Public School Funding, School Quality, and Adult Crime*

E. Jason Baron†

Joshua Hyman‡

Brittany Vasquez§

May 2, 2022

Abstract

This paper asks whether improving the quality of public schools can be an effective long-run crime-prevention strategy in the U.S. Specifically, we examine the effect of school quality improvements early in children's lives on the likelihood that they are arrested as adults. We exploit quasi-experimental variation in school quality due to increases in public school funding, leveraging two natural experiments in Michigan and a novel administrative dataset linking the universe of Michigan public school students to adult criminal justice records. The first research design exploits variation in operating expenditures due to Michigan's 1994 school finance reform, Proposal A. The second design exploits variation in capital spending by leveraging close school district capital bond elections in a regression discontinuity framework. In both cases, we find that students exposed to additional funding during elementary school were substantially less likely to be arrested in adulthood. We show that the social benefits of improving school quality (via increases in funding) are greater than the costs, even when considering only the crime-reducing benefits.

*We received valuable feedback from Peter Arcidiacono, Patrick Bayer, Eric Brunner, George Bulman, Eric Chyn, Eric Edmonds, Ezra Goldstein, Max Gross, Nathan Hendren, Kirabo Jackson, Brian Jacob, Max Kapustin, Julien Lafortune, Juan Carlos Suárez Serrato, and seminar and conference participants at Brown University, Cornell University, Dartmouth College, University of Connecticut, University of Michigan, Williams College, the American Economic Association, the Triangle Economists in Applied Microeconomics seminar, the National Tax Association, the Triangle Economics of Education Workshop, and the Online Economics of Crime Seminar organized by Jennifer Doleac. We appreciate Joseph Ryan, Brian Jacob, and the Child and Adolescent Data Lab for their generosity in sharing data, Jonathan Hartman and Kyle Kwaiser for their help with record linkage, and Jasmina Camo-Biogradlija, Andrea Plevek, and Nicole Wagner Lam for coordinating data access. The project received approval from the University of Michigan's Institutional Review Board: HUM00195369. This research used data structured and maintained by the MERI-Michigan Education Data Center (MEDC). MEDC data are modified for analysis purposes using rules governed by MEDC and are not identical to those data collected and maintained by the Michigan Department of Education (MDE) and/or Michigan's Center for Educational Performance and Information (CEPI). This research was funded with help from training grants R305B170015 and R305B150012 from the U.S. Department of Education's Institute of Education Sciences, and the Early Career Scholars Grant at Policy Impacts. Any opinions, findings, conclusions, or recommendations expressed in this material are those of the authors and do not reflect the views of any other entity.

†Duke University and NBER. Email: jason.baron@duke.edu.

‡Amherst College. Email: jhyman@amherst.edu.

§University of Michigan. Email: vasqbn@umich.edu.

I Introduction

Could improving the quality of public schools be an effective long-run crime-prevention strategy? This question is particularly relevant today, as budget-constrained cities across the country face increasing calls to allocate the marginal crime-prevention dollar away from law enforcement and toward social programs such as public education. Beyond its policy relevance, the relationship between education and crime is also of long-standing interest in economics. The literature has convincingly shown that additional years of schooling lead to less crime (Hjalmarsson, Holmlund and Lindquist, 2015; Lochner and Moretti, 2004; Machin, Marie and Vujić, 2011; Meghir, Palme and Schnabel, 2012). However, as noted in Lochner (2020) and Hjalmarsson and Lochner (2012), there is surprisingly little evidence on the effects of school quality on subsequent criminal behavior. Most evidence on this topic relies on school choice lotteries and shows that students who win admission to a more preferred school are less likely to engage in subsequent criminal activity (Cullen, Jacob and Levitt, 2006; Deming, 2011; Dobbie and Fryer, 2015).

This paper takes an alternative approach to estimating the effects of school quality on adult crime: exploiting quasi-experimental variation in school quality from increases in public school funding. This approach offers two advantages over variation from school choice lotteries. First, estimating the effects of attending a more preferred school may conflate improvements in school quality, such as better teachers or facilities, with exposure to fewer crime-prone peers—an important determinant of criminal development (Bayer, Hjalmarsson and Pozen, 2009; Billings and Hoekstra, 2019; Billings, Deming and Ross, 2019; Jacob and Lefgren, 2003). Relative to school choice interventions, increases in school funding leave less room for changes in peer composition. While it is possible for families to re-sort across districts in response to changes in school funding, the resulting changes in peer composition are likely to be substantially smaller than those from attending a new school with an entirely new set of peers. Second, although the seminal work in Cullen, Jacob and Levitt (2006), Deming (2011), and Dobbie and Fryer (2015) shows that attending a more preferred school reduces subsequent crime, such schools have high demand and only a limited supply of seats. It is important for policy purposes to understand whether these effects can be replicated by improving existing schools instead—an arguably more scalable policy intervention.

We leverage two natural experiments in Michigan which yield plausibly exogenous variation in school funding. The first research design exploits variation in operating expenditures due to Michigan's 1994 school finance reform, Proposal A, in an instrumental variables framework. The second research design exploits variation in capital spending by leveraging close school district capital bond elections using a regression discontinuity (RD) design. Both operating (e.g., teacher salaries) and capital expenditures (e.g., building renovations) affect school quality, but in very different ways. Understanding whether improvements in these different aspects of school quality can reduce adult crime is important for policy, and can shed light on the nature of criminal development by exploring the relationship between distinct inputs in the education production function and subsequent crime.

To implement these research designs, we assemble a novel administrative dataset linking the universe of individual public school and adult criminal justice records in Michigan. To explore mechanisms, we also link these data to juvenile detention records from Michigan and nationwide postsecondary enrollment information. Our data contain ten cohorts of first-time kindergarten students in Michigan public schools between 1995 and 2004, consisting of nearly 1.2 million individual students.[1] The timing of the natural experiments and availability of our administrative data sources requires us to focus on increases in school funding during elementary school. Thus, this paper should be viewed as an examination of whether investments in public schools early in children's lives can reduce their likelihood of committing crime as adults.

Our first natural experiment yields variation in operating expenditures across school districts and student cohorts due to the funding formula imposed by Proposal A. Under this reform, local control over operating expenditure levels was taken almost entirely away from districts, becoming centralized at the state level. Spending was sharply increased in previously low-spending school districts and essentially frozen for previously high-spending ones. We exploit these differential changes in expenditures across cohorts and school districts to identify the causal impact of increases in operating expenditures. Similar to prior work examining the impacts of Proposal A on educational attainment (Hyman, 2017), we instrument for the time-path of actual district spending with the time-path of the district's assigned spending by the state under the new funding formula.

We find large effects of additional operating expenditures on adult crime. Specifically, we find that students exposed to 10% additional operating expenditures from kindergarten through third grade are 2 percentage points, or 15%, less likely to be arrested in adulthood (through age 30). Exploring heterogeneity, we find that the effects are concentrated in baseline low-performing and low-income school districts, and driven by reductions across all major crime types. Although we only observe criminal justice records in Michigan, we provide evidence that the results are not driven by out-of-state migration.

Using our detailed administrative data following students from primary through postsecondary education, we examine the mechanisms through which additional operating expenditures reduce adult crime. Consistent with the design of Proposal A, which targeted operating expenditures, we show that additional funding from the reform is not associated with increases in capital expenditures. Students exposed to additional operating expenditures experience improvements in school quality via reductions in class sizes and teacher turnover, increases in teacher salaries and experience, and the hiring of additional district and school leadership such as vice-principals, who are heavily involved in issues surrounding student discipline and truancy.

We next examine whether these school quality improvements translated into improvements in student performance and behavior. Treated students have higher test scores in fourth grade, though these gains disappear by middle school. Despite the test score fadeout, we find that students experience considerable improvements in behavioral proxies for non-cognitive skills: treated students have substantially lower absenteeism rates in middle school and are less likely to be placed in a

[1]Throughout the paper, we reference academic years by their spring semesters. For instance, we refer to the 1994-95 academic year as 1995.

juvenile detention center, a measure of juvenile delinquency. Students exposed to additional school funding are also more likely to graduate high school and earn a postsecondary degree.

These gains in children's academic and behavioral outcomes could lead to a reduction in adult crime through several channels. Improvements in educational attainment could increase the opportunity cost of adult crime through better labor market outcomes (Becker, 1968; Lochner, 2004). At the same time, the improvements in proxies for non-cognitive skills may be partly responsible for the reduction in crime; recent work shows that teachers who improve attendance and reduce disciplinary incidents can significantly reduce the probability of a student's future arrest, likely due to improvements in socio-emotional skills (Rose, Schellenberg and Shem-Tov, 2021). Finally, it is possible that by increasing student attendance in school during a key period of criminal development, additional funding prevents students from accumulating criminal capital outside of school (Bell, Costa and Machin, 2021).

To explore the relative contribution of each of the above channels, we conduct mediation analyses and show that increases in educational attainment explain roughly 20% of the overall effect. These findings are consistent with prior studies showing that additional years of schooling reduce crime (Hjalmarsson, Holmlund and Lindquist, 2015; Lochner and Moretti, 2004; Machin, Marie and Vujić, 2011; Meghir, Palme and Schnabel, 2012), but also show that focusing on educational attainment alone understates the crime-reducing benefits of better schools. Importantly, reductions in absenteeism explain a much larger portion of the effect (roughly 40%). While the literature examining the effects of school funding has traditionally focused on outcomes such as test scores and educational attainment, this result suggests that additional funding may primarily impact later-in-life crime through improvements in socio-emotional "soft" skills and by keeping children engaged in school during a critical period of criminal capital formation. As a result, previous knowledge of how school funding impacts student outcomes does not accurately predict the extent to which increasing funding will impact adult crime.

We rule out other explanations such as positive peer effects from changing peer composition; we find little evidence that treated students were exposed to a different set of peers in later grades. A likely explanation for this result is that treated school districts under Proposal A were primarily in rural areas with limited options for families to enroll their children in nearby districts. We also rule out that our main result is driven by a mechanical short-term reduction in crime from staying in school longer. We find sustained crime reductions through ages 22–30, a time when nearly all youth are finished with their schooling.

Although school inputs such as class size and teacher compensation are crucial dimensions of school quality, the physical condition of school infrastructure is another important input. Public expenditures on school facilities in the United States totaled roughly $80 billion in 2015, and make up approximately 13% of all K-12 public school spending.[2] Thus, it is important to understand whether increasing capital outlays can also reduce adult crime, particularly in light of recent studies suggesting

[2] Authors' calculations from the National Center for Education Statistics. These expenditures are reported in constant 2017-18 dollars and reflect the sum of capital outlays and interest on school debt.

it may have limited effects on test scores and educational attainment (Baron, 2022; Brunner, Hoen and Hyman, 2022; Cellini, Ferreira and Rothstein, 2010; Martorell, Stange and McFarlin Jr, 2016).

To estimate the effects of additional capital expenditures on adult criminality, we exploit the fact that, although Proposal A centralized school districts' level of operating expenditures, it left the level of funding for capital projects up to local school districts. If a district wishes to raise funds for capital purposes, then it must ask for voter approval to increase property taxes in a local election. While districts that win elections are likely to differ both in observable and unobservable ways from districts in which the election fails, these differences can be mitigated by focusing on narrow elections: a school district that narrowly wins an election is likely to have similar preferences for education spending to a district in which the election narrowly fails. Our research design leverages the universe of close elections in Michigan from 1996–2004 in an RD framework to examine the effects of additional capital expenditures on adult crime.

In line with our analysis of operating expenditures, we examine the long-run effects of capital spending for primary school students. We do this by assigning to each capital bond election the adult arrest rate of the students in kindergarten in the district and year of the election. We find that kindergarteners in school districts that narrowly pass a capital election are 20% less likely to be arrested in adulthood. Narrowly winning districts experience a large and immediate increase ($2,200 or 175%) in capital expenditures per pupil in the year following the election. The increase is much smaller in the second and third years, with an average increase of $940, or 44% in the three years following the election, and no increases thereafter. After smoothing these capital expenditures over time to account for the fact that they produce durable goods, we calculate an elasticity of adult arrests with respect to capital spending of approximately -1. While large, this elasticity is roughly two thirds as large as the elasticity with respect to operating expenditures (-1.5). Nevertheless, the fact that two distinct research designs, each with different assumptions, indicate that improving school quality reduces adult crime bolsters the credibility of a causal relationship.

Exploring mechanisms, we first confirm that winning an election has no impact on operating expenditures. Instead, winning an election impacts school quality through improvements in school infrastructure; we analyze data describing the purpose of each proposed bond, and show that most capital projects in our context fund the improvement and renovation of instructional facilities, primarily targeting elementary and middle schools. Fewer bonds fund new building construction, investments in athletic facilities, or utilities.

How could investments in school infrastructure reduce students' likelihood of committing crime as adults? Children and parents from disadvantaged communities may be pessimistic about the returns to consistently attending school when school facilities are old and in need of repair. Indeed, children living in disadvantaged communities tend to have high rates of school truancy, disengagement, and chronic absenteeism, all of which are highly predictive of adverse long-term outcomes (Jacob and Lovett, 2017). Modernizing and upgrading existing infrastructure may thus improve student and parental engagement. Accordingly, we find that students in narrowly winning school districts are

25% less likely to be chronically absent in middle school, the earliest grade for which we observe attendance records. We find evidence of modest improvements in test scores and high school graduation, but our analysis is underpowered to detect effects on these outcomes with precision. The large declines in chronic absenteeism, and the smaller and imprecise effects on test scores and educational attainment, suggest that keeping at-risk students engaged in school during a key period of criminal capital formation may be an important channel through which investments in infrastructure reduce adult crime. We find less evidence for other explanations such as changes in peer composition.

Lastly, we conduct several cost-benefit calculations to assess the cost-effectiveness of school funding at preventing crime, and to examine how it compares to other educational and crime-prevention measures. Considering only the crime-reducing benefits of school spending, we calculate that its Marginal Value of Public Funds (MVPF) is greater than one under our most conservative assumptions. In other words, society receives more than $1 in benefits for every $1 in government costs. We also show that the crime reduction due to an additional dollar of school funding is similar to that of early childhood educational investments (e.g., Head Start and the Perry Preschool Project), and hiring additional sworn police officers.

The primary contribution of this study is to provide evidence that improving school quality reduces adult crime. Exploiting variation in school quality from increases in funding improves on previous work using school choice lotteries by isolating the effects of school quality from changes in peer composition and by focusing on an arguably more scalable policy lever.

There is remarkably little evidence on the relationship between school funding and adult crime. Johnson and Jackson (2019) use data from the Panel Study of Income Dynamics (PSID) to show that increasing K-12 public school funding improves a range of long-term outcomes for poor children, including self-reported incarceration, particularly when preceded by Head Start exposure. We provide the first comprehensive examination of the effects of school funding on adult crime, complementing the work in Johnson and Jackson (2019) in a number of ways. First, Johnson and Jackson (2019) estimate the joint impact of increases in operating and capital expenditures, and are unable to disentangle their relative effects. We show that increases in both operating and capital expenditures lead to large reductions in adult crime. Second, the PSID relies on relatively small samples (the sample of poor children in Johnson and Jackson (2019) consists of roughly 6,000 individuals, of whom 8% report any incarceration spell), and self-reported crime measures which tend to be under-reported (Deming, 2009, 2011). We examine the universe of public school students in a large state and use administrative criminal justice records as opposed to self-reported incarceration. Third, our detailed administrative data following children from kindergarten through postsecondary education and adulthood allow us to explore the specific intermediate outcomes and mechanisms through which each expenditure type influences later-in-life crime.

This study also contributes to the broader literature on public school spending. The primary economic justification for the public provision of education is one of positive externalities. Yet economists examining the benefits of school funding have mainly focused on the private returns to students such as test scores, educational attainment, and wages.[3] To understand both the optimal level and method of provision of school funding, it is necessary to understand school funding's broader social returns. Positive externalities, such as reductions in adult criminality, are particularly important for thinking about the social return to investments in public education.

Our results also have important implications for the literature examining the effects of additional capital expenditures in public schools. While studies examining the effects of additional operating expenditures generally find improvements in student outcomes, evidence on the effectiveness of capital expenditure increases is less clear (Jackson, 2018*a*; Jackson and Mackevicius, 2021). A number of recent studies have shown that additional capital expenditures have little impact on test scores (Baron, 2022; Brunner, Hoen and Hyman, 2022; Cellini, Ferreira and Rothstein, 2010; Martorell, Stange and McFarlin Jr, 2016) and high school graduation (Baron, 2022; Brunner, Hoen and Hyman, 2022).[4] The results in our study provide evidence that capital expenditures can improve important long-term outcomes such as crime, even in the absence of clear improvements in cognitive outcomes.

Finally, we contribute to the economics of crime literature identifying effective crime reduction strategies. There are numerous high-quality studies in this literature evaluating the criminal deterrence effects of either increasing the size of the police force (Chalfin and McCrary, 2018; Chalfin et al., 2021; Mello, 2019) or implementing tougher sanctions (Bell, Jaitman and Machin, 2014; Katz, Levitt and Shustorovich, 2003). More recently, a growing literature emphasizes the potential efficiency gains of early policy interventions that prevent the development of offenders in the first place, including studies of access to mental healthcare (Jácome, 2020), cognitive behavioral therapy (Heller et al., 2017), lead exposure (Billings and Schnepel, 2018; Grönqvist, Nilsson and Robling, 2020), and early childhood education (Anders, Barr and Smith, 2022; Garcia, Heckman and Ziff, 2019; Heckman, Pinto and Savelyev, 2013). We study a particularly salient, ubiquitous, and arguably scalable early intervention—improving school quality by increasing funding to public schools—and show that it is a viable policy tool to prevent criminal development.

II Background

This section describes the institutional changes in Michigan that gave rise to the two natural experiments that we study in this paper.

Prior to Proposal A, Michigan financed public K-12 education primarily through the local property

[3]Specifically, recent quasi-experimental studies primarily relying on variation from school finance reforms (SFRs) have shown that additional school resources improve short- and medium-term student outcomes such as test scores and educational attainment (Brunner, Hyman and Ju, 2020; Hyman, 2017; Lafortune, Rothstein and Schanzenbach, 2018), and longer-term outcomes such as wages, employment, and intergenerational mobility (Biasi, 2021; Jackson, Johnson and Persico, 2016; Rothstein and Schanzenbach, 2021). See Jackson (2018*a*) for a detailed literature review.

[4]Importantly, in the aforementioned studies, as in our paper, the typical capital project generally targets renovations of existing structures. In contrast, recent studies examining the impacts of larger-scale new school construction projects in urban school districts find large improvements in student test scores (Lafortune and Schönholzer, 2021; Neilson and Zimmerman, 2014).

tax.[5] As a consequence of growing spending inequalities across school districts and rapidly increasing property tax burdens, in July 1993 the Michigan State Legislature eliminated the property tax as a source of local school finance. In response, voters approved Proposal A—which became effective for the 1995 academic year—and financed public education primarily through state revenue sources such as the sales tax.

Proposal A assigned each district a per-pupil operating spending amount known as the foundation allowance (or foundation grant). Districts were not allowed to spend less than their allowance. However, while the allowance comprised the overwhelming majority of district operating expenditures, it did not institute a strict spending ceiling for districts. School districts could spend more than their assigned allowance through either state categorical grants or federal aid. Thus, as we show below, while the relationship between the allowance and operating spending is very strong, it is not one to one.

Proposal A began equalizing funding across school districts during its first year (1995) using the following formula:

$$Allowance = \begin{cases} 4,200 & \text{if } x \leq 3,950 \\ x + 250 & \text{if } x \in [3,950, 4,200] \\ 0.961x + 414.35 & \text{if } x \in [4,200, 6,500] \\ x + 160 & \text{if } x \geq 6,500 \end{cases}$$

where *Allowance* is the per-pupil foundation allowance in 1995 and x is 1994 revenue from state and local sources.

The reform then set into motion a set of incremental increases to the allowance—inversely related to a district's position in the 1994 revenue distribution—that would further equalize revenues across districts in the years after 1995. Importantly, the amount of the foundation allowance awarded to each school district each year was always a deterministic function of district-level revenue in 1994 and overall changes in state budget appropriations to public education. Given that growth in appropriations is plausibly exogenous to individual districts, it is possible that—conditional on a district's 1994 revenue—district-level unobservables related to criminal activity are uncorrelated with changes in the district's allowance.

Panel A of Figure 1 shows (real) average per pupil operating expenditures over time for districts grouped by their 1994 revenue percentile. The figure shows that, despite trending similarly in the years leading up to the reform, initially low- and high-revenue districts experienced drastically different changes in spending following Proposal A. By 2003, average spending in the bottom half of the 1994 revenue distribution had risen to the level of average spending in the third quartile, essentially equalizing spending across the bottom half of Michigan districts.

Panel B illustrates how the time path of the allowance varied by a district's 1994 revenue, and

[5] For more information regarding Michigan's school finance system pre- and post-Proposal A, see Courant, Cullen and Loeb (2003).

shows that most of the observed equalization in spending was driven by the implementation of the foundation allowance. Through 2002, initially low-revenue districts experienced substantial annual real increases in the allowance, while the allowance remained flat for initially high-revenue districts. These figures show that there is little identifying variation in the allowance after 2003. As a result, and given that our student-level data begin in 1995, we focus on student exposure to changes in the allowance and spending during the 1995–2003 period.

Proposal A was similar in many dimensions to other state school finance reforms and represents a fairly representative case study with which to examine long-run impacts of school funding. Most importantly, as with most other reforms, Proposal A substantially increased school spending among previously low-revenue districts, providing a powerful and arguably generalizable natural experiment. Proposal A was motivated by (and designed in the interest of) "adequacy," as were the dozens of reforms that occurred since the late 1980s. An important difference between Proposal A and other school finance reforms during the adequacy era is that it disproportionately impacted areas with a large share of White students. Though this is also true of the average school finance reform in the adequacy era (see Table 1 in Rothstein and Schanzenbach (2021)), it is more pronounced in Michigan. Still, we show below that the marginal allowance dollar was allocated similarly to the average dollar in our sample, which suggests our estimates could be externally valid.

While Proposal A centralized school districts' level of operating spending at the state level, it left the level of funding for capital projects up to local school districts. In other words, construction, modernization, renovations, and repairs of Michigan public educational facilities are still financed primarily through local property taxes. Specifically, if a district wishes to raise funds for capital purposes, then it must ask for voter approval to increase property taxes in a local election. If voters approve the request, then funds for these capital improvement projects are raised through the sale of school bonds—borrowing funds that are paid back at interest over time with the increase in local property taxes approved by the voters. A simple majority vote by district residents is required for the initiative to pass.

Michigan law restricts how school districts can use the funds raised through a bond referendum. Allowable uses include the construction of school buildings, remodeling existing structures, asbestos abatement, athletic and physical education facility development and improvements, and school bus purchases. School districts can also purchase technology, but this is limited to hardware and devices that transmit, receive, or compute information for pupil instructional purposes only. The purchase of operating systems and customized application software is allowed only if purchased with the initial hardware.

School districts are not allowed to use the additional funds on any operating expenditures such as employee salaries and benefits, school supplies, textbooks, or small repairs or maintenance of existing structures. Importantly, once all new construction is complete, a school district must have an audit conducted for the specific project by an independent auditor appointed by the State of Michigan. This institutional feature ensures that school districts cannot simply pass bond elections and use the funds for operating expenditures. Accordingly, we show empirically in Sections IV.B and V.B that additional operating funds through the foundation allowance stick in operating accounts, and additional funds raised through bond elections stick in capital outlay accounts.

From January 1996 to December 2004, the sample period of our main RD analysis, there were 955 unique capital bond elections. There are few restrictions on the dates school districts can hold a local election. A local school board can either call a special election or hold the election at a regular primary or general election date (in the months of May, August, or November).[6] Table A1 shows summary statistics for all elections held by Michigan public school districts during this time period. Of the 518 Michigan public school districts examined in this study, 383 (74%) held at least one election. 49% of all elections passed; elections were relatively close: on average, the percent of votes in favor for a given election was slightly below 50%. The average winning election asked voters for permission to borrow $29 million, or $10,797 per pupil.

III Data Sources and Analysis Samples

III.A Data Sources and Matching Across Administrative Data Systems

We use administrative data from the Michigan Department of Education (MDE), Center for Educational Performance and Information (CEPI), National Student Clearinghouse (NSC), and Michigan State Police (MSP) to test the effects of additional school funding during primary school on adult criminal justice involvement. For a more detailed discussion of each of the data sources, the sample construction, and matching across administrative data systems, see Online Appendix D.

The starting point for our analysis consists of ten cohorts of first-time kindergarten students in Michigan public, non-charter, schools during the 1995 through 2004 academic years. These cohorts include 1,171,367 students across 518 school districts. We use MDE/CEPI administrative datasets to follow these students throughout their educational trajectories in Michigan. Specifically, this dataset allows us to measure intermediate outcomes such as fourth and eighth grade math test scores on the state standardized exam, school attendance in eighth grade, and high school graduation. The microdata contain information on where students enroll each year, allowing us to track students across schools and districts over time and to observe whether a student was ever enrolled in an educational program at one of Michigan's 23 juvenile detention centers (JDCs). Enrollment in a JDC is a behavioral outcome that indicates youth contact with the juvenile justice system; individuals younger than 17 years old may be held in a JDC after being arrested. We focus on placement in a JDC instead of other behavioral outcomes commonly available in administrative education datasets, such as school suspensions or expulsions, because these measures are not consistently reported in the Michigan data.

Education records contain individual-level covariates such as sex, race/ethnicity, and an indicator

[6] Figure A1 shows the distribution of capital bond election months. The figure shows that a large number of elections were held in August—Michigan's primary elections date.

for free or reduced-price lunch (FRPL) eligibility that we control for in our main specifications. We measure student demographics and intermediate outcomes such as attendance in grade eight because, with the exception of test scores, these variables are unavailable prior to 2003, which is the year the first cohort reached eighth grade. We also match students in these cohorts to the NSC, which contains postsecondary enrollment and degree receipt information. NSC data are nationwide, allowing us to observe whether a particular student ever enrolled in (or graduated from) a postsecondary education program outside the state.[7] To characterize the school districts where students in our sample are enrolled, we collect several district-level covariates measuring revenues and expenditures, local school choice, demographic, and economic conditions.

We then match the students in these cohorts to an arrest-level dataset from MSP containing the universe of adult arrests in Michigan from January 2012 through May 2020. The Michigan Education Data Center (MEDC) linked the K-12 public school records to the adult crime data using a probabilistic matching algorithm. Because these data sources do not contain a common numeric identifier, MEDC staff linked the records based on first name, last name, date of birth, and gender. The linkage performed well; overall, 83% of records in the adult crime data matched to a public school student with a high degree of certainty (over 99.6%). Importantly, one should not expect a 100% match rate because some individuals arrested in Michigan could have gone to school in a different state, been enrolled in a private school, or been homeschooled. Online Appendix D discusses the matching process and match rates in greater detail.

For individuals who are arrested at age 17 or older (the age at which individuals are considered adults by the Michigan justice system during our sample period), the arrest data include the date of the arrest, whether the arrest was for a misdemeanor or felony offense, and the exact offense (e.g., assault or larceny). We use this information to construct adult crime outcomes including an indicator for whether the student was ever arrested in Michigan, and arrest status by particular types of crime (e.g., violent or property).

Finally, to estimate the causal effects of additional capital expenditures on adult crime, we obtained a capital bond election-level dataset from MDE. This dataset reports, for each election, the date of the election, the cost of the capital project, voter turnout and votes in favor, a description of the intended use of the bond, and the district's unique identifier.

We next describe our two analysis samples, one used in the analysis of Proposal A-induced increases in operating expenditures ("Proposal A sample"), and the other used in the RD design ("election sample"). Each sample is comprised of different sets of cohorts, as data availability and the timing of each of the natural experiments differs for each design. For instance, the 1995 cohort is the first exposed to Proposal A and the first for which our microdata are available. Furthermore, as described below, the 2000 cohort is the last cohort exposed to plausibly exogenous variation from Proposal A for at least a few years, since the equalization effects of Proposal A ended by 2003. In contrast, the election-level dataset is available from 1996 on, and this analysis is not constrained by

[7]For more information regarding the NSC, see Dynarski, Hemelt and Hyman (2015).

the equalization window of Proposal A, allowing us to add additional cohorts to this analysis. As a result, we construct two distinct analyses samples, each using the most possible data subject to the timing and data constraints of each particular strategy.

III.B Proposal A Analysis Sample and Summary Statistics

The sample for the Proposal A analysis consists of six cohorts of first-time kindergarten students in Michigan public, non-charter schools from 1995 to 2000. These cohorts include 717,042 students across 518 school districts. Given the timing of Proposal A, first-stage variation, and availability of the adult arrest data, we focus our Proposal A analysis on the operating expenditures that students are exposed to from kindergarten through third grade.[8] As mentioned above, there is little identifying variation in the allowance after 2003, by which time the most recent kindergarten cohort reaches grade three. To ensure consistency across cohorts, we restrict our analysis to examining expenditures in grades three and below. However, we show that the results are robust to this choice of grade range.

Given the time period (2012–2020) covered by the arrest data, and the age of students in our sample, each cohort will be "eligible" for the arrest outcome at different ages. For instance, we can observe whether students in the 1995 cohort (most of whom were born in 1990) were ever arrested from ages 22 through 30. For students in the 2000 cohort (most of whom were born in 1995), we observe whether they were ever arrested from ages 17 through 25. While we can observe every student in our dataset from ages 22–25 in the arrest records, this age window is quite restrictive. Thus, in our primary specifications we simply measure whether a student was ever arrested during the respective age window in which they could match to the arrest data. However, because the age profiles of criminal justice involvement may differ by treatment status, we show below that our results are robust to alternative dependent variables such as "ever arrested by age 25" or "ever arrested during the ages of 22 through 25."

Table 1 describes the sample. Column 1 consists of all students in our base population, while Columns 2 and 3 consist of students who were never arrested and those who were arrested as adults at least once, respectively. Of the 717,042 students in the analysis sample, 85,533 (12%) were arrested at least once. Black and low-income students were disproportionately involved in the criminal justice system. For instance, 53% of children eventually arrested were eligible for FRPL in 8th grade, despite their making up just 32% of the population. Students with adult criminal justice involvement had noticeably lower test scores in fourth and eighth grade, lower baseline attendance rates in eighth grade, and were substantially less likely to graduate high school or college. Students with adult criminal justice involvement were also significantly more likely to be placed in a JDC during ages 10–16 (5% versus 1%).

Column 4 describes children enrolled in initially low-revenue school districts—those in the bottom

[8]Personally identifiable information for Michigan public school students is only available for cohorts of students born in 1989 or later. As a result, we were not able to match older cohorts (e.g., first-time fourth-graders in 1995) to adult arrest records. The earliest cohorts we could match were first-time kindergarten students from 1995 on.

quartile of the 1994 revenue distribution—while Column 5 describes high-revenue school districts—those in the top three quartiles. The adult arrest rate of students in initially low-revenue districts is 11%, compared to 12% in initially high-revenue districts. Moreover, while 23% of the base population lives in a rural school district (Column 1), this share differs dramatically for initially low- and high-revenue districts (50% versus 19%). Figure 2—which shows a map of Michigan districts shaded to reflect their 1994 revenue—shows that this heterogeneity is driven by the fact that initially low-revenue districts are primarily in rural areas, towns, and smaller cities, whereas initially high-revenue districts tend to be in larger cities.

III.C Capital Bond Election Analysis Sample

To estimate the effects of narrowly winning a capital bond election, we construct an election-level dataset. We focus on elections during the 1996 through 2004 academic years. 1996 is the first year that bond election data became publicly available, and 2004 is the last kindergarten cohort for which we can observe criminal justice contact through at least age 21.[9]

The construction of the election-level analysis sample is straightforward. For each election e of school district d in focal year t, we merge in outcomes of interest for that specific district. For instance, suppose that Detroit Public Schools had an election in 2002. We map the outcome of this election to the long-term outcomes of first-time kindergarteners in Detroit Public Schools in 2002, such as the share of kindergarteners who are eventually arrested as adults. We construct similar measures for the intermediate outcomes of these students such as "share of kindergarteners in Detroit in 2002 who graduate high school by age 19," or "average fourth grade math test scores." Finally, for each focal election, we merge in district-level outcomes such as total capital outlays per pupil in district d in $t-2$ (to conduct a series of placebo and balance tests) or in $t+1$, to measure the "first-stage" effect of narrowly winning an election on total capital outlays per pupil.

The final sample consists of 955 elections from the 1996 through 2004 academic years, corresponding to the 383 unique public school districts in Michigan that held at least one election during this time period. Column 1 of Table 2 describes the election-level sample. On average, districts in the sample spent roughly $900 on capital two years before their focal election. They spent approximately $9,000 in operating expenditures.

[9]Our results are robust to this choice of cohorts. For instance, the point estimates shown below are nearly identical if we instead focus on elections during the 1996 through 2006 sample period and measure arrest rates by age 19 for the youngest cohort. However, because we are interested in measuring any contact with the adult criminal justice system, and we want to allow sufficient time to observe such contact, our main election analysis focuses on the 1996–2004 cohorts.

IV First Natural Experiment: Michigan's Proposal A

IV.A Empirical Strategy

A naïve analysis of the relationship between primary school funding and adult crime might regress children's later-in-life criminal activity on the amount of school spending that the child was exposed to during primary school. Even with controls for a wide range of observable characteristics including district fixed effects, estimates from such a regression would likely be biased because children in well-funded schools differ along unobservable dimensions from those in schools with fewer resources. For instance, it may be that parental preferences for education and other factors correlated with the district's spending are still in the time-varying error term. Thus, as long as spending decisions are under the control of the school district, simple comparisons of better- and worse-funded districts cannot recover causal estimates of school spending.

To address this concern, we exploit plausibly exogenous variation in operating expenditures induced by Michigan's 1994 school finance reform and the resulting change in the school funding formula. This approach has been used to estimate the effects of additional school funding on test scores (Papke, 2005) and educational attainment (Hyman, 2017) in Michigan. Similar approaches have been used nationally to estimate the impacts of additional school funding on high school dropout rates (Hoxby, 2001) and intergenerational mobility (Biasi, 2021).

As discussed above, the foundation allowance awarded to each district each year was always a deterministic function of district-level revenue in 1994 and growth in state budget appropriations to K-12 public schools. Because the allowance was designed to equalize school funding across initially low- and high-revenue districts, spending in initially low-revenue districts was sharply increased in the years following the reform, while virtually unchanged in higher-revenue districts. We exploit this plausibly exogenous variation in spending across cohorts and districts using a research design akin to a Bartik approach (Bartik, 1991; Borusyak, Hull and Jaravel, 2022; Goldsmith-Pinkham, Sorkin and Swift, 2020), where the local "shares" are the districts' levels of 1994 revenue and the "common shocks" are state budget appropriations to education.

As Goldsmith-Pinkham, Sorkin and Swift (2020) discuss, the most natural way to convey the intuition behind the identifying variation in this setting is to highlight key groups of school districts which best illustrate the exposure design. Thus, we begin by presenting first stage and reduced form results visually in a more typical difference-in-differences framework with a binary treatment, where initially low-revenue districts are the "treatment" group and initially high-revenue ones are the "control" group. Specifically, we estimate the following specification:

$$Y_{idc} = \phi_0 + \phi_1 Treated_d + \phi_2 Treated_d \times \Phi_c + \nu_d + \Phi_c + \epsilon_{idc} \tag{1}$$

where Y_{idc} is an outcome (either the student's average operating spending during kindergarten through third grade or an indicator for ever arrested) for student i who attended district d from kindergarten through third grade in cohort c; $Treated_d$ is a dummy variable equal to one if the student's district during kindergarten through third grade was in the bottom quartile of the 1994 revenue distribution (and thus was "more treated" by the reform); ν_d and Φ_c are district and cohort fixed effects, respectively. Essentially, this specification is equivalent to a more traditional difference-in-differences analysis, where "treatment" is defined as being in an initially low-revenue district.

The coefficients on the cohort fixed effects measure how the outcome variable changes across cohorts (relative to the first cohort) for students in the top three quartiles of the 1994 revenue distribution. The coefficients ϕ_2 measure how the outcome changes across cohorts in initially-low-revenue districts relative to initially-high-revenue districts. We plot estimates of ϕ_2, as well as their 95% confidence intervals, in Figure 3.

Panel A of the figure shows the intuition for the first-stage relationship: the increase across cohorts in operating expenditures during kindergarten through third grade was greater for students in initially low-revenue districts relative to students in initially high-revenue ones. The difference-in-differences estimates are statistically significant for each cohort relative to the 1995 cohort. Panel B of the figure shows the reduced form effect on the probability of being arrested as an adult: students in initially low-revenue districts experienced a relatively larger decline across cohorts in their adult arrest probabilities.

To formally estimate the effects of increasing school funding, we instrument for a school district's operating expenditures with the district's foundation allowance using a two-stage least squares (2SLS) estimator:

First Stage:

$$log(spend)_{idc} = \delta_0 + \delta_1 log(allow)_{idc} + X_{idc}\theta + \mu_d + \pi_c + \varepsilon_{idc} \tag{2}$$

Second Stage:

$$Y_{idc} = \beta_0 + \beta_1 log(\widehat{spend})_{idc} + X_{idc}\Theta + \alpha_d + \gamma_c + \nu_{idc} \tag{3}$$

where Y_{idc} is defined as above; $log(spend)_{idc}$ is the log of the average operating spending that the student was exposed to from kindergarten through third grade;[10] X_{idc} is a vector containing student demographics such as sex, race/ethnicity, and FRPL eligibility, as well as interactions of cohort fixed effects with measures of baseline (1995) school choice, economic, and demographic characteristics of the district that the student attended from kindergarten through third grade.[11] The vector also includes interactions of cohort fixed effects with baseline district-level adult arrests per student, which we create by mapping precinct-level MSP adult arrest data to school districts during 1997, the earliest year available, and dividing by 1997 district enrollment.[12] The specification also includes

[10]We follow Johnson and Jackson (2019) and Jackson, Johnson and Persico (2016) and measure spending in logs as opposed to levels in order to benchmark the magnitude of our estimates to these two studies. Because this specification assumes that a dollar of spending may have less effect for a high-spending than a low-spending district, however, we show in Table 5 that the effects are nearly identical if we measure spending in levels instead.

[11]See Table 4 for the full list of covariates.

[12]Given that the baseline arrest control is technically measured after Proposal A, we show robustness of our main

district (μ, α) and cohort (π, γ) fixed effects. In the first stage, we instrument for $log(spend)_{idc}$ using $log(allow)_{idc}$, the log of the average allowance in the student's district from kindergarten through third grade. Finally, we cluster standard errors at the district level.

β_1 is the parameter of interest and, under assumptions that we probe below, represents the causal effect of increases in operating expenditures. Specifically, $\beta_1/10$ measures the effect of a child's exposure to 10% more operating spending during kindergarten through third grade.

Identifying Assumptions

Two main assumptions must be satisfied for our approach to yield consistent estimates of the effects of additional operating expenditures. We discuss and probe each of these assumptions below.

Relevance: This assumption requires that the district's foundation allowance strongly predicts the district's operating expenditure ($\delta_1 \neq 0$). The third row of Table 4 reports estimates of δ_1 from the first-stage specification in Equation 2. The relationship between the instrument and spending is strong. A one percent increase in the foundation allowance increases operating expenditures by 0.742 percent, with a first-stage F-statistic of 253.

Exogeneity: As discussed in Goldsmith-Pinkham, Sorkin and Swift (2020), the exogeneity assumption needed for consistency in our setting is about exogeneity conditional on observables, which include district and cohort fixed effects. Thus, this assumption implies that the "shares" (or position in the 1994 revenue distribution) are exogenous to *changes* in the outcome variable, as opposed to *levels*. In other words, our approach requires that, in the absence of Proposal A, the adult crime rates of initially low- and high-revenue districts would have trended similarly. One specific reason one may be concerned about the validity of this assumption in our context is that, since low 1994 revenue districts are disproportionately rural, our results could be driven by differential crime trends across urban and rural areas. However, it is important to note that, while the rapid decline in crime throughout the 1990s was widespread, it is well known that the largest decreases were occurring in cities (Kneebone and Raphael, 2011). Given that "more treated" districts in our context are in rural areas, this result implies that relatively rapid declines in crime in more urban areas would attenuate our estimates.

Empirically, as with difference-in-differences research designs, one can assess the plausibility of the exogeneity assumption by examining whether school districts in different positions of the 1994 revenue distribution followed similar trends prior to Proposal A. However, directly testing for parallel pre-trends in adult crime or related outcomes is challenging in our setting. Because Proposal A affects students in all grades, truly untreated cohorts would have graduated high school prior to 1995. As a result, kindergarten cohorts in the early 1990s would still be impacted by the reform, and thus we cannot simply "extend" Figure 3 back to include pre-treatment estimates—as is the case in a more typical difference-in-differences design. In fact, if we wanted to examine the adult arrest rates

results to excluding this variable in Section IV.B. We also show robustness to alternative specifications such as including region-by-cohort fixed effects. See Online Appendix D for details about mapping precinct-level MSP data to school districts.

of truly untreated cohorts, we would need to look at kindergarten cohorts prior to 1983. This test, however, is uncompelling given the large gap between post-1994 kindergarten cohorts and untreated cohorts. This logic also holds for shorter-term outcomes: the first kindergarten cohort whose fourth grade test scores would be unaffected by Proposal A is the 1990 cohort; for eighth grade attendance, it would be the 1986 cohort.

Due to these challenges, we offer two alternative tests of the identifying assumption. First, we test for parallel trends in contemporaneous district-level characteristics. Specifically, we regress the $(t-(t-1))$ change in district-level fiscal and socio-demographic outcomes on a continuous measure of 1994 district revenue from 1990 to 1994. Columns 1–7 of Table 3 show that districts were not trending differentially by 1994 revenue in either (1) fiscal outcomes and measures of school quality such as operating expenditures per pupil, teacher salaries, and class sizes, or (2) socio-demographic characteristics such as the share of students who are not White, the share of FRPL students, the share of students receiving special education services, and the district's total enrollment. Across all columns, the parameter estimate on 1994 revenue is small, supporting the assumption that district characteristics were not trending differentially by 1994 revenue prior to the reform. As an example, the estimates suggest that a 10% higher revenue in 1994 is associated with a $(t-(t-1))$ change in the percent of FRPL students of 0.05 percentage points in the years preceding Proposal A.

While we cannot examine pre-trends in the adult arrest rates of cohorts just before the reform, to offer evidence that crime rates in initially high- and low-spending districts would have trended similarly if not for Proposal A, we present an analogous test using cohorts who started school *after* the effects of Proposal A dissipated. Specifically, as Figure 1 shows, Proposal A induced variation in spending across districts and cohorts between 1995 and 2002. Thus, a test of the exogeneity assumption is that the adult arrest rates of kindergarten cohorts from 2003 on do not differ by their districts' initial position in the 1994 revenue distribution.

Figure 4 shows changes in student outcomes across cohorts in initially low-revenue districts (relative to initially high-revenue ones) for cohorts starting in 2003. The left-hand side of the figure replicates the estimates shown in Figure 3 for our main analysis sample, whereas the right-hand side shows "placebo" estimates from an identical specification but estimated on a sample of kindergarten cohorts from 2003–2008. Panel (b) presents a placebo first stage: consistent with Figure 1, it shows no change across cohorts in operating expenditures during K–3 for students in initially low-revenue districts relative to students in initially high-revenue ones. While Panels (c), (e), and (g) show that students in our analysis sample experienced declines in the probability of an adult arrest as well as improvements in intermediate outcomes such as test scores and attendance rates, Panels (d), (f), and (h) show no evidence that the outcomes of students in cohorts starting in 2003 differed by 1994 revenue. We take this as compelling evidence that the exogeneity assumption is plausible in our context.

IV.B Main Results

2SLS Estimates

Table 4 shows the 2SLS effects of additional operating expenditures on adult crime. The first row of Column 1 shows the estimate of β_1 from Equation 3, where the dependent variable is an indicator for whether the student was ever arrested as an adult. The table also shows the control mean of the dependent variable. Although there is no "control" group in our context, we define the control mean as the average of the dependent variable for districts in the top quartile of the 1994 revenue distribution (the initially highest-revenue districts). As is common in the school spending literature, the table also shows the effect of a 10% increase in K–3 spending in percent terms: dividing β_1 by ten and then dividing by the control mean.

The table shows that students exposed to 10% (roughly $1,000) additional operating expenditures per year for four years (kindergarten through third grade) have a 2 percentage point lower chance of being arrested as adults—a 15% decline in the probability of being arrested, relative to a control mean of 13%. To shed light on the economic significance of these effects, we first benchmark our results to Johnson and Jackson (2019), the only other study to estimate the effects of school spending on adult crime. Johnson and Jackson (2019) show that increasing K–12 per-pupil spending by 10% reduces the likelihood of adult incarceration by 5–8 percentage points, depending on previous Head Start exposure. Assuming spending returns are linear in years of exposure suggests increasing school funding during kindergarten through third grade by 10% reduces the probability of incarceration by 1.5–2.5 percentage points. This represents a 20–30% reduction, relative to an average incarceration rate of 8% in their sample. While arrests and incarcerations are certainly distinct outcomes, the estimates on adult arrests in our study are smaller (15%). Our estimates are also roughly half of the size as those from the reduction in arrests (20–35%) due to cognitive behavioral therapy in the Becoming a Man Program—which directly aimed to reduce crime among vulnerable youth (Heller et al., 2017).

The subsequent columns of Table 4 examine which types of crimes are most sensitive to school spending by presenting estimates of β_1 from Equation 3 on indicators for whether or not the student was ever arrested on a felony or a misdemeanor charge (Columns 2–3), and whether or not the student was ever arrested for a violent, property, drug, or public order crime (Columns 4–7). We observe similar declines for both felonies and misdemeanors (17% and 14%, respectively). While we observe declines across violent, property, drug, and public order crimes, our estimates are largest for violent (19%) and public order offenses (25%). The most common examples of violent crimes in the MSP data are assault, battery, robbery, and sexual assault. Examples of property crimes include larceny, fraud, and damage to property; drug crimes include possession and delivery of a controlled substance; and public order crimes include traffic violations, disorderly conduct, and purchase or consumption of alcohol by a minor.[13]

[13] Table A2 shows that declines in public order offenses are driven by traffic offenses and obstructing, escaping, or fleeing police officers.

Robustness

The results presented so far indicate that additional primary school funding reduces adult crime. Table 5 presents a variety of alternative specifications that are meant to probe the robustness of the main results of the paper. Column 1 shows our baseline estimate. In Column 2, we report results using the district's logged total expenditures as opposed to the district's logged operating expenditures. As expected—given that Proposal A directly targeted operating expenditures—the estimate is nearly identical to our baseline result. Column 3 reports estimates using the district's operating expenditures in levels ($1,000s) as opposed to logs. The 2.5 percentage point reduction in crime from a $1,000 increase in K–3 spending is similar to, though slightly larger than, our baseline estimate. Column 4 reports the results of a reduced form regression. Specifically, we estimate Equation 2 with an indicator for ever arrested on the left-hand side of the equation. This reduced form estimate, divided by the first stage estimate of 0.75, approximates our baseline 2SLS point estimate of -2 percentage points.

In Column 5, we drop from our sample school districts in the top quartile of the 1994 distribution. As shown in Figure 1, these districts spend substantially more than the remaining districts; thus, one may be concerned that they are systematically different and should not be included in the sample. We find that the effect of spending is similar, though somewhat larger, after removing the top quartile of districts. To ensure that our results are not driven by a single district, in Column 6 we exclude from the sample the 12% of students who attended Detroit Public Schools in kindergarten. The effect size in percent terms (12%) is similar, though slightly smaller, than our baseline estimate. In Column 7, we exclude the baseline district adult arrest rate control interacted with cohort fixed effects from the specification. As previously mentioned, this variable is measured during 1997, which is after Proposal A and thus could in theory be affected by the reform. The point estimate is nearly identical after excluding this control. Column 8 shows that the estimate is also robust to controlling for region-by-cohort fixed effects.

As discussed in Section III.B, our main analysis focuses on spending during kindergarten through third grade because there is little variation in the foundation allowance after 2003, by which time the most recent kindergarten cohort reaches grade three. To ensure consistency across cohorts, we restrict to grades three and below. The final column in Table 5 presents robustness of our main results to the choice of grade range, showing results when using spending during kindergarten through fourth, fifth, and sixth grade instead.[14] The point estimates are larger in magnitude as the grade range increases, but are generally statistically indistinguishable from our baseline estimate.[15]

As described above, given the time period (2012–2020) covered by the arrest data, and the age of students in our sample, each cohort of students is "eligible" for the arrest outcome at different ages

[14]The estimate for each grade range is from a separate regression. We do not examine spending in later grades because the first-stage F-statistic becomes substantially smaller, and because roughly 20% of students in Michigan leave public schools during high school primarily due to dropout.

[15]Consistent with limited variation in the allowance after 2003, the first-stage relationship gets weaker as we expand the grade range (K–4 F-stat: 228; K–5 F-stat: 189; K–6 F-stat: 161).

(e.g., we can observe whether students in the 1995 cohort were ever arrested from ages 22 through 30, whereas for students in the 2000 cohort we can only observe arrests from ages 17 through 25). Our primary specification simply measures whether a student was ever arrested during the respective age window in which he/she could be observed in the arrest records. Table A3 shows that our main finding (that increasing school funding significantly reduces the probability of being arrested in adulthood) is robust to alternative dependent variables such as "ever arrested by age 20" or "ever arrested during the ages of 22 through 25"—the age range in which we could observe every student in the arrest records.

Finally, one may be concerned that additional school funding causes students to leave Michigan and commit crimes as adults in other states, which we do not observe in the Michigan adult crime data. That is, the observed reduction in crime could be driven by increased out-of-state migration rather than actual decreases in criminality. We explore the extent to which out-of-state migration may influence our main findings in Table A4. We find that additional spending during kindergarten through third grade does not impact the probability that children leave the state during grades K–12 (Column 1). Column 2 shows that more school spending increases the probability of going to college outside of Michigan. While this estimate may raise some concern, we show in Column 3 that this is not true for students in baseline low-income school districts (where our effects on criminal reduction are largest). We also estimate our main specification on a sample excluding children who left Michigan in K–12 (Column 4), attended college outside of Michigan (Column 5), or attended a K–3 district in a high out-of-state migration county (Column 6). Estimates of school funding on adult crime for these samples are very similar to our main findings, suggesting that our results reflect actual reductions in criminal behavior as opposed to differential out-of-state migration.

Heterogeneity

This section examines whether the effects of additional funding on adult crime were experienced equally by distinct types of districts and students. Specifically, Table A5 presents estimates of β_1 separately for various district and student subgroups. Consistent with previous studies in the school spending literature (Baron, 2022; Jackson, Johnson and Persico, 2016; Lafortune, Rothstein and Schanzenbach, 2018; Papke, 2005), the decline in arrests is more pronounced for students in baseline low-income and low-performing school districts (Columns 1–4). Columns 5 and 6 show that the effects on adult arrests are larger for females. However, simply looking at heterogeneity by gender in overall arrests masks interesting heterogeneity by gender in which types of crimes declined due to additional school funding. Table A6 shows that males had disproportionately large declines in felonies and violent offenses, whereas females had larger declines in misdemeanors, and property and public order offenses.[16]

[16]We do not examine heterogeneity by race/ethnicity because most Black students are concentrated in a small number of urban school districts, with limited variation in funding due to Proposal A. Specifically, although there are 518 school districts in our sample, 50% of Black students attended Detroit Public Schools, 60% attended Detroit, Flint, Grand Rapids, and Saginaw City, 70% were enrolled in only 9 school districts, and 80% were enrolled in only

IV.C Mechanisms

Marginal Dollar Allocation and Improvements in School Quality

This section explores potential mechanisms driving the reduction in adult crime. We begin by exploring how treated school districts allocated the marginal allowance dollar. We perform this exercise in levels, as opposed to logs, to show how each additional *dollar* was spent, such that the sum of the effects across spending types equals the effect on overall spending. Column 1, Row 1 of Table 6 shows that an additional allowance dollar led to an increase in operating expenditures of 58 cents.[17] Of the 58 cent increase, 33 cents went toward instructional expenditures, and 25 cents to non-instructional spending such as school and district leadership (5 cents and 3 cents, respectively). As in Hyman (2017), we test whether this allocation of the marginal dollar is similar to that of the average dollar, which is mostly allocated to instructional expenditures. 57% (= 0.333/0.583) of the marginal dollar was spent on instruction, compared to 62% (=\$6,183/\$9,961) of the average dollar. This difference is not statistically significantly different from zero (*p-value* of 0.442).

Table 7 shows that the increased expenditures on instruction translated into improvements in school inputs. We focus on two key inputs employed in the school quality literature: a school district's average teacher salaries and pupil-teacher ratio. Teacher quality is one of the most important inputs in the education production function (Chetty, Friedman and Rockoff, 2014) and has been shown to impact adult crime (Rose, Schellenberg and Shem-Tov, 2021).[18] Smaller class sizes have been shown to increase standardized test scores and the likelihood of taking college-entrance exams (Krueger and Whitmore, 2001), high school graduation rates (Bloom and Unterman, 2014), college enrollment and completion, (Dynarski, Hyman and Schanzenbach, 2013), and earnings (Fredriksson, Öckert and Oosterbeek, 2013).[19]

Students exposed to 10% more school funding during K–3 experienced a 0.7 (4%) smaller student to teacher ratio during these grades. These students were also taught by teachers earning roughly \$2,500 (5%) higher salaries. Given that districts spent 8 cents of the marginal dollar on school and

19 school districts.

[17] As described in Section II, Proposal A was designed to affect only operating expenditures, and not capital outlays. Given the fungibility of money, however, one may be concerned that school districts nevertheless managed to increase capital expenditures. Such reallocation would conflate our estimated effects of additional operating expenditures with those of additional capital outlays. To explore this concern, we regress the student's K–3 capital outlays on the first-stage specification in Equation 2 in levels. We find no evidence that capital outlays increased as a result of additional allowance dollars. In fact, consistent with the work in Zimmer and Jones (2005), we find that capital outlays decrease with each additional allowance dollar, because higher spending districts that are constrained due to Proposal A issue more bonds. The decline in capital outlays implies that our main estimates of the effects of operating expenditures are conservative, since they are accompanied by declines in capital outlays.

[18] While teacher value-added would be an ideal measure to examine, we are not aware of any dataset linking students and teachers during our sample period in Michigan.

[19] There is little evidence on the effects of these school inputs on adult crime. Cano-Urbina and Lochner (2019) simultaneously estimate the effects of state-level compulsory schooling laws and direct measures of elementary and secondary school quality (pupil/teacher ratios, school term length, and teacher wage rates) on female incarceration and arrest rates throughout adulthood. The study assumes throughout that schooling laws and school quality levels are exogenous to subsequent female crime, and finds small and mixed direct effects of school quality on incarceration and arrests.

district leadership, we also examine impacts of spending on the ratio of pupils to school and district leadership (e.g., principals, vice principals, superintendents, and assistant superintendents). We find substantial reductions in these ratios. Principals and superintendents can have a strong influence on school culture, and often play a large role in responding to disciplinary and truancy incidents (Bacher-Hicks, Billings and Deming, 2019). Thus, improvements in these school inputs could play an important role in student engagement and subsequent crime reductions.

To further explore impacts on school quality, we collected data from the U.S. Department of Education Schools and Staffing Survey (SASS). This survey samples a random cross-section of school districts every few years and asks questions related to staffing levels, instructional salaries, and so on. We use three waves of the restricted-access SASS for Michigan school districts prior to Proposal A, and three waves after, to examine the effects of Proposal A on other school and teacher quality dimensions. As in our main analysis, we find that additional school spending from Proposal A reduced class sizes and increased average teacher salaries (Columns 1 and 2, Table A7). We also find a reduction in the number of new teachers hired (Column 3) and an increase in the district's average teacher experience (Column 4), driven by an increase in the number of highly-experienced teachers (Column 6). Therefore, the declines in class sizes and increases in teacher salaries appear to be driven by a reduction in teacher turnover, particularly among more experienced teachers. We find a small positive effect on base teacher salary levels, but no evidence of impacts on the fraction of teachers with a graduate degree, the fraction of teachers who are certified, or the length of the school year—all commonly used measures of school and teacher quality.

Altogether, this section shows that additional school funding from Proposal A reduced class sizes and teacher turnover, increased teacher salaries, teacher experience, and the number of school and district leadership staff that may play an important role in disciplinary and truancy incidents.

Impacts on Intermediate Outcomes

A main advantage of our detailed, administrative data is that we are able to follow students from primary school through adulthood. This allows us to understand how additional school spending during primary school impacts students' subsequent educational trajectories, which in turn may shed light on the mechanisms through which additional spending impacts adult criminality. Panel A of Table 8 shows that additional spending improves a variety of academic and behavioral outcomes that likely contribute to the reduction in adult crime.

Children exposed to 10% more spending each year during kindergarten through third grade have 12% of a standard deviation higher math test scores during fourth grade (Column 1). Interestingly, these improvements fade out completely by middle school (Column 2). This test score fadeout, followed by effects reappearing in adulthood, is consistent with the effect pattern found in other educational interventions (e.g., Head Start (Deming, 2009)), and is suggestive of long-term crime effects operating through improvements in non-cognitive skills. Indeed, we find that treated students experience considerable improvements in subsequent behavioral proxies for non-cognitive skills: treated students are 8 percentage points (54%) less likely to be chronically absent (defined as missing over 10% of school days) in eighth grade, and have a 53% lower share of missed school days. Students exposed to additional funding are also 0.3 percentage points (24%) less likely to be placed in a JDC. Finally, treated students have higher educational attainment: they are three percentage points (3.4%) more likely to graduate high school and two percentage points (4%) more likely to graduate college.[20]

These improvements in intermediate outcomes could contribute to the estimated reduction in adult crime through several channels. Because of the well-established positive relationship between educational attainment and labor market outcomes, it is possible that improvements in educational attainment increase the opportunity cost of crime through better labor market opportunities (Becker, 1968; Lochner, 2004). It is also possible that the improvements in proxies for non-cognitive skills are partly responsible for the reduction in crime: teachers who improve school attendance and reduce disciplinary incidents can significantly reduce the probability of a student's future arrest, likely due to improvements in socio-emotional skills (Rose, Schellenberg and Shem-Tov, 2021). Finally, increasing student engagement in school during a key period of criminal development could prevent students from accumulating criminal capital outside of the school (Bell, Costa and Machin, 2021).

Any combination of these channels could contribute to the observed reduction in adult criminality and we cannot definitively disentangle the relative importance of each. However, controlling for each of these intermediate outcomes in our main specification and measuring how each changes our baseline estimate of the effects of additional spending could be informative. This type of mediation analysis is akin to that in Oreopoulos, Brown and Lavecchia (2017) and Heckman and Pinto (2015). Column 1, Panel A of Table 9 replicates our baseline estimate in Table 4, but estimated on the sample of students with non-missing intermediate outcome measures.[21] Column 2 shows that our main estimate is largely unchanged when we control for fourth-grade mathematics test scores. Each subsequent column controls for a different intermediate outcome in the same way.

Controlling for high school graduation attenuates the baseline estimate from -22.7 percentage points to -18.6, indicating that high school graduation explains approximately 18% of the overall effect.[22] This finding is consistent with prior work showing that additional years of schooling reduce crime (Hjalmarsson, Holmlund and Lindquist, 2015; Lochner and Moretti, 2004; Machin, Marie and Vujić, 2011; Meghir, Palme and Schnabel, 2012), but shows that focusing only on educational attainment would understate the crime-reducing benefits of improved public schools. As another way to illustrate this point, consider the high school graduation effect in our context (2.7 percentage points) and the estimated effect of high school graduation on arrests from Lochner and Moretti (2004) (a 9% reduction in arrests from a 10 percentage point increase in the high school graduation rate).

[20]Note that educational attainment, and in particular college degree receipt, is not strictly an "intermediate outcome," as it may be realized after a student is arrested at age 17 or later.

[21]We do this to avoid conflating sample compositional changes with attenuation in the main treatment effect due to channels operating through the intermediate outcome.

[22]We do not control for college completion in the specification reported in Column 7 since this outcome may be realized *after* an individual has already been arrested. We also omit 8th grade math test scores, since they were completely unaffected by school funding. However, controlling for both of these variables in Column 7 does not further attenuate the point estimate.

These estimates suggest that the effects we find on high school graduation would reduce arrests by only 2.4% (= $(2.7/10) \times 9$). Thus, the crime-reducing effects of school funding operating through educational attainment in our context are approximately 16% (= 2.4%/15%) of the overall effect, which is strikingly similar to the 18% found through the mediation analysis.

Controlling for proxies for non-cognitive skills such as absenteeism (Column 4) attenuates the estimate from -22.7 to -13.7, suggesting that this channel explains roughly 40% of the overall effect. These results highlight that, although cognitive outcomes and educational attainment are important mechanisms, additional funding primarily impacts later-in-life crime through improvements in socio emotional "soft" skills and by keeping children engaged in school during a critical period of criminal capital formation. As a result, previous knowledge of how school funding impacts student outcomes such as educational attainment and test scores does not accurately predict how school funding will impact adult crime. For instance, even though Jackson, Johnson and Persico (2016) show that additional school funding increases educational attainment, and Lochner and Moretti (2004) show that increases in years of schooling reduce arrests, our results suggest that this channel explains a modest portion of the crime-reducing effect of school funding. These results also highlight the importance of not focusing exclusively on cognitive outcomes when evaluating the overall effects of funding, a finding consistent with recent work showing the multi-dimensionality of teacher and school quality (Beuermann et al., 2022; Jackson, 2018*b*; Petek and Pope, 2021; Rose, Schellenberg and Shem-Tov, 2021).

What About Peer Effects?

In theory, our results could also be consistent with a model of peer influence where differential exposure to fewer crime-prone students reduces the probability that the student will engage in crime later in life (Bayer, Hjalmarsson and Pozen, 2009; Billings and Hoekstra, 2019; Billings, Deming and Ross, 2019; Jacob and Lefgren, 2003). Specifically, if households with preferences for greater school funding respond to changes in spending by "voting with their feet" and moving to a district that received more money due to Proposal A, then our results could reflect improvements in outcomes due to positive changes in peer composition (Tiebout, 1956).

Panel B of Table 8 examines the potential for student re-sorting along a number of different dimensions. Column 1 shows no evidence that students exposed to more school funding during kindergarten through third grade were less likely to switch school districts during grades 4–8. Columns 2–6 examine whether students exposed to more funding during kindergarten through third grade experienced compositional changes in their district's student population during grades 4–8. Although some of the estimates in the table suggest changes in peer composition, they are modest in magnitude, and move in opposite directions. As an example, a 10% increase in spending during kindergarten through third grade leads to a 3% increase in the child poverty rate in the student's district, but also leads to precise, near-zero, changes in median income and reductions in local unemployment (0.6% changes in each). We also find no statistically significant evidence of changes in the fraction of students from baseline low-crime districts.

As an additional check, we re-do the mediation analysis with measures of peer composition during grades 4–8 as intermediate outcomes. Panel B of Table 9 shows that changes in peer composition explain virtually none of the crime-reducing effect of school funding. Whether controlling individually for each of the measures of peer composition, or controlling for all of them simultaneously, the point estimate of the effects on ever arrested is remarkably stable. A likely explanation for this result is that the school districts most impacted by Proposal A are mostly in rural areas with limited options for families to enroll their children in nearby districts.

Overall, this section highlights that improvements in school quality from increases in funding can significantly reduce criminal behavior, even in the absence of accompanying improvements in peer composition.

V Second Natural Experiment: Close Capital Bond Elections

V.A Empirical Strategy

While the total amount of a school district's *operating* expenditures is largely centralized and determined by the state, a school district in Michigan can ask its residents for permission to increase its *capital* expenditures through a local capital bond election. Districts that win elections are likely to differ both in observable and unobservable ways from districts in which the election fails. However, these differences can be mitigated by focusing on narrow elections: a school district that narrowly wins an election is likely to have similar preferences for education spending to a district in which the election narrowly fails. Therefore, to estimate the causal impact of additional capital expenditures, one can use an RD design that compares the long-run outcomes of students in districts that narrowly win an election to those of students in districts in which the election is narrowly defeated. Using the election-level dataset described in Section III.C, we estimate the following specification:

$$Y_{edt} = \Lambda_0 + \Lambda_1 f(VoteShare_{edt}) + \Lambda_2 Win_{edt} + \Lambda_3 f(VoteShare_{edt}) \times Win_{edt} + \epsilon_{edt} \qquad (4)$$

where Y_{edt} is an outcome of interest (e.g., share of first-time kindergarteners in district d in year t who are eventually arrested as adults); $VoteShare_{edt}$ represents election e's (re-centered) vote share in favor; Win_{edt} is an indicator for whether or not district residents approved the election. The parameter of interest is Λ_2, which represents the local average treatment effect (LATE) of narrowly winning an election. In other words, given likely heterogeneous treatment effects, this study cannot speak to how additional capital expenditures influence the long-run outcomes of students in inframarginal elections.

To estimate Λ_2, we use non-parametric methods with optimal bandwidths and bias-correction. Specifically, we estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). We use a triangular kernel function to construct the local polynomial estimator and Calonico, Cattaneo and Titiunik (2014)'s data-driven mean-squared-error optimal bandwidth.

The simple RD design is complicated in our setting by the dynamic nature of bond elections: a district in which the election is narrowly defeated may consider and pass a new proposal in a subsequent year. In our main analysis, we do not account for such "non-compliance" among districts with elections that initially failed. In other words, we simply stack each individual election and estimate intent-to-treat (ITT) effects of a narrow election win. ITT effects represent a combination of (1) the direct effects on outcomes of a narrow election win, and (2) its indirect effects on outcomes operating through the impact on the probability of passing a future election.

We show in Online Appendix B that our results are similar both in magnitude and precision when we use the dynamic RD design introduced by Cellini, Ferreira and Rothstein (2010) and used in Baron (2022) to recover estimates of treatment-on-the-treated (TOT) effects. We focus on estimates of ITT effects in our main analysis for several reasons. First, the issue of non-compliance appears to be small in our context: while the median number of elections per district is two, the median number of passed elections is only one. Second, ITT effects are estimable using standard RD approaches, while the dynamic RD estimates of the TOT effect embed a variety of additional assumptions related to the separability of effects over time and the homogeneity of effects across the distribution of vote shares (we elaborate on this point in Appendix B). Finally, the dynamic RD strategy is most useful when one wishes to trace out dynamic treatment effects on time-varying outcomes, such as test scores. However, the district outcomes of interest in this paper, such as the share of kindergarteners ever arrested as adults, are long-run, time-invariant outcomes.

Identification Assumptions

The RD research design uses close elections to approximate a randomized experiment. This requires that, conditional on having a very close election, a win (or loss) is as good as random. We examine two diagnostics needed for the validity of the RD design based on the distribution of the vote share and pre-election differences in observables between the treatment and control groups.

A key assumption underlying the RD design is that school districts cannot precisely control voting results around the 50% vote share (Lee and Lemieux, 2010). If treatment is indeed as good as random, then it should be equally likely that voters either just pass or just reject the election. As a result, one can infer whether there is manipulation of the vote share by examining the continuity of the vote share distribution around the threshold. Panel (a) of Figure 5 shows a histogram of the vote share for the 955 elections during our sample period. The figure shows no evidence of a discontinuity around the 50% vote share. Additionally, McCrary (2008)'s two-step test shows no statistically significant discontinuity in the density at the cutoff (Panel (b)). The histogram also shows that there is identifying variation in election outcomes near the threshold. 404 (or 47%) of all elections were decided by a margin of less than seven percentage points (the optimal Calonico, Cattaneo and Titiunik (2014) bandwidth for our main specification). 248 (26%) were decided by a margin of less than four percentage points, and 129 (14%) were decided by a margin of less than two.

We also examine whether observables are "locally" balanced in the years prior to the election, which should be the case if treatment assignment is indeed locally randomized. Columns 2 and 3 of Table 2 present summary statistics separately for all winning and losing elections, respectively. Column 4 presents regressions of fiscal outcomes and district characteristics two years before the election ($t-2$) on an indicator for whether the election was approved in time t. The estimates reveal large pre-election differences between winning and losing districts along several outcomes. Two years prior to the election, school districts that eventually pass an election have significantly higher capital and operating expenditures. Furthermore, winning districts have a lower fraction of 5-17 year olds in poverty, a higher median household income, and lower unemployment rates. Column 5 repeats this exercise but keeps only the 129 "close elections" in our sample, consisting of those that were decided by less than two percentage points (the smallest bandwidth used in the main empirical analysis below). Focusing only on close elections eliminates all statistically significant differences between winning and losing districts and substantially shrinks the point estimates.

As an additional check, Figure 6 presents typical RD plots for all elections with a vote share falling within Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth. The figures show average school district characteristics in $t-2$ in one percentage point bins along with a first-order polynomial fit. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the election passed with a vote share in the (50% - 51%] interval. The figures show no evidence of a discontinuity in school district socio-demographic characteristics near the cutoff two years before the election. Altogether, the results in this section suggest little cause for concern regarding the "as good as random" assumption of treatment assignment in close elections.

V.B Main Results

First Stage: Effects on Capital Outlays

Panel (a) of Figure 7 compares total capital outlays per pupil in $t-2$ between districts in which the focal election eventually passed and those in which it eventually failed in t. The figure shows little evidence of a discontinuity near the cutoff two years before the election, reinforcing the "local randomization" assumption described above. In contrast, Panel (b) shows clear evidence that districts that narrowly passed an election in t spent roughly $2,500 more per pupil in $t+1$, relative to districts in which the election was narrowly defeated. Panel (d) of Figure 7 shows that all of the additional funds from a bond election stick in the capital outlay account and are not reallocated to operating expenditures. This evidence of strong "flypaper effects" is common in the literature examining capital expenditure effects (Baron, 2022; Cellini, Ferreira and Rothstein, 2010; Martorell, Stange and McFarlin Jr, 2016).

To more formally quantify the magnitude (and precision) of these estimated effects, Panel A of Table 10 shows the point estimates and associated p-values of local-linear regressions. The first row presents local-linear regression estimates with bias-correction (as in Calonico, Cattaneo and Titiunik (2014)); the second and third rows report two p-values corresponding to the bias-corrected RD estimate: one derived from a conventional variance estimator and one derived from Calonico, Cattaneo and Titiunik (2014)'s derived variance estimator that is robust to the bias-correction process. The table also shows the associated Calonico, Cattaneo and Titiunik (2014) optimal bandwidth, the control mean (defined as the average of the outcome variable among elections rejected by a margin within the optimal bandwidth), and the corresponding percent effect (the point estimate divided by the control mean).

Existing studies in the literature examining the effects of capital bond elections show that capital outlay increases tend to be concentrated in the first three years after the election: districts receive the additional funds from the bond issuance and spend them soon after the election (Baron, 2022; Cellini, Ferreira and Rothstein, 2010; Martorell, Stange and McFarlin Jr, 2016). Indeed, the RD estimate shows that students in narrowly winning districts are exposed to $2,257, or 176%, additional capital outlays the year after the election. On average, students in narrowly winning districts are exposed to $943, or 44%, higher capital outlays in the three years following the election, with no increases thereafter.[23]

Second Stage: Effects on the Adult Arrest Rate

Panel (a) of Figure 8 shows that the share of first-time kindergarteners arrested as adults is lower in narrowly winning school districts (relative to narrowly losing districts). Specifically, the point estimate in Panel B of Table 10 shows that narrowly winning districts had a 2.7 percentage point lower arrest rate, corresponding to a 20% reduction relative to a control mean of nearly 14%. These effects are driven by declines in public order and misdemeanor offenses (Panels (b) and (c), Figure 8).[24]

To compare the magnitude of this effect to that of increases in operating expenditures, we compute the elasticity of adult arrests with respect to capital outlays. As shown in Section IV.B, the corresponding elasticity for operating expenditures is -1.5, since a 10% increase in operating spending from kindergarten through third grade reduces the probability of an adult arrest by 15%. Computing this elasticity for capital spending is complicated by the fact that, while operating expenditures are used toward educational inputs in the same year (such as teacher salaries), capital expenditures purchase durable assets that are productive for many years after their purchase. Ignoring the benefit of a capital expense experienced by students in the future would understate its cost-effectiveness. As

[23] Consistent with the issuance of a bond, Figure A2 shows large impacts of narrowly winning an election on both outstanding long-term debt and debt interest payments.

[24] A compelling placebo check would be to examine the impact of winning the election for students in cohorts not affected by the election. For instance, we could test the effects of narrowly passing an election for seniors in the school district during the year of the election. Unfortunately, we cannot conduct such placebo tests, because students in these cohorts are too old to be matched to the administrative arrest records. As mentioned in Section III, we only observe personal identifiable information for cohorts born in 1990 and later.

such, we follow Jackson and Mackevicius (2021) and smooth the capital expenditure over the life of the durable asset.

Specifically, we distribute the increase in capital expenditures over 15 years, the assumed lifespan for renovations and improvements to buildings in Jackson and Mackevicius (2021), which are the most common use of funds in our context. Because it would be unfair to assume that the asset produces the same benefit in the first year as in year 15 due to depreciation, we follow Jackson and Mackevicius (2021) and assume a 7% annual depreciation rate. As mentioned above, narrowly winning an election increases capital expenditures per pupil by $940 during the first three years after an election, for a total cost of $2,820 over three years. Smoothing the $2,820 over 15 years and depreciating by 7% per year produces an annual per-pupil cost of $188 in year 1 (=$2820/15), $175 (=$188 x 0.93) in year 2, and $163 (=$175 x 0.93) in year 3. This totals $526 during the three years in which the treated kindergarten cohort is in grades 1 through 3. Because the average district capital expenditure per pupil during grades 1 through 3 in our sample is $2,757, the additional $526 represents a 19.1% increase in capital outlays. Thus, the elasticity of adult arrests with respect to capital spending is approximately -1 (=-20.1 / 19.1), or about two thirds of that for operating expenditures.

Robustness

The results so far indicate that additional capital expenditures induced by a narrow election win translate into long-term declines in adult crime. Table A8 presents a variety of alternative specifications that are meant to probe the robustness of our main RD estimates.

We first test the sensitivity of the main estimate to alternative bandwidth selections. The baseline estimates are based on Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, which yields a bandwidth of 7.3 percentage points. Though they vary in terms of their precision, estimates from specifications using a 2, 4, and 6 percentage point bandwidth are similar in magnitude. Second, we show that the results are robust to using a second-order polynomial in the vote share (as opposed to the first-order polynomial used in our main specification). Finally, the results are nearly identical when we restrict the sample to students who remained in their kindergarten district through grade 6 (and were therefore very likely to have been fully exposed to the completed capital projects—which may have taken a few years to finalize). Our main specification does not restrict student movement in subsequent years, as the decision to move could itself be impacted by treatment.

Heterogeneity

Table A9 examines heterogeneity in the effects of narrowly passing an election by school district and student observable characteristics. Narrowly passing an election leads to a 2.5 percentage point decline in the share of female kindergarten students in the district who are arrested as adults. The point estimate on the effect for male students' arrest rates is similar (3.1 percentage points). Exploring heterogeneity by socio-economic status, we find that the effects are larger for FRPL-eligible students.

In terms of baseline district characteristics, effects are similar for baseline low- and high-performing school districts, but are larger for baseline low-income districts. While these results suggest that our effects are primarily driven by declines in the adult arrest probabilities of baseline low-income students and those in baseline low-income districts, it is important to note that the estimates are imprecise and should therefore be interpreted with caution.[25]

V.C Mechanisms

How do School Districts Spend Bond Dollars?

In contrast to operating expenditures, which primarily impact school quality through improvements in teacher quality and class sizes, additional capital expenditures impact school quality through improvements in school infrastructure.[26] Given that our data contain the intended purpose of each bond for most elections in our sample, we are able to broadly characterize the specific projects that additional capital expenditures are meant to fund. Most bond elections request funds for multiple projects (e.g., building a new elementary school, renovating middle school classrooms, and purchasing a new school bus). Table A11 describes the universe of projects across all elections in our sample in terms of (1) the type of project (e.g., new structures/equipment versus renovations/repairs), (2) the type of facility (e.g., a school building or an athletic facility), and (3) the target grades (e.g., elementary school, middle school, or high school).

Roughly one third of projects involved new construction or equipment purchases, while two thirds targeted additions, renovations, or improvements to existing buildings or equipment. Nearly half of all projects targeted instructional facilities or equipment; 17% were for technology, 15% for athletics, 9% for playgrounds, 4% for transportation, 3% for art facilities or equipment, and 2% for mechanical equipment or utilities. Nearly three quarters of projects were for elementary or middle schools.

Although we cannot directly show that these expenditures led to improvements in school infrastructure and equipment, institutional features strongly suggest that this is the case. Specifically, while we do not have data on the physical condition of each school's infrastructure, in Michigan, once all new construction is completed a school district must have an audit conducted for the specific project. This ensures that school districts cannot easily misreport the use of their funds. This institutional feature, the increase in capital outlays, and the lack of impacts on operating expenditures all suggest that narrowly passing a bond election in Michigan improves school districts' infrastructure.

[25]Another interesting source of heterogeneity could be the initial condition of a school district's infrastructure. In other words, there may be diminishing returns to school facility investments: students in districts with buildings in poor condition may experience substantially higher benefits from additional capital expenditures relative to students in districts with adequate baseline infrastructure. Unfortunately, to the best of our knowledge, data on the condition of each public school building in Michigan do not exist during our sample period.

[26]We find no evidence that narrowly passing an election improves inputs such as average teacher salaries, class sizes, or pupil-educational leadership ratios even 10 years after the election (Table A10).

Impacts on Intermediate Outcomes and Peer Composition

How could investments in school infrastructure reduce students' likelihood of committing crimes in adulthood? Students and parents from disadvantaged communities may be pessimistic about the returns to consistently attending school when school infrastructure is in poor conditions. Children living in disadvantaged communities tend to have high rates of school truancy, disengagement, and chronic absenteeism, all of which are closely linked to worse later-in-life outcomes (Jacob and Lovett, 2017). Modernizing and upgrading existing infrastructure may therefore improve student and parental engagement in school by increasing the expected returns to attendance.

Table 11 presents evidence in favor of this hypothesis. Although the estimates are somewhat imprecise, Columns 3 and 4 show that kindergarteners in narrowly winning school districts are roughly 25% less likely to be chronically absent and have an 11.4% lower share of days absent during the school year in eighth grade (the earliest grade for which we can measure attendance for these cohorts). We find suggestive evidence of small improvements in test scores and high school graduation, but these estimates are not statistically significant.[27] For instance, kindergarteners in narrowly winning school districts are 2.4% more likely to graduate high school, but the p-values for this estimate range from 0.283 to 0.354.[28]

The large declines in absenteeism, particularly for students at the bottom of the attendance distribution, and the relatively smaller and imprecise effects on test scores and high school graduation suggest that keeping at-risk students attending (and engaged with) school during a key period of criminal development is an important channel through which investments in infrastructure may reduce adult crime (Bell, Costa and Machin, 2021).[29] As with the impacts of Proposal A, we find little evidence for peer compositional changes.[30]

VI Cost-Benefit Analysis

This section conducts several cost-benefit analyses to examine (1) whether the crime-reducing benefits of increased school spending exceed their costs, and (2) how these costs and benefits compare to other educational and law enforcement interventions. We focus on operating expenditures as opposed to capital expenditures for these analyses.[31]

[27] Hong and Zimmer (2016) use a similar RD design in Michigan to examine effects of capital spending on test scores, finding some evidence of small increases beginning several years after bond passage.

[28] We are unable to examine whether the student ever graduated from a postsecondary education program in the RD analysis because, as described in Section III.C, we include additional cohorts relative to the Proposal A analysis sample, and thus can follow students for a shorter period of time.

[29] Changes in expected returns are not the only plausible mechanism through which attendance may increase. For instance, additional capital expenditures could reduce environmental hazards that may improve student health and attendance. Similarly, improvements in classroom technology could increase student and parental engagement.

[30] Table A12 shows the results of local-linear regressions of district demographic characteristics five and ten years after a narrow election win. None of the estimates are statistically significant. The estimates are generally modest in magnitude and do not reveal consistent evidence of improvements in peer composition.

[31] We do this for three primary reasons. First, given that capital spending is used toward durable assets lasting many years, calculating the costs and benefits of capital spending requires additional assumptions about the productive

VI.A Marginal Value of Public Funds (MVPF)

We begin by calculating the MVPF of public school funding. For additional details regarding the calculations in this section, see Online Appendix C. The MVPF is a benefit-cost framework that produces a common metric for the relative effectiveness of spending on different programs. It compares the benefits that a policy provides to society (society's willingness to pay) to the net cost to the government of implementing it (Hendren and Sprung-Keyser, 2020).

The first step in calculating the MVPF is to estimate society's willingness to pay for additional public school funding. To do so, we measure the social benefit as the reduction in social costs from the effects of additional funding on adult crime. We combine our detailed criminal justice records, which show which types of crimes (if any) students committed in adulthood, with social cost estimates for each crime type in McCollister, French and Fang (2010) and Chalfin (2015). We construct a variable equal to each student's total social cost of adult crime. This variable is equal to zero for students who were never arrested as adults and equal to the sum of the cost of each arrest for those who were arrested. For example, if a student was arrested twice, first for homicide and then for assault, then the student's social cost is the sum of the social costs of homicide and assault. Because students are exposed to additional funding many years before an adult arrest, we discount the social cost of each crime using a 3–5% discount rate (Anders, Barr and Smith, 2022). Using this variable as the outcome in our main specification, we show that the reduction in social costs from increasing school funding by 10% in K–3 ranges from about \$4,400 to \$9,000, depending on the discount rate and specific estimates of the social costs of each crime (Panel A of Table 12).[32]

The next step is to calculate the net cost to the government of increasing school funding, which includes both the direct costs of the additional funding, as well as the cost savings from less criminal activity (for example, government savings from fewer people getting arrested, convicted, and/or incarcerated). This contrasts with typical cost-benefit analyses which do not consider long-run government savings as reductions in a policy's cost. We calculate that the direct cost of increasing school funding in our context ranges from \$5,000 to \$5,200 (Panel B). For the cost savings associated with less criminal activity, we use estimates from Heckman et al. (2010) for the police and court costs associated with each arrest and the incarceration costs for a given incarceration spell. Similar to calculating the reduction in social costs, we estimate our main specification with a dependent variable that is the sum of the cost of each arrest and incarceration for each student in our sample. Panel C shows that cost savings range from about \$700 to \$1,000 depending on the discount rate. Combining the direct cost of increasing school funding with these cost savings, the net cost to the

life-span of the assets and the rate at which the value of these assets depreciate. Second, operating expenditures make up the overwhelming share of of school budgets (roughly 87%). Third, whereas policymakers can directly increase operating expenditures through state appropriations, capital expenditures are a local responsibility in most states.

[32]These estimates are not driven by the social costs of murder. Specifically, a concern may be that, because the social cost of murder (the statistical value of life) is so high, that a single murder may dominate the calculation of social benefits (Heckman et al., 2010). The estimates are only slightly smaller in magnitude and remain statistically significant if we instead assign murders half of the social cost reported in McCollister, French and Fang (2010) and Chalfin (2015), or assign murders the same social costs as assaults.

government of increasing funding is between $4,200 and $4,300.

We calculate the MVPF as the reduction in the social cost of crime divided by the net cost to the government of increasing school funding. The MVPF ranges from 1.0 to 2.1, which means that society receives between $1 and $2 in benefits for every $1 in government costs (Panel D). In other words, even considering only its crime-reducing benefits, and under quite conservative discounting assumptions, the benefits of increasing school funding are larger than the costs. These estimates are similar to those calculated in Hendren and Sprung-Keyser (2020) for social policies targeting children such as child health insurance expansions.

Importantly, our estimates are likely conservative for several reasons. First, we include only crime reductions and exclude other benefits of school funding such as increases in educational attainment and any corresponding increases in earnings. Second, the crime-specific social cost estimates in McCollister, French and Fang (2010) and Chalfin (2015) include only estimates of major index crimes. Therefore, we assign all other crimes in our sample (e.g., drug or traffic offenses) a social cost of zero. Third, the cost savings to the government exclude any savings from fewer juvenile detentions and/or any tax revenue increases from potentially higher earnings.

To illustrate this point, consider the change in the MVPF estimates from incorporating Proposal A's effects on earnings. Previous work in Hendren and Sprung-Keyser (2020), based on earnings projections from estimates of the effects of Proposal A on educational attainment in Hyman (2017), calculates a willingness to pay of $0.62 per $1 of spending, and an MVPF of 0.65. Our estimates of the willingness to pay per $1 of spending range from $0.82 (=$4,378/$5,068, Column 6, Panel A, Table 12) to $1.72 (=$8,969/$5,217, Column 1, Panel A, Table 12). The net cost per $1 of spending in our setting ranges from $0.82 (=1-($967/$5,217)) to $0.86 (=1-($731/$5,068)). Therefore, incorporating increases in earnings (in addition to the crime reduction) in the willingness to pay calculations, we obtain MVPF estimates ranging from 1.75 to 2.9. This analysis highlights (1) that our estimates of the MVPF from crime reductions alone are likely conservative, and (2) that considering social policies' effects on crime can make a striking difference when calculating the MVPF of social policies.

VI.B Comparing School Funding to Other Educational Interventions

We next compare the cost-effectiveness of school funding at preventing crime to other early educational interventions. We create an index of cost-effectiveness by dividing a policy's direct cost by its percentage point impact on the likelihood of ever being arrested. For instance, we find that a $5,217 per-pupil increase in school spending in baseline low-income school districts leads to a 3.1 percentage point reduction in the probability of ever being arrested (Table A5).[33] Thus, the amount of money spent to prevent 1 additional offender is $168,290 (=$5,217 / 0.031).[34]

[33] We focus on the estimated effects for low-income districts because the early childhood interventions that we benchmark our estimates to generally target these districts.

[34] The intuition of this calculation is that if the $5,217 per student were spent on 100 students, it would prevent 3.1 students from ever being arrested. This would be 100 x $5,217 = $521,700 spent to prevent 3.1 offenders, or $168,290 (=$521,700 / 3.1) to prevent 1 additional criminal.

This estimated cost-effectiveness is similar to that of other early childhood education interventions. For example, Anders, Barr and Smith (2022) find that Head Start has a cost to prevent 1 additional criminal of $156,250 (=$1,000 / 0.0064). The analogous cost from the Perry Preschool program is $180,000 (=$1,800 / 0.01) (Heckman et al., 2010).

VI.C Comparing School Funding To Police Spending

One of the most widely studied crime-prevention strategies is increasing the size of the police force. How cost-effective is school funding as a crime-prevention strategy relative to spending on hiring additional police officers?

Chalfin and McCrary (2018) estimate that increasing the police force size by 10% reduces the number of crimes by 4.7%. We begin by calculating the costs associated with increasing the police force size by 10%. According to Chalfin and McCrary (2018), hiring an additional police officer costs roughly $130,000, and there are approximately 262 police officers per 100,000 persons. Given the roughly 15,500 students per 100,000 persons, this implies that there are roughly 262 police officers per 15,500 students in the United States. Therefore, increasing the number of police officers by 10%, at a cost of $130,000 per police officer, yields a cost of $220 per pupil annually.

Given the elasticity of -0.47 found in Chalfin and McCrary (2018), this suggests that the cost per crime averted from investments in police officers is roughly $46.80 (=$220/4.7). However, whereas the decline in crime associated with increases in school funding takes many years to materialize, the decline in crime associated with additional police officers is immediate. Therefore, we discount the $46.80 back 13.5 years, assuming a discount rate of 3%. This yields a cost per crime averted of $31.40. Alternatively, using a 5% and 7% discount rate instead, yields a cost per crime averted of $24.22 and $18.77, respectively.

What about the cost per crime averted of school funding? Using our main specification with the total number of arrests on the left hand side, we find that students exposed to 10% greater school funding during kindergarten through third grade are arrested for 0.087, or 20.2%, fewer crimes as adults. The annual cost of additional school funding in our sample is $401 (=$5,217/13). Therefore, it costs $19.85 of school spending per pupil to avert one additional crime (=$401/20.2), an amount that is quite similar to that of hiring additional police officers.

Another relatively simple way of comparing the effects of school funding to additional police officers is to compare their benefit-cost ratios, considering only the crime-reducing benefits of school spending. Indeed, this comparison may be more accurate, as both our calculated benefit-cost ratio and those for police officers in the existing literature focus on more serious, "index" crimes. The benefit-cost ratios in police spending studies range from 0.8 to 1.6 (Chalfin and McCrary, 2018). These ratios consider only direct (not net) costs of the intervention. As a result, we adjust our calculations from Table 12 to exclude the declines in police, court, and incarceration costs associated with each prevented arrest. After removing these costs, we calculate benefit-cost ratios ranging from 0.9 to 1.9, which are similar to those of increasing the number of police officers. Importantly, our calculated "benefit-cost" ratio includes only the crime-reducing benefits of school funding, but excludes all other potential benefits (e.g., potential increases in earnings).

In summary, an increase in primary school funding appears to be a cost-effective strategy to reduce adult crime. Society's willingness to pay for the crime reductions is greater than the costs, and its crime-prevention cost-effectiveness is comparable to other early childhood education interventions such as Head Start and the Perry Preschool Project, and to increasing the number of sworn police officers.

VII Conclusion

This paper examines whether increasing the quality of public schools can reduce students' likelihood of committing crime as adults. We exploit two distinct sources of plausibly exogenous variation in school funding in Michigan, as well as a novel source of administrative records linking the universe of Michigan public school students to adult arrests. We find that students exposed to additional primary school spending, either operating or capital, experience substantial reductions in the probability of being arrested as adults. This effect is concentrated in baseline low-income and low-performing school districts.

Exploring mechanisms, we show that additional operating expenditures during primary school reduce adult crime through a combination of (1) improvements in students' educational attainment, which likely increase the opportunity cost of crime through better labor market opportunities, (2) improvements in students' socio-emotional skills, and (3) increases in school attendance during a key period of criminal capital formation. In contrast, additional capital expenditures appear to reduce adult crime primarily through the third channel described above: by decreasing the absenteeism rates of at-risk students during a critical period of criminal development. We rule out subsequent changes in peer composition as an important mechanism for both types of expenditures.

Altogether, our study contributes to the literature by showing that improvements in school quality can significantly reduce subsequent criminal behavior, even in the absence of accompanying improvements in peer composition. We conduct various cost-benefit analyses which show that increasing primary school funding is a cost-effective way to reduce crime in the long run. Importantly, we demonstrate that the MVPF of primary school funding is greater than one, even when considering only its crime-reducing benefits.

References

Anders, John, Andrew Barr, and Alexander Smith. 2022. "The Effect of Early Childhood Education on Adult Criminality: Evidence from the 1960s through 1990s." *American Economic Journal: Economic Policy*, Forthcoming.

Bacher-Hicks, Andrew, Stephen B Billings, and David J Deming. 2019. "The school to prison pipeline: Long-run impacts of school suspensions on adult crime." *NBER Working Paper #w26257.*

Baron, E Jason. 2022. "School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin." *American Economic Journal: Economic Policy*, 14(1): 1–39.

Bartik, Timothy J. 1991. *Who benefits from state and local economic development policies? Kalamazoo, MI: WE Upjohn Institute for Employment Research.*

Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building criminal capital behind bars: Peer effects in juvenile corrections." *The Quarterly Journal of Economics*, 124(1): 105–147.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169–217.

Bell, Brian, Laura Jaitman, and Stephen Machin. 2014. "Crime deterrence: Evidence from the London 2011 riots." *The Economic Journal*, 124(576): 480–506.

Bell, Brian, Rui Costa, and Stephen J Machin. 2021. "Why does education reduce crime?" *Journal of Political Economy*, Forthcoming.

Beuermann, Diether, C Kirabo Jackson, Laia Navarro-Sola, and Francisco Pardo. 2022. "What is a good school, and can parents tell? Evidence on the multidimensionality of school output." *Review of Economic Studies*, Forthcoming.

Biasi, Barbara. 2021. "School Finance Equalization Increases Intergenerational Mobility: Evidence from a Simulated-Instruments Approach." *Journal of Labor Economics*, Forthcoming.

Billings, Stephen B, and Kevin T Schnepel. 2018. "Life after lead: Effects of early interventions for children exposed to lead." *American Economic Journal: Applied Economics*, 10(3): 315–44.

Billings, Stephen B, and Mark Hoekstra. 2019. "Schools, Neighborhoods, and the Long-Run Effect of Crime-Prone Peers." *NBER Working Paper #25730.*

Billings, Stephen B, David J Deming, and Stephen L Ross. 2019. "Partners in crime." *American Economic Journal: Applied Economics*, 11(1): 126–50.

Bloom, Howard S, and Rebecca Unterman. 2014. "Can small high schools of choice improve educational prospects for disadvantaged students?" *Journal of Policy Analysis and Management*, 33(2): 290–319.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2022. "Quasi-experimental shift-share research designs." *The Review of Economic Studies*, 89(1): 181–213.

Brunner, Eric, Ben Hoen, and Joshua Hyman. 2022. "School District Revenue Shocks, Resource Allocations, and Student Achievement: Evidence from the Universe of US Wind Energy Installations." *Journal of Public Economics*, 206.

Brunner, Eric, Joshua Hyman, and Andrew Ju. 2020. "School finance reforms, teachers' unions, and the allocation of school resources." *Review of Economics and Statistics*, 102(3): 473–489.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik. 2014. "Robust nonparametric confidence intervals for regression-discontinuity designs." *Econometrica*, 82(6): 2295–2326.

Cano-Urbina, Javier, and Lance Lochner. 2019. "The effect of education and school quality on female crime." *Journal of Human Capital*, 13(2): 188–235.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. 2010. "The value of school facility investments: Evidence from a dynamic regression discontinuity design." *The Quarterly Journal of Economics*, 125(1): 215–261.

Chalfin, Aaron. 2015. "Economic costs of crime." *The encyclopedia of crime and punishment*, 1–12.

Chalfin, Aaron, and Justin McCrary. 2018. "Are U.S. Cities Underpoliced? Theory and Evidence." *Review of Economics and Statistics*, 100(1): 167–186.

Chalfin, Aaron, Benjamin Hansen, Emily K Weisburst, Morgan C Williams, et al. 2021. "Police Force Size and Civilian Race." *American Economic Review: Insights*, Forthcoming.

Chetty, Raj, John N Friedman, and Jonah E Rockoff. 2014. "Measuring the impacts of teachers II: Teacher value-added and student outcomes in adulthood." *American Economic Review*, 104(9): 2633–79.

Courant, Paul, Julie Cullen, and Susanna Loeb. 2003. "K-12 Education in Michigan." In *Michigan at the Millenium: A Benchmark and Analysis of Its Fiscal and Economic Structure.* , ed. Charles Ballard, Paul Courant, Douglas Drake, Ronald Fisher and Elisabeth Gerber.

Cullen, Julie Berry, Brian A Jacob, and Steven Levitt. 2006. "The effect of school choice on participants: Evidence from randomized lotteries." *Econometrica*, 74(5): 1191–1230.

Deming, David. 2009. "Early childhood intervention and life-cycle skill development: Evidence from Head Start." *American Economic Journal: Applied Economics*, 1(3): 111–34.

Deming, David J. 2011. "Better schools, less crime?" *The Quarterly Journal of Economics*, 126(4): 2063–2115.

Dobbie, Will, and Roland Fryer. 2015. "The Medium-Term Impacts of High-Achieving Charter Schools." *Journal of Political Economy*, 123(5): 985–1037.

Dynarski, Susan, Joshua Hyman, and Diane Whitmore Schanzenbach. 2013. "Experimental evidence on the effect of childhood investments on postsecondary attainment and degree completion." *Journal of policy Analysis and management*, 32(4): 692–717.

Dynarski, Susan, Steven Hemelt, and Joshua Hyman. 2015. "The Missing Manual: Using National Student Clearinghouse Data to Track Postsecondary Outcomes." *Educational Evaluation and Policy Analysis*, 37(1S): 53S–79S.

Enamorado, Ted, Benjamin Fifield, and Kosuke Imai. 2019. "Using a probabilistic model to assist merging of large-scale administrative records." *American Political Science Review*, 113(2): 353–371.

Fredriksson, Peter, Björn Öckert, and Hessel Oosterbeek. 2013. "Long-term effects of class size." *The Quarterly journal of economics*, 128(1): 249–285.

Garcia, Jorge Luis, James J. Heckman, and Anna L. Ziff. 2019. "Early Childhood Education and Crime." *Infant Mental Health*, 40(1).

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik instruments: What, when, why, and how." *American Economic Review*, 110(8): 2586–2624.

Grönqvist, Hans, J Peter Nilsson, and Per-Olof Robling. 2020. "Understanding how low levels of early lead exposure affect children's life trajectories." *Journal of Political Economy*, 128(9): 3376–3433.

Heckman, James J, and Rodrigo Pinto. 2015. "Econometric mediation analyses: Identifying the sources of treatment effects from experimentally estimated production technologies with unmeasured and mismeasured inputs." *Econometric reviews*, 34(1-2): 6–31.

Heckman, James, Rodrigo Pinto, and Peter Savelyev. 2013. "Understanding the mechanisms through which an influential early childhood program boosted adult outcomes." *American Economic Review*, 103(6): 2052–86.

Heckman, James, Seong Hyeok Moon, Rodrigo Pinto, Peter Savelyev, and Adam Yavitz. 2010. "The Rate of Return to the HighScope Perry Preschool Program." *Journal of Public Economics*, 94(1): 114–128.

Heller, Sara, Anuj Shah, Jonathan Guryan, Jens Ludwig, Sendhil Mullainathan, and Harold Pollack. 2017. "Thinking fast and slow? Some field experiments to reduce crime and dropout in Chicago." *Quarterly Journal of Economics*, 132(1): 1–54.

Hendren, Nathan, and Ben Sprung-Keyser. 2020. "A unified welfare analysis of government policies." *The Quarterly Journal of Economics*, 135(3): 1209–1318.

Hjalmarsson, Randi, and Lance Lochner. 2012. "The impact of education on crime: International evidence." *CESifo DICE Report*, 10(2): 49–55.

Hjalmarsson, Randi, Helena Holmlund, and Matthew J Lindquist. 2015. "The effect of education on criminal convictions and incarceration: Causal evidence from micro-data." *The Economic Journal*, 125(587): 1290–1326.

Hong, Kai, and Ron Zimmer. 2016. "Does Investing in School Capital Infrastructure Improve Student Achievement?" *Economics of Education Review*, 53: 143–158.

Hoxby, Caroline M. 2001. "All school finance equalizations are not created equal." *The Quarterly Journal of Economics*, 116(4): 1189–1231.

Hyman, Joshua. 2017. "Does money matter in the long run? Effects of school spending on educational attainment." *American Economic Journal: Economic Policy*, 9(4): 256–80.

Jackson, C Kirabo. 2018*a*. "Does school spending matter? The new literature on an old question." *NBER Working Paper #25368.*

Jackson, C Kirabo. 2018*b*. "What do test scores miss? The importance of teacher effects on non–test score outcomes." *Journal of Political Economy*, 126(5): 2072–2107.

Jackson, C Kirabo, and Claire Mackevicius. 2021. "The Distribution of School Spending Impacts." *NBER Working Paper #28517.*

Jackson, C Kirabo, Rucker C Johnson, and Claudia Persico. 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.

Jacob, Brian A, and Lars Lefgren. 2003. "Are idle hands the devil's workshop? Incapacitation, concentration, and juvenile crime." *American economic review*, 93(5): 1560–1577.

Jacob, Brian, and Kelly Lovett. 2017. *Chronic Absenteeism: An Old Problem in Search of New Answers.* Brookings Institution Evidence Speaks Series, Washington, DC.

Jácome, Elisa. 2020. "Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility." *Job Market Paper, Princeton University.*

Johnson, Rucker C, and C Kirabo Jackson. 2019. "Reducing inequality through dynamic complementarity: Evidence from Head Start and public school spending." *American Economic Journal: Economic Policy*, 11(4): 310–49.

Katz, Lawrence, Steven D Levitt, and Ellen Shustorovich. 2003. "Prison conditions, capital punishment, and deterrence." *American Law and Economics Review*, 5(2): 318–343.

Kling, Jeffrey R, Jens Ludwig, and Lawrence F Katz. 2005. "Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment." *The Quarterly Journal of Economics*, 120(1): 87–130.

Kneebone, Elizabeth, and Steven Raphael. 2011. *City and suburban crime trends in metropolitan America.* Brookings Institution Metropolitan Policy Program Washington, DC.

Krueger, Alan B, and Diane M Whitmore. 2001. "The effect of attending a small class in the early grades on college-test taking and middle school test results: Evidence from Project STAR." *The Economic Journal*, 111(468): 1–28.

Lafortune, Julien, and David Schönholzer. 2021. "The Impact of School Facility Investments on Students and Homeowners: Evidence from Los Angeles." *American Economic Journal: Applied Economics*, Forthcoming.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach. 2018. "School finance reform and the distribution of student achievement." *American Economic Journal: Applied Economics*, 10(2): 1–26.

Lee, David S, and Thomas Lemieux. 2010. "Regression discontinuity designs in economics." *Journal of economic literature*, 48(2): 281–355.

Lochner, Lance. 2004. "Education, work, and crime: A human capital approach." *International Economic Review*, 45(3): 811–843.

Lochner, Lance. 2020. "Education and Crime." In *The Economics of Education: A Comprehensive Review, 2nd Edition.* , ed. Steve Bradley and Colin Green, Chapter 9. Elsevier.

Lochner, Lance, and Enrico Moretti. 2004. "The effect of education on crime: Evidence from prison inmates, arrests, and self-reports." *American economic review*, 94(1): 155–189.

Machin, Stephen, Olivier Marie, and Sunčica Vujić. 2011. "The crime reducing effect of education." *The Economic Journal*, 121(552): 463–484.

Martorell, Paco, Kevin Stange, and Isaac McFarlin Jr. 2016. "Investing in schools: capital spending, facility conditions, and student achievement." *Journal of Public Economics*, 140: 13–29.

McCollister, Kathryn E, Michael T French, and Hai Fang. 2010. "The cost of crime to society: New crime-specific estimates for policy and program evaluation." *Drug and alcohol dependence*, 108(1-2): 98–109.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of econometrics*, 142(2): 698–714.

MDC. 2021. *Michigan Department of Corrections 2019 Statistical Report.*

Meghir, Costas, Mårten Palme, and Marieke Schnabel. 2012. "The effect of education policy on crime: an intergenerational perspective." *NBER Working Paper #18145.*

Mello, Steven. 2019. "More COPS, less crime." *Journal of Public Economics*, 172: 174–200.

Miller, Ted, Mark Cohen, and Brian Wiersma. 1996. "Victim Costs and Consequences: A New Look." U.S. Dept. of Justice, Office of Justice Programs, National Institute of Justice.

Neilson, Christopher A, and Seth D Zimmerman. 2014. "The effect of school construction on test scores, school enrollment, and home prices." *Journal of Public Economics*, 120: 18–31.

Oreopoulos, Philip, Robert S Brown, and Adam M Lavecchia. 2017. "Pathways to education: An integrated approach to helping at-risk high school students." *Journal of Political Economy*, 125(4): 947–984.

Papke, Leslie E. 2005. "The effects of spending on test pass rates: evidence from Michigan." *Journal of Public Economics*, 89(5-6): 821–839.

Petek, Nathan, and Nolan Pope. 2021. "The Multidimensional Impact of Teachers on Students."

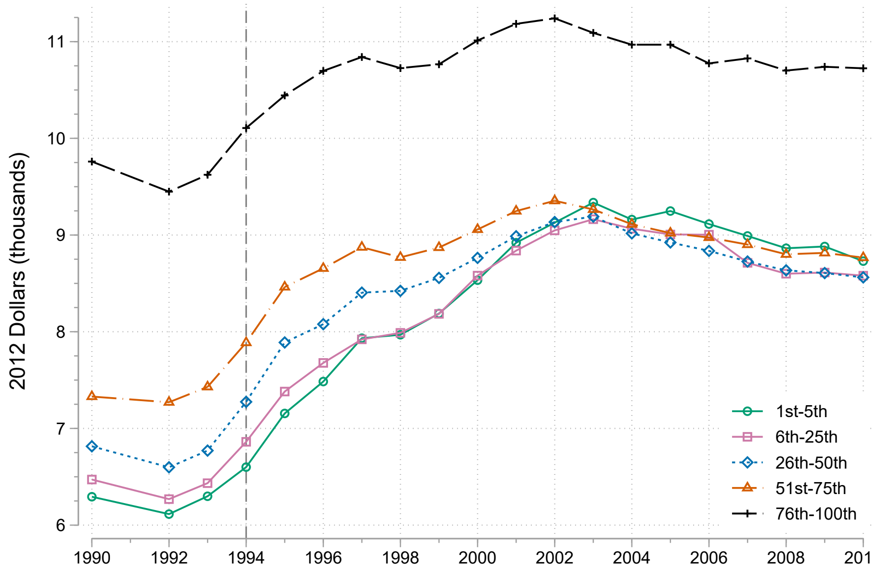
Rose, Evan, Jonathan Schellenberg, and Yotam Shem-Tov. 2021. "The Effects of Teacher Quality on Adult Criminal Justice Contact." *Working Paper.*

Rothstein, Jesse, and Diane Whitmore Schanzenbach. 2021. "Does money still matter? Attainment and earnings effects of post-1990 school finance reforms." *Journal of Labor Economics*, Forthcoming.

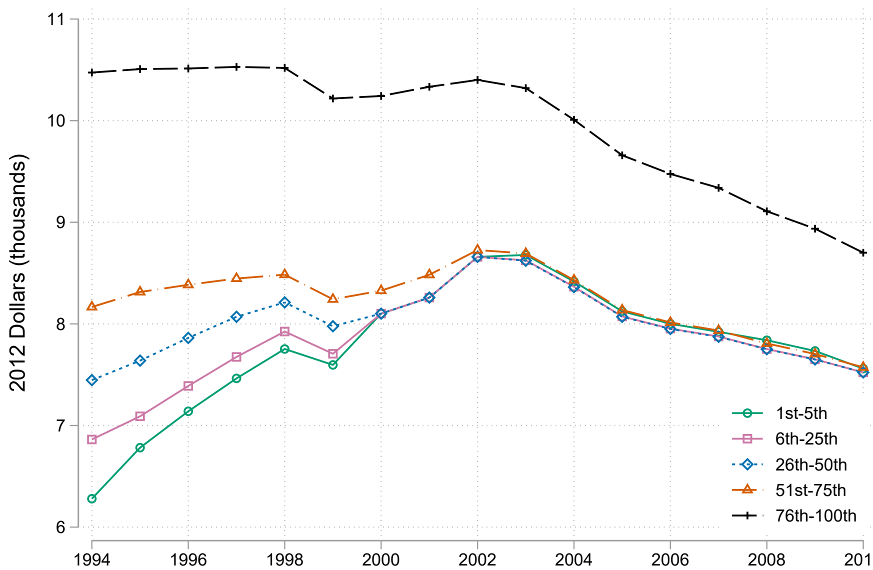
Tiebout, Charles M. 1956. “A pure theory of local expenditures.” *Journal of political economy*, 64(5): 416–424.

Zimmer, Ron, and John T Jones. 2005. “Unintended consequence of centralized public school funding in Michigan education.” *Southern Economic Journal*, 71(3): 534–544.

Figure 1: Time Series of Expenditures and Foundation Allowance by 1994 Revenue Percentile



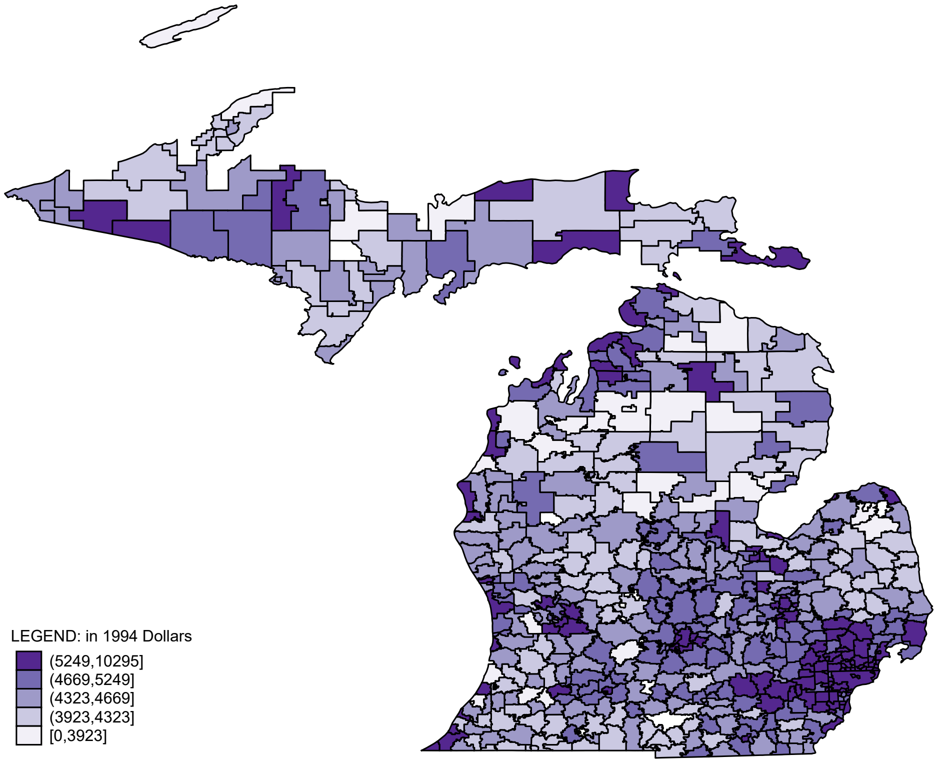
(a) Current Operating Expenditures



(b) Foundation Allowance

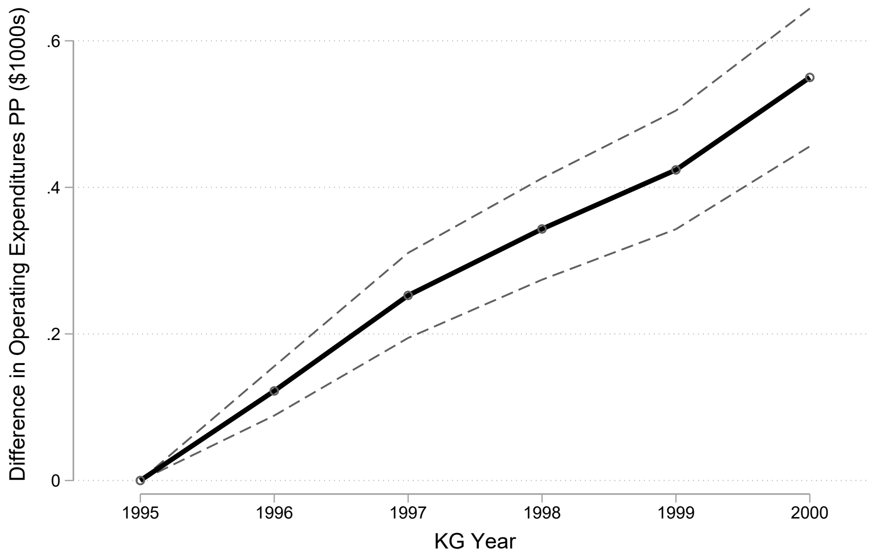
Notes: Panel A of the figure shows real average per-pupil operating expenditures over time for school districts grouped by their 1994 revenue percentile. Panel B plots the real average per-pupil foundation allowance over time for districts grouped by 1994 revenue percentiles. We convert both measures to 2012 dollars using the Employment Cost Index for elementary and secondary school employees provided by the Bureau of Labor Statistics. The 1994 value in Panel B (pre-proposal A) is the district's 1994 revenue from state and local sources.

Figure 2: 1994 Revenue by School District

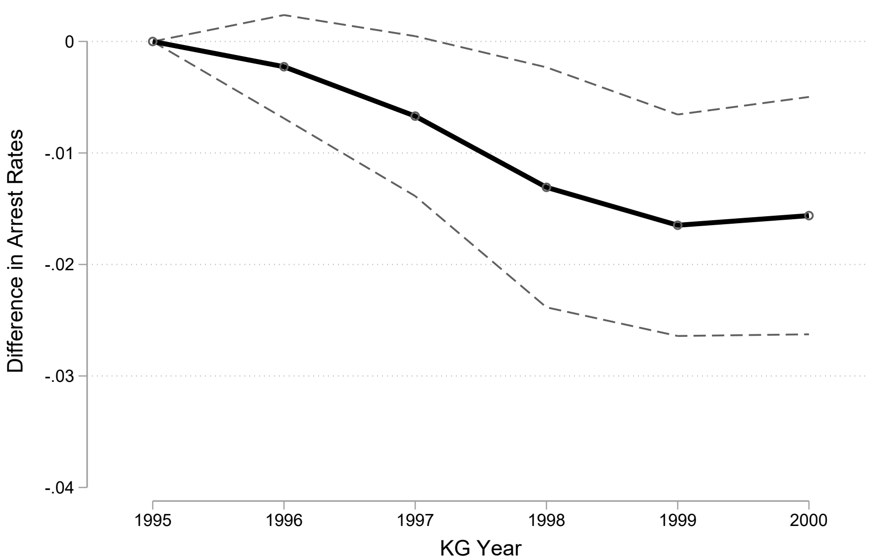


Notes: The figure plots 1994 state and local revenue per pupil for all school districts in Michigan. The darker shades correspond to higher 1994 revenue. These districts tend to appear in urban areas. The 1994 revenue bins reflect the same percentile groupings as in Figure 1 (e.g., first through fifth, sixth through twenty-fifth, and so on).

Figure 3: Differences in Predicted Spending and Arrest Rates by Cohort



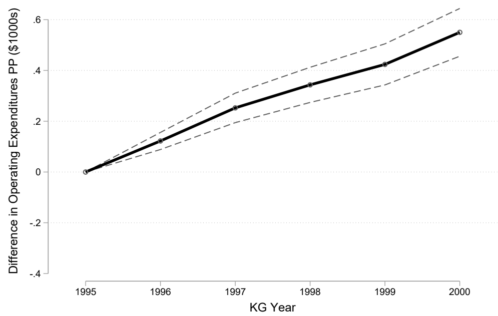
(a) K–3 Operating Expenditures

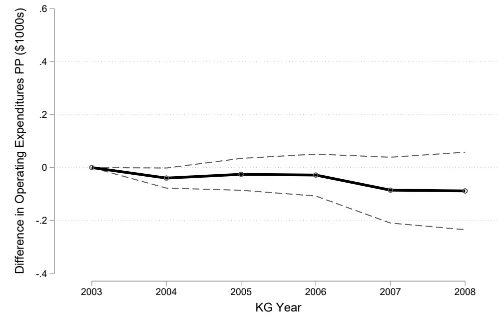


(b) Adult Arrest Rate

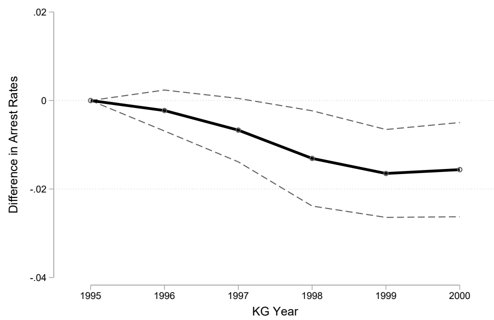
Notes: The figure plots estimates (solid line) of ϕ_2 from Equation 1, as well as their 95% confidence intervals (dashed lines). The dependent variable in Panel A is average operating expenditures in K–3. The outcome variable in Panel B is an indicator equal to one if the student was ever arrested as an adult. Estimates of ϕ_2 measure how the outcome variable changes across cohorts in initially-low-revenue districts relative to initially-high-revenue districts.

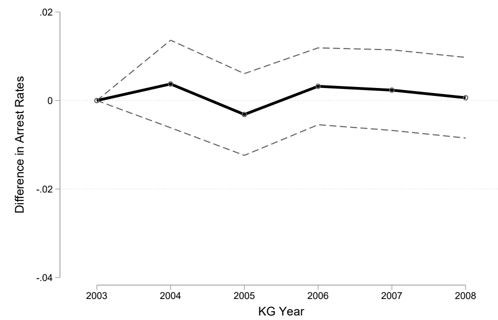
Figure 4: Differences in Outcomes by Cohorts (Treated versus Untreated)



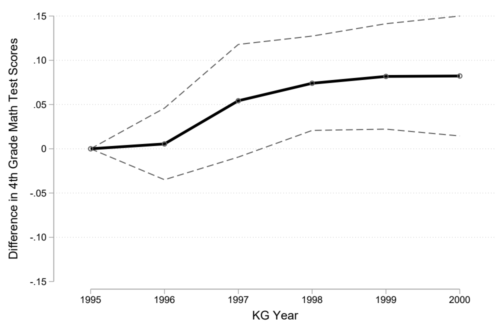


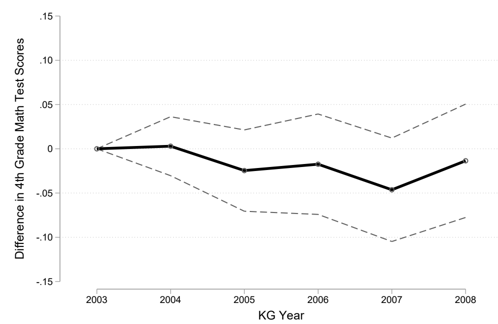
(a) K-3 Operating Expenditures (1995–2000) (b) K-3 Operating Expenditures (2003–2008)



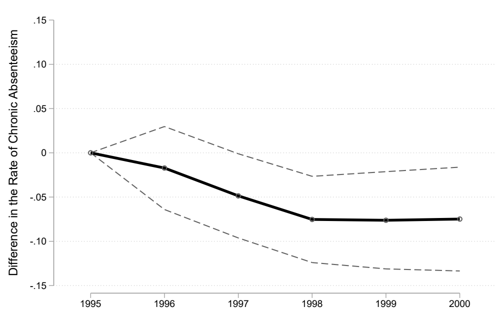


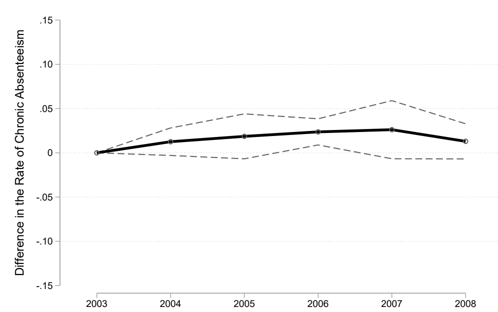
(c) Adult Arrest Rate (1995–2000) (d) Adult Arrest Rate (2003–2008)





(e) G4 Math Scores (1995–2000) (f) G4 Math Scores (2003–2008)

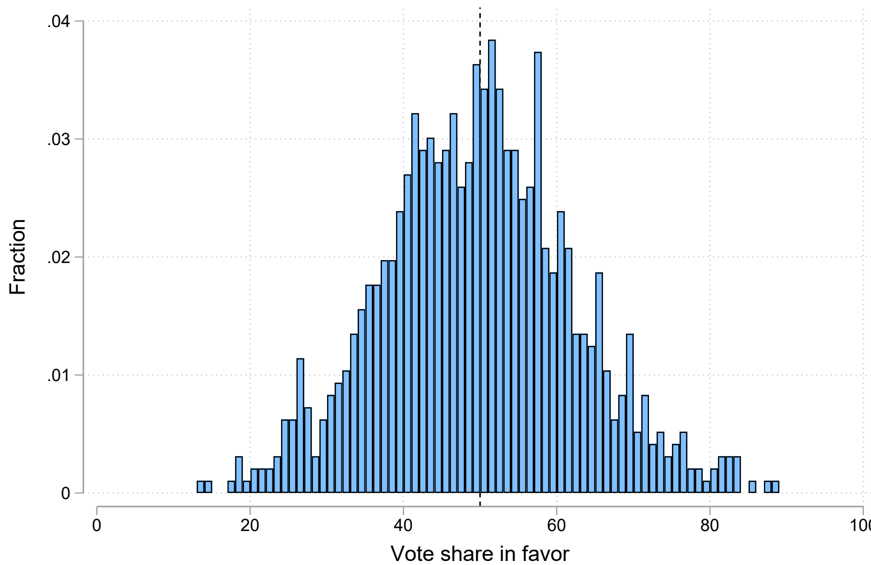




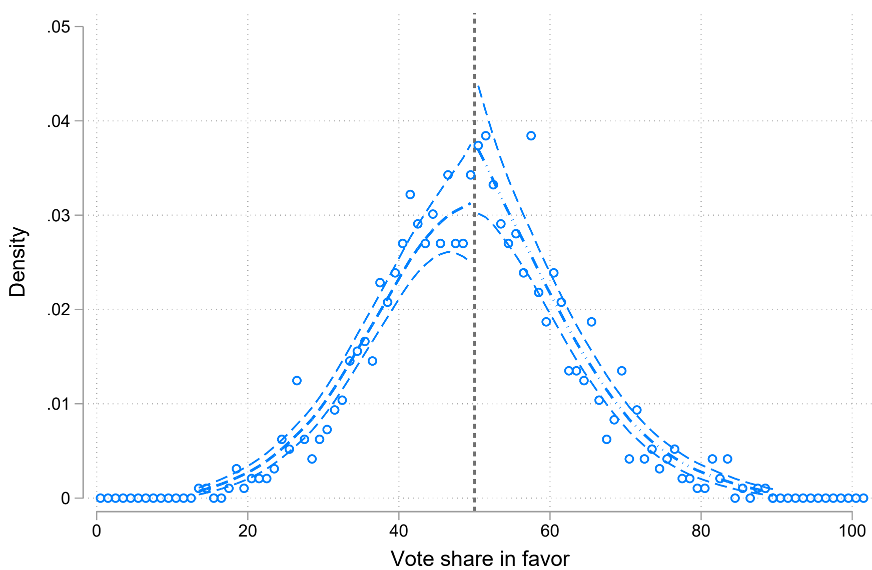
(g) G8 Chronic Absenteeism (1995–2000) (h) G8 Chronic Absenteeism (2003–2008)

Notes: The figure plots estimates and confidence intervals of ϕ_2 from Equation 1. Figures in the left column present estimates for the 1995–2000 kindergarten cohorts; those in the right present estimates for the 2003–2008 cohorts.

Figure 5: Vote Share Manipulation Tests



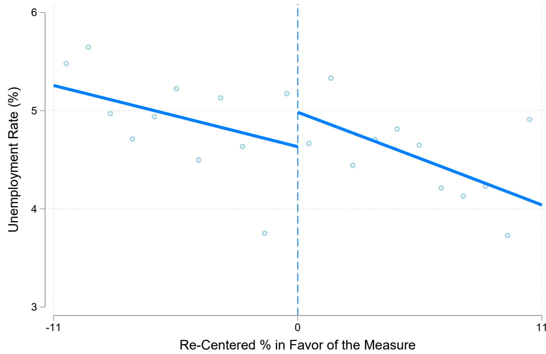
(a) Histogram



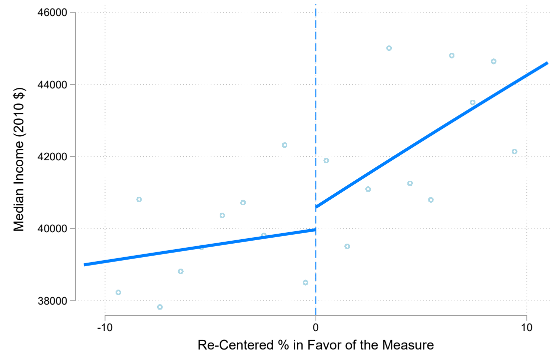
(b) Local-Linear Density Estimator

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

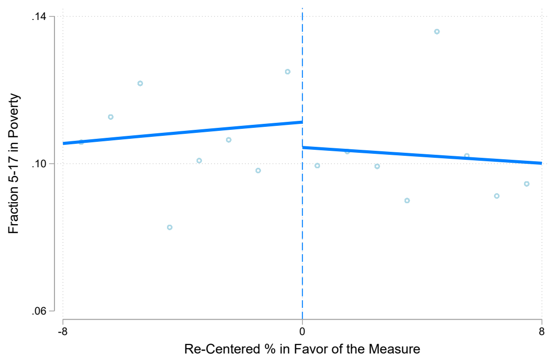
Figure 6: The “Effect” of Narrowly Winning a Capital Election on Pre-Election District Characteristics



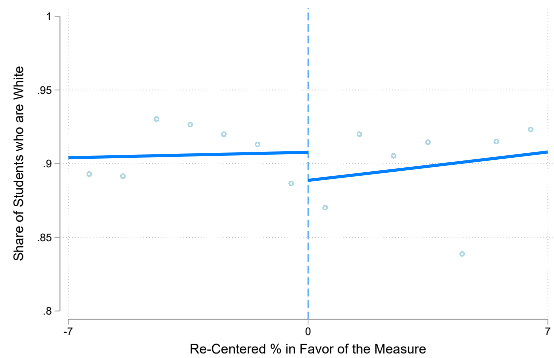
(a) Unemployment Rate (t-2)



(b) Median Income (t-2)



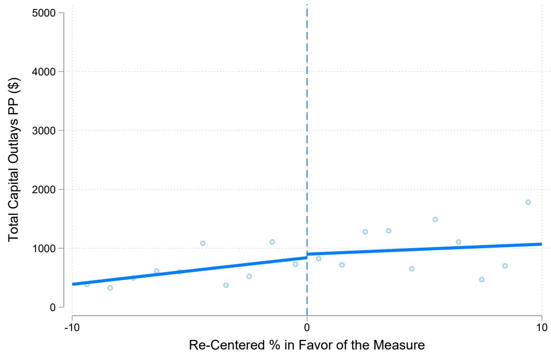
(c) Share in Poverty (t-2)



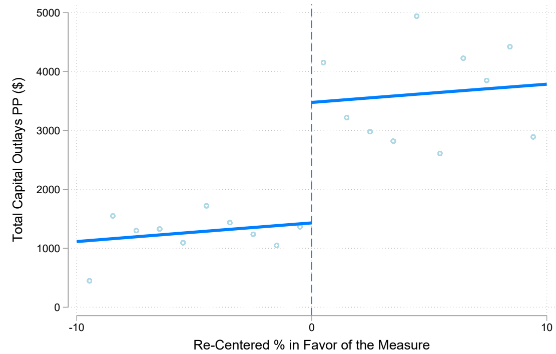
(d) Share of White Students (t-2)

Notes: The figures show average school district characteristics in $t-2$ in one percentage point bins along with a first order polynomial fit for all elections falling within Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth; t represents the year of the focal election. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the election was approved with a vote share in the (50% - 51%] interval. The local polynomial estimator was constructed with a uniform kernel function.

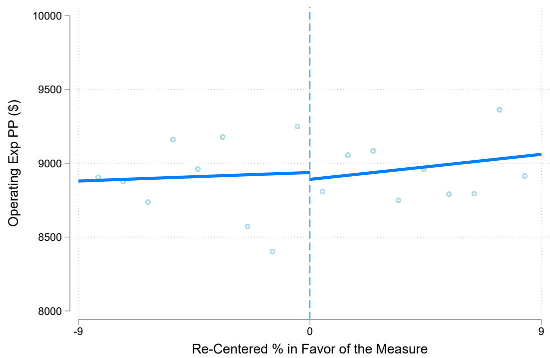
Figure 7: The Effect of Narrowly Winning a Capital Election on Fiscal Outcomes



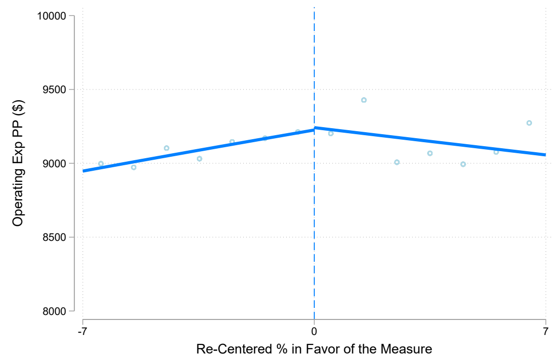
(a) Capital Outlay (t-2)



(b) Capital Outlay (t+1)



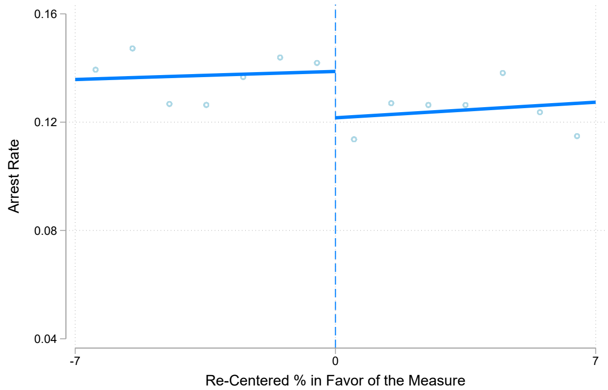
(c) Operating Expenditures (t-2)



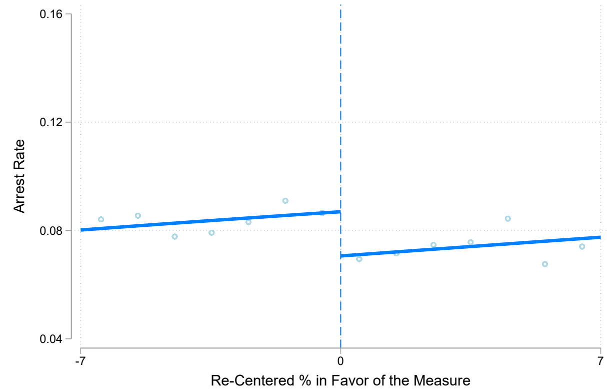
(d) Operating Expenditures (t+1)

Notes: The figures show average school district fiscal outcomes in one percentage point bins along with a first order polynomial fit for all elections falling within Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the election was approved with a vote share in the (50% - 51%] interval. Panels (a) and (c) show outcomes in $t-2$ while Panels (b) and (d) present outcomes in $t+1$; t represents the year of the focal election. The local polynomial estimator was constructed with a uniform kernel function.

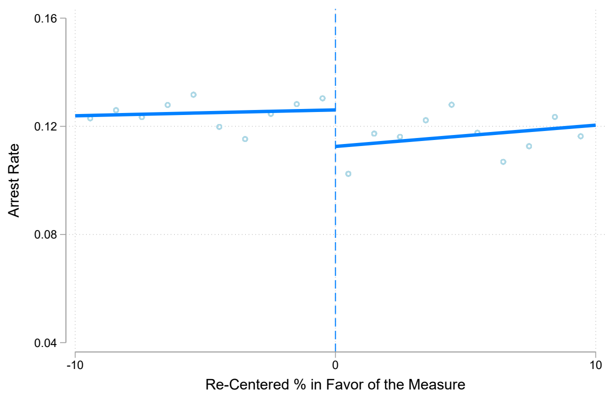
Figure 8: The Effect of Narrowly Winning a Capital Election on Adult Arrests



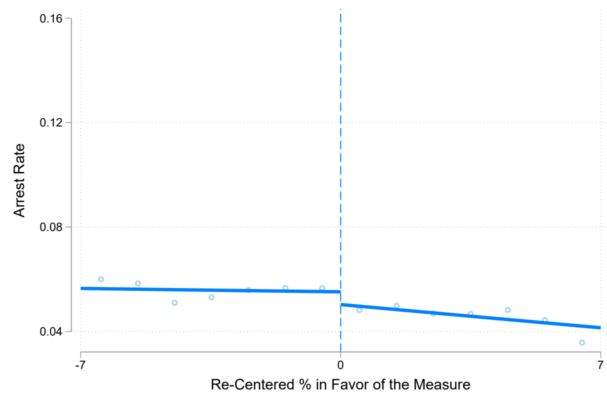
(a) Overall Arrest Rate



(b) Public Order Arrest Rate



(c) Misdemeanor Arrest Rate



(d) Felony Arrest Rate

Notes: The figures show average school district long-term outcomes in one percentage point bins along with a first order polynomial fit for all elections falling within Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the election was approved with a vote share in the (50% - 51%] interval. Panel (a) shows the overall arrest rate: for each election in district d in year t, we calculate the share of first-time kindergarteners in district d in year t who are eventually arrested as adults; t represents the year of the focal election. Panels (b), (c), and (d) repeat this exercise but for the share of individuals arrested on a public order, misdemeanor, and felony offense, respectively. The local polynomial estimator was constructed with a uniform kernel function.

Table 1: Summary Statistics for Proposal A Analysis Sample

	All Students	Never Arrested	Ever Arrested	Initially Low Revenue	Initially High Revenue
Socio-Demographic Characteristics					
Female	0.47	0.49	0.33	0.47	0.47
White	0.71	0.73	0.56	0.89	0.68
Black	0.19	0.17	0.38	0.02	0.22
Hispanic	0.04	0.03	0.04	0.03	0.04
Other	0.03	0.03	0.02	0.03	0.03
Free or Reduced Price Lunch in 8th Grade	0.32	0.29	0.53	0.36	0.32
Intermediate Outcomes					
Std. G4 Math	0.03	0.08	-0.32	0.00	0.04
Std. G8 Math	0.02	0.08	-0.43	0.04	0.02
Chronically Absent in 8th Grade	0.13	0.12	0.24	0.12	0.14
Percent of Total Days Attended in 8th Grade	0.95	0.95	0.92	0.95	0.95
Ever Placed in a JDC	0.01	0.01	0.05	0.01	0.01
Graduated High School	0.80	0.83	0.54	0.81	0.79
Earned a Postsecondary Degree	0.34	0.38	0.11	0.32	0.35
Main Outcome					
Ever Arrested	0.12	0.00	1.00	0.11	0.12
Average in K-3					
Current Operating Spending PP	9,879	9,825	10,272	8,277	10,111
Foundational Allowance PP	9,108	9,118	9,037	7,925	9,279
District-Level, Measured in Kindergarten					
Local Unemployment Rate	4.92	4.91	4.98	5.63	4.82
Median Household Income	42,950	42,948	42,961	36,146	43,936
Fraction Receiving Free or Reduced Price Lunch	0.26	0.25	0.36	0.25	0.26
Percent of 5-17 Year Olds in Poverty	0.14	0.13	0.18	0.14	0.14
Percent Attending Charter Schools	1.10	0.93	2.34	0.39	1.20
Number of Charters in the Distrct	2.18	1.75	5.30	0.07	2.48
Number of Charters in District & Adjoining Districts	5.32	4.59	10.75	0.74	5.99
District Urbanicity During Kindergarten					
Urban	0.24	0.23	0.37	0.05	0.27
Suburban	0.44	0.46	0.37	0.16	0.49
Rural	0.23	0.23	0.18	0.50	0.19
Town	0.09	0.09	0.08	0.30	0.05
Observations	717,042	631,509	85,533	90,758	626,284
Share of Observations	1.00	0.88	0.12	0.13	0.87

Notes. The table shows summary statistics for our main analysis sample. Column 1 consists of all students in our base population, while Columns 2 and 3 consist of students who were never arrested and those who were arrested as adults at least once, respectively. Column 4 describes children enrolled in initially low-revenue school districts—those in the bottom quartile of the 1994 revenue distribution—while Column 5 describes high-revenue school districts—those in the top three quartiles. We convert all spending, revenue, and income measures to 2012 dollars using the Employment Cost Index for elementary and secondary school employees provided by the Bureau of Labor Statistics.

Table 2: Summary Statistics for Election Sample and Balance Tests

	(1) All Elections	(2) Winning Elections	(3) Losing Elections	(4) Unconditional Difference (2) - (3)	(5) Conditional Difference
Panel A: Fiscal Outcomes $(t-2)$					
Capital Outlays PP	919	1,198	602	595***	-99
				(171)	(254)
Operating Expenditures PP	9,040	9,112	8,958	154	5
				(107)	(250)
Long-term Outstanding Debt PP	7,405	9,162	5,415	3,747***	670
				(614)	(2031)
Interest Payments on Outstanding Debt PP	402	504	288	215***	33
				(32)	(126)
Panel B: Demographics $(t-2)$					
Fraction of 5-17 Year Olds in Poverty	0.10	0.09	0.11	-0.02**	-0.01
				(0.01)	(0.02)
Median Household Income	42,448	44,315	40,333	3,982***	709
				(856)	(1892)
Local Unemployment Rate	4.63	4.34	4.96	-0.62***	0.37
				(0.18)	(0.52)
Share of White Students	0.90	0.90	0.90	0.01	0.00
				(0.01)	(0.03)
Number of Elections	955	470	485	955	129

Notes. The table describes the election-level analysis sample two years prior to the focal election. Panel A presents fiscal outcomes, while Panel B presents demographic characteristics. Column 1 presents summary statistics for all 955 elections in the sample. Columns 2 and 3 present these summary statistics separately for all winning and losing elections in the sample, respectively. Column 4 presents regressions of fiscal outcomes and district characteristics two years before the election $(t-2)$ on an indicator of whether or not the election was approved in time t. Standard errors clustered at the school district level are shown in parentheses below the point estimates. Column 5 repeats this exercise but keeps only the 129 "close elections" in our sample, consisting of those that were decided by less than two percentage points (the smallest bandwidth used in the main empirical analysis).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Test for Differential Pre-Trends by 1994 Revenue Levels

	(1) Operating Expenditures PP	(2) Average Teacher Salaries	(3) Pupil/ Teacher Ratio	(4) Percent Non-White	(5) Percent FRPL	(6) Percent Special Education	(7) District Student Enrollment
Log(1994 Revenue PP)	24.412	1,023.695	1.277***	-0.001	0.004	-0.005***	29.820
	(91.475)	(955.868)	(0.371)	(0.013)	(0.006)	(0.001)	(22.649)
Control Mean	8,319	75,442	22.52	23.75	21.04	10.03	3,058
Year FEs	✓	✓	✓	✓	✓	✓	✓
Student Weighted	✓	✓	✓	✓	✓	✓	

Notes. The table shows estimates from a specification where we regress the $(t-(t-1))$ change in district fiscal and socio-demographic characteristics on a continuous measure of the district's (logged) 1994 revenue, as well as year fixed effects. The specification was estimated on a 1990–1994 district $\times$ year balanced panel consisting of the 518 school districts examined throughout the paper. Standard errors clustered at the district level are shown in parentheses below the point estimates. The control mean—the average value of the dependent variable among school districts in the top quartile of the 1994 revenue distribution—is shown below the standard errors.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4: Effect of Operating Expenditures on Adult Criminal Justice Contact

	(1) Any Arrest	(2) Felony	(3) Misdemeanor	(4) Violent	(5) Property	(6) Drug	(7) Public Order
Log (Mean K-3 Spending)	-0.196***	-0.107***	-0.168***	-0.104***	-0.055	-0.057*	-0.234***
	(0.070)	(0.041)	(0.063)	(0.033)	(0.042)	(0.033)	(0.062)
First Stage Coefficient	0.742***	0.742***	0.742***	0.742***	0.742***	0.742***	0.742***
Coefficient SE	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)
F-Statistic	253	253	253	253	253	253	253
Observations	717,042	717,042	717,042	717,042	717,042	717,042	717,042
Control Mean	0.131	0.064	0.120	0.055	0.060	0.047	0.092
Percent Effect	-15.0	-16.7	-14.0	-18.9	-9.2	-12.1	-25.4
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. The third row shows estimates of δ_1 from Equation 2, while the fifth row shows the Kleibergen-Paap Wald F-statistic of this first stage. The table also presents the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 revenue distribution). Finally, the table presents the effect of a 10% increase in spending in percent terms (relative to the control mean). The dependent variable in Column 1 is a dummy variable equal to one if the student was ever arrested as an adult. The outcome in Columns 2–7 is an indicator for whether or not the student was ever arrested for that particular type of offense. The specifications control for district and cohort fixed effects, baseline demographic variables such as sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's baseline district arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Probing Estimates of the Effects of Operating Spending on Adult Arrests

	(1) Baseline	(2) Total Spending	(3) Levels	(4) Reduced Form	(5) Drop Top Quartile	(6) Drop Detroit	(7) Drop Crime Control	(8) Region-by-Cohort FEs	(9) Grade Range
Log (Mean Operating K-3 Spending)	-0.196***				-0.258***	-0.132**	-0.197***	-0.189**	
	(0.070)				(0.073)	(0.066)	(0.070)	(0.079)	
Log (Mean Total K-3 Spending)		-0.191***							
		(0.068)							
Mean Operating K-3 Spending ($1000s)			-0.025**						
			(0.011)						
Log (Mean K-3 Foundation Grant)				-0.146***					
				(0.052)					
Log (Mean Operating K-4 Spending)									-0.200**
									(0.078)
Log (Mean Operating K-5 Spending)									-0.214**
									(0.088)
Log (Mean Operating K-6 Spending)									-0.258**
									(0.101)
Observations	717,042	717,042	717,042	717,042	370,502	637,989	717,042	717,042	717,042
Control Mean	0.131	0.131	0.131	0.131	0.151	0.107	0.131	0.131	0.131
Percent Effect	-15.0	-14.6	-19.1	-11.1	-19.7	-12.2	-15.0	-14.4	[-15.3, -19.7]
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓		✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Column 1 shows our baseline estimate of β_1 from Equation 3. In Column 2, we report estimates of β_1 using the district's *total* expenditures as opposed to *operating* spending. Column 3 reports estimates of β_1 from the 2SLS specification in Equations 2 and 3, but in levels (operating expenditures and foundation allowance in $1000s) as opposed to logs. Column 4 reports the results of a "reduced-form" regression. Specifically, we estimate Equation 2 but replace the student's expenditures in K–3 with an indicator for whether the student was ever arrested as an adult. In Column 5, we drop from our sample school districts in the top quartile of the 1994 distribution. As shown in Figure 1, these districts spend substantially more than the remaining districts; thus, one may be concerned that they are systematically different and should not be included in the sample. In Column 6, we drop from our sample students who attended Detroit Public Schools in K–3. Column 7 drops as a control variable the district's baseline arrests per student interacted with cohort fixed effects. Column 8 additionally controls for region-by-cohort fixed effects. We define "regions" as the eight geographic regions established by the Michigan Governor in 2020 for the purposes of COVID-related reopening guidelines. Finally, in Column 9 we test the effects of spending in different grade ranges: K–4, K–5, and K–6, where each is from a separate regression.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: How Do Districts Spend the Additional Allowance Dollar?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Operating	Instructional	Support Services	Support Services					
				Pupils	Instruct.	General Admin.	School Admin.	Ops. and Maint.	Transp.
Mean K-3 Allowance	0.583***	0.333***	0.249***	0.001	0.041***	0.028***	0.049***	0.101***	0.023**
	(0.057)	(0.039)	(0.039)	(0.016)	(0.013)	(0.008)	(0.008)	(0.023)	(0.011)
Observations	717,042	717,042	717,042	717,042	717,042	717,042	717,042	717,042	717,042
Dep. Var. Mean	9,961	6,183	3,778	570	414	189	642	1,139	430
(A) Dep. Var. Mean / 9,961	1.000	0.624	0.376	0.056	0.041	0.020	0.065	0.113	0.044
(B) Point Estimate / 0.583	1.000	0.571	0.427	0.002	0.070	0.048	0.084	0.173	0.039
P-value (A) = (B)	0.995	0.442	0.445	0.044	0.198	0.046	0.188	0.123	0.785
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests X Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls X Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of γ_1 from Equation 2, but in levels. In other words, the dependent variable in each column is K–3 total expenditures in the particular account in \$1000s, while the independent variable is the K–3 average foundation allowance in \$1000s. Total operating expenditures (Column 1) are equal to the sum of instructional expenditures (Column 2) and expenditures for support services (Column 3). Columns 4–9 examine detailed expenditure accounts within support services. Standard errors are clustered at the district level and shown in parentheses in Row 2. The fourth row of the table shows the sample mean of the expenditure account. We test whether the marginal allowance dollar was spent differently than the average dollar by comparing the fraction of the marginal dollar spent in a given account (B) to the fraction of the average dollar in our sample in that specific account (A). The seventh row reports p-values of a statistical test that A=B. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects of Additional Operating Expenditures on Measures of School Quality

	(1)	(2)	(3)	(4)	(5)
			Pupil/Educational Leadership Ratio		
	Log(Average Teacher Salaries)	Pupil/Teacher Ratio	Superintendents and Principals	Superintendents	Principals
Log (Mean K-3 Spending)	0.461**	-7.4**	-368***	-12,059**	-383***
	(0.191)	(3.0)	(84)	(4,712)	(92)
Observations	717,042	717,042	717,042	717,042	717,042
Control Mean		21.4	286	3,507	332
Percent Effect	5	-4	-13	-34	-12
District and Cohort FE	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. The second row shows standard errors in parentheses, clustered at the district level. The fourth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Finally, the fifth row shows the effect of a 10% increase in spending in percent terms (relative to the control mean). All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects of Operating Spending on Intermediate Outcomes and Peer Composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Intermediate Outcomes						
	G4 Math Score	G8 Math Score	Chronically Absent G8	Share Days Absent G8	Ever JDC	HS Grad	Postsec Grad
Log(Mean K-3 Spending)	1.215***	-0.416	-0.833**	-0.296**	-0.034**	0.268***	0.150**
	(0.432)	(0.300)	(0.325)	(0.122)	(0.017)	(0.087)	(0.066)
Observations	674,369	636,499	652,807	652,807	717,042	684,916	717,042
Control Mean	0.029	-0.013	0.155	0.056	0.014	0.782	0.349
Percent Effect			-53.7	-52.9	-24.3	3.4	4.3
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓
	Panel B: Peer Composition						
	Ever Moved	Fraction 5-17 in Poverty	Percent Unemployment	Median Income	Fraction White	Fraction Low Crime	
Log(Mean K-3 Spending)	0.055	0.109*	-4.738***	3,053***	0.219***	0.114	
	(0.063)	(0.066)	(1.384)	(968)	(0.066)	(0.073)	
Observations	717,042	717,042	717,042	717,042	717,042	717,042	
Control Mean	0.176	0.348	7.305	47,375	0.587	0.683	
Percent Effect	3.1	3.1	-0.6	0.6	3.7	1.7	
District and Cohort FE	✓	✓	✓	✓	✓	✓	
Demographics	✓	✓	✓	✓	✓	✓	
Baseline Arrests	✓	✓	✓	✓	✓	✓	
Baseline District Controls	✓	✓	✓	✓	✓	✓	

Notes. Panel A shows estimates of additional spending on intermediate outcomes (test scores, absenteeism, JDC placement, and high school and postsecondary education). Panel B shows effects on measures of peer composition. All dependent variables in Panel B are measured during a student's fourth through eighth grade. For instance, the dependent variable in the first column is whether the student ever moved school districts during grades 4–8. Similarly, the outcome variable in the third column is the student's district's average percent unemployment in grades 4 through 8. The dependent variable in Column 6 measures the average fraction of students who come from baseline low-crime school districts in the youth's school district during grades 4–8. Baseline "low crime" is defined as below the median of the 1997 district-level arrests per student distribution. The first row of each panel shows estimates of β_1 from Equation 3. $\hat{\beta_1}/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. Given that test scores have been standardized, their effects should be interpreted as standard deviation percent changes. For instance, Column 1 of Panel A shows that a 10% increase in K–3 spending leads to an increase in test scores of 12% of a standard deviation. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Mediation Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Mediation with Intermediate Outcomes							
	Baseline	G4 Math Score	Chronically Absent G8	Share Days Absent G8	Ever JDC	HS Grad	All	
Log (Mean K-3 Spending)	-0.227***	-0.195***	-0.167**	-0.137**	-0.220***	-0.186**	-0.129*	
	(0.070)	(0.069)	(0.066)	(0.070)	(0.069)	(0.073)	(0.068)	
Observations	614,496	614,496	614,496	614,496	614,496	614,496	614,496	
	Panel B: Mediation with Peer Composition							
	Baseline	Ever Moved	Fraction 5-17 in Poverty	Percent Unemployment	Median Income	Fraction White	Fraction Low Crime	All
Log (Mean K-3 Spending)	-0.227***	-0.228***	-0.236***	-0.214***	-0.226***	-0.217***	-0.226***	-0.230***
	(0.070)	(0.069)	(0.071)	(0.072)	(0.071)	(0.073)	(0.070)	(0.073)
Observations	614,496	614,496	614,496	614,496	614,496	614,496	614,496	614,496
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Column 1 presents estimates of β_1 from Equation 3 but estimated on the sample of students with non-missing intermediate outcome measures. We do this to avoid conflating sample compositional changes with attenuation in the main treatment effect due to channels operating through the mediator. Each subsequent column controls for an intermediate outcome of interest. In Panel A, we control for each of the following intermediate outcomes: test scores, chronic absenteeism, share of days absent, ever placed in a JDC, and ever graduated from high school. In Panel B, we control for measures of peer composition in the student's district during grades 4–8: whether the student ever moved, fraction 5-17 in poverty, percent unemployment, median income, share of students who are White, and share of students who come from a baseline low-crime district. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: The Effects of Narrowly Winning an Election on Fiscal and Student Outcomes

		Panel A: Effects on Fiscal Outcomes					
Dependent Variable:		Capital Outlay PP	Capital Outlay PP	Op. Exp. PP	Op. Exp. PP		
		t+1	*[t,t+3]*	*t+1*	*[t,t+3]*		
Election Passed		2,257	943	145	113		
P values from...							
Conv. Variance Estimator		[0.000]	[0.002]	[0.520]	[0.311]		
Robust Variance Estimator		{0.000}	{0.009}	{0.579}	{0.386}		
Control Mean		1,284	2,150	9,088	9,161		
Percent Effect		175.8	43.9	1.6	1.2		
Bandwidth		9.9	10.2	7.1	6.5		
		Panel B: Effects on Adult Criminal Justice Involvement					
Dependent Variable:	Overall Arrest Rate	Felony Rate	Misd Rate	Violent Rate	Property Rate	Drug Rate	Public Rate
Election Passed	-0.027	-0.009	-0.028	-0.003	-0.009	-0.010	-0.021
P values from...							
Conv. Variance Estimator	[0.049]	[0.135]	[0.034]	[0.695]	[0.218]	[0.110]	[0.028]
Robust Variance Estimator	{0.094}	{0.206}	{0.068}	{0.746}	{0.297}	{0.175}	{0.061}
Control Mean	0.136	0.057	0.123	0.046	0.056	0.053	0.084
Percent Effect	-20.1	-15.7	-23.1	-5.9	-15.7	-18.4	-25.6
Bandwidth	7.3	6.8	10.4	7.6	9.3	8.1	7.1

Notes. The table shows the results of local-linear regressions of school districts' fiscal (Panel A) and student outcomes (Panel B). Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each panel presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). Finally, we show the effect in percent terms: the point estimate divided by the control mean, multiplied by 100. We use a triangular kernel function in each specification.

Table 11: The Effects of Narrowly Winning an Election on Intermediate Outcomes

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	G4 Math Score	G8 Math Score	Chronically Absent G8	Share Days Absent G8	HS Grad
Election Passed	-0.037	0.091	-0.025	-0.005	0.018
P values from...					
Conventional Variance Estimator	[0.428]	[0.127]	[0.082]	[0.360]	[0.283]
Robust Variance Estimator	{0.504}	{0.200}	{0.122}	{0.422}	{0.354}
Control Mean	0.033	0.006	0.100	0.046	0.751
Percent Effect			-25.1	-11.4	2.4
Bandwidth	11.0	8.3	9.2	12.4	10.2

Notes. The table shows the results of local-linear regressions of school districts' student intermediate outcomes. Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). Finally, we show the effect in percent terms: the point estimate divided by the control mean, multiplied by 100. We use a triangular kernel function in each specification.

Table 12: MVPF of Public School Funding

	(1)	(2)	(3)	(4)	(5)	(6)
Social Cost Estimates From:	McCollister, French and Fang (2010)			Chalfin (2015)		
Discount Rate:	3%	4%	5%	3%	4%	5%
Panel A: Society's Willingness to Pay						
Log (Mean K-3 Spending)	-89,689.411***	-78,256.279***	-68,178.776***	-57,793.453***	-50,359.459***	-43,784.006***
	(22,497.942)	(19,610.655)	(17,100.498)	(14,563.089)	(12,689.195)	(11,062.189)
Panel B: Direct Cost to the Government						
Grade K-3 Cost	$3,952	$3,952	$3,952	$3,952	$3,952	$3,952
Grade 4-12 Cost	$1,265	$1,188	$1,116	$1,265	$1,188	$1,116
Grade K-12 Cost	$5,217	$5,140	$5,068	$5,217	$5,140	$5,068
Panel C: Cost Savings to the Government						
Log (Mean K-3 Spending)	-9,667.973***	-8,397.941***	-7,309.151***	-9,667.973***	-8,397.941***	-7,309.151***
	(2,670.372)	(2,311.126)	(2,004.982)	(2,670.372)	(2,311.126)	(2,004.982)
Panel D: Estimates of the MVPF						
Willingness to Pay	$8,969	$7,826	$6,818	$5,779	$5,036	$4,378
Net Cost	$4,250	$4,300	$4,337	$4,250	$4,300	$4,337
MVPF	2.1	1.8	1.6	1.4	1.2	1.0

Notes. All monetary amounts are inflated to 2012 dollars. Panel A presents estimates of the social benefits of increasing spending. These are estimates of β_1 from Equation 3 where we replace the dependent variable "ever arrested" with the individual's "total social cost." This variable equals zero for individuals never arrested. For students who were ever arrested, we multiply the social cost of each crime type by the number of arrests of that type. Panel B reports the direct cost to the government of increasing school funding, as discussed in more detail in Online Appendix C. Using the same methods as in Panel A, Panel C reports estimates of β_1 from Equation 3 where the dependent variable is the student's total police, court, and incarceration costs. Panel D presents estimates of the MVPF, equal to society's willingness to pay divided by the net cost to the government of increasing school funding.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

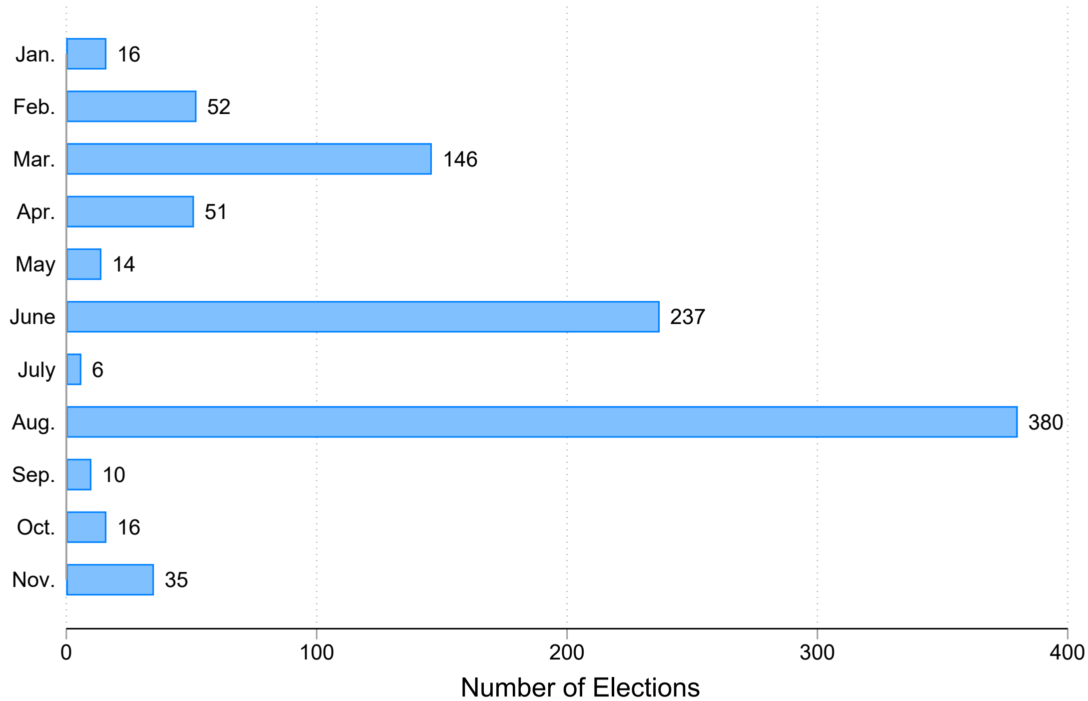
Public School Funding, School Quality, and Adult Crime

E. Jason Baron, Joshua Hyman, and Brittany Vasquez

Online Appendix

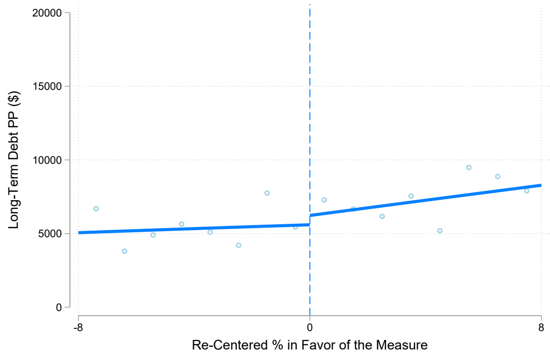
A Supplemental Online Figures and Tables

Figure A1: Distribution of Capital Elections by Month, 1996-2004

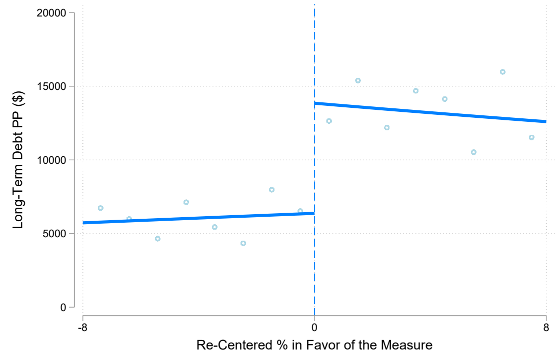


Notes: The figure shows the distribution of elections by election month. Election-level data come from MDE.

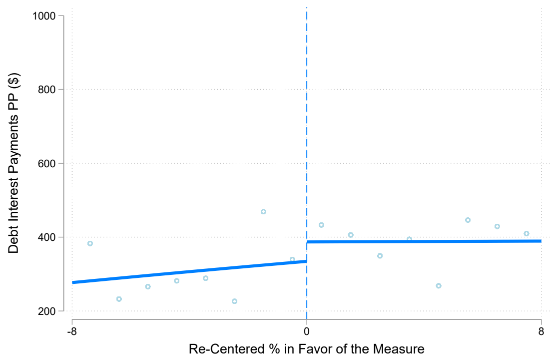
Figure A2: The Effect of Narrowly Winning a Capital Election on Other Fiscal Outcomes



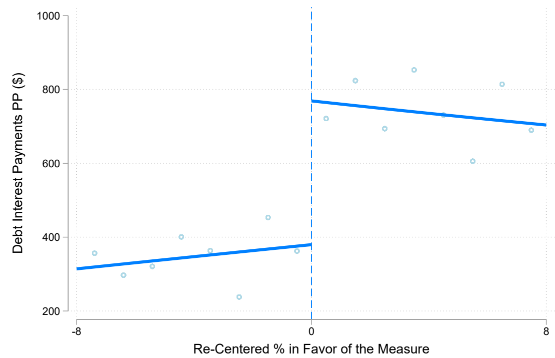
(a) Long-Term Debt (t-2)



(b) Long-Term Debt (t+1)



(c) Debt Interest Payments (t-2)



(d) Debt Interest Payments (t+1)

Notes: The figures show average school district fiscal outcomes in one percentage point bins along with a first order polynomial fit for all elections falling within Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth. Bins are defined by the vote share in favor of the measure. For instance, school districts in bin 1 are those in which the election was approved with a vote share in the (50% - 51%] interval. Panels (a) and (c) show outcomes in $t-2$ while Panels (b) and (d) present outcomes in $t+1$; t represents the year of the focal election. The local polynomial estimator was constructed with a uniform kernel function.

Table A1: Election Summary Statistics (1996-2004)

Variable	(1) N	(2) Mean	(3) Median	(4) Std Dev	(5) Min	(6) Max
Referendum Passed	955	0.49	0	0.5	0	1
Percent in Favor (re-centered)	955	-0.26	-0.24	12.48	-36.96	38.58
Amount Approved PP	470	10,797	9,295	8,231	170	59,093
# Elections Per District	383	2.49	2	1.79	1	14

Notes. The table shows summary statistics for all 955 elections held by Michigan public school districts between 1995-96 and 2003-04, the sample period of our analysis. Data on individual elections are collected and made publicly available by the Michigan Department of Education. The sample size drops in the third row because the amount approved per pupil is conditional on a winning election. Similarly, the number of elections per district is defined at the district level; there are 383 unique school districts that held at least one election during the sample period.

Table A2: Public Order Crime Breakdown

	(1) Ever Liquor	(2) Ever Traffic	(3) Ever Obstruction	(4) Ever Other
Log(Mean K-3 Spending)	-0.023	-0.153**	-0.132***	-0.072***
	(0.018)	(0.066)	(0.024)	(0.021)
Control Mean	0.009	0.072	0.028	0.023
Percent Effect	-25.6	-21.3	-47.1	-31.3
Observations	717,042	717,042	717,042	717,042
District and Cohort FEs	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. The third row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Finally, the fourth row shows the effect of a 10% increase in spending in percent terms (relative to the control mean). All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Robustness to Arrest Age Range

	(1) Ever Arrested	(2) Arrested by 20	(3) Arrested by 21	(4) Arrested by 22	(5) Arrested by 23	(6) Arrested by 24	(7) Arrested by 25	(8) Arrested 22-25
Log(Mean K-3 Spending)	-0.196***	-0.083**	-0.117**	-0.147***	-0.162***	-0.189***	-0.189***	-0.162***
	(0.070)	(0.042)	(0.049)	(0.053)	(0.058)	(0.062)	(0.065)	(0.056)
Control Mean	0.131	0.039	0.059	0.078	0.095	0.107	0.116	0.085
Percent Effect	-15.0	-21.3	-19.8	-18.8	-17.1	-17.7	-16.3	-19.1
Observations	717,042	717,042	717,042	717,042	717,042	717,042	717,042	717,042
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses and clustered at the district level. The third row shows the "control mean"—the average value of the dependent variable in initially high-spending school districts (those in the top quartile of the 1994 state and local revenue distribution). Each column shows the results of separate specifications with different dependent variables. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Migration Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
		Attend College Out-of-State				
	Leave State in K-12	All Districts	Low-Income Districts	Drop K-12 Attriters	Drop Out-of-State College	Drop High Migration Counties
Log (Mean K-3 Spending)	0.019	0.193***	0.048	-0.195***	-0.206***	-0.202***
	(0.026)	(0.055)	(0.078)	(0.072)	(0.072)	(0.074)
Observations	689,265	717,042	327,943	664,845	585,520	513,850
Control Mean	*0.033*	*0.195*	*0.172*	*0.137*	*0.146*	*0.147*
District and Cohort FE	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. The fourth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). The dependent variable in Column 1 is a dummy variable equal to one if the student ever left the state in K–12. We measure this outcome using exit codes that are assigned to students who leave the Michigan Public School system. The dependent variable in Columns 2 and 3 is an indicator variable equal to one if the student ever enrolled in postsecondary education outside of Michigan. This information comes from our K-12–NSC matched dataset. The sample in Column 3 consists only of students enrolled in baseline low-income school districts—those above the median of the 1995 district-level FRPL distribution. In Columns 4 and 5 we drop from the sample any student who (1) left the state in K–12 and (2) ever attended college outside of Michigan, respectively. Finally, in Column 5 we drop students whose K–3 districts are in "high-migration" counties—those in the top quartile of county-level migration rates. We calculate these figures using county-level out-of-state migration rates from the 2005-2009 American Community Survey (ACS).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Heterogeneity

	(1) Ever Arrested	(2) Ever Arrested	(3) Ever Arrested	(4) Ever Arrested	(5) Ever Arrested	(6) Ever Arrested	(7) Ever Arrested	(8) Ever Arrested
Log (Mean K-3 Spending)	-0.306*	-0.115	-0.358**	-0.052	-0.147*	-0.267***	-0.328***	-0.233***
	(0.159)	(0.096)	(0.143)	(0.088)	(0.079)	(0.075)	(0.118)	(0.082)
Subgroup	Low Income	High Income	Low Performing	High Performing	Male	Female	FRPL	Non-FRPL
Observations	327,943	389,099	345,950	371,092	379,968	337,074	231,235	485,807
Control Mean	0.189	0.080	0.182	0.081	0.161	0.097	0.219	0.086
Percent Effect	-16.2	-14.4	-19.7	-6.4	-9.1	-27.5	-15.0	-27.1
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes. The first row of the table shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. The fifth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Each column is a separate regression estimated on the district or student subgroup described in Row 3. Baseline low income is defined as above the median of the 1995 district-level FRPL distribution. Baseline low performing is defined as below the median of the 1995 district-level fourth-grade math test score distribution. Male, female, FRPL, and non-FRPL in Columns 5–8 are measured at the student level. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Heterogeneity by Sex and Crime Type

	(1) Any Arrest	(2) Felony	(3) Misdemeanor	(4) Violent	(5) Property	(6) Drug	(7) Public Order
Panel A: Males							
Log (Mean K-3 Spending)	-0.147*	-0.146**	-0.108	-0.083**	-0.069	-0.096**	-0.183***
	(0.079)	(0.059)	(0.073)	(0.041)	(0.050)	(0.049)	(0.068)
Observations	379,968	379,968	379,968	379,968	379,968	379,968	379,968
Control Mean	0.161	0.092	0.147	0.065	0.072	0.071	0.124
Percent Effect	-9.1	-15.9	-7.3	-12.8	-9.6	-13.5	-14.8
Panel B: Females							
Log (Mean K-3 Spending)	-0.267***	-0.069**	-0.251***	-0.026	-0.048	-0.016	-0.335***
	(0.075)	(0.031)	(0.069)	(0.032)	(0.044)	(0.028)	(0.070)
Observations	337,074	337,074	337,074	337,074	337,074	337,074	337,074
Control Mean	0.097	0.033	0.090	0.027	0.044	0.019	0.066
Percent Effect	-27.5	-20.9	-27.9	-9.6	-10.9	-8.4	-50.8
District and Cohort FE	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓
Baseline Arrests	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls	✓	✓	✓	✓	✓	✓	✓

Notes. The first row of each panel shows estimates of β_1 from Equation 3. $\hat{\beta}_1/10$ represents the effect of a 10% increase in spending per pupil for four years (K–3). The second row shows standard errors in parentheses, clustered at the district level. The fourth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Each column in Panel A is a separate regression, estimated only on the sample of male students. Similarly, each column in Panel B is estimated on the sample of female students. All specifications control for district and cohort fixed effects, the student's sex, race/ethnicity, and FRPL eligibility, and the following baseline district controls interacted with cohort fixed effects: the district's number of arrests per student, local unemployment rate, median household income, fraction receiving FRPL, fraction of 5–17 year olds in poverty, percent of students attending a charter school, and the number of charter schools in the district and in adjoining districts.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects of Proposal A on Measures of School and Teacher Quality From the SASS

	(1) Student-Teacher Ratio	(2) Average Teacher Salaries	(3) Number of New Teachers Hired	(4) Average Teacher Experience	(5) Fraction of Teachers With < 7 Years of Experience	(6) Fraction of Teachers With > 21 Years of Experience	(7) Share of Certified Teachers	(8) Share of Teachers With a Master's Degree	(9) School Year Length (Days)	(10) Base Teacher Salary
Treated × Post	-1.16**	4482.53***	-9.66*	2.19**	-0.01	0.13**	0.05	-0.03	1.20	946.17***
	(0.55)	(1429.53)	(4.98)	(1.08)	(0.04)	(0.06)	(0.05)	(0.06)	(2.12)	(342.05)
Control Mean	18.63	68,534	19.39	16.52	0.19	0.37	0.83	0.60	179.55	41,700
Percent Effect	-6.2	6.5	-49.8	13.3	-5.3	35.1	6.0	-5.0	0.7	2.3

Notes. Each column reports estimates from a separate regression estimated on a district × academic year pooled cross-section. The data for these specifications come from the U.S. Department of Education Schools and Staffing Survey (SASS). We use three waves prior to Proposal A (1988, 1991, 1994) and three waves after the reform (2000, 2004, 2008). The total number of district × academic year observations is 702, with an average of nearly 120 unique school districts per wave. We estimate the following specification: $Y_{dt} = \beta_1(Treat \times Post)_{dt} + \mu_d + \tau_t + \varepsilon_{dt}$, where Y_{dt} is an outcome for district d in year t, and $(Treat \times Post)$ is an indicator variable equal to one for school districts in the bottom quartile of the 1994 revenue distribution, observed after Proposal A. μ_d and τ_t represent school district and year fixed effects, respectively. The parameter of interest is β_1, and under the usual difference-in-differences assumptions, represents the causal effect of Proposal A on Y_{dt}. The first row of the table shows estimates of β_1. Standard errors are shown in parentheses below the point estimates, clustered at the district level. We define the control mean as the average value of the dependent variable for districts in the top three quartiles of the 1994 revenue distribution. Finally, we obtain the effect in percent terms by dividing the estimate of β_1 by the control mean.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Robustness of the Main RD Estimates

Dependent Variable:	(1) Baseline	(2) Bandwidth 2 pp	(3) Bandwidth 4 pp	(4) Bandwidth 6 pp	(5) Second Order Polynomial	(6) Children Who Stayed
Election Passed	-0.027	-0.026	-0.038	-0.038	-0.034	-0.025
P values from...						
Conv. Variance Estimator	[0.049]	[0.296]	[0.034]	[0.012]	[0.049]	[0.071]
Robust Variance Estimator	{0.094}	{0.440}	{0.141}	{0.077}	{0.078}	{0.124}
Control Mean	0.136	0.158	0.155	0.147	0.137	0.133
Percent Effect	-20.1	-16.3	-24.5	-25.7	-25.1	-18.4
Bandwidth	7.3	2	4	6	9.5	7.2

Notes. The table presents the robustness of our main local-linear regression estimate. Column 1 presents the baseline estimate, which was derived using Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth and a first-order polynomial. In Columns 2–4, we probe the sensitivity of the baseline estimate to alternative bandwidths (2, 4, and 6 percentage points). In Column 5, we estimate Λ_2 using a second-order polynomial instead. Finally, Column 6 restricts the sample to students who remained in their kindergarten district through sixth grade.

Table A9: Heterogeneity in RD Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Arrest Rate Females	Arrest Rate Males	Arrest Rate FRPL	Arrest Rate Non-FRPL	Baseline Low Perf.	Baseline High Perf.	Baseline Low Income	Baseline High Income
Election Passed	-0.025	-0.031	-0.025	0.009	-0.027	-0.024	-0.025	-0.013
P values from...								
Conv. Variance Estimator	[0.060]	[0.077]	[0.161]	[0.654]	[0.157]	[0.276]	[0.257]	[0.459]
Robust Variance Estimator	{0.108}	{0.132}	{0.235}	{0.720}	{0.220}	{0.366}	{0.337}	{0.534}
Control Mean	0.084	0.185	0.180	0.178	0.147	0.127	0.153	0.115
Percent Effect	-29.6	-16.5	-13.8	5.1	-18.4	-19.1	-16.1	-11.7
Bandwidth	6.4	8.4	7.6	8.8	6.9	8.1	6.6	8.6

Notes. The table shows the results of local-linear regressions of school districts' outcomes. Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). We also present the effect in percent terms: the point estimate divided by the control mean, multiplied by 100. We use a triangular kernel function in each specification.

Table A10: Effects of Narrowly Passing a Capital Election on School Inputs 10 Years Later

	(1) Log(Average Teacher Salaries)	(2) Pupil/ Teacher Ratio	(3) Pupil/ Superintendent and Principal Ratio
Election Passed	0.039	0.369	-14.9
P values from...			
Conventional Variance Estimator	[0.300]	[0.494]	[0.496]
Robust Variance Estimator	{0.392}	{0.565}	{0.562}
Control Mean		21.9	278.7
Percent Effect	3.9	1.7	-5.4
Bandwidth	8.4	10.5	8.1

Notes. The table shows the results of local-linear regressions of the school district's (logged) average teacher salaries, pupil-teacher ratio, and pupil-superintendent and principal ratio 10 years after the focal election in year t. Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each panel presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). We use a triangular kernel function in each specification.

Table A11: Distribution of Capital Projects in our Sample

Project Type	*Share*
New Structure/Equipment	0.345
Additions/Renovatons/Improvements	0.654
Facilty / Equipment Type	
Instructional	0.487
Technology	0.166
Athletics	0.145
Playground	0.093
Busses / Transportation	0.039
Arts	0.025
Mechanical / Utilities	0.022
Other	0.023
Level	
Elementary School	0.479
Middle School	0.251
High School	0.355
Other	0.012

Notes. The table shows the fraction of all capital projects in our sample by the type of project, the target facility or equipment, and the target grade. The share of projects that target each grade level sum to greater than one because we count projects listed as "middle/high school" as targeting both middle and high school.

Table A12: Examining Sorting Following a Close Election

	Panel A: Outcomes in t+5			
Dependent Variable:	Fraction 5-17 in Poverty	Unemployment Rate (%)	Median Income	Fraction White
Election Passed	-0.019	-0.077	2,409	-0.001
P values from...				
Conventional Variance Estimator	[0.150]	[0.903]	[0.210]	[0.979]
Robust Variance Estimator	{0.227}	{0.920}	{0.298}	{0.982}
Control Mean	0.129	7.514	42,832	0.898
Percent Effect	-14.7	-1.02	5.62	-0.11
Bandwidth	9.2	7.5	9.4	7.2
	Panel B: Outcomes in t+10			
Dependent Variable:	Fraction 5-17 in Poverty	Unemployment Rate (%)	Median Income	Fraction White
Election Passed	-0.015	-0.426	-1,132	0.019
P values from...				
Conventional Variance Estimator	[0.465]	[0.600]	[0.637]	[0.608]
Robust Variance Estimator	{0.552}	{0.661}	{0.691}	{0.666}
Control Mean	0.157	9.846	48,018	0.871
Percent Effect	-9.55	-4.33	-2.36	2.18
Bandwidth	8.2	7.8	10.5	8.6

Notes. The table shows the results of local-linear regressions of school districts' demographics 5 (Panel A) and 10 (Panel B) years after the focal election in year t. Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each panel presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). We use a triangular kernel function in each specification.

B Dynamic Regression Discontinuity Design

Motivation and Description of the Dynamic RD Approach

As discussed in the main text, the simple RD design is complicated in our setting by the dynamic nature of bond elections: a district in which the election is narrowly defeated may consider and pass a new proposal in a subsequent year. In our main analysis, we do not account for such "non-compliance" among districts with elections that initially failed. In other words, we estimate intent-to-treat (ITT) effects of a narrow election win. ITT effects represent a combination of (1) the direct effects on outcomes of a narrow election win, and (2) its indirect effects on outcomes operating through the impact on the probability of passing a future election.

To see this, suppose there are two school districts, A and B, that attempt a capital bond election in time t. Further suppose that A narrowly passes the election, while B narrowly loses. In a setting where districts may attempt and pass multiple elections, it would be difficult to draw inferences from a simple comparison of outcomes between districts A and B in subsequent years. For example, if district A also passes a capital bond referendum in $t+3$, then differences in student outcomes between the two districts in $t+5$ will not solely be due to the election passed in t.

In this section, we use the "one-step" dynamic RD estimator developed by Cellini, Ferreira and Rothstein (2010) to estimate "treatment-on-the-treated" (TOT) effects and isolate only the direct effects of a particular successful election. Estimates of TOT effects yield the causal impacts of successful elections, holding subsequent election outcomes constant. Thus, in the example above, this approach would directly control for the districts' intermediate behavior (from t to $t+5$). Intuitively, the dynamic RD approach compares the outcomes of districts in which a particular election at some point in time was narrowly successful to districts where the election was narrowly defeated—but the sequence of prior and subsequent election outcomes is similar.

Cross-Sectional Setup

More formally, suppose that school district d holds a capital bond election that receives vote share v_d^b. Let $P_d^b = 1(v_d^b > 50)$ be an indicator for a successful election. We can write some district-level outcome y_d (e.g., capital expenditures) as:

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

where γ is the causal effect of a successful bond election on y_d and ϵ_d represents all additional determinants of y_d, with $E[\epsilon_d] = 0$.

RD with Panel Data and Multiple Treatments

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

$$y_{dt} = \sum_{\tau=0}^{\bar{\tau}} [P^b_{d,t-\tau}\gamma_\tau] + \epsilon_{dt} \tag{B.2}$$

There are two possible definitions of the causal effect of a successful election in $t-\tau$ on an outcome in year t. First, one can examine the effect of exogenously passing an election in district d in year $t-\tau$ and "prohibiting" the district from passing any subsequent elections. From Equation B.2, these effects are captured by γ_τ, since the equation holds constant all other elections wins. These effects are known as the "treatment on the treated" (TOT)—γ_τ^{TOT}. Therefore, a consistent estimate of γ_τ^{TOT} will isolate the impact of an election win (with no intermediate election-approved changes to the district's resources) in $t-\tau$ on a district's outcome in t.

An alternative to examining TOT effects is to focus on the impact of passing an election in $t-\tau$ and "allowing" the school district to make decisions regarding subsequent elections as its residents wish. This effect, known as the "intent-to-treat" (ITT), incorporates effects of $P^b_{d,t-\tau}$ on y_{dt} operating through additional bond election wins in intermediate years $\{P^b_{d,t-\tau+1}, P^b_{d,t-\tau+2}, ..., P^b_{dt}\}$. Thus, ITT estimates do not necessarily reflect the impact of additional expenditures solely associated with winning a particular election. For reasons described in the main body of the paper, our primary analysis focused on estimates of the γ_τ^{ITT}'s.

Estimating TOT Effects

A simple regression like Equation B.2 would likely yield biased estimates of the γ_τ^{TOT}'s as factors in ϵ_{dt} are likely to be correlated both with concurrent and past successful elections. However, since there is no evidence of manipulation of the vote share near the 50% threshold in our sample, the correlation between P^b_{dt} and ϵ_{dt} can be kept close to zero by focusing only on close elections. Therefore, to estimate the causal impact of additional capital spending, one can use an RD design that compares outcomes in school districts that narrowly pass an election to those where the election is narrowly defeated. We follow Cellini, Ferreira and Rothstein (2010), Baron (2022), and Martorell, Stange and McFarlin Jr (2016), and implement the main design using a parametric framework that retains all observations in the sample but absorbs variation from non-close elections with flexible controls for the vote share.

Accordingly, we augment Equation B.2 with flexible polynomials of degree g in the vote share, $f_g(v^b_{d,t-\tau})$, and with indicators for the presence of a capital bond election in year $t-\tau$—$m^b_{d,t-\tau}$.[35] After adding school year (θ_t) and district-level (μ_d) fixed effects, the estimating equation becomes:

[35] $v^b_{d,t-\tau} = 0$ if district d did not hold a capital bond election in year $t-\tau$.

$$y_{dt} = \sum_{\tau=\underline{\tau}}^{\overline{\tau}} [P^b_{d,t-\tau}\gamma^{TOT}_{\tau} + m^b_{d,t-\tau}\pi_\tau + f_g(v^b_{d,t-\tau})] + \mu_d + \theta_t + \varepsilon_{dt} \tag{B.3}$$

Intuitively, Equation B.3 identifies the γ^{TOT}_{τ} coefficients by contrasting between school districts where an election in year $t-\tau$ narrowly passed and those where the election was narrowly rejected, but the sequence of previous and subsequent elections and vote shares is similar.

Using Dynamic RD to Estimate Causal Effects of Capital Expenditures

The dynamic RD approach in our setting is complicated by the fact that our main outcome of interest is time invariant (whether the student was ever arrested), and the usual approach is used to recover causal effects on time varying outcomes (e.g., 4th grade test scores in district d in year t). Thus, we estimate the causal effects of additional capital expenditures on the probability that a student is ever arrested in a 2SLS framework. In the first-stage, we use a district-year panel to estimate the TOT effect of district d narrowly winning an election in year $t-\tau$ on its total capital outlays per pupil in year t. In other words, we estimate Equation B.3 with the district's capital outlays per pupil on the left hand side. We then use the "predicted capital expenditures" from this first-stage specification, and relate the probability that a student is ever arrested to predicted capital expenditures in the second stage:

$$EverArr_i = \beta_0 + \beta_1 log(\widehat{CapOutk6})_i + X_i\Theta + \mu_d + \tau_t + \varepsilon_i \tag{B.4}$$

where $EverArr_i$ is a dummy variable equal to one if the student was ever arrested as an adult; $log(\widehat{CapOutk6})_i$ is the (logged) average predicted capital outlays per pupil that the student was exposed to from kindergarten through 6th grade—where the prediction comes from the estimation of Equation B.3; X_i is a vector of individual characteristics including sex, race/ethnicity, and FRPL eligibility; μ_d and τ_t are (kindergarten) district and (kindergarten) cohort fixed effects, respectively. Intuitively, simply relating $EverArr_i$ and $log(CapOutk6)_i$ would clearly yield inconsistent estimates of β_1. However, Equation B.4 allows us to obtain consistent estimates of β_1, since we exploit variation in $log(CapOutk6)_i$ exclusively driven by narrow capital bond election wins.

First Stage: Predicting Capital Expenditures Per Pupil

We estimate Equation B.3 on a school district-year balanced panel from 1996 to 2010 where each district-year observation is used exactly once for the 430 school districts that held at least one election during this sample period.[36] We focus on this sample period because our cohorts of interest

[36] In cases where a school district holds multiple elections in the same year, we keep only the election with the lowest margin of victory (or defeat). However, the results are robust to alternative criteria such as keeping the election with the largest vote share in favor (as in Cellini, Ferreira and Rothstein (2010)) or the first election in each year.

are first-time kindergarteners in 1999 through 2004. Note that in the main body of the paper, we focused on the 1996 through 2004 cohorts instead. We are not able to examine the outcomes of cohorts earlier than 1999 in the dynamic RD analysis because the strategy relies on lags of election win indicators. Given that the first year of available bond election data is 1996, and that we allow for three lags in our main specification, we limit our analysis sample to cohorts from 1999 on. Because we follow students' exposure to additional spending through grade 6, our district-year panel includes each year through 2010 (the 6th grade year of the last cohort in our sample).[37] Standard errors are clustered at the district level. Following Cellini, Ferreira and Rothstein (2010), we specify $f_g(\cdot)$ as a third-order polynomial.

Results from the estimation of Equation B.3 are shown in Figure B1. The figure presents estimates of the dynamic treatment effects of a narrow election win on district-level fiscal outcomes by year relative to the election. The solid line provides a visual representation of estimates of the γ_τ^{TOT}'s while the dashed line shows the corresponding 95% confidence intervals for two years before and up to ten years after the election.

Panels (a), (b), and (c) show that narrowly passing a bond election results in large and immediate increases in capital outlays, outstanding long-term debt, and debt interest payments per pupil. Specifically, while narrowly winning and losing districts were trending similarly in capital outlays prior to the election, in the year following the election, capital spending increased by roughly \$3,000 per pupil in winning districts. This effect began to decline three years after the election, and completely dissipated by year four. This pattern is remarkably similar to the one documented by studies in California (Cellini, Ferreira and Rothstein, 2010), Texas (Martorell, Stange and McFarlin Jr, 2016), and Wisconsin (Baron, 2022). The figures reveal a clear strong first-stage relationship between a narrow election win and capital expenditures. Using estimates of the γ_τ^{TOT}'s, we then predict capital expenditures in district d in year t with Equation B.3 (limiting the specification to three lags—or setting $\bar{\tau} = 3$ based on the dynamics observed in figure B1). The F-statistic of the relationship between capital expenditures in district d in year t and narrow capital bond election wins in $t-3$ through $t-1$ is 277, indicating that there is not a weak instruments problem.

Finally, even though these expenditures are earmarked for local capital improvements, one may be concerned that districts will find a way to divert resources toward non-capital inputs, given the fungibility of expenditures. We find strong evidence against this prediction: there is no evidence of increases in operating expenditures following a narrow election win (Figure B1, Panel (d)). Thus, estimates of the impact of capital bond passage can be interpreted as the effects of school facility investments.

[37]We follow students through sixth grade since Jackson and Mackevicius (2021) show that the effects of capital expenditures take roughly six years to materialize, plausibly due to construction time.

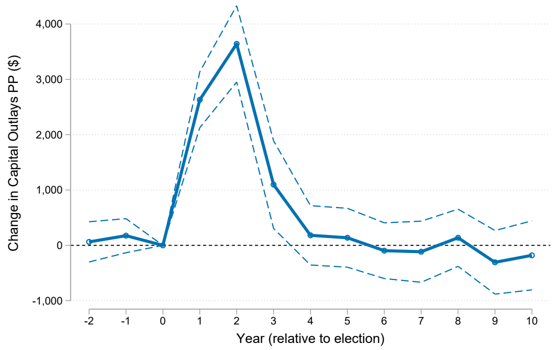
Second Stage: Relating Adult Arrests to Predicted Capital Expenditures

With a predicted capital expenditure per pupil for every district-year in our sample, it is straightforward to relate the probability that student i was ever arrested to the predicted capital expenditures that she was exposed to from kindergarten through grade six; we simply follow students across districts for seven years and average over the amount of capital spending that they were exposed to in each district and year.

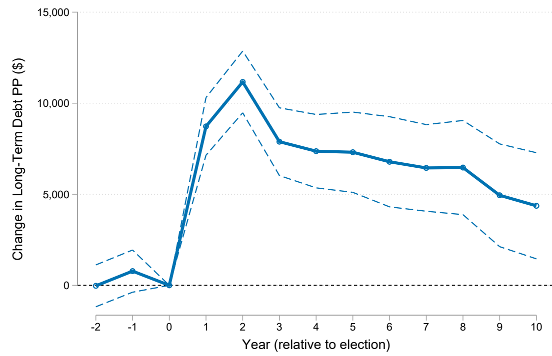
Table B1 shows the results of the estimation of Equation B.4. The table presents estimates of β_1 for three different specifications. The first specification simply presents a bivariate relationship between $EverArr_i$ and $log(\widehat{CapOut}k6)_i$. The second specification adds (kindergarten) district and (kindergarten) cohort fixed effects. Finally, the last specification adds the demographic controls in X_i. We cluster standard errors at the (kindergarten) district level.

The table shows that additional capital expenditures result in statistically significant declines in the probability of ever being arrested as an adult. Specifically, a 100% increase in capital expenditures leads to a 1 percentage point reduction in the probability of being arrested (Column 1). As shown in Figure B1, a typical narrow election win results in an increase in capital outlays per pupil of roughly \$2,500 in the three years after the election. Relative to the average capital outlays per pupil in any given year in our sample (\$900), this corresponds to an average increase of roughly 280% in capital outlays in the three years after the election. Therefore, the point estimates in Column 3 of Table B1 imply that a typical narrow election win leads to a 2.8 percentage point decline in the probability of being arrested, or a 17% decline relative to a control mean of 16.8%. This estimate is nearly identical to estimates of the ITT effects of bond elections presented in the main body of the paper. The similarity between the ITT and TOT estimates is reassuring, but mostly unsurprising given the limited non-compliance in our setting.

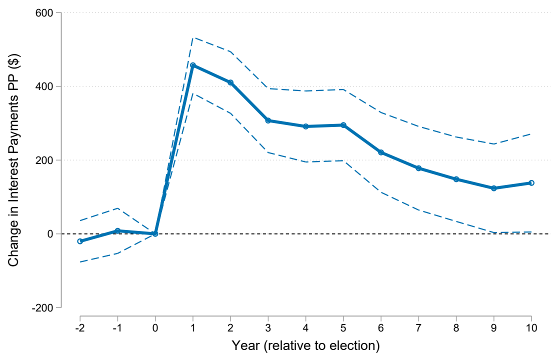
Figure B1: TOT Estimates of Successful Capital Bond Referenda ("First Stage" Evidence)



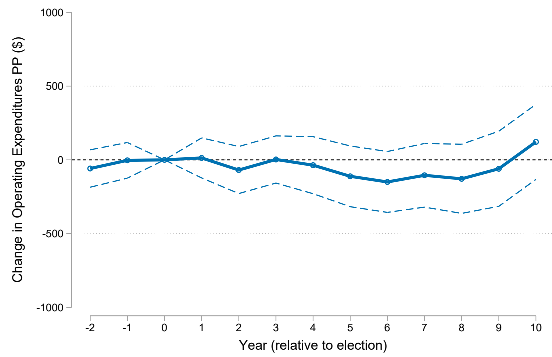
(a) Capital Outlay



(b) Long-Term Debt



(c) Debt Interest Payments



(d) Operations Expenditures

Notes: The figure presents results from the estimation of Equation B.3. The solid line provides a visual representation of estimates of the γ_{τ}^{TOT}'s while the dashed line shows the corresponding 95% confidence intervals for two years before and up to ten years after the election. Standard errors used in the construction of the confidence intervals were clustered at the school district level.

Table B1: TOT Effects of Capital Outlays on Adult Arrests

	(1) Ever Arrested	(2) Ever Arrested	(3) Ever Arrested
Predicted Log Capital Spending K-6	-0.015**	-0.015***	-0.010***
	(0.007)	(0.004)	(0.003)
Observations	618,872	618,872	618,872
Control Mean	0.168	0.168	0.168
District and Cohort FEs		✓	✓
Demographic Controls			✓

Notes.The table shows the results of the estimation of Equation B.4. It presents estimates of β_1 for three different specifications. The first specification simply presents a bivariate relationship between $EverArr_i$ and $log(\widehat{CapOutk}6)_i$. The second specification adds district and cohort fixed effects. Finally, the last specification adds the demographic controls in X_i. Standard errors are clustered at the district level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Additional Details for MVPF Calculation

Society's Willingness to Pay to Increase Public School Funding

For society's willingness to pay, we consider only the crime-reducing benefits of additional school spending, and conservatively ignore all other benefits including increases in educational attainment and earnings. We leverage our detailed arrest data to construct a back-of-the-envelope measure of the discounted social cost of each student's future crimes. Specifically, for each individual in our sample, we multiply the social cost of each crime type by the number of arrests of that type. This total social cost, which equals zero for individuals never arrested, becomes the dependent variable in a regression using our main specification in Equation 3. For instance, if a student is arrested twice, once for homicide and another time for aggravated assault, then the student is assigned the sum of the social costs of homicide and aggravated assault. Because the arrests occur many years after the school spending exposure, we use the age at which each crime is committed, and a discount rate ranging from 3–5% to discount all costs back to age 7.5—the average age in K–3.

We follow the economics of crime literature and use two sets of social cost estimates for each crime type. First, we follow Anders, Barr and Smith (2022) and assign each crime type the social cost estimated by McCollister, French and Fang (2010).[38] Second, we follow Mello (2019) and use the social cost estimates in Chalfin (2015). Panel A of Table 12 shows the results for both sets of social cost estimates, and for discount rates ranging from 3–5%. The estimates vary in magnitude but generally show large reductions in social costs from increased school funding. Specifically, the estimates range from a decline of \$4,378 to \$8,969 in social costs due to a 10% increase in school funding during kindergarten through third grade.

Net Costs of Increasing Public School Funding

The net cost in our context is the direct cost of the additional school spending minus the discounted future reductions in government spending due to declines in police, court, and incarceration costs associated with each prevented arrest. The direct cost of the additional school spending is straightforward to compute. A 10% increase in school spending during kindergarten through third grade costs \$3,952 (10% of \$9,879—the average kindergarten through third grade spending in our sample—for four years $= 0.10 \times \$9{,}879 \times 4$). This \$3,952 is presented in the row labeled "Grade K-3 Cost" in Panel B.

As discussed in Section III.B, our main analysis focuses on spending in K–3 because there is little identifying variation in the foundation allowance after 2003, by which time the most recent

[38] This paper is similar to Miller, Cohen and Wiersma (1996), but is more recent and uses relatively newer methodologies to estimate the social costs of each crime category. The social costs in Miller, Cohen and Wiersma (1996) have been used in cost-benefit analyses of various economics papers, including Heller et al. (2017) and Kling, Ludwig and Katz (2005). The crime-specific social costs in McCollister, French and Fang (2010) include criminal justice expenses associated with police, court, and incarceration costs for each crime. Because we subtract these expenses from total costs to calculate net costs, to avoid double counting we also subtract them from the overall social cost of each crime.

kindergarten cohort reaches grade three. To ensure consistency across cohorts, we restrict to grades three and below. Still, students could be exposed to allowance-induced spending in later grades. For instance, the first cohort could be exposed to additional allowance-induced spending through grade 8. Ignoring this additional spending in later grades would bias our MVPF calculation upward. To understand how much additional spending students in our sample are exposed to in grades 4–12, we regress the average operating expenditure that students were exposed to in these grades on our main specification in Equation 3. We calculate that a 10% increase in K–3 spending leads to a 1.7% increase in spending in grades 4 through 12. Average operating spending in our sample is \$10,018. Thus, an increase of 1.7% in spending for nine years is equal to \$1,533 (=\$10,018 $\times$ 0.017 $\times$ 9). Given that this increased spending comes in later years, we discount it back 6.5 years from age 14, the average age in grades 4–12, to age 7.5, the average age in grades K–3. We present the discounted values in the row labeled "Grade 4–12 Cost" in Panel B, which range from \$1,116 to \$1,265, depending on the assumed discount rate. Total K-12 costs, presented in the third row of the panel, range from \$5,068 to \$5,217.

We next consider the reduction in future government spending from the declines in police, court, and incarceration costs due to the averted arrests. We follow Heckman et al. (2010), who calculate these costs in order to evaluate the cost-effectiveness of the Perry Preschool Project in Michigan. The authors calculate total police and court costs per arrest, as well as total incarceration costs per incarcerated individual in Michigan for the 1982 through 2002 years. We use the costs in 2002, the closest year to the availability of our arrest data (2012–2020). The authors calculate police and court costs per arrest of \$11,468 by taking the total police and court costs in Michigan from the Expenditure and Employment Data for the Criminal Justice System (CJEE) and dividing it by the total number of Michigan arrests in the Uniform Crime Report (UCR) data. Similarly, the authors calculate an incarceration cost per incarcerated individual of \$33,871 by taking the total incarceration costs in Michigan jails and prisons from the CJEE and dividing it by the total number of individuals incarcerated in Michigan jails or prisons (as reported by the Michigan Department of Corrections). We follow Heckman et al. (2010) in assuming identical police, court, and incarceration costs across crime types.

As we do throughout the paper, we inflate the \$11,468 and \$33,871 to 2012 dollars. We assign the resulting police and court cost to every arrest in our data. We then estimate the fraction of arrests in our data that lead to incarceration using information from a 2019 report by the Michigan Department of Corrections (MDC, 2021). We find that 41.6% of felony offenders are placed in a jail or prison in Michigan. We thus assign to each of our felony arrests the incarceration cost multiplied by 0.416 to account for the fact that only 41.6% of these arrests will result in incarceration. We conservatively assume that no misdemeanor arrests led to incarceration, both because only a small portion of misdemeanors do, and because this fraction is not reported in the Michigan Department of Corrections report.

We then create a new variable, equal to the sum of police, court, and incarceration cost per arrest.

We discount these costs from the age at the arrest to age 7.5, the average age in K–3, using a 3%, 4%, and 5% discount rate. We estimate our preferred specification in Equation 3 with this variable on the left hand side, and present the results in Panel C. The police, court, and incarceration cost savings from a 10% increase in grade K–3 spending range between $731 and $967 per pupil. Subtracting these from the total grade K-12 cost yields the net cost, which ranges from $4,250, assuming a 3% discount rate, to $4,337, assuming a 5% discount rate.

Given these estimates of society's willingness to pay and net government costs, we calculate that the MVPF of a 10% increase in K–3 school spending ranges from 1.0 to 2.1 (Panel D).

D Data Appendix

Additional Details on Data Sources

This study uses administrative data from the Michigan Department of Education (MDE), Center for Educational Performance and Information (CEPI), National Student Clearinghouse (NSC), and Michigan State Police (MSP) to test the effects of additional school funding during primary school on adult criminal justice involvement.

The starting point for our analysis consists of ten cohorts of first-time kindergarten students in Michigan public, non-charter, schools during the 1995 through 2004 academic years. These cohorts include 1,171,367 students across 518 school districts. The first time we observe kindergarten cohorts is in 4th grade. This is because, prior to 2003, the only data available for tracking students in Michigan are test-taking records from fourth grade on. Therefore, to identify students in their kindergarten cohorts, we assume that 4th grade students in district d in year t were first-time kindergarteners in district d in year $t-4$. Using data from the first cohort of kindergarten students that we can fully track over time (2003), as well as data on school choice utilization (both inter-district open enrollment plans as well as charter school enrollments) in Michigan over time, we estimate that this assumption is justified for roughly 95% of students in our base population.

We use MDE/CEPI administrative datasets to follow these students throughout their educational trajectories in Michigan. Specifically, this dataset allows us to measure intermediate outcomes such as fourth and eighth grade math test scores on the state standardized exam, school attendance in eighth grade, and high school graduation. The microdata contain information on where students enroll each year, allowing us to track students across schools and districts over time and to observe whether a student was ever enrolled in an educational program at one of Michigan's 23 juvenile detention centers (JDCs). Enrollment in a JDC is a behavioral outcome that indicates youth contact with the juvenile justice system; individuals younger than 17 years old may be held in a JDC after being arrested. We focus on placement in a JDC instead of other behavioral outcomes commonly available in administrative education datasets, such as school suspensions or expulsions, because these measures are not consistently reported in the Michigan data.

Education records contain individual-level covariates such as sex, race/ethnicity, and an indicator for free or reduced-price lunch (FRPL) eligibility that we control for in our main specifications. We measure student demographics and intermediate outcomes such as attendance in grade eight because, with the exception of test scores, these variables are unavailable prior to 2003, which is the year the first cohort reached eighth grade. We match students in these cohorts to the NSC, which contains postsecondary enrollment and degree receipt information. NSC data are nationwide, allowing us to observe whether a particular student ever enrolled in (or graduated from) a postsecondary education program outside the state.[39]

To characterize the school districts where students in our sample are enrolled, we also collect

[39] For more information regarding the NSC, see Dynarski, Hemelt and Hyman (2015).

several district-level covariates measuring revenues and expenditures, local school choice, demographic, and economic conditions. Specifically, based on where and when students were enrolled in primary school, we merge in district-level expenditure data from CEPI, as well as foundation allowance and 1994 district revenue information from the Michigan Senate Fiscal Agency.

The school choice variables include: the percent of students living in the district who attend a charter school, number of charter schools located in the district, and number of charter schools located in the district and adjoining districts. The percent of students living in the district who attend a charter school was constructed using information from CEPI's Public Student Headcount Data and CEPI's Nonresident Student Research Tool. The number of charter schools located in the district and adjoining districts was constructed using charter school addresses and school district geographic boundaries.[40]

The district-level variables characterizing demographic and economic conditions include: the fraction of 5–17 year olds living in poverty in the district, local median household income, fraction of students attending school in the district who are White, fraction of students attending school in the district that are eligible for FRPL, and local average unemployment rate. The fraction of students in the district who are White and fraction eligible for FRPL come from the National Center for Education Statistics (NCES) Common Core of Data (CCD), available starting in 1993. School district population and poverty counts come from the Census Small Area Income and Population Estimates (SAIPE), available since 1995. Median income information is also from SAIPE, but only available at the county level (there are 83 counties and 518 districts in our sample). Local unemployment rates were calculated using monthly city- and county-level unemployment rates from the Bureau of Labor Statistics (BLS). Average rates were calculated for a school year from August through July. If over half of the students in a district attend school in a city for which the rate is available, then we use the student-weighted average rate across cities in the district. If fewer than half of students in the district attend school in a city with an available rate, then we use the county unemployment rate. We convert all spending and income measures to 2012 dollars using the Employment Cost Index for elementary and secondary school employees provided by the BLS.

Our main specifications also control for baseline district-level adult arrests per student, which we create by mapping precinct-level MSP adult arrest data to school districts during 1997, the earliest year available, and dividing by 1997 district enrollment. Because the geographic boundaries of precincts and school districts do not necessarily overlap, mapping precincts to school districts is not straightforward. We gather information from the National Archive of Criminal Justice Data on the latitude and longitude of every precinct in the MSP data. Using GIS and publicly available shape files of school district geographic boundaries in Michigan, we then assign a precinct to a given school district if the precinct's coordinates fall *within* the district's boundaries. For districts with multiple precincts, we add up the total number of arrests across each precinct. For the 16% of small school districts that do not have a precinct within their boundaries, we predict their number of arrests based

[40] We thank Brian Jacob, Tamara Wilder Linkow, and Francie Streich for sharing these data.

on the total number of arrests in their counties and their population.

Finally, to estimate the causal effects of additional capital expenditures on adult crime, we obtained a capital bond election-level dataset from MDE. This dataset reports, for each election, the date of the election, the cost of the capital project, voter turnout and votes in favor, a description of the intended use of the bond, and the district's unique identifier.

Matching Across Administrative Data Systems

We match the students in our base cohorts to an arrest-level dataset from MSP containing the universe of adult arrests in Michigan from January 2012 through May 2020. For individuals who are arrested at age 17 or older (the age at which individuals are considered adults by the Michigan justice system during our sample period), these data include the date of the arrest, whether the arrest was for a misdemeanor or felony offense, and the exact offense (e.g., assault or larceny). We use this information to construct adult crime outcomes including an indicator for whether the student was ever arrested in Michigan, and arrest status by particular types of crime (e.g., violent or property).

The Michigan Education Data Center (MEDC) linked the K-12 public school records to the adult crime data using a probabilistic matching algorithm. Because these data sources do not contain a common identifier, MEDC staff linked the records based on first name, last name, date of birth, and gender using the Fellegi-Sunter model implemented via the *fastlink* R package (Enamorado, Fifield and Imai, 2019). The linkage performed well; for each of the matched records, the software rates the certainty level of the match using a posterior probability. Overall, 83% of records in the adult crime data matched to a public school student with a high degree of certainty (over 99.6%). This match rate is nearly identical for males and females, and MEDC staff closely validated the match by manually matching a randomly selected subset of 200 records. Importantly, one should not expect a 100% match rate because some individuals arrested in Michigan could have gone to school in a different state, been enrolled in a private school, or been homeschooled.