Public School Funding, School Quality, and Adult Crime

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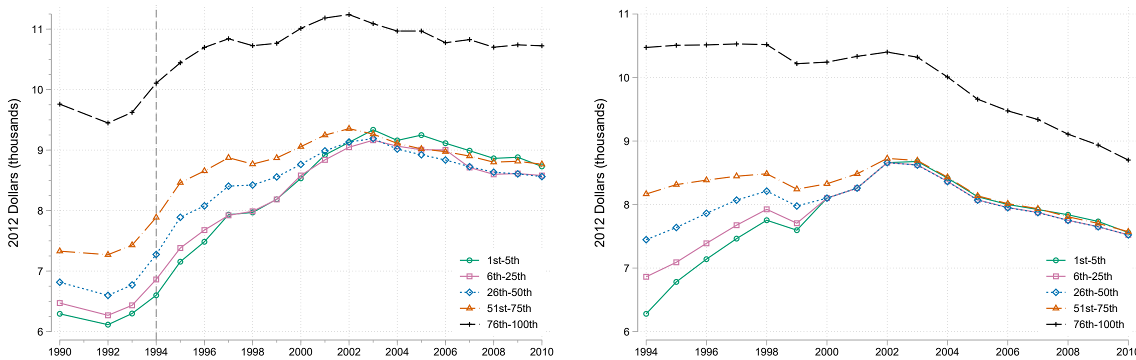
Online Appendix

A Supplemental Online Figures and Tables

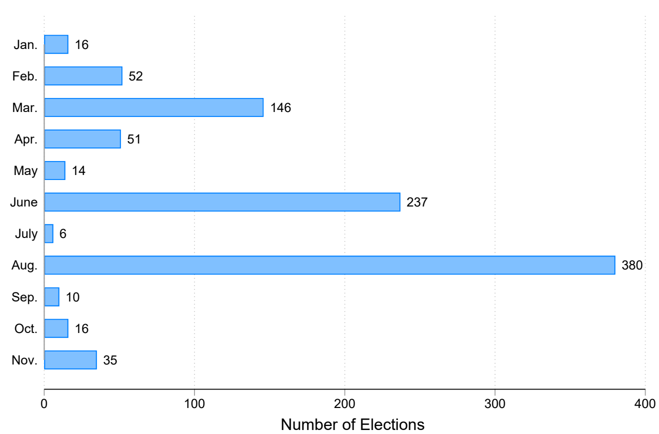
Figure A1: Time Series of Four-Year Average Expenditures and Foundation Allowance by 1994 Revenue Percentile



(a) Operating Expenditures (b) Foundation Allowance

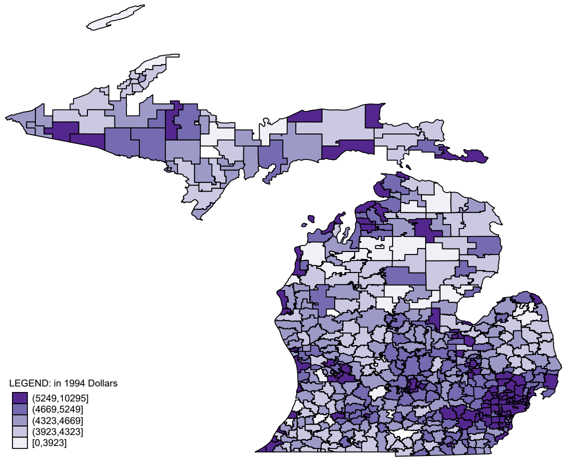
Notes: Panel A of the figure shows real (four-year) average per-pupil operating expenditures over time for school districts grouped by their 1994 revenue percentile. Panel B plots the real (four-year) average per-pupil foundation allowance over time for districts grouped by 1994 revenue percentiles. Four-year average spending (allowance) in year t is calculated as the average of spending (allowance) in t through $t + 3$. We convert all measures to 2012 dollars using the Employment Cost Index for elementary and secondary school employees provided by the Bureau of Labor Statistics.

Figure A2: Distribution of Capital Elections by Month, 1996-2004



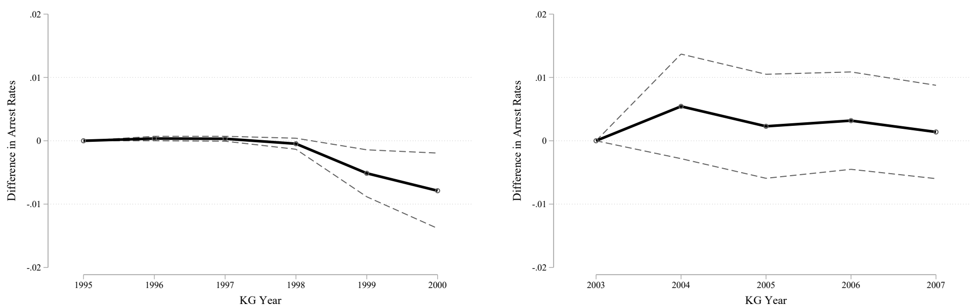
Notes: The figure shows the distribution of elections by election month. Election-level data come from MDE.

Figure A3: 1994 Revenue by School District



Notes: The figure plots 1994 state and local revenue per pupil for all school districts in Michigan. The darker shades correspond to higher 1994 revenue. These districts tend to appear in urban areas. The 1994 revenue bins reflect the same percentile groupings as in Figure 1.

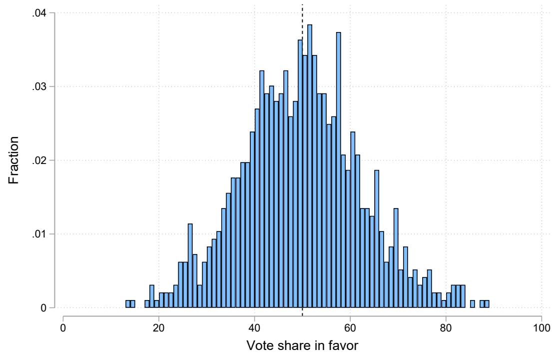
Figure A4: Differences in Adult Crime (Treated versus Placebo Cohorts)



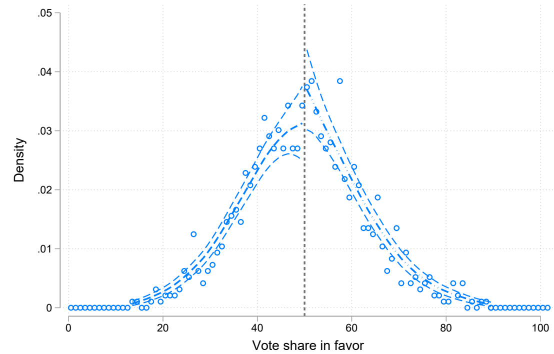
(a) Arrested by Age 19 (1995–2000) (b) Arrested by Age 19 (2003–2008)

Notes: The figure plots estimates and confidence intervals of ϕ_2 from Equation 1. The left column presents estimates for the 1995–2000 KG cohorts; the right column present estimates for the 2003–2008 cohorts. In contrast to Panels (c) and (d) in Figure 2 (in which the dependent variable is whether the student was ever arrested during the respective age window in which they could match to the arrest records), the outcome variable in this figure is whether the student was ever arrested by age 19. We do this to make sure that we measure criminal justice contact during the same age window for both sets of KG cohorts. The point estimates for the first four years in Panel (a) are mostly mechanical zeroes. KG students in 1995 (most of which were born in 1990) can only be observed in the arrest records from ages 22–30. Similarly, KG students in 1998 (most of which were born in 1993) can only be observed from ages 19 to 27. While this approach is not perfect (since, for example, students in the 2007 cohort are only observed during ages 10–18 in the criminal justice records), it is a more consistent measure of criminal justice contact across the two sets of cohorts. Finally, note that, while the point estimates in Panel (a) of this figure are smaller in magnitude than the point estimates in Panel (c) of Figure 2, the control mean is also significantly lower: 0.030 versus 0.131.

Figure A5: Vote Share Manipulation Tests



(a) Histogram



(b) Local-Linear Density Estimator

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

Table A1: Election Summary Statistics (1996-2004)

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

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

Table A2: Summary Statistics for Election Sample

Sample:	(1) All Elections	(2) Winning Elections	(3) Losing Elections
Fiscal Outcomes (t-2)			
Capital Outlays PP	919	1,198	602
Operating Expenditures PP	9,040	9,112	8,958
Demographic Characteristics(t-2)			
Fraction of 5-17 Year Olds in Poverty	0.10	0.09	0.11
Median Household Income	42,448	44,315	40,333
Local Unemployment Rate	4.63	4.34	4.96
Share of White Students	0.90	0.90	0.90
Number of Elections	955	470	485

Notes. The table describes the election-level analysis sample two years prior to the focal election. Panel A presents fiscal outcomes, while Panel B presents demographic characteristics. Column 1 presents summary statistics for all 955 elections in the sample. Columns 2 and 3 present these summary statistics separately for all winning and losing elections in the sample, respectively.

Table A3: Test of Differential Pre-Trends by 1994 Revenue Levels

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

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

Table A4: Probing Estimates of the Effects of Operating Spending on Adult Arrests

Robustness Check:	(1) Baseline	(2) Total Spending	(3) Levels	(4) Reduced Form	(5) Drop Top Quartile	(6) Drop Detroit	(7) Drop Crime Control	(8) Region-by-Cohort Fes
Log (Mean Operating K-3 Spending)	-0.196***				-0.258***	-0.132**	-0.197***	-0.189**
	(0.070)				(0.073)	(0.066)	(0.070)	(0.079)
Log (Mean Total K-3 Spending)		-0.191***						
		(0.068)						
Mean Operating K-3 Spending ($1000s)			-0.025**					
			(0.011)					
Log (Mean K-3 Foundation Grant)				-0.146***				
				(0.052)				
F-Statistic	253	283	94		180	211	255	194
Observations	717,042	717,042	717,042	717,042	370,502	637,989	717,042	717,042
Control Mean	0.131	0.131	0.131	0.131	0.151	0.107	0.131	0.131
Percent Effect	-15.0	-14.6	-19.1	-11.1	-19.7	-12.2	-15.0	-14.4
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓		✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Column 1 shows our baseline estimate of β_1 from Equation 3. In Column 2, we report estimates of β_1 using the district's *total* expenditures as opposed to *operating* spending. Column 3 reports estimates of β_1 from the 2SLS specification in Equations 2 and 3, but in levels (operating expenditures and foundation allowance in $1000s) as opposed to logs. Column 4 reports the results of a "reduced-form" regression. Specifically, we estimate Equation 2 but replace the student's expenditures in K–3 with an indicator for whether the student was ever arrested as an adult. This reduced form estimate, divided by the first stage estimate of 0.75, approximates our baseline 2SLS point estimate of -2 percentage points. In Column 5, we drop from our sample school districts in the top quartile of the 1994 distribution. As shown in Figure 1, these districts spend substantially more than the remaining districts; thus, one may be concerned that they are systematically different and should not be included in the sample. In Column 6, we drop from our sample students who attended Detroit Public Schools in K–3. Column 7 drops as a control variable the district's baseline arrests per student interacted with cohort fixed effects. See Online Appendix D for details about mapping precinct-level MSP data to school districts. Column 8 additionally controls for region-by-cohort fixed effects. We define "regions" as the eight geographic regions established by the Michigan Governor in 2020 for the purposes of COVID-related reopening guidelines.

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

Table A5: Robustness of our Baseline Estimates to Measuring Spending in Different Grades

	(1)	(2)	(3)	(4)	(5)	(6)
Spending Measured During:	K–3	K–4	K–5	K–6	K–7	K–8
Log(Mean Spending)	-0.196***	-0.206***	-0.225***	-0.242***	-0.276***	-0.319***
	(0.070)	(0.074)	(0.082)	(0.087)	(0.100)	(0.116)
F-Statistic	253	240	205	180	149	133
Observations	717,042	717,042	717,042	717,042	717,042	717,042
Control Mean	0.131	0.131	0.131	0.131	0.131	0.131
Percent Effect	-15.0	-15.7	-17.2	-18.5	-21.1	-24.4
District and Cohort FEs	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓

Notes. The first row in Column 1 shows our baseline estimate of β_1 from Equation 3. Each additional column presents estimates of β_1, but in a specification where we continue to measure exposure to the foundation grant during K–3, but measure operating expenditures during different grade ranges. We do not examine spending in grades beyond eight because, as shown in the table, the first-stage F-statistic becomes substantially smaller as we increase the grade range, and because roughly 20% of students in Michigan leave public schools during high school primarily due to dropout. The declining F-statistic is consistent with limited variation in the allowance after 2003. The second row shows standard errors in parentheses, clustered at the district level. We also present the Kleibergen-Paap Wald F-statistic of the first-stage relationship from Equation 2, and 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 all columns is a dummy variable equal to one if the student was ever arrested as an adult.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Robustness to Arrest Age Range

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

Notes. The first row of the table shows estimates of β_1 from Equation 3. The second row shows standard errors in parentheses and clustered at the district level. The third row shows the "control mean"—the average value of the dependent variable in initially high-spending school districts (those in the top quartile of the 1994 state and local revenue distribution). Each column shows the results of a separate regression, where the dependent variable for that regression is listed at the top of the column.

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

Table A7: Robustness to Out-of-State Migration

	(1)	(2)	(3)	(4)	(5)	(6)
		Attend College Out-of-State				
Dependent Variable:	Leave State in K-12	All Districts	Low-Income Districts	Drop K-12 Attriters	Drop Out-of-State College	Drop High Migration Counties
Log (Mean K-3 Spending)	0.019	0.193***	0.048	-0.195***	-0.206***	-0.202***
	(0.026)	(0.055)	(0.078)	(0.072)	(0.072)	(0.074)
Control Mean	0.033	0.195	0.172	0.137	0.146	0.147
Observations	689,265	717,042	327,943	664,845	585,520	513,850
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. The second row shows standard errors in parentheses, clustered at the district level. The third row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). The dependent variable in Column 1 is a dummy variable equal to one if the student ever left the state in K–12. We measure this outcome using exit codes that are assigned to students who leave the Michigan Public School system. The dependent variable in Columns 2 and 3 is an indicator variable equal to one if the student ever enrolled in postsecondary education outside of Michigan. This information comes from our K-12–NSC matched dataset. The sample in Column 3 consists only of students enrolled in baseline low-income school districts—those above the median of the 1995 district-level FRPL distribution. In Columns 4 and 5 we drop from the sample any student who (1) left the state in K–12 and (2) ever attended college outside of Michigan, respectively. Finally, in Column 6 we drop students whose K–3 districts are in "high-migration" counties—those in the top quartile of county-level migration rates. We calculate these figures using county-level out-of-state migration rates from the 2005-2009 American Community Survey (ACS).

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

Table A8: Heterogeneity By Socio-Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Ever Arrested	Ever Arrested	Ever Arrested	Ever Arrested	Ever Arrested	Ever Arrested	Ever Arrested	Ever Arrested
Log (Mean K-3 Spending)	-0.306*	-0.115	-0.358**	-0.052	-0.147*	-0.267***	-0.328***	-0.233***
	(0.159)	(0.096)	(0.143)	(0.088)	(0.079)	(0.075)	(0.118)	(0.082)
Subgroup	Low Income	High Income	Low Performing	High Performing	Male	Female	FRPL	Non-FRPL
Observations	327,943	389,099	345,950	371,092	379,968	337,074	231,235	485,807
Control Mean	0.189	0.080	0.182	0.081	0.161	0.097	0.219	0.086
Percent Effect	-16.2	-14.4	-19.7	-6.4	-9.1	-27.5	-15.0	-27.1
District and Cohort 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. The second row shows standard errors in parentheses, clustered at the district level. Each column is a separate regression estimated on the district or student subgroup described in the third row. The fifth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Baseline low income is defined as above the median of the 1995 district-level FRPL distribution. Baseline low performing is defined as below the median of the 1995 district-level fourth-grade math test score distribution. Male, female, FRPL, and non-FRPL in Columns 5–8 are measured at the student level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Heterogeneity by Sex and Crime Type

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

Notes. The first row of each panel shows estimates of β_1 from Equation 3. The second row shows standard errors in parentheses, clustered at the district level. The fourth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Each column in Panel A is a separate regression, estimated only on the sample of male students. Similarly, each column in Panel B is estimated on the sample of female students. The dependent variable in Column 1 is whether 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 in adulthood.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: How Do Districts Spend the Additional Allowance Dollar?

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

Notes. The first row of the table shows estimates of γ_1 from Equation 2, but in levels. In other words, the dependent variable in each column is K–3 total expenditures in the particular account in \$1000s, while the independent variable is the K–3 average foundation allowance in \$1000s. Total operating expenditures (Column 1) are equal to the sum of instructional expenditures (Column 2) and expenditures for support services (Column 3). Columns 4–9 examine detailed expenditure accounts within support services. Standard errors are clustered at the district level and shown in parentheses in Row 2. The fourth row of the table shows the sample mean of the expenditure account. We test whether the marginal allowance dollar was spent differently than the average dollar by comparing the fraction of the marginal dollar spent in a given account (B) to the fraction of the average dollar in our sample in that specific account (A). The seventh row reports p-values of a statistical test that A=B.

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

Table A11: Effects of Additional Operating Expenditures on Measures of School Quality

	(1)	(2)	(3)	(4)	(5)
			Pupil/Educational Leadership Ratio		
Dependent Variable:	Log(Average Teacher Salaries)	Pupil/Teacher Ratio	Superintendents and Principals	Superintendents	Principals
Log (Mean K-3 Spending)	0.461**	-7.4**	-368***	-12,059**	-383***
	(0.191)	(3.0)	(84)	(4,712)	(92)
Observations	717,042	717,042	717,042	717,042	717,042
Control Mean		21.4	286	3,507	332
Percent Effect	5	-4	-13	-34	-12
District and Cohort 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. The second row shows standard errors in parentheses, clustered at the district level. The fourth row shows the "control mean"—the average value of the dependent variable in initially high-revenue school districts (those in the top quartile of the 1994 state and local revenue distribution). Finally, the fifth row shows the effect of a 10% increase in spending in percent terms (relative to the control mean). We omit the control mean in Column 1 because the dependent variable in this column is logged.

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

Table A12: Effects of Proposal A on Measures of School and Teacher Quality From the SASS

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

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

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

Table A13: Effects of Operating Spending on Intermediate Outcomes

Dependent Variable:	(1) G4 Math Score	(2) G8 Math Score	(3) Chronically Absent G8	(4) Share Days Absent G8	(5) Ever JDC	(6) HS Grad	(7) Postsec Grad
Log (Mean K-3 Spending)	1.215***	-0.416	-0.833**	0.296**	-0.034**	0.268***	0.150**
	(0.432)	(0.300)	(0.325)	(0.122)	(0.017)	(0.087)	(0.066)
Observations	674,369	636,499	652,807	652,807	717,042	684,916	717,042
Control Mean			0.155	0.944	0.014	0.782	0.349
Percent Effect			-53.7	-52.9	-24.3	3.4	4.3
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓

Notes. The table shows estimates of additional spending on intermediate outcomes. The first row of the table shows estimates of β_1 from Equation 3. The second row shows standard errors in parentheses, clustered at the district level. Given that test scores have been standardized, their effects should be interpreted as standard deviation percent changes. For instance, Column 1 shows that a 10% increase in K–3 spending leads to an increase in test scores of 12% of a standard deviation.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Mediation Analysis of the Effects of Operating Spending on Adult Crime (Intermediate Outcomes)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Holding Constant:	Baseline	G4 Math Score	Chronically Absent G8	Share Days Absent G8	Ever JDC	HS Grad	All
Log (Mean K-3 Spending)	-0.227***	-0.195***	-0.167**	-0.137**	-0.220***	-0.186**	-0.129*
	(0.070)	(0.069)	(0.066)	(0.070)	(0.069)	(0.073)	(0.068)
Observations	614,496	614,496	614,496	614,496	614,496	614,496	614,496
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓

Notes. Column 1 presents estimates of β_1 from Equation 3 but estimated on the sample of students with non-missing intermediate outcome measures. We do this to avoid conflating sample compositional changes with attenuation in the main treatment effect due to channels operating through the mediator. Each subsequent column presents estimates of β_1 from specifications that additionally control for the intermediate outcome of interest. The dependent variable is an indicator for whether the student was ever arrested as an adult. The second row shows standard errors in parentheses, clustered at the district level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Heterogeneity in Intermediate Outcomes by Socio-Demographics

Dependent Variable:	(1) G4 Math Score	(2) G8 Math Score	(3) Chronically Absent G8	(4) Share Days Absent G8	(5) Ever JDC	(6) HS Grad	(7) Postsec Grad
	Panel A: Low Income						
Log (Mean K-3 Spending)	1.291**	-0.528	-0.915**	0.293**	-0.070**	0.178	0.115
	(0.644)	(0.430)	(0.443)	(0.120)	(0.030)	(0.161)	(0.110)
	Panel B: High Income						
Log (Mean K-3 Spending)	0.573	-0.526	-0.309	0.175	-0.003	0.358***	0.177*
	(0.564)	(0.501)	(0.323)	(0.162)	(0.016)	(0.122)	(0.107)
	Panel C: Low Performing						
Log (Mean K-3 Spending)	1.396**	-0.355	-1.166***	0.311***	-0.072**	0.281*	0.086
	(0.596)	(0.414)	(0.440)	(0.112)	(0.029)	(0.161)	(0.099)
	Panel D: High Performing						
Log (Mean K-3 Spending)	0.621	-0.627	-0.228	0.200	-0.002	0.269**	0.170*
	(0.562)	(0.487)	(0.307)	(0.181)	(0.016)	(0.112)	(0.094)
	Panel E: Male						
Log (Mean K-3 Spending)	1.174***	-0.542*	-0.852***	0.292**	-0.065**	0.247**	0.119
	(0.429)	(0.299)	(0.319)	(0.120)	(0.028)	(0.104)	(0.085)
	Panel F: Female						
Log (Mean K-3 Spending)	1.256***	-0.292	-0.818**	0.302**	-0.000	0.282***	0.176**
	(0.457)	(0.343)	(0.336)	(0.124)	(0.014)	(0.089)	(0.084)
	Panel G: FRPL						
Log (Mean K-3 Spending)	1.723***	-0.462	-1.376***	0.413***	-0.034	0.383***	0.092
	(0.543)	(0.402)	(0.438)	(0.125)	(0.031)	(0.148)	(0.084)
	Panel H: Non-FRPL						
Log (Mean K-3 Spending)	1.039**	-0.434	-0.470*	0.220*	-0.040**	0.421***	0.264***
	(0.437)	(0.355)	(0.279)	(0.129)	(0.017)	(0.099)	(0.085)
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓

Notes. The first row in each panel shows estimates of β_1 from Equation 3. The second row shows standard errors in parentheses, clustered at the district level. Every estimate comes from a separate regression estimated on the district or student subgroup described in the panel title and where the dependent variable is listed in the corresponding column. Baseline low income is defined as above the median of the 1995 district-level FRPL distribution. Baseline low performing is defined as below the median of the 1995 district-level fourth-grade math test score distribution. Male, female, FRPL, and non-FRPL status are measured at the student level.

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

Table A16: How Do Districts Spend the Additional Allowance Dollar? (by Subgroup)

Share of Marginal Dollar Allocated to:	(1) Instruction	(2) Support Services
All School Districts	0.571	0.427
Baseline Low Income	0.726	0.274
Baseline High Income	0.569	0.431
Baseline Low Performing	0.743	0.257
Baseline High Performing	0.585	0.415

Notes. The table presents the fraction of the marginal dollar spent in either the instructional or support services account. The first row reports these estimates for all 518 school districts in our sample (as previously reported in Row (B) of Table A10). The remaining rows repeat this exercise, but separately for different subgroups of school districts. There are 259 school districts in each category. Baseline low income is defined as above the median of the 1995 district-level FRPL distribution. Baseline low performing is defined as below the median of the 1995 district-level fourth-grade math test score distribution.

Table A17: Effects of Operating Spending on Subsequent Peer Composition

Dependent Variable:	(1) Ever Moved	(2) Fraction 5-17 in Poverty	(3) Percent Unemployment	(4) Median Income	(5) Fraction White	(6) Fraction Low Crime
Log (Mean K-3 Spending)	-0.091	-0.012	-2.454*	-38,288***	0.156**	0.187*
	(0.061)	(0.076)	(1.383)	(8,793)	(0.073)	(0.107)
Observations	695,551	699,191	699,191	699,193	699,191	717,042
Control Mean	0.113	0.357	8.102	49,116	0.592	0.652
Percent Effect	-8.1	-0.3	-3.0	-7.8	2.6	2.9
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. The second row shows standard errors in parentheses and clustered at the district level. All of dependent variables are measured during a student's "transition years"—grades 6 and 9. For instance, the dependent variable in the first column is whether the student was ever "new" to a school district in either grade 6 or 9. Similarly, the outcome variable in the third column is the student's district's average percent unemployment rate in grades 6 and 9. We measure these outcomes during transition years since students are most likely to move during these grades. The dependent variable in Column 6 measures the average fraction of students who come from baseline low-crime school districts in the youth's school district during grades 6 and 9. Baseline "low crime" is defined as below the median of the 1997 district-level arrests per student distribution.

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

Table A18: Mediation Analysis of the Effects of Operating Spending on Adult Crime (Peer Composition)

Holding Constant:	(1) Baseline	(2) Ever Moved	(3) Fraction 5-17 in Poverty	(4) Percent Unemployment	(5) Median Income	(6) Fraction White	(7) Fraction Low Crime	(8) All
Log (Mean K-3 Spending)	-0.198***	-0.194***	-0.198***	-0.195***	-0.213***	-0.196***	-0.199***	-0.193***
	(0.071)	(0.070)	(0.072)	(0.073)	(0.070)	(0.073)	(0.071)	(0.072)
Observations	695,551	695,551	695,551	695,551	695,551	695,551	695,551	695,551
District and Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Demographic Controls	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Arrests × Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline District Controls × Cohort FEs	✓	✓	✓	✓	✓	✓	✓	✓

Notes. Column 1 presents estimates of β_1 from Equation 3 but estimated on the sample of students with non-missing measures of peer composition. We do this to avoid conflating sample compositional changes with attenuation in the main treatment effect due to channels operating through the mediator. Each subsequent column presents estimates of β_1 from specifications that additionally control for the peer composition measure of interest. The dependent variable is an indicator for whether the student was ever arrested as an adult. The second row shows standard errors in parentheses, clustered at the district level. All peer composition variables are measured during grades 6 and 9.

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

Table A19: Balance Tests for the RD Design

Dependent Variable ($t-2$):	(1) Capital Outlays PP	(2) Operating Expenditures PP	(3) Fraction 5-17 in Poverty	(4) Median Income	(5) Unemployment Rate	(6) Share White
Election Passed	43	-58	-0.021	618	0.344	-0.001
P values from...						
Conv. Variance Estimator	[0.877]	[0.820]	[0.227]	[0.729]	[0.481]	[0.986]
Robust Variance Estimator	{0.893}	{0.849}	{0.300}	{0.768}	{0.557}	{0.988}
Control Mean	621	8,891	0.110	39,478	4.924	0.906
Percent Effect	6.9	-0.7	-19.1	1.6	7.0	-0.1
Bandwidth	10.3	8.5	8.0	9.8	11.0	6.9

Notes. The table shows the results of local-linear regressions of school districts' characteristics measured in $t-2$, on whether the election eventually passed in t. Specifically, we present estimates of Λ_2 from Equation 4. We estimate local-linear regressions with robust bias-corrected confidence intervals and inference procedures following the approach developed in Calonico, Cattaneo and Titiunik (2014). The first row of each panel presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from a variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). Finally, we show the effect in percent terms: the point estimate divided by the control mean, multiplied by 100. We use a triangular kernel function in each specification.

Table A20: Robustness of the Main RD Estimates

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

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

Table A21: Heterogeneity in RD Estimates by Socio-Demographics

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

Notes. The table shows the results of local-linear regressions of school districts' outcomes. The dependent variable in Columns 1–4 is the arrest rate for students in that particular subgroup. These specifications are estimated on the sample of all school districts. Columns 5–8 show estimates of Λ_2 from Equation 4, but estimated only on the sample of school districts in that particular subgroup. The first row presents local-linear regression estimates with bias-correction. The second and third rows report two p-values corresponding to the bias-corrected estimate: one derived from a conventional variance estimator and one derived from variance estimator robust to the bias-correction. The table also presents Calonico, Cattaneo and Titiunik (2014)'s mean-squared-error optimal bandwidth, and the control mean (the average of the dependent variable for losing districts with a vote share within the optimal bandwidth). We also present the effect in percent terms: the point estimate divided by the control mean, multiplied by 100. We use a triangular kernel function in each specification.

Table A22: Effects of Narrowly Passing a Capital Election on School Inputs 10 Years Later

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

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

Table A23: Distribution of Capital Projects in our Sample

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

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

Table A24: Heterogeneity in RD Estimates by Capital Project Type

	(1)	(2)	(3)	(4)	(5)	(6)
	Project Type		Facility/Equipment Type		Level	
Sample:	New Structure/ Equipment	Additions/ Renovations/ Improvements	Instructional	Non-Instructional	Elementary/ Middle School	High School
Election Passed	-0.053	-0.020	-0.026	-0.005	-0.034	-0.003
P values from...						
Conventional Variance Estimator	[0.045]	[0.204]	[0.076]	[0.769]	[0.055]	[0.884]
Robust Variance Estimator	{0.075}	{0.285}	{0.124}	{0.803}	{0.101}	{0.901}
Control Mean	0.162	0.131	0.169	0.101	0.163	0.164
Percent Effect	-32.7	-15.3	-15.4	-5.0	-20.9	-1.8
Bandwidth	6.7	7.7	8.9	7.7	6.4	8.4

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

Table A25: The Effects of Narrowly Winning an Election on Intermediate Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	G4 Math Score	G8 Math Score	Chronically Absent G8	Share Days Absent G8	Ever JDC	HS Grad
Election Passed	-0.037	0.091	-0.025	-0.005	0.001	0.018
P values from...						
Conventional Variance Estimator	[0.428]	[0.127]	[0.082]	[0.360]	[0.801]	[0.283]
Robust Variance Estimator	{0.504}	{0.200}	{0.122}	{0.422}	{0.834}	{0.354}
Control Mean			0.100	0.046	0.012	0.751
Percent Effect			-25.1	-11.4	8.3	2.4
Bandwidth	11.0	8.3	9.2	12.4	7.5	10.2

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

Table A26: Examining Sorting Following a Close Election

	(1)	(2)	(3)	(4)
Panel A: Outcomes in t+5				
Dependent Variable:	Fraction 5-17 in Poverty	Unemployment Rate (%)	Median Income	Fraction White
Election Passed	-0.019	-0.077	2,409	-0.001
P values from...				
Conventional Variance Estimator	[0.150]	[0.903]	[0.210]	[0.979]
Robust Variance Estimator	{0.227}	{0.920}	{0.298}	{0.982}
Control Mean	0.129	7.514	42,832	0.898
Percent Effect	-14.7	-1.02	5.62	-0.11
Bandwidth	9.2	7.5	9.4	7.2
Panel B: Outcomes in t+10				
Dependent Variable:	Fraction 5-17 in Poverty	Unemployment Rate (%)	Median Income	Fraction White
Election Passed	-0.015	-0.426	-1,132	0.019
P values from...				
Conventional Variance Estimator	[0.465]	[0.600]	[0.637]	[0.608]
Robust Variance Estimator	{0.552}	{0.661}	{0.691}	{0.666}
Control Mean	0.157	9.846	48,018	0.871
Percent Effect	-9.55	-4.33	-2.36	2.18
Bandwidth	8.2	7.8	10.5	8.6

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

B Dynamic Regression Discontinuity Design

Motivation and Description of the Dynamic RD Approach

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

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

We focus on estimates of ITT effects in our main analysis for several reasons. First, the issue of non-compliance appears to be small in our context: while the median number of elections per district is two, the median number of passed elections is only one. Second, ITT effects are estimable using standard RD approaches, while the dynamic RD estimates of the TOT effect embed a variety of additional assumptions related to the separability of effects over time and the homogeneity of effects across the distribution of vote shares. Finally, the dynamic RD strategy is most useful when one wishes to trace out dynamic treatment effects on time-varying outcomes, such as test scores. The district outcomes of interest in this paper, such as the share of kindergarteners ever arrested, are time-invariant outcomes.

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

Cross-Sectional Setup

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

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

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

RD with Panel Data and Multiple Treatments

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

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

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

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

Estimating TOT Effects

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

Accordingly, we augment Equation B.2 with flexible polynomials of degree g in the vote share, $f_g(v^b_{d,t-\tau})$, and with indicators for the presence of a capital bond election in year $t-\tau$—$m^b_{d,t-\tau}$.[21] After adding school year (θ_t) and district-level (μ_d) fixed effects, the estimating equation becomes:

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

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

Using Dynamic RD to Estimate Causal Effects of Capital Expenditures

The dynamic RD approach in our setting is complicated by the fact that our main outcome of interest is time invariant (whether the student was ever arrested), and the usual approach is used to recover causal effects on time varying outcomes (e.g., 4th grade test scores in district d in year t). Thus, we estimate the causal effects of additional capital expenditures on the probability that a student is ever arrested in a 2SLS framework. In the first-stage, we use a district-year panel to estimate the TOT effect of district d narrowly winning an election in year $t-\tau$ on its total capital outlays per pupil in year t. In other

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

words, we estimate Equation B.3 with the district's capital outlays per pupil on the left hand side. We then use the "predicted capital expenditures" from this first-stage specification, and relate the probability that a student is ever arrested to predicted capital expenditures in the second stage:

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

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

First Stage: Predicting Capital Expenditures Per Pupil

We estimate Equation B.3 on a school district-year balanced panel from 1996 to 2010 where each district-year observation is used exactly once for the 430 school districts that held at least one election during this sample period.[22] We focus on this sample period because our cohorts of interest are first-time kindergarteners in 1999 through 2004. Note that in the main body of the paper, we focused on the 1996 through 2004 cohorts instead. We are not able to examine the outcomes of cohorts earlier than 1999 in the dynamic RD analysis because the strategy relies on lags of election win indicators. Given that the first year of available

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

bond election data is 1996, and that we allow for three lags in our main specification, we limit our analysis sample to cohorts from 1999 on. Because we follow students' exposure to additional spending through grade 6, our district-year panel includes each year through 2010 (the 6th grade year of the last cohort in our sample).[23] Standard errors are clustered at the district level. Following Cellini, Ferreira and Rothstein (2010), we specify $f_g(\cdot)$ as a third-order polynomial.

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

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

Finally, even though these expenditures are earmarked for local capital improvements,

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

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

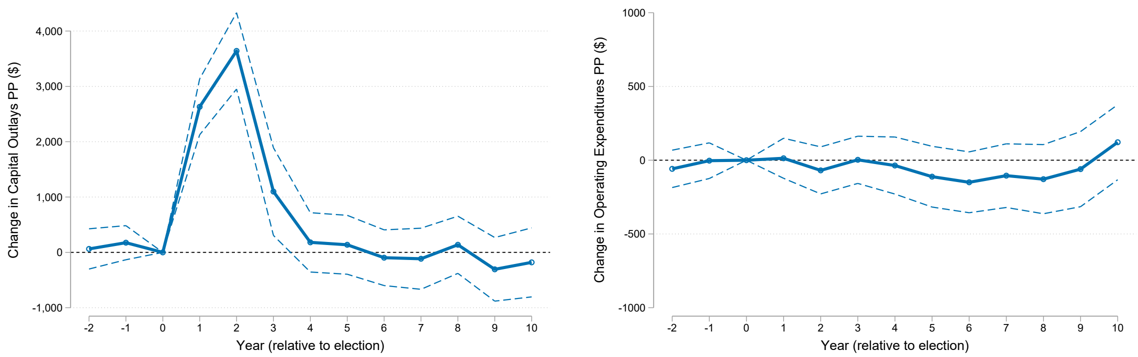
Second Stage: Relating Adult Arrests to Predicted Capital Expenditures

With a predicted capital expenditure per pupil for every district-year in our sample, it is straightforward to relate the probability that student i was ever arrested to the predicted capital expenditures that she was exposed to during K–6; we simply follow students across districts for seven years and average over the amount of capital spending that they were exposed to in each district and year.

Table B1 shows the results of the estimation of Equation B.4. The table presents estimates of β_1 for three different specifications. The first specification simply presents a bivariate relationship between $EverArr_i$ and $log(\widehat{CapOutk6})_i$. The second specification adds (kindergarten) district and (kindergarten) cohort fixed effects. Finally, the last specification adds the demographic controls in X_i. We cluster standard errors at the (kindergarten) district level.

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

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



(a) Capital Outlay

(b) Operating Expenditures

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

Table B1: TOT Effects of Capital Outlays on Adult Arrests

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

Notes.The table shows the results of the estimation of Equation B.4. It presents estimates of β_1 for three different specifications. The first specification simply presents a bivariate relationship between $EverArr_i$ and $log(\widehat{CapOutk6})_i$. The second specification adds district and cohort fixed effects. Finally, the last specification adds the demographic controls in X_i. Standard errors are clustered at the district level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Detailed MVPF Calculations and Additional Cost/Benefit Comparisons

This section provides the details of our MVPF calculations and describes how the costs and benefits of increased school spending compare to other educational and law enforcement interventions. We focus on operating expenditures as opposed to capital expenditures for the MVPF calculation for three reasons. First, given that capital spending is used toward durable assets lasting many years, calculating the costs and benefits of capital spending requires additional assumptions about the productive life-span of the assets and the rate at which the value of these assets depreciate. Second, operating expenditures make up the overwhelming share of school budgets (roughly 87%). Third, whereas policymakers can directly increase operating expenditures through state appropriations, capital expenditures are a local responsibility in most states.

Society's Willingness to Pay to Increase Public School Funding

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

We follow the economics of crime literature and use two sets of social cost estimates for each crime type. First, we follow Anders, Barr and Smith (2022) and assign each crime type the social cost estimated by McCollister, French and Fang (2010).[24] Second, we follow Mello (2019) and use the social cost estimates in Chalfin (2015). Panel A of Table 3 shows the results for both sets of social cost estimates, and for discount rates ranging from 3–5%. The estimates vary in magnitude but generally show large reductions in social costs from increased school funding. Specifically, the estimates range from a decline of \$4,378 to \$8,969 in social costs due to a 10% increase in school funding during K–3.

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

Net Costs of Increasing Public School Funding

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

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

As discussed in Section III.B, our main analysis focuses on spending in K–3 because there is little identifying variation in the foundation allowance after 2003, by which time the most recent kindergarten cohort reaches grade three. To ensure consistency across cohorts, we restrict to grades three and below. Still, students could be exposed to allowance-induced spending in later grades. For instance, the first cohort could be exposed to additional allowance-induced spending through grade 8. Ignoring this additional spending in later grades would bias our MVPF calculation upward. To understand how much additional spending students in our sample are exposed to in grades 4–12, we regress the average operating expenditure that students were exposed to in these grades on our main specification in Equation 3. We calculate that a 10% increase in K–3 spending leads to a 1.7% increase in spending in grades 4 through 12. Average operating spending in our sample is \$10,018. Thus, an increase of 1.7% in spending for nine years is equal to \$1,533 (=\$10,018 $\times$ 0.017 $\times$ 9). Given that this increased spending comes in later years, we discount it back 6.5 years from age 14, the average age in grades 4–12, to age 7.5, the average age in grades K–3. We present the discounted values in the row labeled "Grade 4–12 Cost" in Panel B, which range from \$1,116 to \$1,265, depending on the assumed discount rate. Total K-12 costs, presented in the third row of the panel, range from \$5,068 to \$5,217.

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

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

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

Estimates of the MVPF

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

(2020) for social policies targeting children such as child health insurance expansions.

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

To illustrate this point, consider the change in the MVPF estimates from incorporating Proposal A's effects on earnings. Previous work in Hendren and Sprung-Keyser (2020), based on earnings projections from estimates of the effects of Proposal A on educational attainment in Hyman (2017), calculates a willingness to pay of \$0.62 per \$1 of spending, and an MVPF of 0.65. Our estimates of the willingness to pay per \$1 of spending range from \$0.82 (=\$4,378/\$5,068, Column 6, Panel A, Table 3) to \$1.72 (=\$8,969/\$5,217, Column 1, Panel A, Table 3). The net cost per \$1 of spending in our setting ranges from \$0.82 (=1-(\$967/\$5,217)) to \$0.86 (=1-(\$731/\$5,068)). Therefore, incorporating increases in earnings (in addition to the crime reduction) in the willingness to pay calculations, we obtain MVPF estimates ranging from 1.75 to 2.9. This analysis highlights (1) that our estimates of the MVPF from crime reductions alone are likely conservative, and (2) that considering social policies' effects on crime can make a striking difference when calculating the MVPF of social policies.

Comparing School Funding to Other Educational Interventions

We next compare the cost-effectiveness of school funding at preventing crime to other early educational interventions. We create an index of cost-effectiveness by dividing a policy's direct cost by its percentage point impact on the likelihood of ever being arrested. For instance, we find that a \$5,217 per-pupil increase in school spending in baseline low-income school districts leads to a 3.1 percentage point reduction in the probability of ever being arrested (Table A8).[25] Thus, the amount of money spent to prevent 1 additional offender is \$168,290 (=\$5,217 / 0.031).[26]

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

Comparing School Funding To Police Spending

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

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

Given the elasticity of -0.47 found in Chalfin and McCrary (2018), this suggests that the cost per crime averted from investments in police officers is roughly \$46.80 (=\$220/4.7). However, whereas the decline in crime associated with increases in school funding takes

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

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

many years to materialize, the decline in crime associated with additional police officers is immediate. Therefore, we discount the $46.80 back 13.5 years, assuming a discount rate of 3%. This yields a cost per crime averted of $31.40. Alternatively, using a 5% and 7% discount rate instead, yields a cost per crime averted of $24.22 and $18.77, respectively.

What about the cost per crime averted of school funding? Using our main specification with the total number of arrests on the left hand side, we find that students exposed to 10% greater school funding during K–3 are arrested for 0.087, or 20.2%, fewer crimes as adults. The annual cost of additional school funding in our sample is $401 (=$5,217/13). Therefore, it costs $19.85 of school spending per pupil to avert one additional crime (=$401/20.2), an amount that is quite similar to that of hiring additional police officers.

Another relatively simple way of comparing the effects of school funding to additional police officers is to compare their benefit-cost ratios, considering only the crime-reducing benefits of school spending. Indeed, this comparison may be more accurate, as both our calculated benefit-cost ratio and those for police officers in the existing literature focus on more serious, "index" crimes. The benefit-cost ratios in police spending studies range from 0.8 to 1.6 (Chalfin and McCrary, 2018). These ratios consider only direct (not net) costs of the intervention. As a result, we adjust our calculations from Table 3 to exclude the declines in police, court, and incarceration costs associated with each prevented arrest. After removing these costs, we calculate benefit-cost ratios ranging from 0.9 to 1.9, which are similar to those of increasing the number of police officers. Importantly, our calculated "benefit-cost" ratio includes only the crime-reducing benefits of school funding, but excludes all other potential benefits (e.g., potential increases in earnings).

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

D Data Appendix

Additional Details on Data Sources

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

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

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

Education records contain individual-level covariates such as sex, race/ethnicity, and an indicator for free or reduced-price lunch (FRPL) eligibility that we control for in our main specifications. We measure student demographics and intermediate outcomes such as attendance in grade eight because, with the exception of test scores, these variables are unavailable prior to 2003, which is the year the first cohort reached eighth grade. We match students in these cohorts to the NSC, which contains postsecondary enrollment and degree receipt information. NSC data are nationwide, allowing us to observe whether a particular student ever enrolled in (or graduated from) a postsecondary education program outside the state.[27]

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

The school choice variables include: the percent of students living in the district who attend a charter school, number of charter schools located in the district, and number of charter schools located in the district and adjoining districts. The percent of students living in the district who attend a charter school was constructed using information from CEPI's Public Student Headcount Data and CEPI's Nonresident Student Research Tool. The number of charter schools located in the district and adjoining districts was constructed using charter school addresses and school district geographic boundaries.[28]

The district-level variables characterizing demographic and economic conditions include:

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

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

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

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

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

Matching Across Administrative Data Systems

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

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

E Elasticity of Arrests With Respect to Capital Expenditures

To compare the magnitude of the effect of increases in capital expenditures to that of increases in operating expenditures, we compute the elasticity of adult arrests with respect to capital outlays. As shown in Section IV.B, the corresponding elasticity for operating expenditures is -1.5, since a 10% increase in operating spending from kindergarten through third grade reduces the probability of an adult arrest by 15%. Computing this elasticity for capital spending is complicated by the fact that, while operating expenditures are used toward educational inputs in the same year (such as teacher salaries), capital expenditures purchase durable assets that are productive for many years after their purchase. Ignoring the benefit of a capital expense experienced by students in the future would understate its cost-effectiveness. As such, we follow Jackson and Mackevicius (2021) and smooth the capital expenditure over the life of the durable asset.

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