P=1, V=v]$ and $E[R|P=0, V=v]$ are continuous at the passage threshold, v (Imbens and Lemieux 2008). If agents selecting V have "Imperfect control," and cannot choose an exact value, it creates some randomness in V, which justifies this assumption (Lee and Lemieux 2010). In this setting, perfect control would be evidence of voter fraud.[11] Although I cannot test this assumption directly, I provide evidence that there is no discontinuity in district characteristics from prior to the election in Section 5.

4 Data

Estimating the models described in the previous section requires data on school district fiscal information, including private contributions, tax revenues, and records of local referenda to raise taxes for schools. I collect these data from four Midwestern states, Michigan, Minnesota,

[11] In some other settings using elections for RD estimates, there is evidence that this assumption fails. For example, close unionization elections tend to swing against the union when Republicans control the National Labor Relations Board and for the union when Democrats do (Frandsen 2014). However, these issues are unlikely to apply here.

Ohio, and Wisconsin. In this section, I describe the datasets used in my analysis.

4.1 Fiscal Data

To estimate the impacts on private contributions to schools, I use data from administrative school finance records from four states. This is a previously unused source of information on contributions to schools.

Previous research on private contributions to schools primarily used non-profit tax filings to determine the quantity of donations received by local schools (Brunner and Sonstelie 2003; Brunner and Imazeki 2004; Nelson and Gazley 2014). As discussed by Figlio and Kenny (2009), these data are potentially inaccurate for two reasons. First, only non-profits with greater than $50,000 in revenues are required to file with the IRS.[12] In 2013, 65.3% of school districts that reported receiving some contributions received less than $50,000.[13] Second, it requires identifying non-profits that support a given school. These papers typically use keywords in the organization's name and categorization along with the address given in the tax filings to determine the district in which the organization is located. This is necessarily inexact.

Rather than relying on IRS filings or survey reports, I use new data from administrative reports of school district revenue from private contributions. States require that local school districts report detailed accounting information to state officials. These include detailed accounts of the sources of all revenues. In most states, one subcategory of local revenues is private contributions. The Wisconsin Uniform Financial Accounting Requirements manual states that this includes "Gifts, fundraising, contributions, and development." This does not include grants that the district may have received.

[12] Prior to tax year 2010, this threshold was $25,000. While organizations with revenues under these amounts can voluntarily file 990 forms, they are not required to.

[13] In fact, this understates the problem since non-profit organizations often support an individual school rather than the entire district. Less aggregation among the nonprofits makes it likely that fewer of them will reach the $50,000 threshold for filing.

I collected administrative data of school district finances as reported to their state government through public records requests to state departments of education or via publicly available sources. I obtained these data for the fiscal years 2004 through 2014 in Michigan, fiscal years 2001 through 2014 in Minnesota, fiscal years 1992 through 2014 in Ohio, and fiscal years 1992 through 2014 in Wisconsin. [14] In 2004, Wisconsin altered their accounting system which nearly doubled the average level of reported private contributions. Changes like this make it difficult to show changes over time in contributions accurately. However, they do not bias the models presented above, which all include state by year fixed effects that can control for these changes. All tax revenue and expenditure data come from the U.S. Census Bureau's Annual Survey of School System Finances from 1992 to 2014.

4.2 Referenda Data

I compiled a database of bond and tax referenda for the four states from a variety of sources. The Wisconsin Department of Public Instruction catalogs referenda from 1990 through 2014. This includes referenda to approve bond sales for capital projects, to approve "recurring" taxes that permanently raise the tax revenue limit, and to approve "non-recurring" taxes that temporarily raise tax rates without permanently changing the limit.

Ohio referenda come from two sources. First, I use referenda data digitized from records held by the Ohio Secretary of State for years 2004 through 2016. Second, I supplement with bond election records for 1985-2003 from the Ohio Municipal Advisory Council. In total, these data include referenda to approve both general fund tax increases and to approve bond sales for capital investments.[15]

Michigan referenda come from the state Department of Treasury for the years 1996

[14] Beginning in 2006, the U.S. Census Bureau's Annual Survey of School System Finances includes the level of gifts received. The median absolute difference between the census and state data in contributions per student is $0.16

[15] Isen (2014) uses similar tax referenda in Ohio to estimate fiscal spillovers in Ohio.

through 2014. As discussed in Section 2, Michigan school districts cannot increase taxes for operational expenses, so these data cover only referenda to approve bond sales for capital investments(Conlin and Thompson 2014). Specifically, these data cover those bonds accepted by the state under the Michigan School Bond Loan Program that allows districts to borrow at lower interest rates.

4.3 Descriptive Evidence

This research design depends upon districts proposing tax changes that require voter approval. My analysis is limited to districts where the school board brought tax proposals before the voters. This is not required; school boards have the option of staying below the tax levels that require voter approval. As a result, districts in which votes are required may differ from those that do not. If this were the case, the estimates that result would not necessarily be valid among a broader range of districts. To investigate this, Panel 1 of Table II compares summary statistics in 2012-13 for districts who never vote on a referendum, who vote on at least one referendum, and those who have a referendum that falls close to the threshold. Approximately half of districts in the four states hold a referendum in the sample period. Those that never hold a referendum have higher revenues and expenditures on average than those that do. In Ohio, districts require voter approval to exceed a tax rate not a tax level, so districts with high property values may not choose to hold referenda.

Because this is a regression discontinuity design, I am estimating a local average treatment effect for those districts that have referenda near the threshold. The likelihood that this study's results would hold true in other districts depends in part, on whether districts with referenda near the passing threshold are unusual.

Districts that have at least one referenda are very similar on observable characteristics to those that have at least one close referenda. Panel 2 of Table II compares the characteristics of districts where a referendum failed the next year to those where a referendum passed

Table II: Summary statistics for school districts with and without referenda.

		(1) District Characteristics			(2) All Votes		(3) Narrow Votes	
	All districts	Never has ref	1+ refs	1+ close refs	Failed	Passed	Failed	Passed
	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd
N	1,983	343	1,640	1,171	4,818	5,584	1,752	1,747
Avg # of refs	5.23	0.00	6.33	7.79	10.81	9.14	10.97	9.88
	(4.96)	(0.00)	(4.77)	(4.75)	(5.23)	(5.25)	(5.14)	(5.39)
Expenditures PP	12,339	12,012	12,394	12,446	11,619	11,833	11,595	11,747
	(3,141)	(4,081)	(2,952)	(2,880)	(2,972)	(2,921)	(2,857)	(2,795)
Total Revenue PP	12,681	12,348	12,737	12,822	11,482	11,910	11,479	11,754
	(2,854)	(4,085)	(2,589)	(2,418)	(2,308)	(2,503)	(2,282)	(2,334)
Local Tax Rev PP	4,087	4,217	4,066	4,331	4,090	4,313	4,222	4,438
	(2,977)	(4,165)	(2,728)	(2,578)	(2,170)	(2,563)	(2,277)	(2,429)
% Rev fr Local Tax	30.99	31.06	30.98	33.15	35.76	35.97	36.84	37.63
	(17.24)	(21.08)	(16.52)	(16.31)	(16.53)	(17.81)	(17.09)	(17.37)
Contributions PP	58.6	45.4	60.7	61.2	31.6	37.9	34.4	36.0
	(93.0)	(88.1)	(93.6)	(92.1)	(55.9)	(63.3)	(56.7)	(62.7)
Enrollment	2,350	1,928	2,438	2,613	2,790	2,915	3,121	3,109
	(4,085)	(5,007)	(3,862)	(3,812)	(3,752)	(4,686)	(4,421)	(4,314)

Notes: Part (1) shows mean district characteristics by whether they are observed proposing any tax increases and if a increase passes or fails by a margin of less than 5%. Part (2) shows mean characteristics from the year prior to the vote, for each referenda observed, by whether the vote passed or failed. Expenditure and revenue figures are per pupil in 2014 dollars. Enrollment and Fiscal data from NCES CCD.

Note: Panel (1) shows mean district characteristics by whether they are observed proposing any tax increases and if a increase passes or fails by a margin of less than 10%. Panel (2) shows mean characteristics from the year prior to the vote, for each referenda observed, by whether the vote passed or failed. Dollar figures are per pupil in 2014 dollars. Enrollment and Fiscal data from Census/NCES Annual Survey of School System Finances.

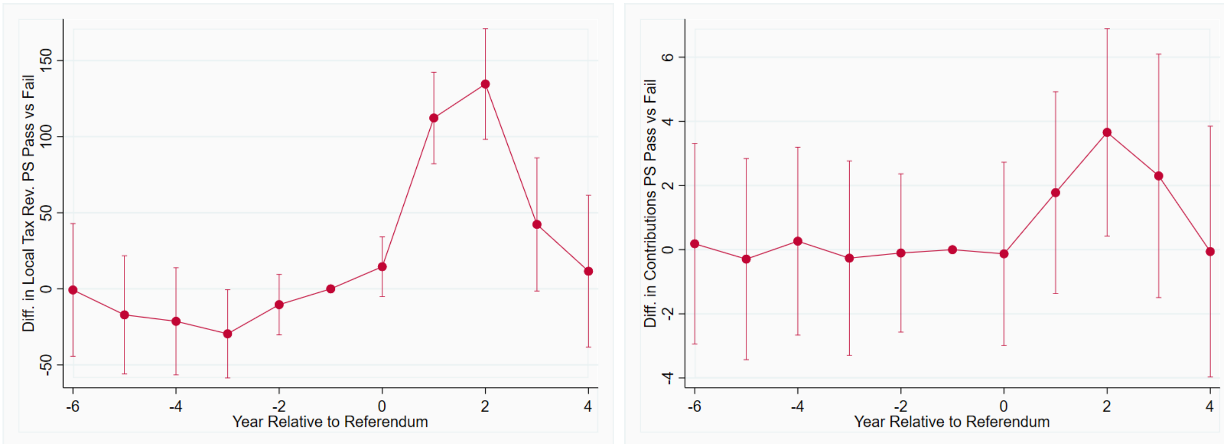
the next year. On observable characteristics, they are remarkably similar. Referenda that passed tend to be in districts that received slightly more private contributions, though the difference is far from statistically significant.

Before turning to the regression discontinuity results, I examine briefly what the raw data show. Figure IIa plots an "event study" type analysis, showing the difference in tax revenues per student between referenda that eventually pass and those that eventually fail both before and after the vote after controlling for referenda and state by year fixed effects. Prior to the vote, districts whose referenda later pass are trending similarly to those whose referenda fail. One and two years following the vote, the difference between districts where a referendum passed vs those where one failed is unmistakable. Three years after the vote, this difference has eroded. As in Cellini, Ferreira, and Rothstein (2010), this is because districts where a referendum failed are far more likely to pass one in subsequent years than those where one passed. School boards often decrease the tax level they are asking for and propose a new increase one or two years later.

Likewise, Figure IIb shows the trends in private contributions per student leading up to and following a referendum. Here, there is slight evidence of an *increase* in donations 2 years following a successful referenda. While this would suggest *crowd-in*, it may reflect the positive correlation between potential outcomes and votes in favor. The regression discontinuity design allows separating the two.

Since these graphs compare taxes and contributions both before and after a referendum, time-invariant district characteristics would not result in a false conclusion. However, districts might pass tax referenda because of time varying characteristics. For instance, new residential construction might require new funding and change the composition of potential donors. Were this the case, the conclusion from this simple analysis could be unfounded.

This is the advantage of the regression discontinuity design. Rather than assuming that districts whose referenda failed are an adequate counter-factual for those whose referenda



(a) Local tax revenues

(b) Private contributions

Figure II: Event study analysis of referenda passage

Note: Graph shows β_τ from the following regression $y_{i\tau} = \beta_\tau P_i + \zeta_i + \alpha_\tau + \kappa_{s,t(i)+\tau} + u_{i\tau}$, where P_i is whether referenda i won, ζ_i are referenda fixed effects, α_τ are relative year fixed effects, and κ are state by fiscal year fixed effects. Error bars show 95% confidence interval. Includes only referenda with data from all relative years shown.

passed, I focus instead on those districts whose referenda passed or failed narrowly.

5 Instrument Validity and the Effect of Referenda on Revenues

In this section, I first show standard tests for validity of the regression discontinuity design. I then show that the design produces a strong first stage result of the effect of referendum passage on revenues.

While there may be political campaigns operating both for and against a referendum, it should not be possible for any actor to exactly target an election vote share. In any election, even with well-run campaigns, there is uncertainty about who will go to the polls and how they will vote. If this were not the case, we would expect to see excess mass in the distribution of vote shares around the passage threshold. Figure III presents a histogram of vote shares relative to the threshold across all referenda. There is no evidence of clumping

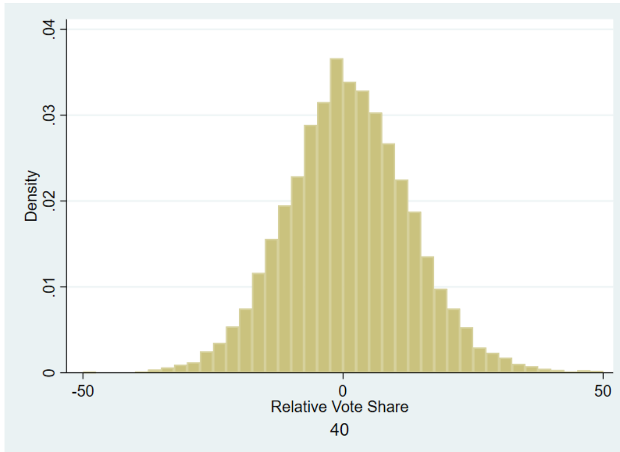


Figure III: Histogram of referenda vote shares

Note: Histogram of vote shares relative to the passage threshold.

around the threshold.

If agents are unable to manipulate which side of the threshold a referendum ends up, then unobservable characteristics of districts should be continuous across the passage threshold. While this is not directly testable, I provide evidence in support by testing whether district characteristics in the years prior to a referendum are discontinuous at the threshold. Table III presents estimates of these discontinuities. None of these estimated discontinuities differ significantly from zero at even the 10% confidence level. Furthermore, there is no discernible pattern to these results.[16]

In order to use the regression discontinuity as an instrument for district revenues there must be a strong discontinuity in revenues at the passing threshold. I first demonstrate this graphically. Figure IV shows the mean level of tax revenues by bins of the share of votes in favor of passing the referenda, relative to the votes required. Due to the panel nature of the empirical strategy, it is worth taking a moment to describe the creation of this graph. I split observations in the year following a referendum into quantiles of the vote share. Rather than

[16]When the full estimation strategy with a panel data set is estimated as if the referendum occurred in a year prior to when it actually did, 6 of 20 estimates differ from zero at the 5% level. However, unlike the effect on post-referenda fiscal outcomes, these results disappear when the model includes referenda-specific time trends. Appendix A shows these results.

Table III: Effect of passing a referendum on prior characteristics

	1 yr. before	2 yr. before	3 yr. before	4 yr. before
Exp. PS	79.9	0.5	-118.8	-51.9
	(136.8)	(143.3)	(136.4)	(141.3)
Curr. Exp PS	-14.8	-17.7	-47.4	-40.7
	(72.4)	(70.3)	(67.4)	(64.9)
Cap. out. PS	68.8	-4.7	-54.3	-28.6
	(100.1)	(106.9)	(104.6)	(109.8)
Rev. PS	128.3	52.9	-92.0	-87.4
	(107.5)	(108.3)	(100.3)	(95.5)
Loc. tax rev. PS	176.5	156.6	107.8	117.2
	(105.3)	(103.2)	(103.4)	(104.1)

Note: Regression discontinuity estimates of the effect of passing a referendum on outcomes fixed before the referenda. Each estimate comes from a separate regression that includes only within 9 percentage points of the passage threshold and include linear relationships between the vote share and the outcomes on either side of the threshold. Each model includes state by year fixed effects. Standard errors are clustered at the school district level. All dollar figures are in 2014 dollars.

calculating the mean of the unconditional level of tax revenues in each quantile, I regress local taxes per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then calculate the mean residual from this regression for each quantile, add back the mean level of local tax revenues per student and plot these values.

Figure IV demonstrates that local tax revenues are discontinuous at the passage threshold. Table IV displays a point estimate corresponding to this effect, as well as the effect on subsequent years. These estimates come from estimating Equation 2. One year following the vote, districts whose referenda passed have $256 higher revenues than those whose failed. There is a strong discontinuity at the referenda passage threshold in total expenditures, current expenditure, capital outlays, and local tax revenues per student. These are apparent but weak the year of the referenda, very strong one year and two years after and then weaken.[17] None are statistically significant four years after the vote.

[17] These first stage results are robust to a number of different specifications. In particular, this first stage

I use the passage of a referendum as an instrument for public funding for schools. To do this requires a measure of the level of public funding. The two obvious possibilities are expenditures and revenues, both of which passing a referendum affects. However, expenditures are not a good measure of the additional provision of school services the referenda has provided. Because many referenda authorize capital expenditures they have a much larger impact on expenditures than they do on revenues. However, while capital expenditures are spent in large sums, households feel their benefits over many years. The expenditure itself is not a good measure of the provision of the public good. As a result, it is preferable to focus on tax revenues. With this strong instrument for revenues in hand, I turn to the main results.

6 Results

In the sections that follow, I describe the results of these methods. First, I report the main results of how tax revenues crowd out private contributions. Then, I discuss how to interpret this result given the level of contributions relative to taxes.

6.1 Effect of taxes on contributions

To examine the results of taxes on contributions, I begin with a graphical analysis of the effect of passing a referendum on contributions to schools. Figure V shows this reduced form impact of the passage of a referendum on private contributions per student to the school district one (a) and two (b) years after the vote. The construction of this figure is akin to Figure IV and is discussed in Section 5. Unlike in Figure IV, there does not appear to be any shift in contributions at the passage threshold.

is detectable without the panel data structure using the simple regression discontinuity design shown in Equation 1. In addition, adding referenda specific time trends to the panel data equation leaves the results qualitatively similar.

Table IV: Effect of passing a referendum on expenditures and revenues

	Contr. PS	Exp. PS	Curr. Exp PS	Cap. out. PS	Loc. tax rev. PS
Yr. of ref	-1.80	265**	37	188*	83**
	(2.29)	(118)	(36)	(99)	(37)
1 yr. later	-0.07	912***	139***	665***	256***
	(2.44)	(135)	(40)	(117)	(43)
2 yrs. later	4.16	1,256***	177***	949***	271***
	(2.93)	(163)	(44)	(141)	(50)
3 yrs. later	-2.01	623***	134***	390**	159***
	(3.03)	(196)	(50)	(164)	(55)
4 yrs. later	0.44	-85	151***	-279*	133**
	(3.26)	(192)	(56)	(164)	(58)
5 yrs. later	-1.80	-360**	103*	-404***	119*
	(3.66)	(178)	(58)	(142)	(64)
6 yrs. later	-2.84	-116	46	-183	76
	(3.92)	(159)	(59)	(132)	(67)

Note: Regression discontinuity estimates of the effect of passing a referendum on fiscal outcomes following the referenda. Each column comes from one model estimated with Equation 2, includes all referenda within 10 percentage points of the passage threshold, and $f_\tau(V_i)$ is linear with different slopes on either side of the threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years before and six years after the referenda. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All figures are in 2014 dollars.

Table V: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions.

	(1)	(2)
Loc. tax rev. PS	0.0081	0.0107
	(0.0104)	(0.0124)
Bandwidth	7.41	7.64

Note: Both columns are estimated by Equation 3, instrumenting for local tax revenue with the passage of a tax referenda. Column 2 also controls for referenda-specific time trends. Both include only referenda within the bandwidth shown of the passage threshold and include linear relationships between the vote share and the outcomes on either side of the threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations prior to the referenda and from one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Table V shows point estimates of the effect of tax revenues on private contributions from the fuzzy regression discontinuity design. Column 1 shows results from Equations 2 and 3 as the first and second stages respectively. A one-dollar increase in local revenues increases slightly contributions per student by 0.81 cents. While this estimate is not significantly different from zero, there is no evidence of crowd-out. In fact, the estimate allows crowding out of more than 1.22 cents per dollar to be rejected along with crowd in of more than 2.8 cents per dollar.

This result is robust to other specifications. Because there is more than one year of data prior to each referenda, referenda specific trends are identified along with the referenda fixed effects that are already present. This controls for the possibility of differing pre-trends across the passage threshold. Column 2 shows the results with this addition. The point estimate moves only slightly. The result is also robust to alternative bandwidths which is shown in Appendix C.

Table VI: Dynamic Effects of Taxes on Contributions

A. Structural parameters						
β_0	β_1	β_2	β_3	β_4	β_5	β_6
-0.001	0.041	-0.051	0.047	-0.043	0.033	-0.045
(0.020)	(0.038)	(0.047)	(0.049)	(0.052)	(0.045)	(0.033)
B. Total effect of $1 increase in taxes on contributions						
Year of change	1 yr later	2 yrs later	3 yrs later	4 yrs later	5 yrs later	6 yrs later
-0.001	0.041	-0.010	0.037	-0.006	0.026	-0.019
(0.020)	(0.023)	(0.030)	(0.028)	(0.035)	(0.029)	(0.031)

Note: Panel A shows the effect of contemporaneous and lagged revenues on contributions with parameters as specified in Equation 4, estimated with a fuzzy regression discontinuity design with revenues instrumented for with the passage of referenda in prior years. Panel B shows the effect of a one-dollar increase in taxes on contributions in subsequent years calculated using the estimates in Panel A.

6.1.1 Dynamic effects

Panel A of Table VI shows the effect of contemporaneous and lagged tax revenues on contributions estimated using Equations 2 and 4 as the first and second stages respectively. β_k represents the effect of tax revenues per student k years earlier on contributions per student. These estimates are not significantly different from zero at any point, and they rule out large effects of contemporaneous or lagged revenues on contributions. However, they are difficult to interpret. When manipulating taxes, policy-makers are likely to change more than only one-year's taxes.

Using these parameters, Figure VI shows the effect of a permanent one-dollar increase in taxes on contributions in the subsequent years. In the year of the tax change, the change only influences contributions through its effect of contemporaneous taxes. One year after the tax change, it can influence contributions through both contemporaneous and the prior year's taxes. As in the estimates presented in Section 6.1, there is no evidence of crowd out. However, the confidence interval quickly expands as more parameters are involved in the calculation. Panel B of Table VI shows the point estimates associated with this graph.

6.2 Interpretation

The estimate in Table V clearly indicates that government revenues have at most a small impact on contributions. However, this comes from an increase in government revenues per student, approximately \$256, which dwarfs the average level of contributions per student, approximately \$35. Since donors cannot reduce their contributions below zero, is there a limit to the level of crowd-out that I could have found?

A naive estimate suggests that the maximum amount of possible crowd-out would be 13.7 cents per dollar. The largest amount of crowd-out in the 95% confidence interval for my estimate, 1.22 cents, is small relative to a dollar of government revenue, less so when compared to a 13.7-cent maximum.

However, this does not accurately represent the maximum possible level of crowd-out. The first stage of the empirical strategy estimates the difference in tax revenues between districts that passed and those that failed referenda. The 13.7 cents calculation assumes that \$256 difference in tax revenue comes entirely as an increase in revenue among those whose referenda passed and that districts whose referenda failed remain at the status quo. In this case, then under total crowd-out, districts that passed referenda would reduce contributions to zero and those whose referenda failed would remain at their status quo, resulting in estimated crowd-out of 13.7 cents.

If instead, districts whose referenda passed remain at their status quo and the \$256 difference came entirely as a decrease in revenue among those whose referenda failed, then it would be possible to observe full dollar for dollar. In fact, there would be no limit on the level of crowd-out that it would be possible to estimate. The truth is inbetween these extremes. In many cases real tax revenue will decline if the referenda fails. In some cases, a prior tax is expiring. In other cases the law is written to require frequent votes to maintain constant funding in real dollars.

I present two other results indicating that the lack of crowd-out is not due simply to low

Table VII: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions excluding zeros.

	No Zeros	Above Median Contr
Loc. tax rev. PS	0.0083	0.0296
	(0.0136)	(0.0284)
Bandwidth	7.54	6.69

Note: Column 1 excludes districts with no contributions in the year before the referenda (12.9%) and Column 2 also excludes those with below median contributions. Otherwise estimates are the same as in Table V.

initial levels of contributions. If there truly was crowd-out but the relative dearth of contributions prevented detecting it, I would expect there to be greater crowd-out among districts with higher levels of contributions prior to their referendum. There is no evidence this is the case. Limiting estimation to districts with positive, or above the median contributions does not impact the result. Table VII presents fuzzy regression discontinuity results of the effects of local tax revenues on private contributions with these restrictions. Neither moves the point estimates towards crowd-out in an appreciable way.

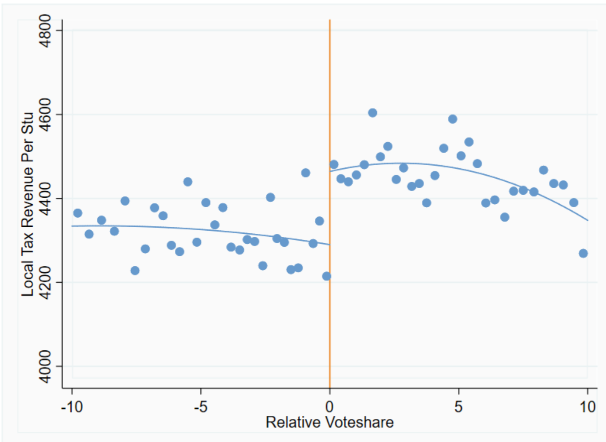
7 Conclusion

These results suggest that sharp increases in tax funding for schools led to at most very small changes in private contributions to them. I can reject crowd-out greater than 1.22 cents per dollar of increased taxes, and any crowd-in larger than 2.8 cents. In this setting, increased tax funding for schools comes directly from the pocketbooks of local residents. It is surprising then, that they do not cut back on their contributions to the same schools.

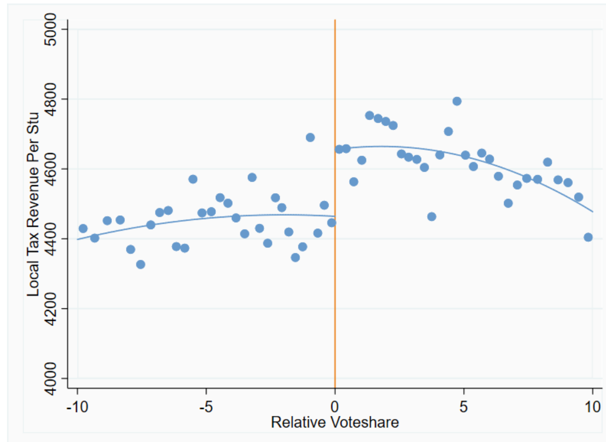
These results show that when school districts modify their budgets, private contributions will not change. This is the case with modest changes to school budgets. Dramatic changes to school finances, like those involved with large-scale school finance reforms may produce larger responses. However, in terms of the marginal changes that districts make annually, school districts need not worry that their funding decisions will have a perceptible impact on the donations they receive from parents and other community members.

This result stands in contrast to prior research that finds non-negligible crowd-out. These papers find that a dollar of additional government funding crowds out private spending by 20 cents (Hungerman 2005), 70 cents (Andreoni and Payne 2011b), one dollar (Andreoni and Payne 2011a), or 3 cents (Gruber and Hungerman 2007).

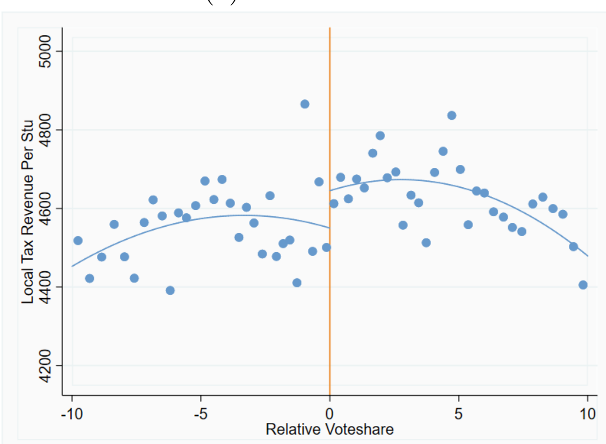
Variations in the settings of these studies that offer potential explanations for the differing results. Andreoni & Payne (2011b; 2011a) show that estimates of crowd-out differ depending on the source of funds. Most, or in some cases all, of the estimated crowd-out comes from foundation and non-profit behavioral responses rather than directly from donors. In fact, while Hungerman (2005) finds that charitable activities were crowded out he finds no evidence that donations were. In this setting, there are fewer institutional players between donors and the public goods. School contributions typically come to schools through parent teacher organizations or local education foundations. These are small, single purpose entities.



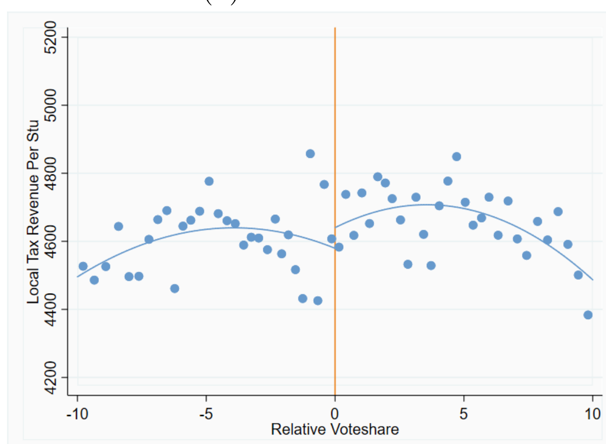
(a) 1 Year After



(b) 2 Years After



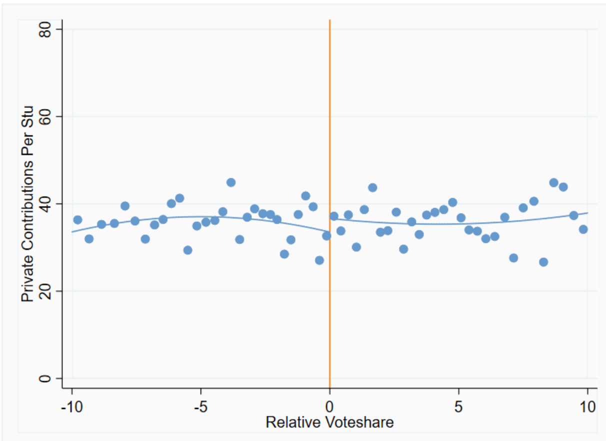
(c) 3 Years After



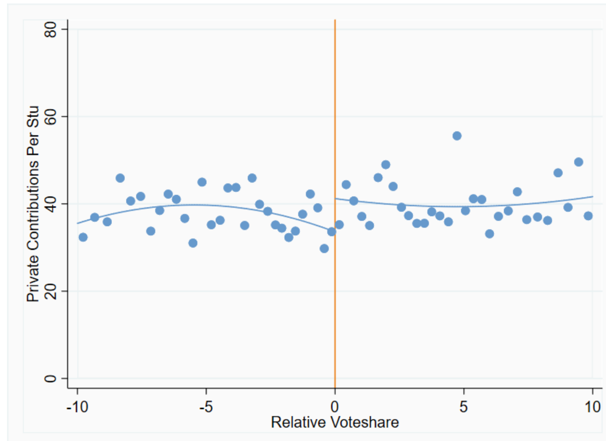
(d) 4 Years After

Figure IV: Effect of referenda passage on local tax revenue per student.

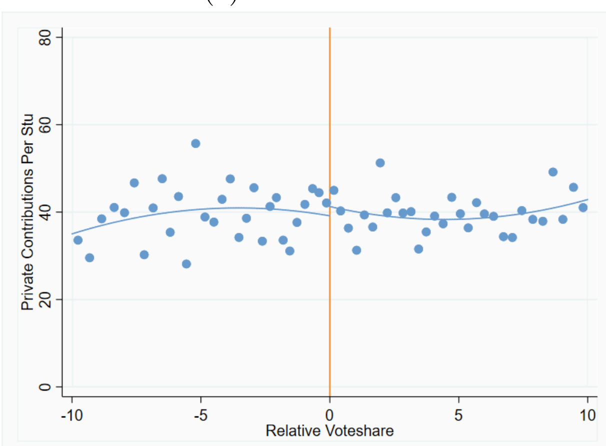
Note: These figures graphically present the first stage impact of referenda passage on local tax revenues per student, γ_1 in Equation 2. To construct these figures, I use the panel dataset described in Section 5 and regress local taxes per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then split the observations into quantiles of vote shares, calculate the mean residual in each bin, and add back the mean level of local tax revenues per student. Lines represent quadratic polynomials fitted to these residuals.



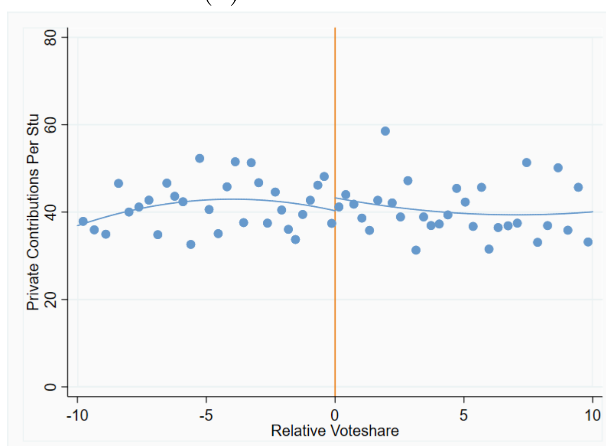
(a) 1 Year After



(b) 2 Years After



(c) 3 Years After



(d) 4 Years After

Figure V: Contributions by vote share following a referenda

Note: These figures graphically present the reduced form impact of referenda passage on contributions, γ_1 and γ_2 in Equation 2, but with the outcome of contributions per student. To construct these figures I use the panel dataset described in Section 5 and regress contributions per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then split the observations into quantiles of vote shares, calculate the mean residual in each bin, and add back the mean contributions per student.

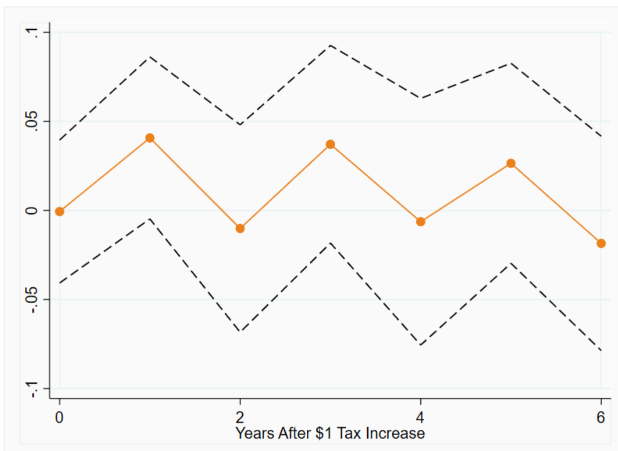


Figure VI: Dynamic Impact of $1 Increase in Taxes on Contributions

Note: Figure shows the effect of a one-dollar increase in taxes on contributions in subsequent years. Estimates are using the parameters given in Panel A of Table VI.

Appendices

A Further tests of RD validity

In section 5, I present estimates of the discontinuity in pre-referenda fiscal outcomes. Here, I provide additional "placebo test" results using the full panel data estimation strategy discussed in Section 3. I alter the date of the referenda to be two or more years prior to its actual date and estimate the first stage model. Table A1 shows estimates from this analysis. Only 1 of 20 estimates show weak evidence of a discontinuity at the threshold of the subsequent referenda.

Table A1: Placebo test estimates of passing a referendum on prior outcomes

	Exp. PS	Curr. Exp PS	Cap. out. PS	Loc. tax rev. PS	Contr. PS
$win_1^{\tau=1}$	144.2	-6.8	126.4	36.6	0.3
	(131.0)	(37.6)	(113.1)	(37.0)	(2.4)
$win_1^{\tau=2}$	23.2	-16.5	27.8	2.2	3.7
	(144.3)	(38.5)	(124.2)	(37.3)	(2.4)
$win_1^{\tau=3}$	-97.6	-52.7	-16.3	-40.7	6.7
	(131.4)	(36.0)	(114.4)	(35.6)	(2.6)*
$win_1^{\tau=4}$	-90.1	-51.5	-41.6	-25.6	-0.2
	(142.2)	(32.0)	(125.4)	(34.9)	(2.8)

Note: Regression discontinuity estimates of the effect of passing a referendum as if the referenda had occurred τ years prior to its actual date. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

B Effects of state versus local revenues

Table A2: Effect of passing a referendum on expenditures and revenues by state.

	Yr of ref	1 yr. ltr	2 yrs ltr	3 yrs ltr	4 yrs ltr
Mich Exp. PS	264	1,260	1,558	545	-137
	(308)	(432)***	(785)**	(884)	(669)
Mich Cap. out. PS	262	953	1,231	417	-72
	(240)	(376)**	(659)*	(696)	(550)
Mich Curr. Exp PS	117	23	-27	-64	-108
	(116)	(140)	(129)	(183)	(178)
Mich Loc. tax rev. PS	-67	44	-307	-224	-171
	(94)	(122)	(289)	(227)	(241)
Minn Exp. PS	-42	546	1,734	687	214
	(290)	(333)	(451)***	(413)*	(435)
Minn Cap. out. PS	-85	224	958	427	-371
	(237)	(293)	(377)**	(338)	(349)
Minn Curr. Exp PS	19	261	579	277	524
	(115)	(93)***	(115)***	(160)*	(174)***
Minn Loc. tax rev. PS	-2	27	337	246	261
	(72)	(85)	(98)***	(111)**	(137)*
Ohio Exp. PS	379	678	1,034	1,117	-17
	(170)**	(177)***	(193)***	(275)***	(285)
Ohio Cap. out. PS	299	493	831	893	-143
	(146)**	(149)***	(160)***	(228)***	(250)
Ohio Curr. Exp PS	3	95	110	99	75
	(36)	(45)**	(52)**	(53)*	(58)
Ohio Loc. tax rev. PS	93	286	299	120	66
	(38)**	(45)***	(54)***	(63)*	(68)
Wisc Exp. PS	279	1,483	1,121	-611	-229
	(177)	(274)***	(345)***	(370)*	(294)
Wisc Cap. out. PS	158	1,188	888	-885	-440
	(144)	(265)***	(327)***	(328)***	(232)*
Wisc Curr. Exp PS	95	145	110	192	196
	(91)	(102)	(100)	(112)*	(136)
Wisc Loc. tax rev. PS	59	326	205	138	132
	(80)	(87)***	(100)**	(116)	(120)

Note: Regression discontinuity estimates of the effect of passing a referendum. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Table A3: Effect of passage on expenditures and revenues by state and type of referenda

	Yr. of ref	1 yr. ltr	2 yrs. ltr	3 yrs. ltr	4 yrs. ltr
Minn, Non-Bond, Cap. out. PS	-107	-341	227	311	-216
	(284)	(285)	(272)	(380)	(376)
Minn, Non-Bond, Curr. Exp PS	3	256***	608***	287*	504***
	(131)	(95)	(122)	(173)	(190)
Minn, Non-Bond, Loc. tax rev. PS	-31	-18	228**	71	126
	(87)	(99)	(103)	(111)	(146)
Minn, Bond, Cap. out. PS	203	2,677***	3,507**	793	-1,329
	(466)	(891)	(1,488)	(1,002)	(1,132)
Minn, Bond, Curr. Exp PS	140	235	490	170	767*
	(261)	(281)	(305)	(452)	(457)
Minn, Bond, Loc. tax rev. PS	144	319**	897***	1,297***	1,135***
	(111)	(145)	(260)	(329)	(302)
Ohio, Non-Bond, Cap. out. PS	377*	434**	158	-58	-125
	(218)	(213)	(199)	(215)	(230)
Ohio, Non-Bond, Curr. Exp PS	7	105*	165**	198***	160**
	(46)	(57)	(67)	(67)	(73)
Ohio, Non-Bond, Loc. tax rev. PS	140***	286***	287***	77	10
	(51)	(57)	(67)	(80)	(89)
Ohio, Bond, Cap. out. PS	116	563***	1,982***	2,386***	-289
	(98)	(135)	(266)	(419)	(447)
Ohio, Bond, Curr. Exp PS	4	77	15	-60	-50
	(55)	(66)	(77)	(82)	(85)
Ohio, Bond, Loc. tax rev. PS	9	281***	303***	186**	149
	(52)	(76)	(92)	(93)	(94)
Wisc, Non-Bond, Cap. out. PS	-75	27	-676	-676*	-89
	(236)	(365)	(476)	(343)	(350)
Wisc, Non-Bond, Curr. Exp PS	118	320	217	246	310
	(186)	(203)	(182)	(196)	(251)
Wisc, Non-Bond, Loc. tax rev. PS	89	267	55	-22	52
	(155)	(163)	(179)	(215)	(219)
Wisc, Bond, Cap. out. PS	354**	2,185***	2,359***	-855**	-685**
	(156)	(347)	(423)	(426)	(265)
Wisc, Bond, Curr. Exp PS	50	-7	-7	80	80
	(70)	(78)	(87)	(101)	(113)
Wisc, Bond, Loc. tax rev. PS	-16	286***	215**	145	105
	(66)	(83)	(98)	(111)	(126)

Note: Regression discontinuity estimates of the effect of passing a referendum. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda X year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

C Alternative specifications of regression discontinuity

Table A5: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions with alternative bandwidths.

	Contr. PS
bw=2.41	0.0039
	(0.0240)
bw=3.41	0.0090
	(0.0205)
bw=4.41	0.0112
	(0.0131)
bw=5.41	0.0052
	(0.0140)
bw=6.41	0.0100
	(0.0108)
bw=7.41	0.0081
	(0.0104)
bw=8.41	0.0085
	(0.0107)
bw=9.41	0.0022
	(0.0105)
bw=10.41	0.0019
	(0.0101)
bw=11.41	0.0021
	(0.0089)
bw=12.41	-0.0004
	(0.0085)

* $p < 0.05$; ** $p < 0.01$

Note: Table V presents results with bandwidths set according to Imbens and Kalyanaraman (2011). This table shows the same models with alternative bandwidths.

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