Public School Funding, School Quality, and Adult Crime*

E. Jason Baron[†] Joshua Hyman[‡] Brittany Vasquez[§]

March 2024

Abstract

This paper asks whether increasing public school funding can be an effective long-run crime-prevention strategy in the U.S. Specifically, we examine the effect of increases in funding early in children's lives on the likelihood that they are arrested as adults. We exploit quasi-experimental variation 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 increasing school funding are greater than the costs, even when considering only the crime-reducing benefits.

*We thank Will Dobbie and three anonymous referees for their helpful comments and suggestions. We also thank Peter Arcidiacono, Patrick Bayer, Eric Brunner, George Bulman, Eric Chyn, Eric Edmonds, Ezra Goldstein, Max Gross, Nathan Hendren, Caroline Hoxby, Kirabo Jackson, Brian Jacob, Max Kapustin, Julien Lafortune, Juan Carlos Suárez Serrato, and seminar and conference participants at Brown University, Cornell University, Dartmouth College, Einaudi Institute for Economics and Finance, University of Connecticut, University of Michigan, Williams College, the American Economic Association, the NBER Crime Summer Institute, the Triangle Economists in Applied Microeconomics seminar, the National Tax Association, the Triangle Economics of Education Workshop, the Online Economics of Crime Seminar organized by Jennifer Doleac, and the Policy Impacts Annual Conference. 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 increasing public school funding be an effective long-run crime-prevention strategy? And does the answer depend on the type of funding—for example, operating versus capital? These questions are 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.

We examine these questions by leveraging two natural experiments in Michigan which yield plausibly exogenous variation in two distinct types of school funding. The first research design exploits variation in operating expenditures (e.g., teacher salaries) due to Michigan's 1994 school finance reform, Proposal A, in an instrumental variables framework. The second design exploits variation in capital spending (e.g., building renovations) by leveraging close school district capital bond elections via a regression discontinuity (RD) design.

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 Michigan juvenile detention records 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 students.[1] The timing of the natural experiments and availability of our data 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

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

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 each year during K–3 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.

We then examine mechanisms through which additional operating expenditures reduce adult crime, finding that more funding leads to reductions in class sizes and teacher turnover, increases in teacher salaries and experience, and the hiring of additional district and school leadership who are heavily involved in issues surrounding student discipline and truancy. These changes translate into improvements in student test scores in fourth grade, though these gains disappear by middle school. Despite the test score fadeout, students experience considerable improvements in behavioral proxies for non-cognitive skills: substantially lower absenteeism rates in middle school and a lower likelihood of being placed in a juvenile detention center. Students exposed to additional school funding are also more likely to graduate high school and earn a postsecondary degree.

To examine the relative contribution of these various improvements toward students' adult crime reduction, we conduct mediation analyses and show that increases in educational attainment explain less than 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), but also show that focusing on educational attainment alone understates the crime-reducing benefits of increasing public school funding. Reductions in absenteeism explain a much larger portion of the effect (roughly 40%). We rule out that the crime reduction is due to positive peer effects from changing peer composition.

Lastly, considering only its crime-reducing benefits, we assess the cost-effectiveness of increasing operating expenditures by showing 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.

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 U.S. 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 expenditures can also reduce adult crime, particularly in light of recent studies suggesting 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 crime, we exploit the fact that Proposal A 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. Our empirical approach leverages the universe of close elections in Michigan from 1996–2004 via an RD design. We find that kindergarteners in districts that narrowly pass an election are 20% less likely to be arrested in adulthood. After smoothing the capital expenditures over time (since they produce durable goods), we calculate an elasticity of adult arrests with respect to capital spending of approximately -1. While large, this is smaller than the elasticity with respect to operating expenditures (-1.5).

[2]Authors' calculations from the National Center for Education Statistics.

Still, the fact that two distinct research designs find that increasing school funding reduces adult crime bolsters the credibility of a causal relationship.

Exploring mechanisms, we show that most capital projects fund improvements to instructional facilities, primarily elementary and middle schools. We then show that students in narrowly winning school districts are 25% less likely to be chronically absent in middle school, the earliest grades in 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 small 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. These results also suggest that capital expenditures can improve important long-term outcomes such as crime, even in the absence of clear improvements in cognitive outcomes.

Our paper contributes to several related literatures in economics. First, we contribute to the literature examining the relationship between education and crime. While the literature has shown that additional years of schooling lead to less crime (Hjalmarsson, Holmlund and Lindquist, 2015; Lochner and Moretti, 2004; Machin, Marie and Vujić, 2011), there is less evidence on the effects of *school quality* on criminal behavior. Most evidence relies on school choice lotteries and shows that students who win admission to a preferred school are less likely to engage in subsequent criminal activity (Cullen, Jacob and Levitt, 2006; Deming, 2011; Dobbie and Fryer, 2015). Exploiting variation in school quality from increases in public school funding complements this literature in two key ways. First, estimating the effects of attending a 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. Second, preferred schools attended via lottery wins have high demand and only a limited supply of seats. It is important for policy purposes to understand whether crime can be reduced by improving existing schools instead—an arguably more scalable policy intervention.

We also contribute to the literature examining the effects of public school spending. The primary economic justification for the public provision of education is one of positive externalities. Yet the literature has 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.

There is little evidence on the relationship between school funding and adult crime. Johnson and Jackson (2019) use data from the 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. 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 may be under-reported (Deming, 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, we are able to explore the specific intermediate outcomes and mechanisms through which each expenditure type influences later-in-life crime.

[3]See Jackson (2018) for a detailed literature review. A notable exception is Asker, Brunner and Ross (2022), which examines effects on voting and volunteerism.

Finally, we contribute to the economics of crime literature identifying effective crime reduction strategies. Numerous studies evaluate the criminal deterrence effects of 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; Bhuller et al., 2020). 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 and show that it is a viable policy tool to prevent criminal development.

II Background

II.A Michigan's 1994 Proposal A

Prior to Proposal A, Michigan financed public K-12 education primarily through the local property tax. 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. 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 three quartiles of districts.

Panel B illustrates how the time path of the allowance varied by a district's 1994 revenue, and 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.[4]

Proposal A was similar in many ways to other school finance reforms. As with most other reforms, Proposal A substantially increased school spending among previously low-revenue districts. Proposal A was designed in the interest of "adequacy," as were the other reforms that occurred since the late 1980s. Treated districts under Proposal A had similar spending levels to those treated by other reforms during the adequacy era (Lafortune, Rothstein and Schanzenbach, 2018). An important difference between Proposal A and other reforms during this period is the extent to which it disproportionately targeted rural areas with a large share of White students. Though this is 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.

The rural focus of the reform suggests that our results speak primarily to rural crime, and thus yield several interesting insights for the economics of crime. Crime is not solely an urban phenomenon, yet there is little research in the economics of crime focused on areas outside of large, urban centers. Rural areas increasingly struggle with regards to educational attainment and labor market opportunities, leading to increased criminal activity and substance abuse, and contributing to political polarization; indeed, a remarkable trend in the U.S. over the past two decades has been the convergence of rural and urban crime rates (Abraham and Ceccato, 2022). We show below that the share of children in our sample who are arrested in adulthood is similar across children in initially low-spending districts (which tend to be in rural areas) and initially high-spending districts. While it is true that violent offenses are more common in initially high-spending districts, overall crime rates are surprisingly similar. Thus, this study contributes to the small literature evaluating policies to reduce rural crime (Abraham and Ceccato, 2022).

[4]Because we measure the amount of spending and foundation allowance during K–3, we also present this variation as four-year averages in Figure A1.

II.B Capital Bond Elections in Michigan

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 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.

From January 1996 to December 2004, the sample period of our RD analysis, there were 955 unique capital bond elections. 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).[5] Table A1 shows summary statistics for all elections held by districts during this time period. Of the 518 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.

[5]Figure A2 shows the distribution of capital bond election months.

III Data Sources and Analysis Samples

III.A Data Sources

We use data from the Michigan Department of Education (MDE), Michigan's Center for Educational Performance and Information (CEPI), National Student Clearinghouse (NSC), and Michigan State Police (MSP). For a detailed discussion on each of the data sources, the sample construction, and matching across 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.

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. Michigan public school records have also been matched 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.[6] 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. 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.

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

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

operating expenditures that students are exposed to from K–3.[7] 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 during the ages of 22 through 25."

Table 1 describes the sample. Column 1 consists of all students in the sample, while Columns 2 and 3 consist of students who were never arrested and those who were arrested at least once, respectively. Of the 717,042 students, 12% were arrested at least once. Black and low-income students were disproportionately likely to be arrested. For example, 53% of children eventually arrested were eligible for FRPL, despite comprising 32% of the sample.

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. The adult arrest rate of students in initially

[7] 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.

low-revenue districts is 11%, compared to 12% in initially high-revenue districts. The adult arrest rate for violent offenses is 3.7% and 5.3%, respectively. 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 A3—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 from 1996 through 2004. 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.

The construction of the election-level analysis sample is straightforward. For each election e in 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 1996 through 2004, corresponding to the 383 unique public school districts in Michigan that held at least one election during this time period. Column 1 of Table A2 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. Column 2 shows that eventually winning school districts are positively selected: two years before the election, they have a higher median household income and a lower local unemployment rate.

IV First Natural Experiment: Michigan's Proposal A

IV.A Empirical Strategy

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 educational attainment in Michigan (Hyman, 2017).

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, a natural way to convey the intuition behind the identifying variation in this setting is to highlight key groups of districts which best illustrate the exposure design. We begin by presenting first stage and reduced form results visually in a more typical difference-in-differences (DID) 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 K–3 or an indicator for ever arrested) for student i who attended district d from K–3 in cohort c; $Treated_d$ is a dummy variable equal to one if the student's district during K–3 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 DID 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 2.

Panel A shows the intuition for the first-stage relationship: the increase across cohorts in operating expenditures during K–3 was greater for students in initially low-revenue districts relative to students in initially high-revenue ones. The DID estimates are statistically significant for every cohort. Panel C 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:

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

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

where Y_{idc} is defined as above; $log(spend)_{dc}$ is the log of the average operating spending that the student was exposed to from K–3; 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 K–3.[8] 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.[9] The specification also includes district (μ, α) and cohort (π, γ) fixed effects. In the first stage, we instrument for $log(spend)_{dc}$ using $log(allow)_{dc}$, the log of the average allowance in the student's district during K–3. 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 K–3.

Identification Assumptions

Two main assumptions must be satisfied for our approach to yield consistent estimates of the effects of additional operating expenditures.

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 2 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

[8]See Table 2 for the full list of covariates.

[9]We show robustness of our main results to excluding this variable in Section IV.B.

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. 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 DID 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" Panels A and C in Figure 2 back to include pre-treatment estimates. In fact, if we wanted to examine the adult arrest rates 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 district-level characteristics. Specifically, we regress the year $t-1$ to year t change in district-level fiscal and demographic outcomes on a continuous measure of 1994 district revenue from 1990 to 1994. Table A3 shows that districts were not trending differentially by 1994 revenue in fiscal outcomes, measures of school quality, or demographic characteristics. The parameter estimate on 1994 revenue is always small and generally statistically insignificant. As an example, the estimates suggest that a 10% higher revenue in 1994 is associated with a year-to-year change in the percent of students in special education of 0.05 percentage points in the years preceding Proposal A.[10]

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 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.

The right-hand side panels of Figure 2 show changes in student outcomes across cohorts in initially low-revenue districts (relative to initially high-revenue ones) for cohorts starting in 2003. Essentially, these panels present "placebo" estimates from identical specifications to panels in the left-hand side of the figure, but estimated on a sample of kindergarten cohorts from 2003–2008. For example, 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 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.[11]

[10]Similarly, the point estimate in Column 3 suggests that a 10% higher revenue in 1994 is associated with a 0.128, or 0.57%, year-to-year change in the pupil/teacher ratio.

[11]The dependent variable in Panels (c) and (d) is whether the student was ever arrested during the respective age window in which they could match to the arrest records. To ensure that these results are not driven by the differential age window during which we measure criminal justice contact, Figure A4 repeats this exercise but using a specification where the dependent variable is whether the student was ever arrested by age 19 instead.

IV.B Main Results

2SLS Estimates

Table 2 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).

The table shows that students exposed to 10% (roughly $1,000) additional operating expenditures per year for four years (K–3) have a 2 percentage point lower chance of being arrested as adults—a 15% decline 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 K–3 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 2 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%).

Robustness

Table A4 presents alternative specifications to probe robustness, such as using districts' total as opposed to operating expenditures, operating expenditures in levels ($1,000s), and showing a reduced form regression; other specifications drop districts in the top quartile of the 1994 distribution, students who attended Detroit Public Schools, and the baseline district adult arrest rate control interacted with cohort fixed effects. The last specification controls for region-by-cohort fixed effects. Effects of additional funding on adult crime remain negative, statistically significant, and similar to our baseline estimate in percent terms.

As discussed in Section III.B, our main analysis focuses on spending during K–3. This may create an exclusion violation if exposure to a higher allowance during K–3 leads to increases in the amount of spending beyond grade three. To address this concern, we show in Table A5 that our main results are robust to the choice of grade range during which we measure additional spending. Further, when estimating the MVPF in Section IV.D, we explicitly account for any additional funding that the student may be exposed to in later grades as a result of the instrument. Finally, when we estimate our main specification but control directly for the amount of spending that the student is exposed to in grades 4–12, we obtain a similar point estimate to that in Table 2: -0.185 (SE$=0.067$).

As described in Section III.B, our primary specification measures whether a student was ever arrested during the respective age window in which he/she could be observed in the arrest records. Table A6 shows that our main finding 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 can observe every student in the arrest records.

Finally, one may be concerned that additional funding causes students to leave Michigan and commit crimes as adults in other states, which we do not observe in our data. We explore the extent to which out-of-state migration may influence our main findings in Table A7. We find that additional spending during K–3 does not impact the probability that children leave the state during grades K—12. While we find that more spending increases the probability of going to college outside of Michigan, 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, attended college outside of Michigan, or attended a K–3 district in a high out-of-state migration county. These estimates are similar to our main findings, suggesting that our results reflect actual reductions in criminal behavior as opposed to differential out-of-state migration.

Heterogeneity

Table A8 presents estimates for various district and student subgroups. Consistent with the literature (Baron, 2022; Jackson, Johnson and Persico, 2016; Lafortune, Rothstein and Schanzenbach, 2018), 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 A9 shows that males had disproportionately large declines in felonies and violent offenses, whereas females had larger declines in misdemeanors and public order offenses.

IV.C Mechanisms

Marginal Dollar Allocation and Improvements in School Quality

Table A10 shows that an additional allowance dollar led to an increase in operating expenditures of 58 cents. Of the 58 cent increase, 33 went toward instructional expenditures, and 25 to non-instructional spending such as school and district leadership (5 and 3 cents, respectively). 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% of the marginal dollar was spent on instruction, compared to 62% of the average dollar, though the difference is not statistically significant.

Table A11 shows that increases in expenditures translated into improvements in school inputs. Students exposed to 10% more school funding during K–3 experienced a 0.7 (4%) smaller student-teacher ratio during these grades. These students were also taught by teachers earning roughly $2,500 (5%) higher salaries. Additional funding also reduced the ratio of pupils to school and district leadership (e.g., principals, vice principals, superintendents, and assistant superintendents). Principals and superintendents can have a strong influence on school culture, and often play a 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. As in our main analysis, we find that additional school spending from Proposal A reduced class sizes and increased average teacher salary (Table A12). We also find a reduction in the number of new teachers hired and an increase in average teacher experience, driven by an increase in the number of highly-experienced teachers. Therefore, the declines in class sizes and increases in teacher salaries appear to be driven by a reduction in turnover, particularly among more experienced teachers.

Impacts on Intermediate Outcomes

Table A13 shows that additional spending improves a variety of academic and behavioral outcomes. Children exposed to 10% more spending each year during K–3 have 12% of a standard deviation higher math test scores during fourth grade, though these improvements fade out completely by middle school. This test score fadeout, followed by effects reappearing in adulthood, is consistent with the effect pattern found in other educational interventions, and is suggestive of long-term crime effects operating through improvements in non-cognitive skills. Indeed, we find considerable improvements in behavioral proxies for non-cognitive skills: an 8 percentage point (54%) decline in chronic absenteeism (defined as missing over 10% of school days) in eighth grade, a 53% lower share of missed school days, and a 0.3 percentage point (24%) reduction in the probability of JDC placement. Treated students also 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.[12]

These improvements in intermediate outcomes could contribute to the estimated reduction in adult crime through several channels. Improvements in educational attainment could increase the opportunity cost of crime through better labor market opportunities (Lochner, 2004). Alternatively, increases in attendance may contribute to reducing the probability of a student's future arrest by signaling improvements in socio-emotional skills (Rose, Schellenberg and Shem-Tov, 2021), or by keeping students in school during a key period of criminal development commonly occurring outside of the school (Bell, Costa and Machin, 2021).

We cannot definitively disentangle the relative importance of these channels. However, controlling for intermediate outcomes in our main specification and measuring how each changes our baseline estimate of the effects of additional spending could be informative. Controlling for high school graduation attenuates the baseline estimate from -22.7 percentage points to -18.6 (Table A14), indicating that high school graduation explains approximately 18% of the overall effect. 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), but shows that focusing only on educational attainment would understate the crime-reducing benefits of additional school spending.[13]

[12]These effects are broadly consistent with Jackson, Johnson and Persico (2016), who find that a 10% increase in funding for all 12 schooling years led to 0.31 more years of education.

[13]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).

Controlling for proxies for non-cognitive skills such as absenteeism attenuates the estimate from -22.7 to -13.7, suggesting that this channel explains roughly 40% of the overall effect. This suggests that 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. Thus, 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 only a modest portion of the crime-reducing effect of school funding.[14]

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 later-in-life crime: 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 changes in peer composition.

Table A17 examines the potential for student re-sorting along a number of different dimensions. We find no evidence that students exposed to more school funding during K–3

These estimates suggest that the effects we find on high school graduation would reduce arrests by only 2.4% (= (2.7/10) × 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.

[14] Table A15 presents heterogeneity in the effects of additional funding on intermediate outcomes. Consistent with heterogeneity in the impacts of funding on crime, impacts on attendance are larger for students in disadvantaged districts. This is consistent with the mediation analysis, which shows that attendance may be an important mechanism. Moreover, consistent with the findings in Hyman (2017), improvements in educational attainment are concentrated among students in advantaged school districts. There are at least two potential reasons for this pattern. First, Table A16 shows that advantaged and disadvantaged districts spend the marginal dollar differently: advantaged districts tend to spend a larger share in support services (relative to instruction). Second, it could be that the marginal student impacted by the additional funding differs across the two types of districts.

were more or less likely to switch school districts during "transition years" (grades 6 and 9), in which students are most likely to move, or that they experienced compositional changes in their district's student population during those years. Although some of the estimates suggest changes in peer composition, they are modest in magnitude, and move in opposite directions.[15] 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 increases in school funding can significantly reduce criminal behavior, even in the absence of accompanying improvements in peer composition.

IV.D Marginal Value of Public Funds (MVPF)

This section examines whether the crime-reducing benefits of increased operating expenditures exceed their costs by calculating the MVPF of public school funding. 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).[16]

The first step in calculating the MVPF is to estimate society's willingness to pay for additional 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, 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 social cost of adult crime. This

[15] As an additional check, we re-do the mediation analysis with measures of peer composition during transition years as intermediate outcomes. Table A18 shows that changes in peer composition explain virtually none of the crime-reducing effect of school funding.

[16] See Online Appendix C for additional details regarding the calculations in this section, as well as additional comparisons of how the costs and benefits of increased school spending compare to other educational and law enforcement interventions.

variable is equal to zero for students who were never arrested and equal to the sum of the cost of each arrest for those who were arrested. 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 roughly \$4,400 to \$9,000, depending on the discount rate and specific estimates of the social costs of each crime (Panel A of Table 3).

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 (e.g., government savings from fewer people getting arrested). 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. 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 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. That is, even considering only its crime-reducing benefits, and under quite conservative assumptions, the benefits of increasing school funding are larger than the costs.

V Second Natural Experiment: Close 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. To estimate the causal impact of additional capital expenditures, this section uses 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:

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

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 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.[17]

The RD research design uses close elections to approximate a randomized experiment. This requires that, conditional on having a close election, a win (or loss) is as good as random. If treatment is indeed as good as random, then it should be equally likely that voters either just pass or just reject the election. A histogram of the vote share for the 955 elections during our sample period shows no evidence of a discontinuity around the 50% vote share, and McCrary (2008)'s two-step test shows no statistically significant discontinuity in the density at the cutoff (see Figure A5).

We also examine whether observables are "locally" balanced before the election, which should be the case if treatment assignment is indeed locally randomized. Table A19 presents point estimates and associated p-values of local linear regressions of school district characteristics two years before the election ($t-2$) on an indicator for whether the election was eventually approved in year t. The table shows that there are no statistically significant differences in pre-treatment characteristics between eventually (narrowly) winning and losing school districts.

[17]We focus on estimates of ITT effects in our main analysis for several reasons: (1) the issue of non-compliance appears to be small in our context, (2) dynamic RD estimates embed a variety of additional assumptions, and (3) the dynamic RD is most useful to examine time-varying outcomes, such as test scores; however, our main outcome of interest is time-invariant. See Online Appendix B for more details.

V.B Main Results

Panel (a) of Figure 3 shows clear evidence that districts that narrowly passed an election in t spent roughly $2,500 more per pupil on capital outlays in $t + 1$, relative to districts in which the election was narrowly defeated. As expected, given that bond elections target only capital outlays, Panel (b) shows that there was no effect on operating expenditures. To more formally quantify the magnitude (and precision) of these estimated effects, Panel A of Table 4 shows the point estimates and associated p-values of local-linear regressions. 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.

Panel (c) of Figure 3 shows that the share of first-time kindergarteners arrested as adults is lower in narrowly winning school districts. The point estimate in Panel B of Table 4 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 (d) and (e), Figure 3). 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. After smoothing the capital expenditure over the life of the durable asset, we estimate that 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 (see Online Appendix E for details).

Table A20 shows that our results are robust to specifications using alternative bandwidth selections, a second-order polynomial in the vote share, and restricting 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). Table A21 finds limited heterogeneity by student gender or by baseline district academic performance, but shows suggestive evidence that effects are larger for FRPL-eligible students and among baseline low-income districts.

V.C Mechanisms

We find no evidence that narrowly passing an election improves inputs such as average teacher salary or class size (Table A22). Instead, it seems likely that additional capital expenditures impact school quality primarily through improvements in school infrastructure. Table A23 shows that roughly one third of all elections in our sample proposed 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 targeted elementary or middle schools.[18] After each winning project is completed, districts are audited to ensure funds were used as proposed. These audits, the increase in capital outlays, and the lack of impacts on operating expenditures and related inputs all suggest that narrowly passing a bond election improves school infrastructure.

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 attending school when infrastructure is in poor conditions. Better infrastructure may therefore improve student and parental engagement in school by increasing expected returns to attendance. Table A25 presents evidence in favor of these hypotheses. While the estimates are somewhat imprecise, kindergarteners in narrowly winning school districts are about 25% less likely to be chronically absent in eighth grade (the earliest grade we can measure attendance). We find suggestive evidence of small improvements in test

[18] Table A24 examines heterogeneity in the effects of narrowly passing an election by the type of capital project. Effects are larger for bonds targeting new structures or equipment, bonds targeting instructional structures and equipment, and bonds targeting investments in elementary or middle schools in the district.

scores and high school graduation, but they are not statistically significant.[19]

The large declines in chronic absenteeism, and relatively smaller and imprecise effects on test scores and high school graduation, suggest that keeping at-risk students attending school during a key period of criminal development is an important channel through which investments in infrastructure reduce adult crime (Bell, Costa and Machin, 2021).[20] Of course, changes in expected returns are not the only plausible mechanism; additional capital expenditures could reduce environmental hazards that may improve student health and attendance. Similarly, new or improved instructional infrastructure could lead to better technology in the classroom or additional classes offered, which may engage students and parents and improve school attendance.

VI Conclusion

This paper examines whether increasing funding for 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

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

[20] We find little evidence of peer compositional changes: Table A26 shows no effects on demographic characteristics five and ten years after a narrow election win.

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, we contribute to the literature by showing that, beyond widely documented private returns, increases in school funding can provide positive externalities via crime reductions in the long run. Importantly, we demonstrate that the MVPF of primary school funding is greater than one, even when considering only these crime-reducing benefits.

References

Abraham, Jonatan, and Vania Ceccato. 2022. "Crime and safety in rural areas: A systematic review of the English-language literature 1980–2020." *Journal of Rural Studies*, 94: 250–273.

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.

Asker, Erdal, Eric Brunner, and Stephen Ross. 2022. "The Impact of School Spending on Civic Engagement: Evidence from School Finance Reforms." *NBER Working Paper #30711.*

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.

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.

Bhuller, Manudeep, Gordon B Dahl, Katrine V Løken, and Magne Mogstad. 2020. "Incarceration, recidivism, and employment." *Journal of Political Economy*, 128(4): 1269–1324.

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.

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.

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

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.

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 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, 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.

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, 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, 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.

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. "Does school spending matter? The new literature on an old question." *NBER Working Paper #25368.*

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.

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.

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.

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

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.

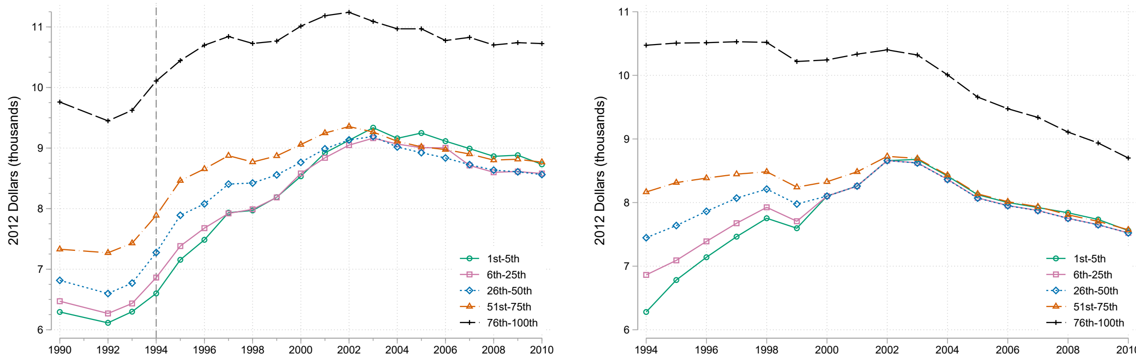
McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of econometrics*, 142(2): 698–714.

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

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.

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

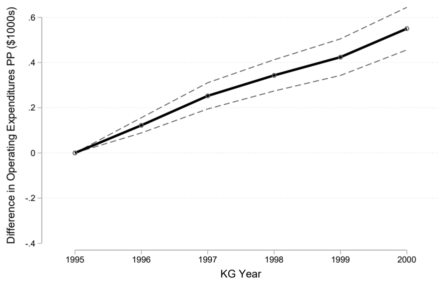


(a) Operating Expenditures

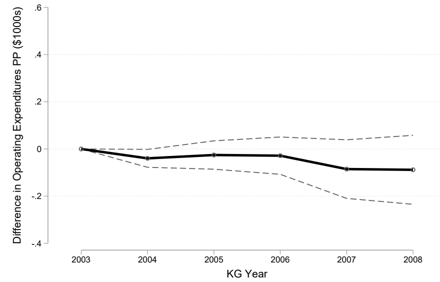
(b) Foundation Allowance

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.

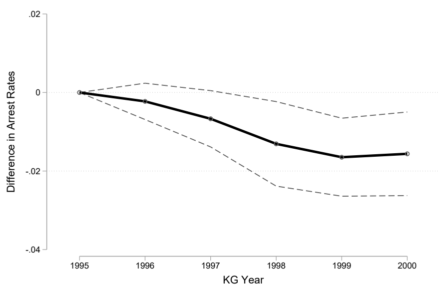
Figure 2: Differences in Predicted Spending and Arrest Rates by Cohort



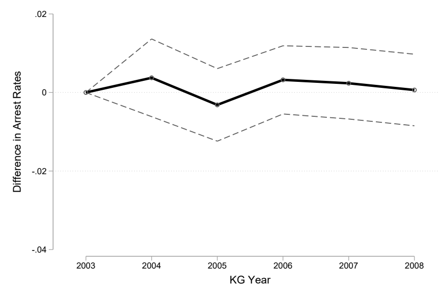
(a) K-3 Expenditures (1995–2000)



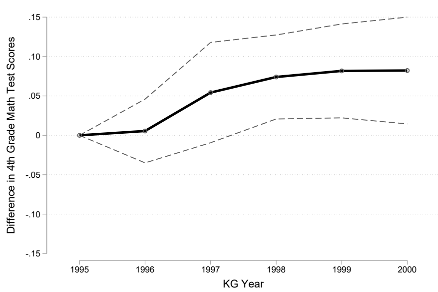
(b) K-3 Expenditures (2003–2008)



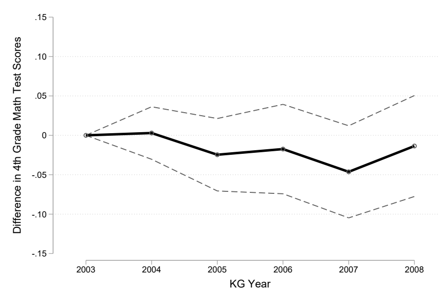
(c) Adult Arrest Rate (1995–2000)



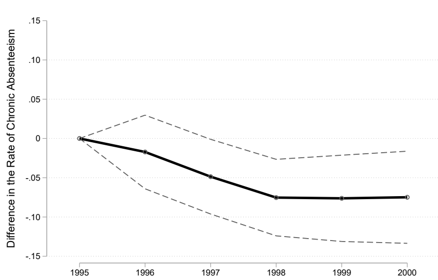
(d) Adult Arrest Rate (2003–2008)



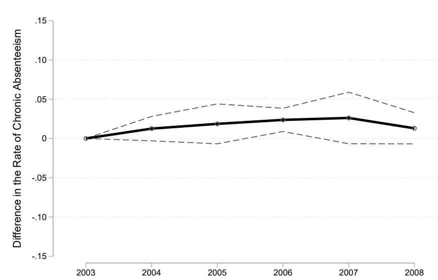
(e) G4 Math Scores (1995–2000)



(f) G4 Math Scores (2003–2008)



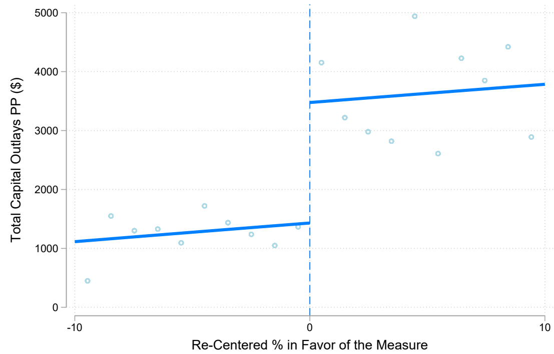
(g) G8 Absenteeism (1995–2000)



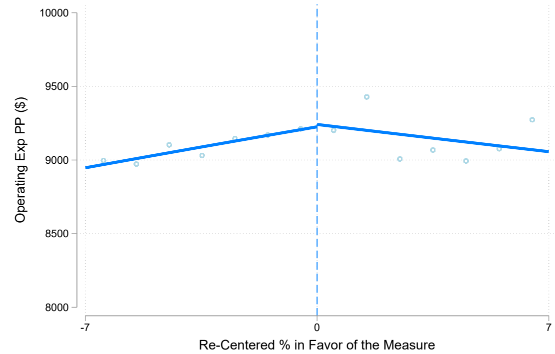
(h) G8 Absenteeism (2003–2008)

Notes: The figure plots estimates and 95% confidence intervals of ϕ_2 from Equation 1. The left column presents estimates for the 1995–2000 KG cohorts; the right column presents estimates for the 2003–2008 cohorts.

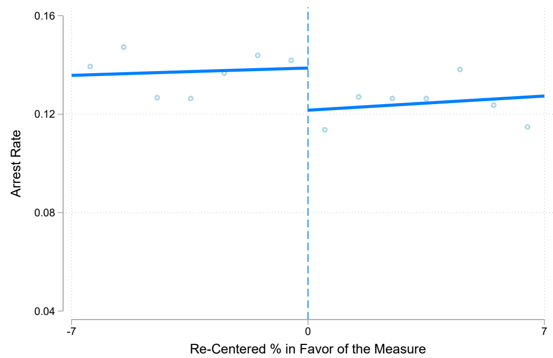
Figure 3: The Effect of Narrowly Winning an Election on Fiscal and Student Outcomes



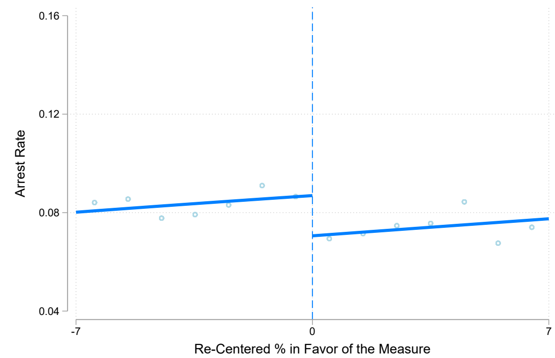
(a) Capital Outlays (t+1)



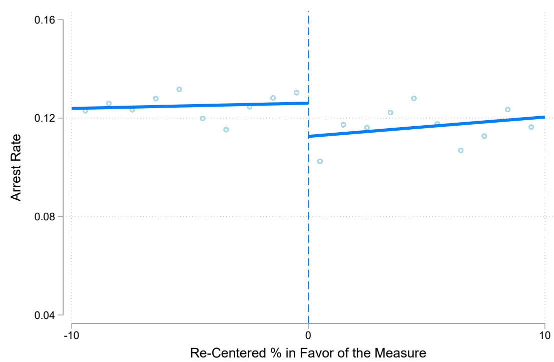
(b) Operating Expenditures (t+1)



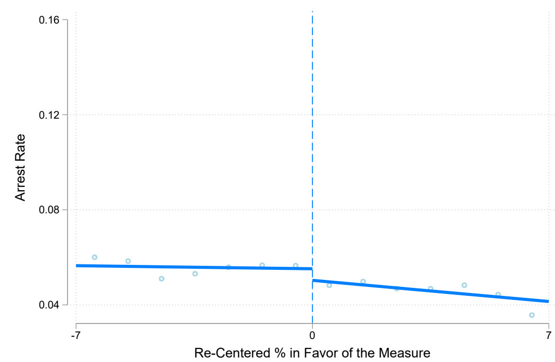
(c) Overall Arrest Rate



(d) Public Order Arrest Rate



(e) Misdemeanor Arrest Rate



(f) Felony Arrest Rate

Notes: The figures show average school district fiscal and long-term student 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. t represents the year of the focal election. The local polynomial estimator was constructed with a uniform kernel function.

Table 1: Summary Statistics for Proposal A Analysis Sample

Sample:	(1) All Students	(2) Never Arrested	(3) Ever Arrested	(4) Low Revenue	(5) 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 Outcomes					
Ever Arrested	0.12	0.00	1.00	0.11	0.12
Ever Arrested on a Violent Crime	0.05	0.00	0.40	0.04	0.05
Ever Arrested on a Property Crime	0.05	0.00	0.45	0.05	0.05
Ever Arrested on a Drug Crime	0.04	0.00	0.37	0.04	0.04
Ever Arrested on a Public Order Crime	0.08	0.00	0.68	0.07	0.08
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. Column 1 consists of all students in the sample, 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 BLS.

Table 2: 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 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 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 3: 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						
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						
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: 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.

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

Table 4: The Effects of Narrowly Winning an Election on Fiscal and Student Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effects on Fiscal Outcomes							
Dependent Variable:	Capital Outlay PP *t+1*	Capital Outlay PP *[t,t+3]*	Op. Exp. PP *t+1*	Op. Exp. PP *[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 Crime							
Dependent Variable:	Overall Rate	Felony Rate	Misdemeanor Rate	Violent Rate	Property Rate	Drug Rate	Public Order 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.