Labor versus Capital in the Provision of Public Services: Estimating the Marginal Products of Inputs in the Production of Student Outcomes[1]

Ali Enami (corresponding author)
Department of Economics, The University of Akron, Akron, OH, 44325.
Email: aenami@uakron.edu

James Alm
Department of Economics, Tulane University, New Orleans, LA, 70118.
Email: jalm@tulane.edu

Rodrigo Aranda
Georgia Policy Labs, Andrew Young School of Policy Studies, Georgia State University, Atlanta, GA, 30303. Email: rarandabalcazar@gsu.edu

Highlights

- We estimate the effects of school expenditures on student outcomes in Ohio schools.
- We apply a dynamic regression discontinuity design to school funding referenda.
- Additional expenditures do not affect student outcomes in the *average* school.
- However, *operating* expenditures have a positive effect in *higher poverty* schools.

Abstract

This paper uses data on Ohio school districts to estimate the short- and long-term impact of different types of school expenditures on student outcomes. Our identification strategy employs a dynamic regression discontinuity design that relies upon the exogenous variation in public school funding created by marginally approved or failed local referenda to fund Ohio schools. We find that additional school expenditures on operating, minor capital, and major capital categories do not have a statistically significant effect on the student test scores of the *average* public school. Importantly, however, *operating* expenditures have a large and statistically significant impact on student performance in *higher poverty* school districts. We also examine possible channels (e.g., class size, attendance, discipline, and teachers' compensation) through which each type of expenditure may affect outcomes, and we find that teachers' compensation is the only channel that is affected by additional operating and minor capital expenditures.

Keywords: public school funding, operating expenditure, minor and major capital expenditures, student achievement, school district referendum.

JEL Classifications: H75, I20, R51.

---

[1] We are grateful to Alan Barreca, Douglas Harris, Nora Lustig, Douglas Nelson, Jonathan Pritchett, Steven Sheffrin, Benjamin Sperisen, and participants in the Murphy Institute seminar series at Tulane University, the Southern Economic Association Conference, Texas Christian University, Rowan University, and The University of Akron for their valuable comments on earlier versions of this paper. We are especially grateful to David Brasington and Adam Isen for generously sharing their databases with us and to Michelle Beaulieu for her excellent research assistance. Financial support from the Department of Economics and the School of Liberal Arts at Tulane University, as well as the Institute for Humane Studies at George Mason University, is acknowledged. The views expressed here are solely ours and do not necessarily represent the views of the institutions with which we are affiliated.

1. Introduction

Human capital is one of the most important determinants of success in the job market (Becker, 1964; Goldin & Katz, 2009). Educational systems produce human capital by combining various inputs with the available educational technology. We expect that, as with other production systems, an increase in inputs in the educational system will lead to a higher level of output in the form of human capital. The main purpose of this paper is to examine the empirical evidence for this relationship in the public school system, by examining the impact of various types of school expenditures on student test scores in Ohio public schools.[2] Our identification strategy employs a dynamic regression discontinuity design that relies upon the exogenous variation in public school funding created by marginally approved or failed local referenda to fund Ohio schools. We find that there is no short- or long-term impact on student test scores of additional operating expenditures or of capital expenditures for the *average* Ohio public school districts in our sample. We do find evidence that additional operating expenditures leads to higher compensation for the instructional staff, but this effect does not translate into better student outcome. Importantly, however, we also find that school districts in *poorer* neighborhoods see a statistically significant and positive effect from additional operating expenditures. While our parameter estimates for the impact of various types of school expenditures are not always statistically different from zero for an average school district in Ohio, the point estimates for major construction projects are consistently smaller, negative, and more often statistically significant. We also examine possible channels by which additional expenditures may affect student outcomes, and our estimates suggest that additional teachers' compensation is the most likely channel.

The relationship between various types of school expenditures and student outcomes has important policy implications. Public schools in the Unites States are mostly funded through local property taxes, which means that wealthier districts have more resources that poorer districts to put towards education. This variation in funding has led to an inequality of opportunity for students (Jennings, 2012), which in turn leads to a lower proportion of children from lower income families moving into higher income brackets (Breen, 2010; Brown, 2013). One possible reform is a centralized school funding system that provides all school districts with identical per pupil allowance might solve this problem, but such a system is not politically viable in many states,

[2] School expenditures are proxies for different inputs (labor versus capital) in the human capital production function of schools, and test scores are a common proxy for the main outcome of these inputs on human capital.

much less across the whole country. Another possible reform is to focus federal and state resources on programs/projects that have the most impact on improving the educational attainment of students in disadvantaged school districts. However, this reform requires estimates of the impacts of different categories of school expenditures on student outcomes, along with information on the channels through which these expenditures work. This is our purpose here. By examining the effects of various types of expenditures (i.e. operating, minor capital, and major capital expenditures) on educational outcomes and the channels through which these effects take place, we aim to provide policymakers with valuable information on the trade-offs of investing in different programs and projects.

Previous empirical studies show mixed results on the relationship between financial resources and student outcomes. The Coleman report (Coleman et al., 1966) was the first attempt to study the role of "school quality."[3] This report found that, on average, school quality has only a small effect on student achievement; the socio-economic status of the student's family is a much more important determinant. However, this report also found that the least economically advantaged groups benefit the most from higher school quality. Follow-up studies have found evidence both for the Coleman report (Hanushek, 1986; Cellini, Fereira, & Rothstein, 2010; Neilson & Zimmerman, 2014; Hong 2016; Martorell, Stange, & McFarlin, 2016) and against it (Greenwald, Hedges, &Laine, 1996; Card & Krueger, 1996; Jackson, Johnson, & Persico, 2016). Even meta-analyses disagree about which side of the argument has produced more reliable evidence in support of their case (Hanushek, 1994; Hedges et al., 1994). In an attempt to better understand the true effects of school expenditures on student outcome, some studies have shifted their focus to long-term outcomes such as income in adulthood. These results have also been inconclusive (Card & Krueger, 1996), although more recent papers have tended to find that school spending does indeed lead to better educational outcomes and eventually to higher incomes (Jackson, Johnson, & Persico, 2016; Hyman, 2017).

Despite this extensive body of work and its many important contributions, there are some shortcomings in previous work. One issue is the inability of most previous work to estimate the impact of specific categories of school expenditures on student outcomes. Even recent studies

[3] In this report, the quality of schools is measured based on "…curriculums offered [(i.e. academic, commercial, and vocational)], school facilities such as textbooks, laboratories, and libraries, such academic practices as testing for aptitude and achievement, and the personal, social, and academic characteristics of the teachers and the students bodies in the schools" (Coleman et al., 1966).

estimate the impact on student outcomes either of total expenditures only (e.g., Jackson, Johnson, & Persico, 2016; Kogan, Lavertu, & Peskowitz, 2016; Lavertu & St. Clair, 2017) or of major capital expenditures only (e.g., Cellini, Fereira, & Rothstein, 2010; Hong & Zimmer, 2016; Martorell, Stange, & McFarlin, 2016). To our knowledge, our paper is the first to provide causal estimates of the effects of each of the three major school spending categories - operational expenditures (e.g., teacher salaries and supplies), minor capital expenditures (e.g., maintenance and painting), and major capital expenditures (e.g., buildings and major renovations) on student outcomes.

Relatedly, and more importantly, much of the earlier studies were unable to identify the causal impact of expenditures on outcomes, relying mainly on simple correlational analysis. The most important one is the correlational nature of most studies, specially the earlier works, that analyze the impact of school spending on student outcomes (Jackson, Johnson, & Persico, 2016). More recent work has often addressed this issue (Cellini, Fereira, & Rothstein, 2010; Hong & Zimmer, 2016; Jackson, Johnson, & Persico, 2016; Martorell, Stange, & McFarlin, 2016; Hyman, 2017). Even so, the identification strategy of these studies remains controversial. In a perfectly designed experimental study, one would randomly assign school districts to different expenditure levels in each category and then compare their resulting student outcomes. This is of course not feasible, but one can mimic such an experiment with a quasi-experimental approach that relies on marginally approved or rejected school funding referenda in a regression discontinuity design (Cellini, Fereira, & Rothstein, 2010). This is what we do here. Ohio laws require school districts to rely on local referenda to change the amount of revenue that they receive through local resources, and state law also requires districts to specify the purpose of additional requested funding. The results of these referenda are not random. For instance, schools that badly need additional funding might be more likely to win a referendum for new funding. However, by restricting our attention to referenda where the margin of victory is small, one gets results that are almost random; that is, there is little difference between a district that wins a referendum by a few votes and one that loses a referendum by a few votes. This quasi-random assignment of funding to districts means that we are able to identify the causal effect of the additional funding (and additional funding for different purposes) on student outcomes.

Most previous studies did not identify specific channels by which education expenditures may affect student outcomes. Our regression discontinuity design also allows us to estimate the possible channels through which additional funding for various purposes affects student outcomes. These possible channels include: class size (student to teacher ratio), student attendance rate, discipline (disciplinary actions per 100 students), and average expenditure on instructional staff.

Previous work generally focuses on an *average* school. We are able to estimate the effect of extra funding on an average school, as well as on schools located in relatively *poorer* neighborhoods and schools located in relatively *richer* ones. Moreover, throughout our analysis, we account for the heterogenous treatment assignment (i.e., variation in the amount of per-pupil funding that different types of schools receive following a successful referendum).

Finally, most previous work did not consider the effects of increased school expenditures on student outcomes over time. Since school funding may not affect student outcomes immediately, we look at both short-term (1 year) and long-term (up to 5 years) effects.[4] The main practical challenge of studying the long-term effect is that those school districts that marginally reject additional local school funding (i.e. the control group) may approve it in the next referendum. Such non-compliance would lead to attenuation bias, so that it would appear as if the extra funding has no effect on the educational outcome in the long-run. To account for the dynamic nature of school funding, we use a modified version of the estimation technique originally proposed by Cellini, Fereira, and Rothstein (2010), a dynamic regression discontinuity design. In their paper, the effect of approving additional funding for a school district in year t affects student outcomes in year $t+\tau$, both directly and indirectly through its effect on the probability of approving other funding proposals between these two points in time. These other proposals also have a direct and an indirect effect on student outcomes in year $t+\tau$, which we can consider and estimate. Our modification to the original dynamic regression discontinuity design proposed by Cellini, Fereira, and Rothstein (2010) is to allow for the school district to receive funding for various types of school expenditures (i.e., operating, minor capital, and major capital expenditures). The methodology section of this paper discusses this technique in greater detail.

Overall, then, the main contributions of our paper to the literature are several. To the best of our knowledge, ours is the first paper that gives causal estimates of the effects on student outcomes of different expenditure types in the public school system, and provides these estimates for different types of school districts both in the short run and in the long run. Indeed, while we limit

[4] In the case of major capital expenditures, we also look at effects up to 10 years following the approval of a project by voters.

our analysis in this paper to three types of expenditures – operating, minor capital, and major capital expenditures – one can easily extend this framework to accommodate a more granular breakdown of the types of school expenditures. Our paper also examines various channels through which each type of funding may affect student test scores. Previous work either did not examine any channels or did so in a limited way.[5]

Throughout our analysis, the student outcome that we examine is the district-level math proficiency rate[6] in school years 1996-97 through 2014-15.[7] These tests are administered to 9th graders prior to 2006 and 10th graders afterward.[8] Although our parameter estimates are mainly positive, we do not find a statistically significant effect for the additional operating and minor capital expenditures in either the short- or the long-term. However, our parameter estimates for major construction projects are generally negative and more often statistically significant, pointing to no impact for this type of expenditure on student outcomes, a result that is in line with Cellini, Fereira, and Rothstein (2010) and Mortell et al. (2016) who do not find any significant effect of additional major capital expenditures on test scores in California or Texas public schools.[9] Moreover, we find that additional expenditures on instructional staff are positively affected by both operating and minor capital expenditures (which suggests the diversion of funding from maintenance projects); this result is in line with Fryer (2013) and Ree et al. (2018), who do not find a causal relationship between financial incentives for teachers on student outcomes. We also find that additional funding for school districts, regardless of whether it is for operational, minor capital, or major capital expenditures, has a negligible effect on class size, attendance rate, and students' discipline. Finally, we find that additional operating expenditure has a much greater and statistically significant effect on school districts located in higher poverty school districts, between three and ten times of the average effect depending on the year. This finding is similar to that of

[5] That is, the examined channels were limited to a very few ones, sometimes only one, such as the classroom quality, or the researchers examined various channels only in the context of major capital expenditures.

[6] The Ohio Department of Education categorizes students in five groups based on their test scores: "limited", "basic", "proficient", "accelerated", and "advanced". Throughout this paper, we calculate the proficiency rate based on those students who score "proficient" or above in a test. The minimum percentage of correct answers necessary to be proficient in a subject varies slightly for different subjects and from one year to another, but it is generally about 42 percent.

[7] We use this specific test score because it is available for more years.

[8] We normalize the math proficiency rates in each year to account for the changes in tests and the grades of students who took them.

[9] Some studies have found statistically significant effects of building schools on educational outcomes in the U.S. (e.g., Hong and Zimmer, 2016) and in developing countries (e.g., Duflo, 2001).

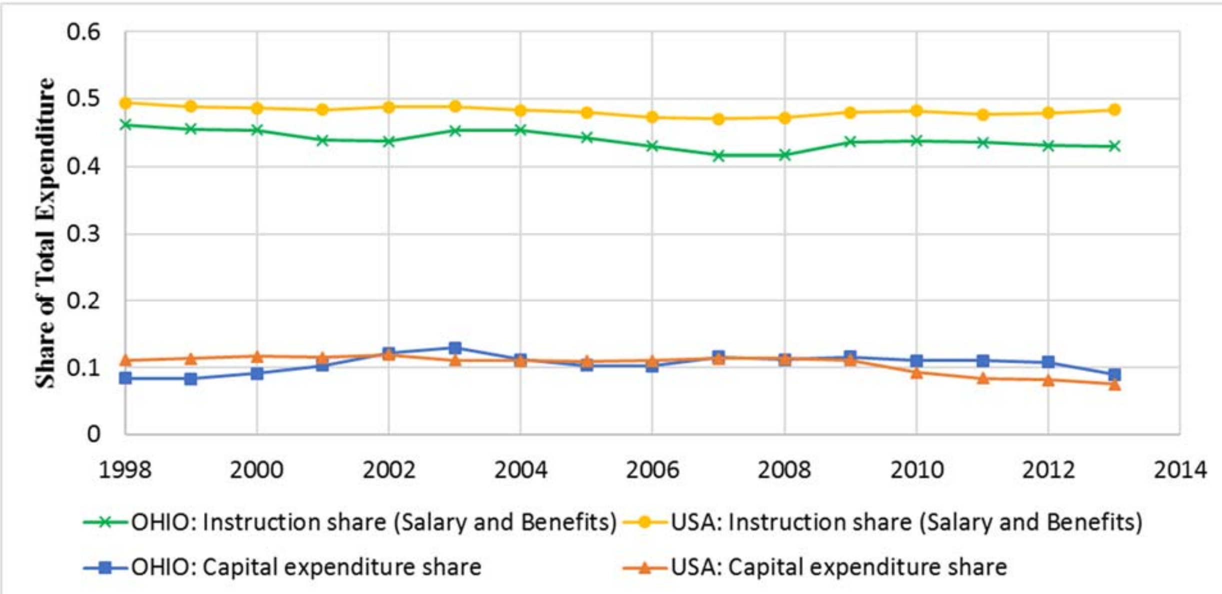
Jackson, Johnson, and Persico (2016) and Lafortune, Rothstein, and Schanzenbach (2018) and contrary to that of Hyman (2017).

The rest of this paper is organized as follows. Section 2 reviews the various aspects of public school funding in Ohio. Section 3 describes the empirical methodology of this study and provides a detailed description of the adapted dynamic regression discontinuity design utilized in this paper. Section 4 introduces the data sources along with descriptive statistics and the results of the parallel trend tests. Section 5 presents the results of the preferred model along with various robustness checks. Section 6 concludes with suggestions for future research.

2. Public School Funding in Ohio

In the 2015-16 school year, public schools in Ohio spent about $9,915 per pupil (Ohio Department of Education (ODE), 2017). Between 1991 and 2013, Ohio has been ranked about 19th on average among 50 states and the District of Columbia in per pupil expenditures (NCES, 2017). About 10 percent of the total school expenditures in Ohio has been consistently spent on construction projects, school maintenance, and renovations, around 45 percent is spent on the salary and benefits of the instructional staff, and the remaining 45 percent is divided between expenditures on non-instructional staff, supplies and supports, and "non-specified" expenditures spent on (NCES, 2017). As Figure 1 shows, Ohio is very similar to the rest of the U.S. in terms of the share of expenditures on capital structures versus instructional staff.

Figure 1. Capital expenditure and spending on the instructional staff in Ohio vs. the United States as a share of total expenditure

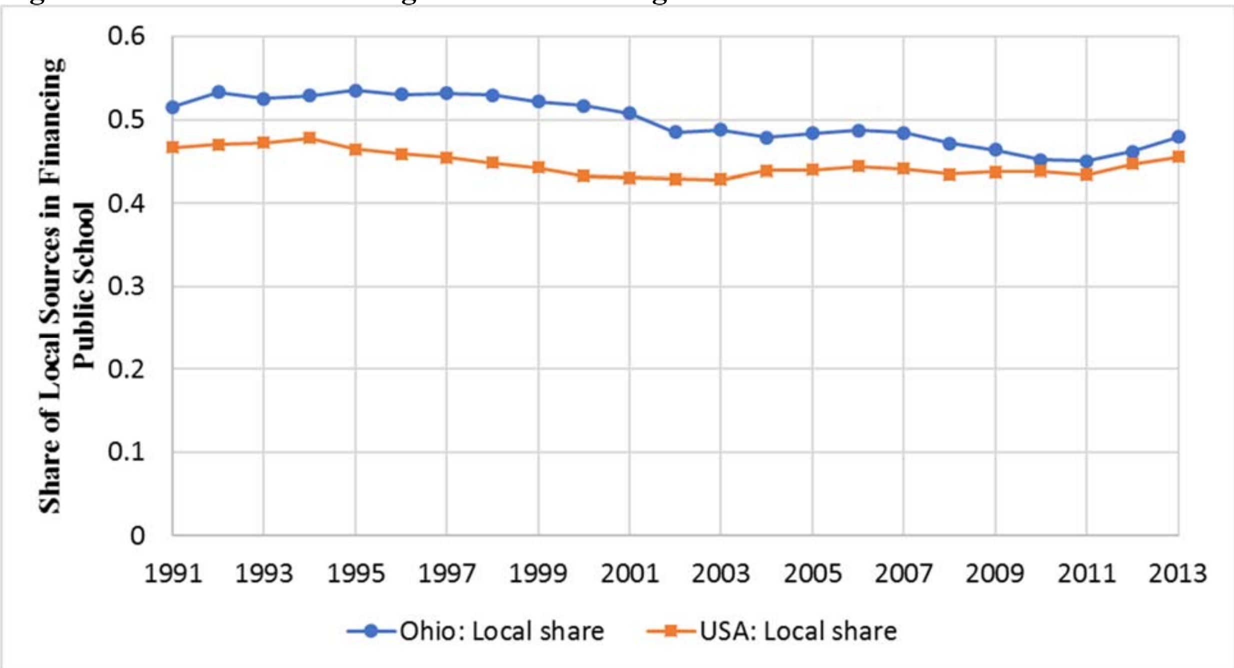


Source: U.S. Department of Education, National Center for Education Statistics, Common Core of Data (CCD), and own calculations.

As of March 2017, Ohio has 612 traditional school districts that cover the whole state and are mainly funded through the local sources.[10] As Figure 2 shows, about 50 percent of the total school budget in Ohio has been consistently financed by local sources, which makes this state representative of the U.S. as a whole (NCES, 2017). The remaining 50 percent is divided between resources provided through the state (40 percent) and the federal government (10 percent). The federal funding for schools in Ohio is similar to the rest of the country in that resources are allocated through nation-wide programs such as No Child Left Behind or the American with Disability Act.

[10] We limit the domain of this study to these traditional school districts. As of March 2017, Ohio has 49 joint vocational school districts, and approximately 370 public community schools (i.e. charter schools) that are not included in this analysis.

Figure 2. Share of school budget financed through the local sources



Source: U.S. Department of Education, National Center for Education Statistics, Common Core of Data (CCD) and own calculations.

State funding for schools in Ohio has experienced various changes over the years and has been the subject of four state Supreme Court litigations.[11] The main approach has been to fund schools based on a per-pupil formula that takes into account the ability of school districts to raise taxes (ODE, 2017).[12] This is known as the "foundation" formula, which determines how much funding a school district must receive through the state government to achieve the minimum per pupil funding (also determined by the state government). The problem with this approach is that legislators use the available budget to determine the minimum per pupil revenue that a school district should receive and not the actual needs of students, which usually exceed the available budget. Historically, this approach has resulted in the under-provision of funding for low-income districts and the creation of one of the most unequal school funding systems in the U.S. (Pittner, Carleton, & Casto, 2010). Four Ohio Supreme Court decisions all found the school funding system

[11] For a good review of the historical and recent approaches to school funding in Ohio, see Pittner, Carleton, and Casto (2010)

[12] The only exception to this main approach happened in fiscal years 2010 and 2011, in which an "Evidence Based Model" was used. This model identified the needs of students in different groups in financial terms and then allocated money to schools based on the population of students in these groups. This model was abandoned following the election of Governor John Kasich in 2011.

to be unconstitutional, but did not force a specific new system.[13] As a result, the main problem with the system (i.e. its reliance on local funding) has remained unchanged, and Ohio was still ranked high (19th) in terms of inequality in its public-school funding in 2009 (Pittner, Carleton, & Casto, 2010). However, these litigations have had some positive impacts, such as the creation of the Ohio School Facilities Commission in 1997, which provided matching funds for districts to build new schools (OFCC, 2017).[14]

Local funding for school districts in Ohio is provided largely through property and income taxes. In 2017, 190 school districts in Ohio use income taxes (0.25 percent to 2 percent) in addition to the property taxes to support their mission (ODT, 2017). School districts can also issue bonds in order to finance major capital investments and (additional) property taxes are used to pay back the borrowed money and interest. Ohio laws require any income tax in excess of 1 percent municipal tax and any property tax that exceeds an aggregate 10 mills per dollar valuation of a property to be subjected to the decision of voters, and a single majority vote is sufficient to pass a new tax (OSOS, 2017).[15] The school districts are required to identify the "purpose" of a tax, which allows us to determine whether the purpose of a tax is to fund a type of "operational expenditure" versus "minor capital expenditure". The main keywords for categorizing a proposal as "operating expenditure" are "operating expenses", "current expenses", "current operating expenses", "avoid an operating deficit", and "educational services". Similarly, the main keywords to categorize a proposal as "minor capital expenditure" are "construction", "(general) permanent improvement", "renovate", "(classroom) maintenance", "facilities", and "improvements". A negligible number of proposals have keywords from both categories or an unclear purpose, and they have been categorized as both operating and minor capital expenditures. Bonds are always classified as a "major capital expenditure". Appendix A provides examples for a property tax levy (hereafter "levy"), a bond, and an income tax proposal. Appendix A also presents year-by-year summary

[13] The cases are known as *DeRolph* (I through IV) *vs. State,* and were litigated between 1997 and 2002 by the Supreme Court of Ohio. The first case was filed in a lower court in 1991.

[14] As of February 2017, the share of the state in these projects ranges from 12 percent to 98 percent in various projects, depending on the wealth of the district. There is also one project that is funded 100 percent by the state. School districts are ranked based on their need, and they have 13 months from the time that their project is approved to raise their local share (through bond referendums). If a school district fails to raise its local share within this time limit, it is considered a "lapsed" district, but it can still receive the funding at a later time if it gets the necessary vote to raise the local share of the funding for its project (OFCC, 2017).

[15] Practically, all new property taxes that have passed in the past two decades are in addition to this 10 mills threshold.

statistics for all school district referenda in Ohio from 1996 to 2015 for bonds, levies, and income tax referenda.

What makes Ohio special compared to other states is 1976 legislation known as House Bill (HB) 920. According to HB 920, any property tax approved by voters is frozen based on the valuation of a property in the year of referendum, and therefore the dollar value that is received by a school district remains constant for the duration of the approved levy. As a result, HB 920 has forced district boards of education in Ohio to use new referenda regularly (and also to rely on the income tax) to keep up with the financial needs of their districts. This feature, i.e., the existence of numerous referenda, allows our parameter estimates to have lower standard errors. We discuss our methodology in the next section.

3. Empirical Methodology

School funding that is provided through local referenda has a unique dynamic. Although one can rely on referenda that are marginally approved or rejected by voters (i.e. a sharp regression discontinuity) as a way to create a quasi-randomized assignment of funding to school districts, those school districts that fail to receive funding may ask for it again in the next period and receive it then. As a result, there is a non-compliance issue that is quite different from a usual fuzzy regression discontinuity design that cannot be addressed by the techniques devolved for this type of regression discontinuity.[16]

Without accounting for the dynamic nature of the school funding through referenda, one can estimate the intent-to-treat (ITT) effect by simply comparing the educational outcome of school districts that receive extra funding in referenda with small vote margins (i.e. the treatment group) to school districts that fail to receive additional funding in similar other close referenda (i.e. the control group). Unfortunately, ITT estimates are biased toward zero due to the non-compliance problem mentioned above. The main goal of this section is to show how one can go from the easily estimated ITT effects to the more relevant but complex treatment-on-the-treated (TOT) effects. The latter is the effect of school funding on student outcomes in the absence of the non-compliance issue. In other words, the TOT effect shows how much extra funding affects various educational

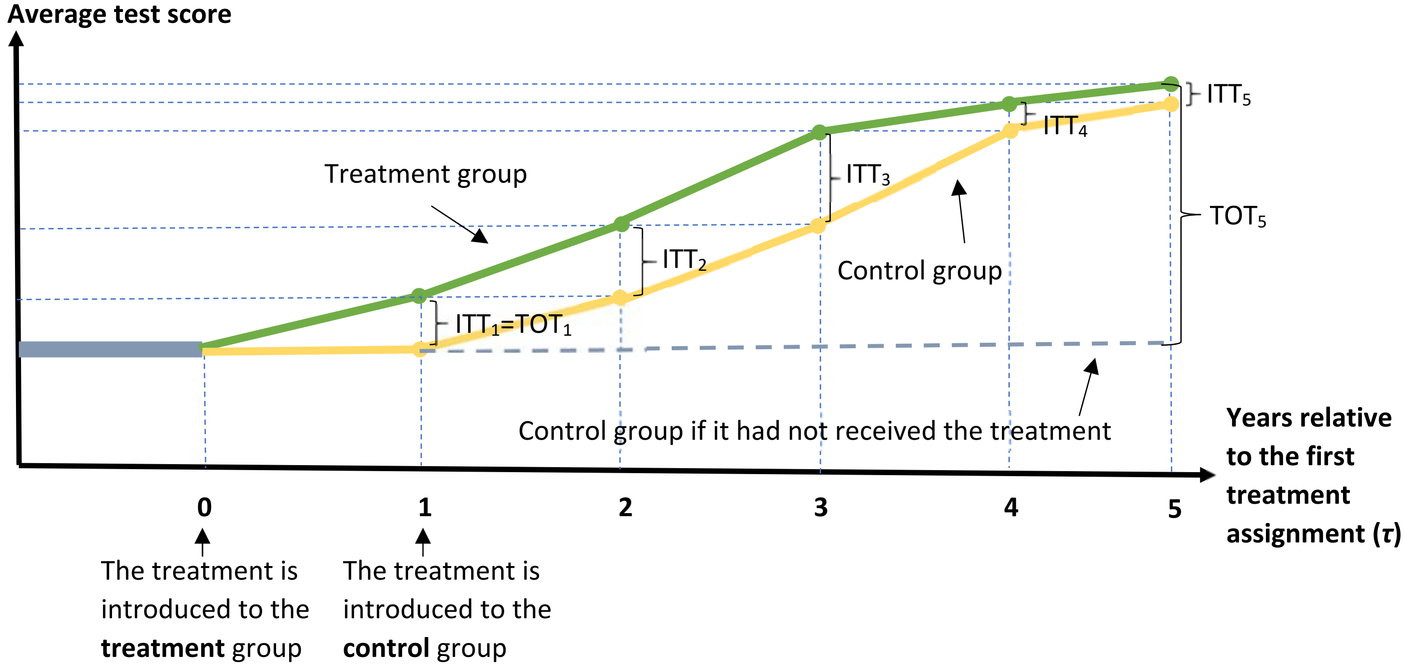
[16] The type of non-compliance that the usual fuzzy regression discontinuity models address is the one that happens at the time of treatment assignment. The non-compliance in the case of school funding mainly happens in the years following the year of treatment assignment.

outcomes if one could randomly assign school districts to the treatment and control groups and prevent the schools in each group from leaving their designated group.

In order to evaluate the TOT effect of school funding on student outcomes, we utilize an extension of the dynamic regression discontinuity design (dynamic RDD), proposed by Cellini, Fereira, and Rothstein (2010). Figure 3 provides a visual representation of their dynamic RDD model developed. Assume there are two groups of school districts that are otherwise equivalent. One group (i.e. the treatment group) receives extra funding in year zero, while the other group (i.e. the control group) receives the same funding with a one-year delay. Assume there is no other funding available in the following years. If the effect of funding on educational outcome only depends on the number of years that has passed since the treatment (and not the calendar year that the treatment takes place)[17], the control group follows the "same path" as the treatment group, as shown in Figure 3. In all years following the initial treatment, the usual models can only detect the difference between the treatment and control group at the end of that year (i.e. the ITT effects). However, one can construct the TOT effect for a given year by adding up the ITT effects between year zero and the year of interest. Figure 3 shows, for example, how TOT_5 (i.e. TOT effect at the end of the fifth year) is constructed from ITT effects in that year and all previous years.

[17] This is the core assumption of our estimation technique. It is a reasonable assumption in the context of school funding because there is no reason to believe that, everything else equal, more funding in one year has a different effect comparing to other years that is not captured in the calendar year and relative year fixed effects that are included in our preferred model.

Figure 3. The relationship between the Treatment-on-the-Treated (TOT) estimator and the Intent-to-Treat (ITT) estimators



Note: The treatment-on-the-treated (TOT) estimator in the long run is equal to the sum of intent-to-treat (ITT) estimators (assuming the control group receives a similar treatment one year later than the treatment group and it is otherwise similar to the treatment group).

In our example, we have zero compliance in the control group and throughout this six-year period none of the two groups receive any additional funding beyond the one time that they receive it (in year zero or one). In practice, we account for all deviations from this simple example, and so the TOT estimator then becomes equal to the weighted sum of these ITT effects with weights being the probability of receiving more funding in the future conditional on receiving additional funding today.

The extension of this dynamic RDD model utilized in our paper accounts for the fact that schools may receive different types of funding as opposed to one specific type of expenditure. Other authors have commonly studied major capital expenditures to build new school facilities (Cellini, Fereira, & Rothstein, 2010 Hong and Zimmer, 2016; Martorell, Stange, & McFarlin, 2016). In our paper, we divide the types of school funding into three major categories: operational expenditures (O), major construction projects (C), and smaller capital improvements and maintenance (S). However, one can extend our model to include a more detailed break-down of the types of funding that a school district receives, something that is not possible with our current database.

Equation (1) shows a simple model that relates the outcome of interest to different types of potential treatments:[18]

$$Y_{j,t+\tau} = k_\tau + \alpha_\tau O_{j,t} + \beta_\tau C_{j,t} + \gamma_\tau S_{j,t} + u_{j,t+\tau} \quad \forall \tau \geq 0 \qquad \textbf{(1)}$$

where $Y_{j,t+\tau}$ is an output of interest in school district *j* measured τ years after the time of treatment assignment (i.e. *t*), k_τ is a constant term representing the average value of the outcome of interest in the schools that receive no funding τ years after the time of treatment assignment (i.e. *t*), and $u_{j,t+\tau}$ is the error term. $O_{j,t}$, $C_{j,t}$, and $S_{j,t}$ are three indicator variables associated with the type of additional expenditures authorized by the voters at time *t* for school district *j*. These indicator variables are defined as follows:[19]

$$O_{j,t} = 1(District\ j\ approves\ additional\ operational\ expenditures) \cong 1(v_{j,t}^{O} > v^*)$$

$$C_{j,t} = 1(District\ j\ approves\ additional\ major\ capital\ expenditures) \cong 1(v_{j,t}^{C} > v^*) \qquad \textbf{(2)}$$

$$S_{j,t} = 1(District\ j\ approves\ additional\ minor\ capital\ expenditures) \cong 1(v_{j,t}^{S} > v^*)$$

where $v_{j,t}^{O}$, $v_{j,t}^{C}$, and $v_{j,t}^{S}$ are the vote share in favor of approving additional operational expenditures, additional major, and minor capital expenditures, respectively and v^* is the required threshold vote share in favor of a type of expenditure to be considered passed. In Ohio, this threshold is 50 percent.

In this framework, if the assignment of funding was random, then α_τ, β_τ, and γ_τ would be the causal effects of interest that show how much additional funding for various types of expenditures affects the outcome of interest τ years after the time of treatment assignment. This is generally not the case since the votes cast for or against a measure on the ballot are not random on the two extremes of vote distribution (e.g., 10 percent versus 90 percent). However, this is not a problem if there is some exogenous variation in the vote shares (Cellini, Fereira, & Rothstein 2010).

[18] Our notations are borrowed from Cellini, Fereira, and Rothstein (2010).
[19] The use of the “approximately equal” sign accounts for some practical aspects of the data. For example, assume that a school district has two measures on the ballot in one period to increase its operational expenditures. Assume also that one of them is approved with 100 to 99 votes while the other one is rejected by 101 to 98 votes. Since one of the measures is approved, the value of $O_{j,t}$ is one, but the vote share (calculated using the average values of votes for and against the measures) is less than 0.5 (i.e. 198/398).

Specifically, for referenda that are close to the threshold vote share, one can argue that it is almost as good as a random assignment for a school district to end up on one side (and receive additional funding) as on the other side (and not receive any funding).

Following Cellini, Fereira, and Rothstein (2010), we use a regression discontinuity technique that retains all of the referenda but also accounts for the variation that comes from the referenda not close to the threshold vote share by adding a polynomial of order g in vote shares, i.e., $P_{g,\tau}(v_{j,t}^{O}, v_{j,t}^{C})$.[20] As Cellini, Fereira, and Rothstein (2010) show (and we extend their mathematical derivation in Appendix B), the addition of this polynomial makes the main variables of interest independent of the error term. Therefore, $O_{j,t}$, $C_{j,t}$, and $S_{j,t}$ can be identified using the regression model in Equation (3):

$$Y_{j,t+\tau} = k_\tau + \alpha_\tau^{ITT} O_{j,t} + \beta_\tau^{ITT} C_{j,t} + \gamma_\tau^{ITT} S_{j,t} + P_{g,\tau}(v_{j,t}^{O}, v_{j,t}^{C}, v_{j,t}^{S}) + \varepsilon_{j,t+\tau} \quad \forall\tau \geq 0 \qquad \textbf{(3)}$$

Note that ITT superscript is added to Equation (3) to emphasize in the interpretation of the estimated parameters that we do not control for any funding that the treatment and control school districts may receive in years between t and $t + \tau$.

While α_τ^{ITT}, β_τ^{ITT}, and γ_τ^{ITT} are unbiased in estimating the ITT effect of additional school funding on student outcomes, they are not estimated efficiently in Equation (3). The error term, $\varepsilon_{j,t+\tau}$, includes unobserved covariates that are variable at the district-referendum level but are constant over time. If one pools the data used to estimate different values of τ in equation (3) to estimate all of them together, various fixed effects can be added to the model to increase precision. The new model for this pooled data is: [21]

$$Y_{j,t,\tau} = k + \sum_{\tau=0}^{L}\left(\sum_{x=0}^{L}\left\{\left[\alpha_\tau^{ITT} O_{j,t} + \beta_\tau^{ITT} C_{j,t} + \gamma_\tau^{ITT} S_{j,t} + P_{g,\tau}(v_{j,t}^{O}, v_{j,t}^{C}, v_{j,t}^{S})\right][1(\tau = x)]\right\}\right) + \sum_{x=0}^{L} 1(\tau = x) + R_{j,t} + T_t + \varepsilon_{j,t,\tau} \qquad \textbf{(4)}$$

where $1(\tau = x)$ is an indicator variable identifying the data related to individual ITT models that were estimated separately in equation (3) for each τ (i.e., the "relative year fixed effects"), $R_{j,t}$ is

[20] Our preferred model uses a polynomial of order three.

[21] Our preferred model also includes some additional control variables to help with precision: a control variable for the amount of state funding allocated to the major capital projects in a school district, and three indicator variables to identify the type of the proposed tax, i.e. whether it is a new tax, renewal/replacement of an existing one, or removal/reduction of an existing one. Given the existence of a polynomial in the vote share, none of these variables is required for the identification of the effect of interest.

district-referendum fixed effects, and T_t is the calendar year fixed effects. Following Cellini, Fereira, and Rothstein (2010), in the pooled model we also add data from three years prior to a referendum, which helps with identifying various fixed effects in the model and increases precision.

The next step is to estimate TOT effects using these ITT effects. Mathematically, one can view the ITT parameters as the result of taking the total derivative of $Y_{j,t+\tau}$ with respect to $O_{j,t}$ (i.e., $\alpha_\tau^{ITT} = \frac{dY_{j,t+\tau}}{dO_{j,t}}$), $C_{j,t}$ (i.e., $\beta_\tau^{ITT} = \frac{dY_{j,t+\tau}}{dC_{j,t}}$), or $S_{j,t}$ (i.e., $\gamma_\tau^{ITT} = \frac{dY_{j,t+\tau}}{dS_{j,t}}$). These total derivatives can be decomposed into a series of partial derivatives (each of which has a TOT interpretation), as shown in the following equation:

$$\begin{aligned}
\alpha_\tau^{ITT} &= \alpha_\tau^{TOT} + \textstyle\sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \pi_h^O) + (\beta_{\tau-h}^{TOT} * \pi_h^C) + (\gamma_{\tau-h}^{TOT} * \pi_h^S)] \\
\beta_\tau^{ITT} &= \beta_\tau^{TOT} + \textstyle\sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \varphi_h^O) + (\beta_{\tau-h}^{TOT} * \varphi_h^C) + (\gamma_{\tau-h}^{TOT} * \varphi_h^S)] \qquad \textbf{(5)} \\
\gamma_\tau^{ITT} &= \gamma_\tau^{TOT} + \textstyle\sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \omega_h^O) + (\beta_{\tau-h}^{TOT} * \omega_h^C) + (\gamma_{\tau-h}^{TOT} * \omega_h^S)]
\end{aligned}$$

where $\alpha_{\tau-h}^{TOT} = \frac{\partial Y_{j,t+\tau}}{\partial O_{j,t+h}}$, $\beta_{\tau-h}^{TOT} = \frac{\partial Y_{j,t+\tau}}{\partial C_{j,t+h}}$, and $\gamma_{\tau-h}^{TOT} = \frac{\partial Y_{j,t+\tau}}{\partial S_{j,t+h}}$ for $h \geq 0$. Moreover, $\pi_h^O = \frac{dO_{j,t+h}}{dO_{j,t}}$, $\pi_h^C = \frac{dC_{j,t+h}}{dO_{j,t}}$ and $\pi_h^S = \frac{dS_{j,t+h}}{dO_{j,t}}$ represent the (intent-to-treat) effects of approving a measure to only increase operational expenditures in year *t* on the approval of additional operational, major capital, and minor capital expenditures in year $t + h$, respectively. The terms $\varphi_h^O = \frac{dO_{j,t+h}}{dC_{j,t}}$, $\varphi_h^C = \frac{dC_{j,t+h}}{dC_{j,t}}$, and $\varphi_h^S = \frac{dS_{j,t+h}}{dC_{j,t}}$ as well as $\omega_h^O = \frac{dO_{j,t+h}}{dS_{j,t}}$, $\omega_h^C = \frac{dC_{j,t+h}}{dS_{j,t}}$, and $\omega_h^S = \frac{dS_{j,t+h}}{dS_{j,t}}$, have a very similar definition to π_h^O, π_h^C, and π_h^S. These values can be estimated using equation (4) and an appropriate left-hand side variable.[22] The right-hand side variables in equation (5) demonstrate channels through which approving additional operational (capital) expenditures in year t affects the outcome of interest in year $t + \tau$ directly (i.e., $\alpha_\tau^{TOT}/\beta_\tau^{TOT}/\gamma_\tau^{TOT}$), as well as indirectly through

[22] In practice, the values of $O_{j,t}$, $C_{j,t}$, and $S_{j,t}$ are determined based on the first time that a measure appears on the ballot for school district *j* to add additional funding for year *t*. This procedure generates a more conservative parameter estimate in case a school district tries to change its initial assignment to the control group (or the type of treatment) in follow up referenda for that specific year. However, the values of $O_{j,t+h}$, $C_{j,t+h}$, and $S_{j,t+h}$, which appear as the left-hand side variables, are determined based on the actual treatment assignment of a school district in year *t+h*.

affecting the probability of approving various types of future funding in years between t and $t + \tau$ (i.e., the terms included in the summations).

By rearranging equation (5) we can find the value of the TOT estimators. First, note that, when $\tau = 0$, the ITT, and TOT effects are the same because the control and treatment groups do not have time to change their treatment assignments. Therefore:

$$\alpha_0^{ITT} = \alpha_0^{TOT}, \quad \beta_0^{ITT} = \beta_0^{TOT}, \quad \gamma_0^{ITT} = \gamma_0^{TOT} \tag{6}$$

For any $\tau > 0$ we have:

$$\alpha_\tau^{TOT} = \alpha_\tau^{ITT} - \sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \pi_h^O) + (\beta_{\tau-h}^{TOT} * \pi_h^C) + (\gamma_{\tau-h}^{TOT} * \pi_h^S)]$$

$$\beta_\tau^{TOT} = \beta_\tau^{ITT} - \sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \varphi_h^O) + (\beta_{\tau-h}^{TOT} * \varphi_h^C) + (\gamma_{\tau-h}^{TOT} * \varphi_h^S)] \tag{7}$$

$$\gamma_\tau^{TOT} = \gamma_\tau^{ITT} - \sum_{h=1}^{\tau}[(\alpha_{\tau-h}^{TOT} * \omega_h^O) + (\beta_{\tau-h}^{TOT} * \omega_h^C) + (\gamma_{\tau-h}^{TOT} * \omega_h^S)]$$

Note that the Delta method produces the standard errors for these TOT parameters.

Up to this point, the treatment groups have been defined using an indicator variable for receiving additional operational and/or capital expenditures. Our model uses per pupil expenditure in thousands of 2015 dollars for the treatment variables (i.e. $O_{j,t}$, $C_{j,t}$, and $S_{j,t}$) to account for the heterogeneity of funding approved for different school districts.

4. Data

We use various data sources. The first and the most important one is the referendum reports from the Ohio Secretary of State Office (OSOS).[23] Ohio holds state-wide referenda multiple times a year. The main ones take place in May and November of each year with some special elections on February, March, and August. The referendum reports provide the list of issues on the ballot and the number of votes for and against it. Moreover, these reports provide a brief description of each issue with all necessary information to identify the purpose of the requested funding for a school district (i.e. operational versus capital expenditures), the dollar value of the requested

[23] Our data had to be extracted from these reports. Adam Isen generously shared part of the data base that he prepared for his paper and therefore reduced the amount of data entry for this paper. For more information about his work, see Isen (2014).

funding, and the period of the time that the new tax will be in place. All dollar values are expressed in 2015 dollars.

Appendix A presents year-by-year descriptive statistics of these funding proposals. Here we report an aggregated description of the referendum data in Table 1.[24] Note that for every district-year combination we only use the first time that a district requests extra funding in the form of additional taxes to commence in that year (if any) in order to identify the type of treatment assignment that it receives. For example, if a school district asks to levy a tax for a period of 5 years to begin in year 2010 in multiple referenda, the result of the first referendum is used to determine its treatment assignment. As mentioned before, this is to produce more conservative estimates in case some school districts in the control group (who do not receive any funding the first time they ask for it for a specific school year) were able to switch their assignment in follow up referenda before that specific school year ends.

Table 1 presents the summary statistics of the aggregated data based on the purpose of the issue on the ballot. The total number of referenda for operating expenditures is slightly higher than the combined number of referenda for various types of capital expenditures. On average, the size of a tax proposal to increase operational expenditures is about 3.7 times more than a proposal to increase minor capital expenditures in a year, or $1,016 and $276 per pupil expenditure in 2015 dollars, respectively. Major capital expenditures have an average (life-time) size of about $10,486 (in 2015 dollars) per pupil.

Additional operating expenditures are usually approved for a shorter period of time compared to the minor capital expenditures, 5 and 9 years respectively. Even so, about 26 percent of the proposals for additional operating expenditures and 24 percent of the proposals for additional minor capital expenditures do not have a time limit, and the related taxes are proposed to be collected in perpetuity. The average number of years to repay the principal and interest for the issued bonds related to major capital expenditures is about 26 years. The highest approval rate of referenda belongs to minor capital expenditures (67 percent), followed by operating expenditures (58 percent), and then major capital expenditures (46 percent). Finally, the percentage of close referenda in each category, defined as those with vote share between 45 and 55 percent, are 32

[24] Out of 612-613 school districts that are present in our data base for years between 1996 and 2015, we include 602 of them in our analysis to ensure comparability over time.

percent, 28 percent, and 37 percent for operating, minor capital, and major capital expenditures, respectively.

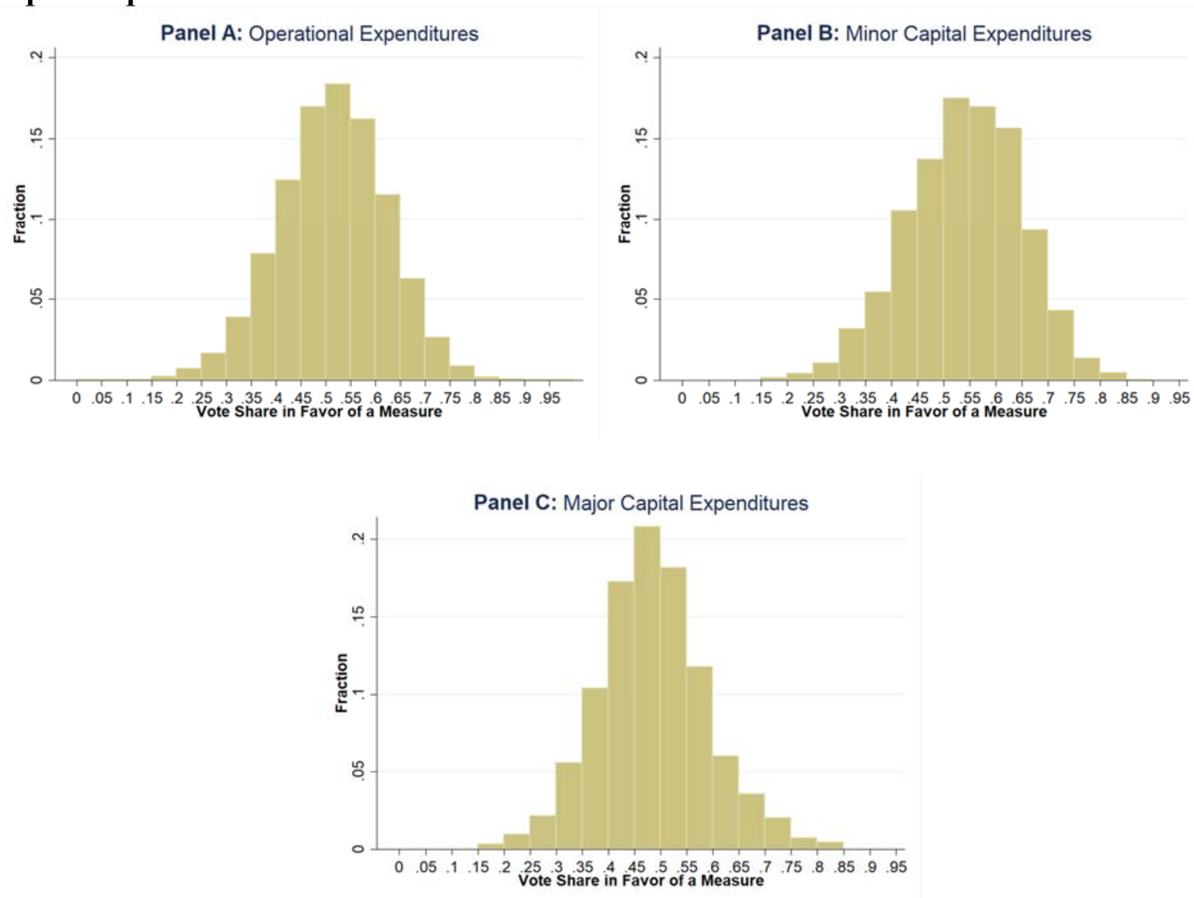
Table 1. Aggregated summary of the referendum data

Type of referenda	Number of unique school dist.	Number of referenda	Per pupil average size requested (2015$)	Length (year)	% CPT	% Pass	% Close
Operational Exp.	545	3350	$1,0167/year	5.43	26.60	58.39	32.15
Capital Exp.							
Minor	508	1452	$276/year	9.08	24.17	68.60	28.24
Major	523	1074	$10,486/life-time	25.68	0.19	45.72	37.43

Sources: Referendum reports from the OSOS and own calculations.
Notes: Only referenda related to the first time a school district requested additional funding in the form of a new tax to be commenced in a specific year are included. The data covers years from 1996 to 2015 for all 602 Ohio school districts included in this analysis. The total number of unique school districts with at least one referendum over this period is 594. Length refers to the average length of issues that are not considered for "continuing period of time (CPT)". For major capital expenditures, Length refers to the number of years it will take to repay the principal and interest of the issued bonds. "% CPT" refers to the percentage of the issues that are considered for CPT. "% Close" refers to the percentage of the referenda that have a vote margin between 45 percent and 55 percent (included) in favor of the issue on the ballot.

Figure 4 shows the distribution of vote share in favor of measures to receive additional funding for operational (Panel A), minor capital (Panel B), and major capital expenditures (Panel C). It is worth noting that the bell-shape of the vote distribution in all panels, as well as the similarity of the number of measures that are marginally rejected or approved, provides us with more confidence in our identification technique that relies on the randomness of the treatment assignment for schools that are close to the 50 percent vote margin. To make sure vote share distribution is continuous around the 50 percent threshold, we conducted the McCrary (2008) test and, at the 95 percent confidence level, we failed to reject the hypothesis that the vote share distribution is continuous around this threshold.

Figure 4. Distribution of vote share in favor of measures to spend more operational and capital expenditures



Sources: Referendum reports from the OSOS and own calculations.

Notes: The data covers years from 1996 to 2015 for 602 Ohio school districts included in this paper. The total number of unique school districts with at least one referendum over this period is 594. All school district referenda are included in these graphs. The total number of referenda included in Panels A, B, and C are 4831, 1891, and 1549 respectively.

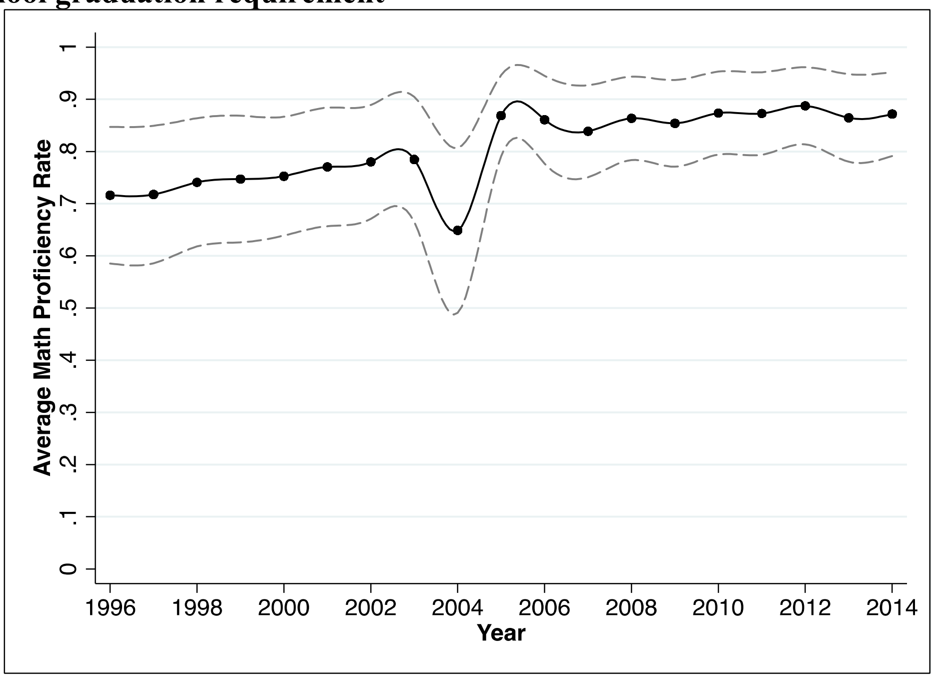
Our second main data base is the results of the math proficiency tests for students' subject to high school graduation tests. Prior to the school year 2005-06, the proficiency in 9th grade core courses was required for graduation. Starting in the school year 2005-06, the Ohio Graduation Test was introduced and required proficiency in 10th grade core courses. The high school graduation math proficiency data covers school years from 1996-97 to 2014-15.[25]

Figure 5 shows the school district average proficiency rates in math. Average math proficiency rates for Ohio appear to be constant over time for each grade but increase by about 10 percentage

[25] David Brasington generously shared his data for 1996-97 to 2004-05 period with us. For the application of this data in his work see, for example, Brasington and Haurin (2009). The data for 2005-06 forward is available through ODE website.

points between grades 9 and 10. The standard deviation also decreases for 10th graders. In earlier years, the value of standard deviation is about 13 percent, while in later years it reduces to about 8 percent. We normalize the math proficiency rates of a school district in each year to account for the fact that the test may have changed from one year to another and also that they are administered to students of different grades over this period of time.

Figure 5. Average math proficiency rate of school districts in Ohio for students' subject to the high school graduation requirement



Sources: David Brasington personal data base (1996-2004), ODE (2005-2014), and own calculations.
Notes: The data includes 602 Ohio school districts included in this paper. The year corresponds to the Fall semester of a school year. The dotted lines represent the standard deviation. Math proficiency rates for 1996-2003 is for 9th graders, 2004 is for 8th graders (due to the absence of data for 9th or 10th graders), and the rest is for 10th graders.

Other important variables that we use are the district level total per pupil expenditures, average expenditures on instructional staff, student to teacher ratio (all available through NCES), attendance rate, and disciplinary actions per 100 students, all available through ODE. Table 2 provides summary statistics for these variables. There is substantial variation in the size of school districts that are included in our analysis. While the average and median number of students in a school district are 2,850 and 1,787 respectively, there are school districts that serve as few as 67 and as many as 76,504 students. The average per pupil expenditure in 2015 dollars is about $12,000, while the average expenditure on instructional staff is about $96,000. Both variables have a significant variation across schools and over time. The average number of students per teacher is approximately 17. The variable ranges from only 4 students to 54 students per teacher. Attendance rate is the variable with the lowest range. Over the past decade, Ohio school districts experienced between 89 to 98 percent attendance rate, amounting to an average of 96 percent. Finally, the number of disciplinary actions per 100 students ranges from 0 to 261 disciplinary actions per 100 students. The average and median values, however, are about 16 and 10 actions per 100 students respectively, which shows most of school districts do not experience much trouble with their students. These results highlight the fact that school districts in Ohio are very different in terms of the type of students that they serve.

Table 2. District level descriptive statistics of main variables of interest

Variable	Years Included	Mean	Standard Deviation	Min	Max	Median
Number of Students	1997-2013	2,850	4,610	67	76,504	1,787
Per Pupil Expenditure ($2015)	1997-2013	11,879.90	3,933.61	1,664.04	147,527.73	10,866.16
Average Expenditure on Instructional Staff ($2015)	1997-2013	95,969.62	14,065.76	39,120.14	170,903.02	94,462.06
Student to Teacher Ratio	1997-2013	17.39	2.36	3.70	53.50	17.36
Attendance Rate (%)	2005-2015	96.13	1.67	89.00	97.50	97.50
Disciplinary Actions per 100 Students	2005-2015	15.93	20.47	0.00	260.90	9.70

Sources: NCES, ODE and own calculations.
Notes: The data includes 602 out of 612 Ohio school districts that are included in this paper. Year corresponds to the Fall semester of a school year.

We also need to examine whether the school districts that succeed in securing various types of additional funding (the treatment groups) and those who fail to do so (the control group) are otherwise similar. Specifically, one needs to test whether the treatment and control groups are balanced with respect to various outcome variables before they receive the extra funding.

Table 3 provides the results of the balance tests comparing the treatment and control school districts prior to treatment assignment. Each variable listed in the first column is measured in year t-1, and is separately regressed on a number of variables related to the treatment assignment in year t. Columns 2-4 show the type of treatment assignment in year t, i.e., receiving additional operating, minor capital, and major capital expenditures. Each regression includes a polynomial of order three in vote share for each type of requested funding (i.e., operating, minor capital, and major capital), three indicator variables to identify the type of the proposed tax (i.e., a new tax, renewal/replacement of an existing one, or removal/reduction of an existing one), and school district and year fixed effects.

Table 3. Balance tests comparing the treatment and control groups prior to treatment assignment

Variable	Operating Expenditures	Minor Capital Expenditures	Major Capital Expenditures
Standardized Math Score	-0.02428	0.02247	0.00201
	(0.01759)	(0.05675)	(0.00250)
Proficiency in Reading: 10th Grade	-0.01738	0.00271	-0.00386
	(0.01813)	(0.06067)	(0.00444)
Value Added: Composite	0.26402	0.63538	0.03347
	(0.28482)	(0.80936)	(0.08149)
Value Added: Math	0.31746	0.57289	0.03765
	(0.34261)	(0.74941)	(0.08570)
Total School Expenditure	0.19205*	-0.14487	-0.02661*
	(0.11350)	(0.33409)	(0.01495)
Expenditure on Teachers	-0.51957*	0.54290	-0.06209
	(0.26814)	(1.00993)	(0.04570)
Pupil to Teacher Ratio	-0.05960	-0.01962	-0.00638
	(0.06504)	(0.20555)	(0.01153)
Attendance Rate	0.00037	-0.00226	0.00007
	(0.00055)	(0.00167)	(0.00011)
Disciplinary Action per 100 Students	-0.55049	2.04050	-0.00503
	(0.54432)	(1.93347)	(0.11164)
Percentage of New Students	-0.04683**	-0.00340	-0.00405
	(0.02199)	(0.03902)	(0.00367)
Percentage of Female Students	-0.00022	-0.00030	0.00008
	(0.00100)	(0.00266)	(0.00019)
Percentage of non-white students	-0.00187	0.00330	-0.00008
	(0.00127)	(0.00331)	(0.00026)

4-year Graduation rate	0.00741	0.02144	0.00008
	(0.00459)	(0.01525)	(0.00071)
Child Poverty Rate	0.00085	0.00164	-0.00005
	(0.00128)	(0.00324)	(0.00028)
Total Enrollment	5.76573	-39.44295	-0.60984
	(30.72255)	(83.77827)	(5.09609)
Percentage of Dropouts	0.00049	-0.00144	0.00001
	(0.00150)	(0.00290)	(0.00027)
Percentage of Students with Free or Reduced Price Lunch	0.00235	-0.00044	0.00015
	(0.00182)	(0.00677)	(0.00029)

Sources: NCES, ODE, ODT, OSOS, Census, David Brasington personal data base for district level math proficiency rates, and own calculations.

Notes: Standard errors are clustered at the school district level. *, **, and *** represent significance levels of 10 percent, 5 percent and 1 percent, respectively. The data includes 602 out of 612 Ohio school districts that are included in this paper. Standardized Math Score is for students that are subjected to the Ohio high school graduation tests and are standardized for each year. All other variables, expect for proficiency in reading and graduation rate, cover all students in a school district. Each variable listed in the first column is measured in year t-1 and is separately regressed on a number of variables related to the treatment assignment in year t. Columns 2-4 show the type of treatment assignment in year t, i.e., receiving additional operating, minor capital, and major capital expenditures (in per pupil thousands of 2015 dollars). Each regression includes a polynomial of order three in vote share for each type of requested funding (i.e., operating, minor capital, and major capital), three indicator variables to identify the type of the proposed tax (i.e., a new tax, renewal/replacement of an existing one, or removal/reduction of an existing one), and school district and year fixed effects.

As the coefficient estimates in Table 3 reveal, the treatment and control groups are similar with regard to almost all variables included here before a referendum takes place. The only exceptions are: total school expenditure, which is statistically significant at the 90 percent level for operating and major capital expenditures, expenditure on teachers (significant at the 90 percent level for operating expenditures), and percentage of new students (significant at the 95 percent level of confidence for operating expenditures). Given the number of concurrent hypotheses that are tested here, it is expected that a very few of them turn out to be statistically significant by sheer accident. Therefore, we believe that it is safe to conclude that school districts in the treatment and control groups are similar prior to the treatment assignment.

In addition to the balance tests of variables related to the school districts listed in Table 3, we conducted a series of balance tests with respect to the comparability of referenda that were approved or rejected. These tests are necessary to assure that referenda that were approved were not systematically different from those that were rejected. We tested for the size of the requested funding, the length of the tax proposal, the percentage of referenda requested for the continuing period of time (CPT), the type of proposal being “new or additional tax”, and the type of proposal being “renew or replacement tax”. The results of this analysis are presented in Appendix C and show that the characteristics of proposals are generally similar.

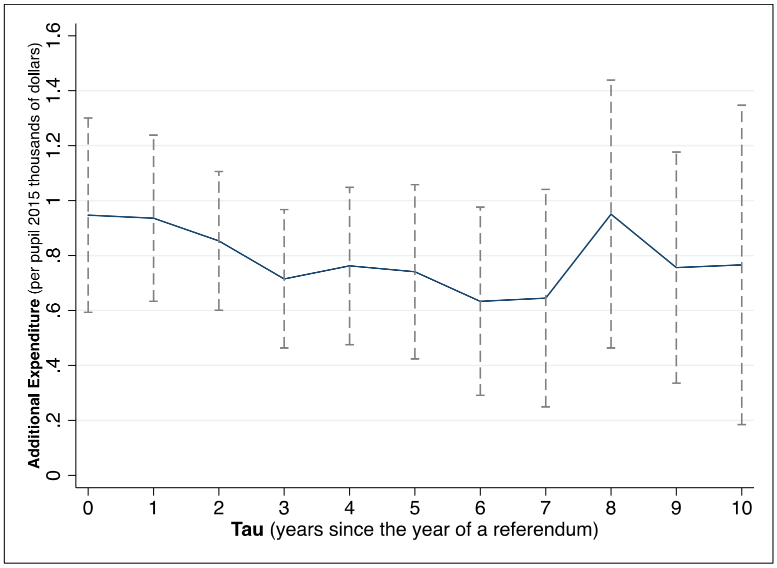
5. Results

Before presenting the main results, it is important to examine how approving various types of additional funding affects the total expenditure in a school district. Figure 6 shows these effects for 10 years following a reform in three graphs using simple ITT models of the form shown in equation (3). Panel A shows that approving an additional operating expenditure (measured in per pupil thousands of 2015 dollars per year) increases the total per pupil expenditure by slightly less than the approved size of a referenda (about 21 percent less), and the effect remains relatively constant over time. Panel B shows that approving additional funding for minor capital projects (measured in per pupil thousands of 2015 dollars per year) leads to an increase in the total per pupil expenditure that exceeds the size of the reform (about 70 percent more). This is expected, as school districts may combine available funds from state and federal governments for renovation with the funding that they receive through additional local taxes. Panel C shows that approving an additional $1,000 for a major capital project (measured in 2015 dollars) leads to an additional total per pupil expenditure close to the approved extra funding over the course of five years following the reform (i.e. year zero through four, in which year zero is the year of commencement for an approved additional tax).[26] Overall, these graphs show that the approval of additional taxes by voters leads to a real increase of a similar size in total school expenditures.

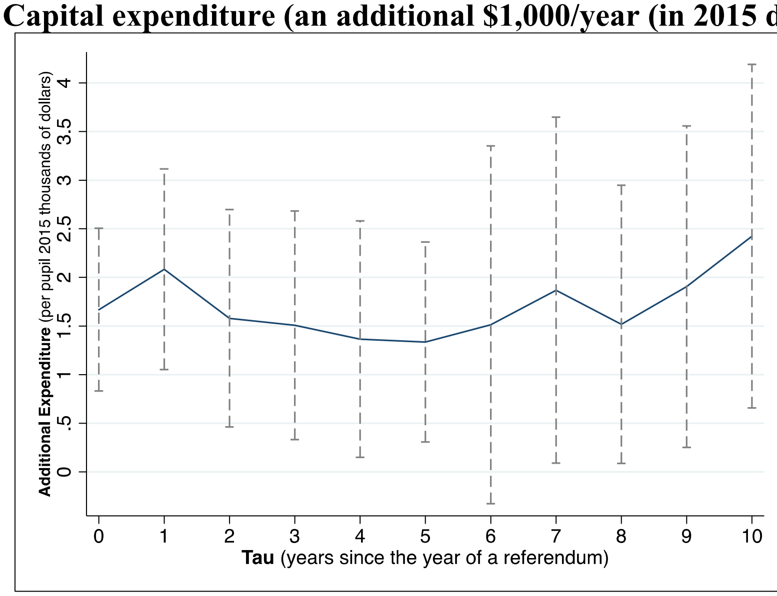
[26] Note that this effect comes solely from the local sources since the availability of data allows the model to control for the additional matching fund that a school district receives from the state for its major capital projects.

Figure 6. The effect of approving various types of additional funding for school districts on per pupil total expenditure

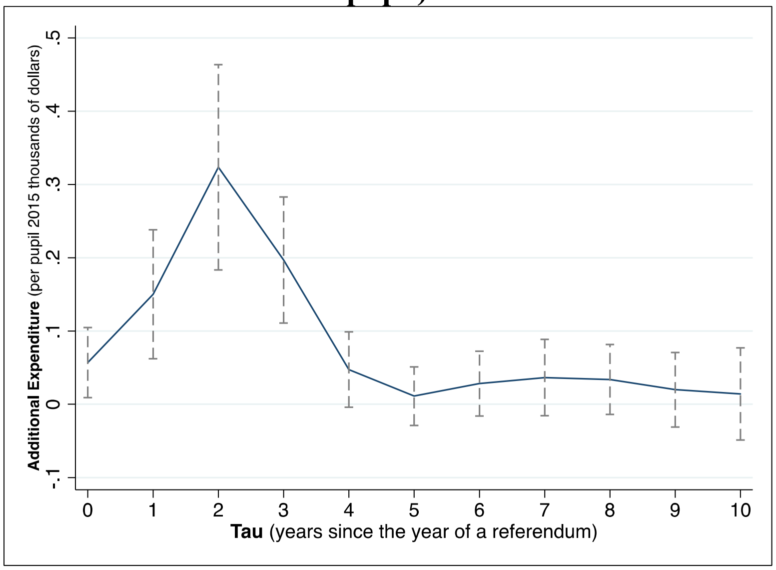
Panel A) Operating expenditure (an additional $1,000/year (in 2015 dollars) per pupil)



Panel B) Minor Capital expenditure (an additional $1,000/year (in 2015 dollars) per pupil)



Panel C) Major Capital expenditure (an additional $1,000/project (in 2015 dollars) per pupil)



Sources: NCES, OSOS, ODT, and own calculations.

Notes: The data includes 602 Ohio school districts of which 594 had at least one referendum between 1996 and 2015. The graphs show simple ITT effects estimated separately for each Tau using equation (3). In each model, the dependent variable is the per pupil total expenditure (measured in thousands of 2015 dollars) Tau years following a referendum. The three main variables of interest graphed in Panels A, B, and C are the size of various additional expenditure approved for a school district (i.e. operating, minor capital, and major capital expenditures, respectively). These variables are also in per pupil thousands of 2015 dollars (the operating and minor capital expenditures are per year, while the major capital expenditure is per project). The solid line in each graph is the ITT effect and the dashed lines are the 95 percent confidence intervals. See the methodology section for the list of control variables included in the analysis.

Table 4 illustrates our main results, showing how approving various types of school expenditures affects the school district level math proficiency rates of students who take Ohio high school graduation tests. The main variables of interest are the size of the approved additional funding for various purposes, which are measured in per pupil thousand of 2015 dollars (per year for operating and minor capital expenditures and per project for major capital expenditures). In both ITT and TOT models, we find no statistically significant effect for additional operating, minor capital, and major capital expenditures at the 95 percent confidence level. Moreover, our ITT and TOT effects are often close to each other. The closeness of these two effects implies that either the passage of funding does not have that much of an impact on student outcomes or the follow up referenda are approved relatively similarly by the treatment and control groups. A priori, a researcher needs to account for the dynamic of school funding. However, once the two effects turned out to be similar, a researcher can rely on the biased but more precise ITT estimation as opposed to the unbiased but less precise TOT estimator.

As Appendix D shows, our ITT effects are robust to a change in methodology in which we estimate the impact of each type of expenditure separately, i.e., by eliminating variables related to other types of expenditure in equation (4). Moreover, our estimation shows that the approval of each type of expenditure mainly affects the approval of future additional expenditures of the same type in a statistically significant way. However, our parameter estimates for these relationships have large standard errors implying potential heterogeneity across school districts.

While we do not find any statistically significant effects for any type of school expenditure at the 95 percent confidence level, our preferred parameter estimates (i.e., TOT effects) are precise enough to be able to reject the existence of a "big" effect for any type of expenditure. Focusing on relative years (τ) in which we have the largest combination of effects size and standard error, we are able to reject an effect size of 0.065 standard deviation increase in math proficiency rate for an additional $1,000/year per pupil operating expenditure. Similarly, for minor and major capital expenditure (and at the 95 percent confidence level), we can reject effect sizes as big as 0.188 and 0.004 standard deviation.

Table 4. The impact of approving various types of additional funding on the school district level standardized math proficiency rate of students who are subject to the Ohio high school graduation tests

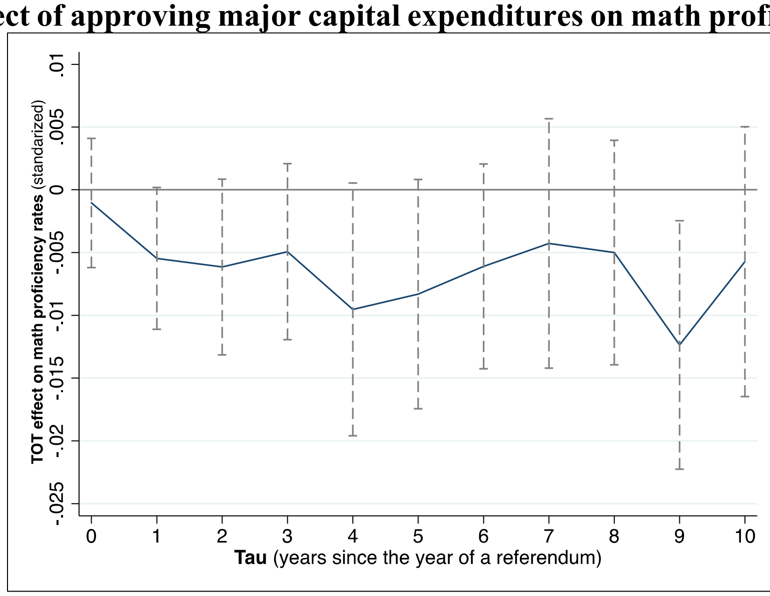
Approved type of expenditure	Years relative to the commencement year of a tax measure (τ)					
	τ = 0	τ = 1	τ = 2	τ = 3	τ = 4	τ = 5
	Panel A. ITT effects					
Operating exp.	0.01938	0.02682	0.01513	0.02179	0.02733	0.00862
	(0.01879)	(0.01885)	(0.02079)	(0.02159)	(0.02225)	(0.02410)
Capital exp.						
Minor	-0.02306	-0.01956	0.05076	0.00511	-0.08751	0.01185
	(0.06130)	(0.06657)	(0.05966)	(0.07184)	(0.08266)	(0.07910)
Major	-0.00107	-0.00537*	-0.00533	-0.00372	-0.00844*	-0.00673*
	(0.00271)	(0.00283)	(0.00347)	(0.00335)	(0.00495)	(0.00398)
	Panel B. TOT effects					
Operating exp.	0.01938	0.03020	0.02037	0.02044	0.01858	-0.00707
	(0.01879)	(0.02060)	(0.02278)	(0.02268)	(0.02242)	(0.02226)
Capital exp.						
Minor	-0.02306	-0.01106	0.06598	0.02278	-0.06837	0.01645
	(0.06130)	(0.06738)	(0.06201)	(0.07361)	(0.08453)	(0.07467)
Major	-0.00107	-0.00552*	-0.00618*	-0.00499	-0.00952*	-0.00829*
	(0.00271)	(0.00297)	(0.00368)	(0.00366)	(0.00526)	(0.00476)

Sources: NCES, ODE, ODT, OSOS, David Brasington personal data base for district level math proficiency rates, and own calculations.

Notes: Clustered standard errors (at the school district level) are in parenthesis. *, **, and *** represent significance levels of 10 percent, 5 percent and 1 percent, respectively. Parameter estimates represent the effect of approving additional expenditures (operating, minor capital, and major capital expenditures) for school districts (measured in per pupil constant-2015 thousands of dollars: per year for operating and minor capital expenditures and per project for major capital expenditures) on the standardized math proficiency rates of students subjected to Ohio high school graduation tests. The effects are measured in various points after the commencement year of a new tax ($\tau = 0$ represents the commencement year, $\tau = 1$ represents one year after the commencement, and so on). All parameters in Panel A are estimated in one regression model based on equations (4) (using $L = 6$). See the methodology section for the list of control variables. Parameters in Panel B are estimated using equation (7) and are based on the parameters estimated in Panel A.

Despite the fact that the parameter estimates for both types of capital expenditures are mainly statistically insignificant, it is worth noting that the major capital expenditures seem to have a consistent negative effect on test scores for at least the first six years included in this analysis. One may argue that major capital projects take a few years to be completed and one should not expect any positive effect until later years. Moreover, the fact that a school's administration is occupied with a major construction project provides a potential explanation for a short term negative effect (e.g., through the reduction in supervision). To assess whether the effect of building a school on test scores appear in later years, we expand the time horizon of the analysis to include up to 10 years following the commencement year of a referendum. To better visualize the result, Figure 7 presents the effect of approving a major capital project (measured in per pupil thousands of constant 2015 dollars) on the students' test scores. As is clear from Figure 7, the effect is never positive throughout the 10 years following the approval of a major construction project. Our finding is generally in line with those of Goncalves (2015) and Conlin and Thompson (2017) who also did not find a short term positive impact on test scores for major construction projects in Ohio. Similar to them, our parameter estimates are negative (but only marginally significant). However, our long term results stand in contrast to the findings of Conlin and Thompson (2017) who find a positive impact of capital stock on student test scores.

Figure 7. The effect of approving major capital expenditures on math proficiency of students



Sources: NCES, ODE, ODT, OSOS, David Brasington personal data base for district level math proficiency rates, and own calculations.

Note: The solid line is the TOT effect using equation (7), which relies on ITT values estimated using a model based on equation (3) (using L=10). See the methodology section for the list of control variables. The main variable of interest is the size of an approved major capital expenditure measured in per pupil thousands of constant-2015 dollars. The standardized math proficiency rates are for students subjected to Ohio high school graduation tests. The dashed lines represent 95 percent confidence intervals.

Our methodology also allows us to analyze various channels through which the additional funding for school districts may affect student achievement. We consider at four channels: student to teacher ratio, student attendance rate, prevalence of disciplinary actions, and average expenditure on instructional staff. Table 5 presents the results of this analysis, which shows that average expenditure on instructional staff (Panel D) is the only variable affected by the approval of additional expenditure and only in the case of operating and minor capital expenditures. The approval of a new tax to spend an additional $1,000 per pupil on operating expenditure leads to an average of $357 extra expenditure (although statistically insignificant) on the instructional staff one year following the commencement of the new tax. This effect increases dramatically in the second year following the commencement of the new tax to $868 and it becomes statistically significant. The effect continues to rise in the following years and reaches $1,504 in the fifth year following the commencement of a new tax.

The impact of additional minor capital expenditure on average expenditure on instructional staff is even higher. The parameter estimates range from $627 to $4,596, indicating either school districts redirect their newly received funding to pay for teachers or use their discretionary funds to pay for teachers as opposed to pay for expenses such as the maintenance of classes. Finally, we do not find any evidence for the impact of approval of additional funding for major construction projects on teacher's compensation.

It is worth noting that, despite the fact that both operating and minor capital expenditures increase the compensation of teachers, we do not observe any robust impact on student test scores, as discussed before.

Table 5. The impact of approving various types of additional funding on the school district level student-to-teacher ratio, attendance rate, disciplinary actions per 100 students, and average expenditure on instructional staff

Approved type of expenditure	Years relative to the commencement year of a tax measure (τ)					
	τ = 0	**τ = 1**	**τ = 2**	**τ = 3**	**τ = 4**	**τ = 5**
			Panel A. Student to Teacher Ratio			
Operating exp.	-0.01245	-0.00053	0.09172	0.07470	0.13113*	0.18608**
	(0.04970)	(0.06007)	(0.07313)	(0.07094)	(0.07292)	(0.07757)
Capital exp.						
Minor	0.07284	0.11313	0.36260	0.27939	0.41702	0.35054
	(0.13777)	(0.17799)	(0.28274)	(0.26502)	(0.26581)	(0.26850)
Major	0.00054	-0.00081	-0.00465	0.00296	0.00238	0.02248
	(0.00689)	(0.01124)	(0.01721)	(0.01893)	(0.02303)	(0.01978)
			Panel B. Attendance Rate (%)			
Operating exp.	-0.00026	-0.00020	0.00023	0.00042	0.00032	0.00056
	(0.00050)	(0.00051)	(0.00053)	(0.00056)	(0.00054)	(0.00056)
Capital exp.						
Minor	0.00255	0.00033	0.00359	0.00399*	0.00241	0.00372*
	(0.00234)	(0.00144)	(0.00228)	(0.00235)	(0.00229)	(0.00212)
Major	-0.00010	-0.00009	-0.00005	-0.00009	-0.00008	-0.00010
	(0.00009)	(0.00010)	(0.00013)	(0.00013)	(0.00015)	(0.00016)
			Panel C. Disciplinary Actions per 100 Students			
Operating exp.	-0.68260	-0.25799	0.31039	0.13867	0.52875	0.52909
	(0.54322)	(0.68208)	(0.71600)	(0.69144)	(0.69211)	(0.72670)
Capital exp.						
Minor	0.18835	0.69659	-1.43032	-2.05625	-1.32601	-1.41205
	(1.53432)	(1.50811)	(1.62322)	(1.60981)	(1.58024)	(1.74690)
Major	0.07795	0.00251	-0.00047	0.01890	-0.00379	-0.03635
	(0.09046)	(0.09982)	(0.14193)	(0.11422)	(0.12906)	(0.15056)
			Panel D. Average Expenditure on Instructional Staff (thousands of constant-2015 dollars)			
Operating exp.	0.03395	0.35686	0.86839**	1.06661***	1.33588***	1.50366***
	(0.27268)	(0.35415)	(0.35285)	(0.39439)	(0.41847)	(0.45140)
Capital exp.						
Minor	0.62660	2.74444***	2.18922**	3.72990***	4.59633***	3.21174*

	(0.84166)	(0.74817)	(0.98101)	(1.14066)	(1.70231)	(1.74494)
Major	0.00354	-0.02322	-0.01323	0.01169	-0.01625	0.00491
	(0.03894)	(0.04847)	(0.06276)	(0.06931)	(0.07319)	(0.08053)

Sources: NCES, ODE, ODT, OSOS, and own calculations.
Notes: Clustered standard errors (at the school district level) are in parenthesis. *, **, and *** represent significance levels of 10 percent, 5 percent and 1 percent, respectively. Parameter estimates represent the effect of approving additional expenditures (operating, minor capital, and major capital expenditures) for school districts (measured in per pupil constant-2015 thousands of dollars: per year for operating and minor capital expenditures and per project for major capital expenditures) on the dependent variable specified for each panel. The effects are measured in various points after the commencement year of a new tax ($\tau = 0$ represents the commencement year, $\tau = 1$ represents one year after the commencement, and so on). Parameters in each panel represent the TOT effects using equation (7) and a corresponding equation (4) (in which $L = 6$). See the methodology section for the list of control variables.

As for other outcomes, Panel A of Table 5 shows that, for the first three years after a tax is approved, there is no effect on the student to teacher ratio, and, in the 4th and 5th years after it is passed, this effect becomes statistically significant. To put this result in context, it implies that five years after approving a referendum for operating expenditures a \$1,000 increase led to a statistically significant increase of the student to teacher ratio equivalent to a 1 percent increase. Panel A also shows no statistically significant effect on student to teacher ratios for minor and major capital expenditures.

Panel B of Table 5 shows no effect of increasing operating, minor, and major expenditures on the attendance rate at the 95 percent confidence level. Similarly, Panel C shows no statistically significant effect of these expenditures on disciplinary actions per 100 students.

These results are quite robust to alternative specifications. We present two robustness checks in Table 6 and a number of other checks in Appendix D. First, we repeat the whole analysis for the reading proficiency rate of the students who are subject to the Ohio high school graduation tests, as shown on Panel A of Table 6.[27] Similar to our results regarding the math proficiency rate, none of the expenditure categories has a statistically significant and positive impact on reading proficiency rate. For the second robustness check (Panel B of Table 6), we use our preferred model from Table 4 but limit the data to only close referenda (i.e. the ones with 45 percent to 55 percent vote share). The results of this robustness check also follow the general pattern observed in Table

[27] The data for this test score are available for years 2005 to 2014.

4. It is worth noting that major construction projects continue to have a negative impact, although it is generally statistically insignificant at the 95 percent confidence level.

Table 6. Robustness checks

Approved type of expenditure	Years relative to the commencement year of a tax measure (τ)					
	τ = 0	**τ = 1**	**τ = 2**	**τ = 3**	**τ = 4**	**τ = 5**
Panel A. Reading proficiency rate (standardized) in Ohio high school graduation tests (OHSGT)						
Operating exp.	-0.00517	0.01526	0.01678	0.00432	0.00523	-0.01327
	(0.01593)	(0.01996)	(0.02009)	(0.01936)	(0.01904)	(0.01921)
Capital exp.						
Minor	0.00301	0.05168	0.05537	0.04202	0.00006	0.05073
	(0.05094)	(0.05587)	(0.04637)	(0.05466)	(0.05312)	(0.05521)
Major	0.00565	0.00109	-0.00122	-0.00161	0.00107	0.00039
	(0.00433)	(0.00372)	(0.00418)	(0.00453)	(0.00449)	(0.00448)
Panel B. Math proficiency rate (standardized) in OHSGT using only close referenda						
Operating exp.	0.01981	0.03894	0.01994	0.03507	0.00023	-0.02024
	(0.02568)	(0.03109)	(0.03670)	(0.03527)	(0.03876)	(0.04232)
Capital exp.						
Minor	-0.02257	0.01906	0.06817	0.02671	-0.12509	0.00645
	(0.06825)	(0.08325)	(0.06741)	(0.08232)	(0.09837)	(0.08376)
Major	0.00148	-0.00473	-0.01010*	-0.00623	-0.01463**	-0.00728
	(0.00474)	(0.00537)	(0.00575)	(0.00660)	(0.00714)	(0.00698)

Sources: NCES, ODE, ODT, OSOS, David Brasington personal data base for district level math proficiency rates, and own calculations.

Notes: Clustered standard errors (at the school district level) are in parenthesis. *, **, and *** represent significance levels of 10 percent, 5 percent and 1 percent, respectively. Parameter estimates represent the effect of approving additional expenditures (operating, minor capital, and major capital expenditures) for school districts (measured in per pupil constant-2015 thousands of dollars: per year for operating and minor capital expenditures and per project for major capital expenditures) on the dependent variable specified for each panel. The effects are measured in various points after the commencement year of a new tax ($\tau = 0$ represents the commencement year, $\tau = 1$ represents one year after the commencement, and so on). Parameters in each panel represent the TOT effects using equation (7) and a corresponding equation (4) (in which $L = 6$). See the methodology section for the list of control variables. Close referenda used for Panel B are the referenda with vote shares between 45 percent and 55 percent.

Our other robustness checks (Appendix D) produce a very similar picture. Specifically, our results are robust to the exclusion of year 2004 (for which we observe a dip in our math proficiency rate) and the method we use to combine vote shares when in the first referendum of a year there are more than one tax proposal on the ballot related to one specific type of expenditure (e.g., two parallel proposals to increase operating expenditure).[28] Moreover, our results on the impact of major construction projects on math proficiency rate remain generally unchanged when we divide the school districts in our sample depending on whether they received matching funding from the state (i.e., OFCC) or not. Finally, we examine the impact of various types of funding on teacher value added index and student mobility, and we do not find any consistent short term or long term effect for any type of school expenditure.

Finally, it is of interest to evaluate the impact of each type of funding on school districts located in poor versus rich neighborhoods. Although the granularity of our data do not allow a direct assessment of the impact of additional funding on poor versus rich *students*, we can study this at the *school district level*. To that end, we look at the school districts in places with high and low poverty rates among children, and we assess whether there is any differential effect of spending increases. To do this, we use the school district level poverty rates among children estimated by the Census (2017) for year 1995 to rank the school districts. Then we split the sample to only include school districts with poverty rates in the top 30 percent ("higher poverty school districts") in one subsample and the ones with poverty rates in the bottom 30 percent ("lower poverty school districts") in another subsample.

Table 7 shows the results of additional school funding on students in the higher and lower poverty school district subsamples. The results in Panel A show that in higher poverty school districts the math proficiency rates increase by about 0.162 standard deviations one year following the commencement of a new tax to increase operating expenditures. This increase is statistically significant until the fifth year after the approval of a new tax. To put this result in perspective, this impact is about three to ten times the magnitude of our main results (i.e., the average effect). Contrary to what we find for operating expenditures, there is no statistically significant effect on math proficiency rates for both minor and major capital expenditures.

Conversely, when we look at the effect on math proficiency rates for the lower poverty districts (Panel B), the results for operating expenditures are much smaller and statistically insignificant. Moreover, the aforementioned decline in math proficiency rates following the approval of a major capital expenditures seems to be driven by school districts with lower poverty

[28] Our preferred method is to take the average of the vote shares across parallel proposals. Our alternative method utilized in our robustness check is to use the highest vote share.

rates. This relationship is negative and statistically significant 2, 4, and 5 years after the passage of such proposal.

Table 7. The differential Impact of approving various types of additional funding on the school district level standardized math proficiency rate of students who are subject to the Ohio high school graduation tests who live in high versus low poverty districts

Approved type of expenditure	Years relative to the commencement year of a tax measure (τ)					
	τ = 0	**τ = 1**	**τ = 2**	**τ = 3**	**τ = 4**	**τ = 5**
	Panel A. Higher poverty school districts					
Operating exp.	0.06433	0.16255**	0.14207*	0.21331**	0.14081*	0.02211
	(0.06659)	(0.07725)	(0.07394)	(0.08349)	(0.08374)	(0.09270)
Capital exp.						
Minor	-0.56813*	-0.13014	0.01837	-0.14240	-0.33645	0.10385
	(0.29058)	(0.31933)	(0.31674)	(0.32539)	(0.41329)	(0.39473)
Major	0.00002	-0.00157	-0.00030	-0.00548	0.00359	-0.00099
	(0.00530)	(0.00631)	(0.00604)	(0.00748)	(0.00762)	(0.00886)
	Panel B. Lower poverty school districts					
Operating exp.	0.00289	0.00759	-0.00378	0.01590	0.02107	0.02927
	(0.02289)	(0.02339)	(0.02350)	(0.02614)	(0.02112)	(0.02160)
Capital exp.						
Minor	-0.02344	-0.07515	-0.02499	-0.03633	-0.06241	-0.02441
	(0.04931)	(0.09106)	(0.06481)	(0.06889)	(0.07705)	(0.07860)
Major	-0.00098	-0.00318	-0.00758**	-0.00270	-0.01251***	-0.00915**
	(0.00355)	(0.00340)	(0.00380)	(0.00471)	(0.00477)	(0.00460)

Source: NCES, ODE, ODT, OSOS, Census, David Brasington personal data base for district level math proficiency rates, and own calculations.

Notes: Clustered standard errors (at the school district level) are in parenthesis. *, **, and *** represent significance levels of 10 percent, 5 percent and 1 percent, respectively. Parameter estimates represent the effect of approving additional expenditures (operating, minor capital, and major capital expenditures) for school districts (measured in per pupil constant-2015 thousands of dollars: per year for operating and minor capital expenditures and per project for major capital expenditures) on the standardized math proficiency rates of students subjected to Ohio high school graduation tests. The effects are measured in various points after the commencement year of a new tax ($\tau = 0$ represents the commencement year, $\tau = 1$ represents one year after the commencement, and so on). Parameters in each panel represent the TOT effects using equation (7) and a corresponding equation (4) (in which $L = 6$). See the methodology section for the list of control variables. Panel A (B) values are estimated using only the portion of data that belongs to the school districts with top (bottom) 30 percent poverty rates among children living in that school district in 1995, i.e. about 180 school districts. About 73 percent (70 percent) of these school districts are still in the same group in 2015.

6. Conclusions

This paper analyzes the effect of various types of public school expenditures on test scores of students in Ohio by comparing the impact of operating versus minor capital versus major capital expenditures on the math proficiency rate of students who are subject to Ohio high school graduation tests. Moreover, this paper examines four channels, i.e., student-to-teacher ratio, student attendance rate, disciplinary actions per 100 students, and average expenditure on instructional staff, through which the additional expenditures in any of these three categories could potentially affect the performance of students.

Regardless of the type of additional school expenditure, we generally do not find a statistically significant effect of these expenditures on student outcomes in an average public school in Ohio. The point estimates for major capital expenditures are generally negative, although mainly statistically insignificant. However, when we limit the sample to only higher poverty school districts, additional operating expenditures have a much larger and statistically significant effect on student test scores that amounts to, depending on the year, three to ten times of the average effect.

Additional funding in any of these three categories also does not have an economically significant impact on the class size (student-to-teacher ratio), the attendance rate, or the discipline of students. However, the average expenditure on instructional staff increases with the approval of additional operating or minor capital expenditures, indicating that school districts potentially divert their maintenance funding to pay for teachers.

Our study informs policy makers at the state and federal level about how to focus already limited resources on areas that have an impact on students' achievements. Various studies, including this one, have not been able to identify any effect for major capital expenditures on student outcomes. However, our study also shows that increasing the operating expenditures of schools in poor neighborhoods has a large impact on less privileged students in these school districts. Focusing spending increases on these school districts is a policy that can create a higher return on state and federal expenditure.

We also find that the additional operating expenditure leads to better payment to teachers in all school districts, which may be one of the reasons why this additional expenditure matters more for poor school districts. This allows these school districts to be more competitive in hiring teachers or increasing incentives for current ones. Further research is necessary to better shed light on each of these channels.

Moreover, future research should focus on other channels through which additional funding can affect student outcomes, such as college preparation courses or extracurricular activities, especially ones that have the most effect on the students of lower socio-economic status. An important limitation of this study is the lack of student-level data, which were not available to us. Such data would allow for a more detailed analysis of who benefits the most from additional school funding.

Finally, it is worth noting that the methodology of this paper can be utilized in other contexts, for example, to study the effect of various types of referenda to increase taxes on housing prices or the number of new businesses. This is a natural extension of existing studies on these topics, as demonstrated by Enami, Reynolds, & Rohlin, 2020).

References

Angrist, J. D., & Pischke, J.-S. (2008). Mostly harmless econometrics: An empiricist's companion. Princeton, NJ: Princeton University Press.

Becker, G. S. (1964). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* Chicago, IL: University of Chicago Press.

Brasington, D. M., & Haurin, D. R. (2009). Parents, peers, or school inputs: Which components of school outcomes are capitalized into house value? *Regional Science and Urban Economics*, 39 (5), 523-529.

Breen, R. (2010). Educational expansion and social mobility in the 20th century. *Social Forces*, 89 (2), 365-388.

Brown, P. (2013). Education, opportunity and the prospects for social mobility. *British Journal of Sociology of Education*, 34 (5-6), 678-700.

Card, D., & Krueger, A. B. (1996). School resources and student outcomes: An overview of the literature and new evidence from North and South Carolina. *The Journal of Economic Perspectives*, 10 (4), 31-50.

Cellini, S. R., Ferreira, F., & Rothstein, J. (2010). The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125 (1), 215-261.

Coleman, J. S. et al. (1966). *Equality of Educational Opportunity*. National Center for Educational Statistics, document number: ED012275. (1966). Available at: http://eric.ed.gov/?id=ED012275.

Conlin, M., & Thompson, P. N. (2017). Impacts of new school facility construction: An analysis of a state-financed capital subsidy program in Ohio. *Economics of Education Review*, 59, 13-28.

Census (2017). *Small Area Income and Poverty Estimates: School District Data Files*. data extracted in July 2017. Washington, D.C.: U.S. Bureau of the Census, available at: https://www.census.gov/did/www/saipe/data/schools/data/index.html.

Duflo, E. (2001). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. *The American Economic Review*, 91 (4), 795-813.

Enami, A., Reynolds, C. L., & Rohlin, S. M. (2020). The effect of property taxes on businesses: Evidence from a dynamic regression discontinuity approach. Manuscript. The University of Akron and Kent State University.

Fryer, R. G. (2013). Teacher incentives and student achievement: Evidence from New York City public schools. *Journal of Labor Economics*, 31 (2), 373-407.
Greenwald, R., Hedges, L. V., & Laine, R. D. (1996). The effect of school resources on student achievement. *Review of Educational Research*, 66 (3), 361-396.
Goldin, C. D., &Katz, L. F. (2009). *The Race Between Education and Technology.* Cambridge, MA: Harvard University Press.
Goncalves, F. (2015). The effects of school construction on student and district outcomes: Evidence from a state-funded program in Ohio. Manuscript. Available at SSRN 2686828.
Hedges, L. V., Laine, R. D., & Greenwald, R. (1994). An exchange: Part I: Does money matter? A meta-analysis of studies of the effects of differential school inputs on student outcomes. *Educational Researcher*, 23 (3), 5-14.
Hanushek, E. A. (1986). The economics of schooling: Production and efficiency in public schools. *The Journal of Economic Literature*, 24 (3), 1141-1177.
Hanushek, E. A. (1994). An exchange: Part II: Money might matter somewhere: A response to Hedges, Laine, and Greenwald. *Educational Researcher*, 23 (4), 5-8.
Hong, K., & Zimmer, R. (2016). Does Investing in school capital infrastructure improve student achievement? *Economics of Education Review*, 53, 143-158.
Hong, K. (2016). School bond referendum, capital expenditure, and student achievement. NYU Wagner Research Paper No. 2886666. Available at SSRN: https://ssrn.com/abstract=2886666 or http://dx.doi.org/10.2139/ssrn.2886666.
Hyman, J. (2017). Does money matter in the long run? Effects of school spending on educational attainment. *American Economic Journal: Economic Policy*, 9 (4), 256-280.
Isen, A. (2014). Do local government fiscal spillovers exist? Evidence from counties, municipalities, and school districts. *Journal of Public Economics*, 110, 57-73.
Jackson, C. K., Johnson, R. C., &Persico, C. (2016).. The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131 (1), 157-218.
Jennings, J. (2012). *Reflections on a Half-Century of School Reform: Why Have We Fallen Short and Where Do We Go from Here?* Washington, D.C.: Center on Education Policy.
Kogan V., Lavertu, S., & Peskowitz, Z. (2016). School district tax referenda, spending cuts, and student achievement." Manuscript. Available at:
https://aefpweb.org/sites/default/files/webform/41/KLP_RDlevy_02172016.pdf.
Lafortune, J., Rothstein, J., & Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10 (2), 1-26.
Lavertu, S., & St. Clair, T., (2017). Beyond spending levels: School district revenue uncertainty and student achievement. AEFP 42nd Annual Conference, March 16-18, 2017. Available at:
https://aefpweb.org/sites/default/files/webform/42/LavertuStClair_20170116.pdf
Martorell, P., Stange, K., & McFarlin, I. (2016). Investing in schools: Capital spending, facility conditions, and student achievement. *Journal of Public Economics*, 140, 13-29.
McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142 (2), 698-714.
National Center for Education Statistics (NCES) (2017). U.S. Department of Education. Common Core of Data (CCD). Data extracted in February 2017. Available at:
https://nces.ed.gov/ccd/elsi/tableGenerator.aspx.
Neilson, C. A., & Zimmerman, S. D. (2014). The effect of school construction on test scores, school enrollment, and home prices. *Journal of Public Economics*, 120, 18-31.
Ohio Department of Education (ODE) (2017). Information and data extracted in February 2017. Available at:
http://bireports.education.ohio.gov
http://education.ohio.gov/Topics/Finance-and-Funding/School-Payment-Reports/State-Funding-For-Schools/Traditional-Public-School-Funding.

ftp://ftp.ode.state.oh.us/MR81/

Ohio Department of Taxation (ODT) (2017). Information and data extracted in February 2017. Available at: http://www.tax.ohio.gov/tax_analysis/tax_data_series/school_district_data/publications_tds_school.aspx http://www.tax.ohio.gov/school_district_income.aspx.

Ohio Facilities Construction Commission (OFCC) (2017). Information and data extracted in February 2017. Available at: http://ofcc.ohio.gov/ServicesPrograms/K-12Schools/CFAP.aspx, http://ofcc.ohio.gov/Resources/Publications.aspx#115657-annual-reports http://ofcc.ohio.gov/NewsEvents/NewsReleases/20120712.aspx.

Ohio Secretary of State. Information (OSOS) (2017). Information and data extracted in February 2017. Available at: https://www.sos.state.oh.us/SOS/Upload/elections/EOresources/general/2013QandI.pdf

Pittner, N. A., Carleton, M..M., & Casto, C. (2010). School funding in Ohio: From DeRolph to the evidence-based model (EBM) and beyond. *Journal of Education Finance*, 36 (2), 111-142.

Ree, J. D., Muralidharan, K., Pradhan, M., &Rogers (2018). Double for nothing? Experimental evidence on an unconditional teacher salary increase in Indonesia. *The Quarterly Journal of Economics*, 133 (2), 993-1039.