*American Economic Journal: Economic Policy 2017, 9(4): 256–280*
*https://doi.org/10.1257/pol.20150249*

# Does Money Matter in the Long Run? Effects of School Spending on Educational Attainment†

*By* JOSHUA HYMAN*

*This paper measures the effect of increased primary school spending on students' college enrollment and completion. Using student-level panel administrative data, I exploit variation in the school funding formula imposed by Michigan's 1994 school finance reform, Proposal A. Students exposed to $1,000 (10 percent) more spending were 3 percentage points (7 percent) more likely to enroll in college and 2.3 percentage points (11 percent) more likely to earn a postsecondary degree. The effects were concentrated among districts that were urban and suburban, lower poverty, and higher achieving at baseline. Districts targeted the marginal dollar toward schools serving less-poor populations within the district.* (*JEL* H75, I21, I22, I28)

Government spending on primary and secondary education accounts for 4.3 percent of US GDP (National Center for Education Statistics 2013). Despite this large government investment, it remains unknown whether education spending improves students' long-run outcomes. One difficulty in answering this question is the lack of plausibly exogenous variation in spending. A second challenge is the necessity of high quality administrative data with which to track students over time, and especially past high school graduation.

An extensive literature has solved the first challenge by exploiting plausibly exogenous changes in education spending due to school finance reform. Throughout the latter half of the twentieth century, dozens of states reformed education financing with the goal of reducing inequalities in education by equalizing spending across school districts. These reforms generally succeeded in (at least partially) equalizing spending between poor and rich school districts (Downes 1992; Murray,

* Department of Public Policy, 1800 Asylum Avenue, Fourth Floor, West Hartford, CT 06117 (email: joshua.hyman@uconn.edu). I thank Susan Dynarski, John Bound, Brian Jacob, and Jeffrey Smith for their advice and support. I am grateful for helpful conversations with Eric Brunner, Paul Courant, Eleanor Dillon, Thomas Downes, Robert Garlick, Michael Gideon, Max Kapustin, Glenn Nelson, Stephen Ross, Caroline Theoharides, and various conference and seminar participants. Thanks to Kathryn Summers at the Michigan Senate Fiscal Agency for data and helpful discussion. I am grateful to three anonymous referees for helpful comments that significantly improved the paper. Thanks to the Institute of Education Sciences, US Department of Education for providing support through Grant R305E100008 to the University of Michigan. Thanks to my partners at the Michigan Department of Education (MDE) and Michigan's Center for Educational Performance and Information (CEPI). This research used data structured and maintained by the Michigan Consortium for Education Research (MCER). MCER data are modified for analysis purposes using rules governed by MCER and are not identical to those data collected and maintained by MDE and CEPI. Results, information, opinions, and any errors are my own and are not endorsed by or reflect the views or positions of MDE or CEPI.

† Go to https://doi.org/10.1257/pol.20150249 to visit the article page for additional materials and author disclosure statement or to comment in the online discussion forum.

Evans, and Schwab 1998; Hoxby 2001; Guryan 2001; Card and Payne 2002; Papke 2005, 2008; and Roy 2011). However, it is less clear whether the changes in spending affected student achievement, with some studies finding positive effects and others finding no effects. Furthermore, the preceeding literature focused almost exclusively on student achievement in school, as opposed to graduation or long-run outcomes.

In this paper, I examine the long-run effects of school spending on students' educational attainment. I exploit variation across districts and over time in the funding formula imposed by Michigan's 1994 school finance reform, Proposal A, as an instrument for spending. Student-level panel data allow me to examine the effects of spending during primary school on students' college entry and completion later in life. Unlike previous studies that examined the effects of lagged district spending on average district achievement, I track students across districts, allowing me to reduce measurement error in the spending variable by observing the spending that students are exposed to each year. This paper is also the first study of student outcomes beyond high school using individual-level panel data.

I find that students exposed to $1,000 or approximately 10 percent more spending per year experienced a 3.0 percentage point (7 percent) increase in college enrollment, and a 2.3 percentage point (11 percent) increase in degree receipt. The larger percent increase in degree receipt than enrollment suggests that many of the students induced into college by the additional spending persisted to completion, but also that the additional spending boosted the graduation rate for students who would have enrolled in the absence of the spending increase.

The results in this paper are somewhat smaller than those found in Jackson, Johnson, and Persico (2016) and Candelaria and Shores (2015), who also examine effects of school finance reform on educational attainment. As far as boosting postsecondary enrollment, the increased school funding was less cost-effective than Head Start (Deming 2009), but more cost-effective than the Tennessee STAR experiment (Chetty et al. 2011; Dynarski, Hyman, and Schanzenbach 2013).

To further explore the effects on postsecondary attainment, I extend my analysis in three ways. First, I examine heterogeneity by district characteristics such as urbanicity, poverty, and baseline achievement levels. Second, using school-level expenditure data, I explore whether school districts strategically allocated the marginal dollar toward schools serving particular student populations. Finally, I examine how the spending changes affected specific inputs to education production, such as class size and teacher salary.

I show that the postsecondary effects were driven by large increases among urban and suburban districts, lower poverty districts, and districts that were higher achieving prior to Proposal A. This heterogeneity stands in contrast to recent studies of school finance reforms (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Schanzenbach forthcoming; and Candelaria and Shores 2015), which found effects primarily among high-poverty districts. I also find that districts allocated the marginal dollar primarily toward schools serving wealthier families within the district. Finally, the spending increases lowered class sizes and the ratio of pupils to administrators, but had little effect on teacher salaries.

Given the debate surrounding the effect of school resources on student outcomes (see Hanushek 2003, Krueger 2003), this paper provides important new evidence

that increases in education expenditures improve the later life outcomes of students. However, as found in other recent studies (e.g., Cascio, Gordon, and Reber 2013), it also provides evidence that local government responses to state or federal education policies can result in benefits accruing to students who may not have been the intended beneficiaries of the policy.

The remainder of this paper is organized as follows. In the next section, I describe Proposal A and summarize the previous literature. In Section II, I describe the data, and in Section III, the methodology. I present the results in Section IV, and Section V concludes.

## I. Background

### A. *School Finance Reform in Michigan*

Prior to 1995, education spending in Michigan was financed primarily through local property taxes. There was essentially no limit on the amount of revenue that a district could raise locally, and consequently education spending across the state was highly unequal. Due to this inequality, and to public outcry over an increasing property tax burden, in July 1993 the Michigan state legislature abolished local school property taxes beginning in 1995.[1] In response, voters passed Proposal A, which relied on state rather than local sources of revenue to finance education funding in Michigan.[2]

Proposal A changed the school funding formula. Each district was assigned a per pupil spending amount known as a foundation allowance. Districts were not allowed to spend less than the allowance on per pupil expenditures and, with few exceptions, were not allowed to raise funds locally to spend more. Proposal A equalized funding across districts in its first year, 1995, because each district's allowance was larger than the district's revenue from state and local sources during 1994 by an amount inversely related to its 1994 revenue. In 1995, allowances were calculated using the formula:

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

where *Allow* is the per pupil foundation allowance and $x$ is 1994 revenue.

Proposal A also set into motion a time path of allowances that was further equalizing and provides the plausibly exogenous variation in spending that I exploit. Figure 1, panel A, illustrates how the time path of the allowance varied by a district's

[1] Here and throughout the paper, I refer to a school year by its spring year, i.e., 1995 refers to the 1994–1995 school year.

[2] For a thorough review of Michigan education finance and Proposal A, please see Courant and Loeb (1997) or Cullen and Loeb (2004).

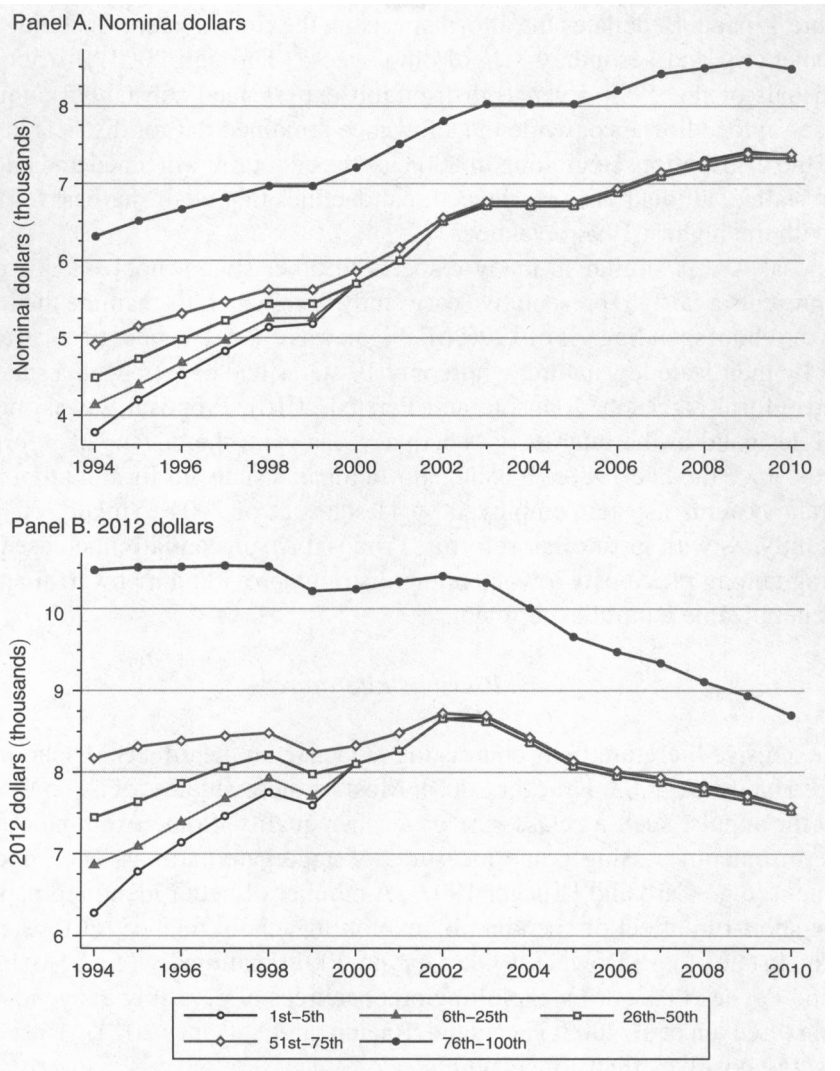


FIGURE 1. FOUNDATION ALLOWANCE OVER TIME BY 1994 REVENUE PERCENTILE

*Notes:* Figures show the average foundation allowance over time for districts grouped by 1994 revenue percentiles. Panel A uses current nominal dollars and panel B uses real 2012 dollars deflated using the Employment Cost Index for elementary and secondary school employees provided by the Bureau of Labor Statistics. The 1994 value (pre-Proposal A) is the district's 1994 revenue.

1994 revenue. The allowance is plotted over time (in nominal dollars), grouping districts by percentiles of the 1994 revenue distribution. The figure shows how the allowance was designed to equalize school funding through the early 2000s by boosting funding in initially low-spending districts without reducing the funding of initially high-spending districts.[3]

[3] Online Appendix Table 1 and online Appendix Figure I provide additional information about the allowance and how it varies across districts and over time.

Figure 1, panel B, deflates the allowance using the Employment Cost Index (ECI) for elementary and secondary school employees.[4] Through 2002, districts in the bottom half of the 1994 revenue distribution experienced substantial annual real increases in the allowance, while the allowance remained flat for districts in the top half of the distribution. Beginning in 2003, as the economy worsened and allowance growth stalled, all districts experienced real declines that were sharpest for the districts with the highest 1994 revenue.

Proposal A was similar in many respects to other state school finance reforms and represents a fairly representative case study with which to examine the long-run effects of school spending. As of 1996, Michigan was one of 31 states that had a reform passed by their state legislature, while only 19 states had experienced a state court-ordered reform (Jackson, Johnson, and Persico 2016). Proposal A was motivated by and designed in the interest of "adequacy," as were the dozens of reforms that occurred since the late 1980s. It changed Michigan's state aid formula to a foundation grant system, a system employed by 41 states as of 2004 (Yinger 2004). Most importantly, as with most other reforms, Proposal A substantially increased school spending among previously low-spending districts, providing a powerful and arguably generalizable natural experiment.

### B. *Previous Literature*

An extensive literature finds contrasting evidence on the effects of school spending (see Hanushek 2003, Krueger 2003). Most of these studies focus on the effects of specific inputs, such as class size or teacher quality. Some examine effects on long-term outcomes using crude measures of aggregate earnings and educational attainment (e.g., Card and Krueger 1992). A number of better identified papers isolate the short-run effect of spending by exploiting school finance reforms either in individual states (e.g., Downes 1992, Guryan 2001), nationally (e.g., Hoxby 2001, Card and Payne 2002), or by exploiting other sources of plausibly exogenous variation (e.g., Leuven et al. 2007; Hægeland, Raaum, and Salvanes 2012). Most of these studies find positive effects of spending.

Most relevant to the present paper are the studies that examine the short-run and medium-run effects of Proposal A, and two recent papers that examine the long-run effects of school finance reforms nationwide on educational attainment. The papers examining Proposal A find positive effects on fourth grade test scores, but no effects on seventh grade scores or on school-level ACT-taking or SAT-taking rates or scores (Papke 2005, 2008; Chaudhary 2009; and Roy 2011).

The first paper examining students' long-run outcomes is Jackson, Johnson, and Persico (2016), which uses the Panel Survey of Income Dynamics (PSID) and exploits the timing of statewide school finance reforms. The authors find that increased spending during childhood leads to more years of completed schooling and higher earnings during adulthood. Candelaria and Shores (2015) use a similar

[4] I use the Employment Cost Index (ECI) as opposed to the Consumer Price Index (CPI), because the ECI more accurately captures changes in the purchasing power of school districts. Online Appendix Figure II shows the time path of the allowance deflated using the CPI.

strategy and the National Center for Education Statistics (NCES) Common Core of Data (CCD) to show that the reforms increased high school graduation rates among high-poverty districts. In Section IVB, I compare the magnitude of my results to those in Jackson, Johnson, and Persico (2016) and Candelaria and Shores (2015).[5]

## II. Data

I use an original dataset containing six cohorts of first-time fourth grade students in Michigan public, non-charter schools between 1995 and 2000 matched to their postsecondary outcomes. Individual test-taking records from the Michigan statewide testing system allow me to identify the universe of students and where they are enrolled. The fourth grade test is the first that all students are required to take, and a comparison of these microdata with publicly available aggregate fourth grade head counts shows very similar total numbers.

The two key outcomes of interest are whether students enroll in postsecondary school within 10 years after fourth grade and earn a postsecondary degree within 14 years. Postsecondary enrollment and degree receipt information was obtained by matching students to the National Student Clearinghouse (NSC), a nonprofit organization that houses such information on over 90 percent of undergraduate students nationwide (Dynarski, Hemelt, and Hyman 2015).

Based on where and when students were enrolled in primary school, I merged in school-level and district-level expenditure data obtained from Michigan's Center for Educational Performance and Information, as well as the foundation allowance and 1994 district revenue information obtained from the Michigan Senate Fiscal Agency. The expenditure data contain information on spending by category, such as instruction, administration, and operations and maintenance. The school-level expenditure data omit any district-level spending categories, such as district administration and central business office.

The Michigan microdata contain several variables that are used as controls in the analysis: time-invariant student demographic information such as sex and race, as well as time-varying characteristics such as free and reduced-price lunch, limited English proficiency (LEP), and special education (SPED) status.[6] The microdata also contain scores on state assessments during grades 4, 7, and 11, as well as high school graduation status.

Finally, I obtained several district-level variables measuring school choice participation (e.g., the percentage of students living in the district who attend a charter school), demographics (e.g., population density), and economic conditions (e.g., local median household income), which are also included as controls.[7] See online Appendix 1 for a more thorough data description.

[5] Lafortune, Rothstein, and Schanzenbach (forthcoming) is less relevant to my study as it examines the long-run effects of school finance reforms on achievement rather than attainment. They find that the reforms led to sustained increases in achievement among low-income districts.

[6] I measure these time-varying characteristics during grade 12, because they are first available in 2003, the year the first cohort reached 12th grade.

[7] The full list of variables includes: percent of students living in the district who attend a charter school; percent of students living in the district who use interdistrict school choice to attend a traditional public school in another district; percent of students attending a traditional public school in the district who live in another district

TABLE 1—SAMPLE MEANS OF MICHIGAN FOURTH GRADE COHORTS

| | All districts and cohorts (1) | All cohorts, by 1994 district revenue | | All districts | |
|---|---|---|---|---|---|
| | | Bottom half (2) | Top half (3) | 1995 cohort (4) | 2000 cohort (5) |
| Demographics | | | | | |
| Female | 0.489 | 0.488 | 0.490 | 0.493 | 0.491 |
| White | 0.741 | 0.907 | 0.671 | 0.757 | 0.726 |
| Black | 0.179 | 0.028 | 0.242 | 0.162 | 0.199 |
| Hispanic | 0.030 | 0.025 | 0.032 | 0.026 | 0.032 |
| Other race | 0.039 | 0.031 | 0.042 | 0.042 | 0.034 |
| Free or reduced-price lunch | 0.214 | 0.220 | 0.212 | 0.153 | 0.276 |
| Limited English | 0.007 | 0.003 | 0.009 | 0.003 | 0.009 |
| Special education | 0.098 | 0.094 | 0.100 | 0.076 | 0.103 |
| Status as of grade 12 | | | | | |
| Observed in data | 0.725 | 0.757 | 0.711 | 0.707 | 0.739 |
| Observed in data in grade four district | 0.549 | 0.616 | 0.521 | 0.539 | 0.555 |
| Educational attainment | | | | | |
| Graduates high school | 0.827 | 0.841 | 0.821 | 0.823 | 0.828 |
| Enrolls in postsecondary school | 0.448 | 0.431 | 0.455 | 0.440 | 0.469 |
| Earns postsecondary degree | 0.201 | 0.192 | 0.205 | 0.207 | 0.197 |
| Average in grades four through seven: | | | | | |
| Foundation allowance (2012$) | 9,078 | 8,015 | 9,524 | 9,009 | 9,175 |
| Operating expenditure (2012$) | 9,797 | 8,418 | 10,375 | 9,432 | 10,158 |
| During fourth grade year, district-level: | | | | | |
| Percent attending charter | 1.15 | 0.40 | 1.46 | 0.00 | 2.72 |
| Percent attending school outside of home district | 1.65 | 1.98 | 1.50 | 0.00 | 3.07 |
| Population per square mile | 215 | 28 | 294 | 211 | 213 |
| Local unemployment rate | 4.9 | 5.3 | 4.8 | 6.1 | 3.6 |
| Local median household income (2012$) | 60,537 | 54,456 | 63,088 | 58,828 | 61,697 |
| Urbanicity during fourth grade | | | | | |
| Urban | 0.219 | 0.013 | 0.305 | 0.210 | 0.223 |
| Suburban | 0.452 | 0.229 | 0.546 | 0.453 | 0.453 |
| Rural | 0.329 | 0.759 | 0.148 | 0.337 | 0.324 |
| Number of districts | 518 | 259 | 259 | 518 | 518 |
| Number of students | 746,834 | 220,720 | 526,114 | 119,991 | 129,576 |

*Notes:* The sample is all first-time fourth-graders in Michigan public (non-charter) schools during 1994–1995 through 1999–2000. Free lunch, special education, and limited English proficiency status are measured during grade 12. College enrollment and degree receipt include any postsecondary institution and are measured within two and six years, respectively, after scheduled on-time high school graduation based on fourth grade cohort year.

Table 1 reports sample means for the 746,834 students and 518 districts in the sample. Eighteen percent of the sample is black (column 1), although this percentage differs dramatically for districts in the bottom half and top half of the 1994 revenue distribution (3 percent, column 2 versus 24 percent, column 3, respectively). This heterogeneity reflects the fact that districts with low 1994 revenue are primarily rural areas, towns, and smaller cities, whereas districts with high 1994 revenue are

(i.e., gains from interdistrict choice); number of charter schools located in the district; number of charter schools located in the district and adjoining districts; population per square mile in the district (i.e., population density); fraction of 5–17 year olds living in poverty in the district; local median household income (in 2012 dollars); fraction of students attending school in the district who are black; fraction of students attending school in the district who are eligible for free lunch; and local average unemployment rate.

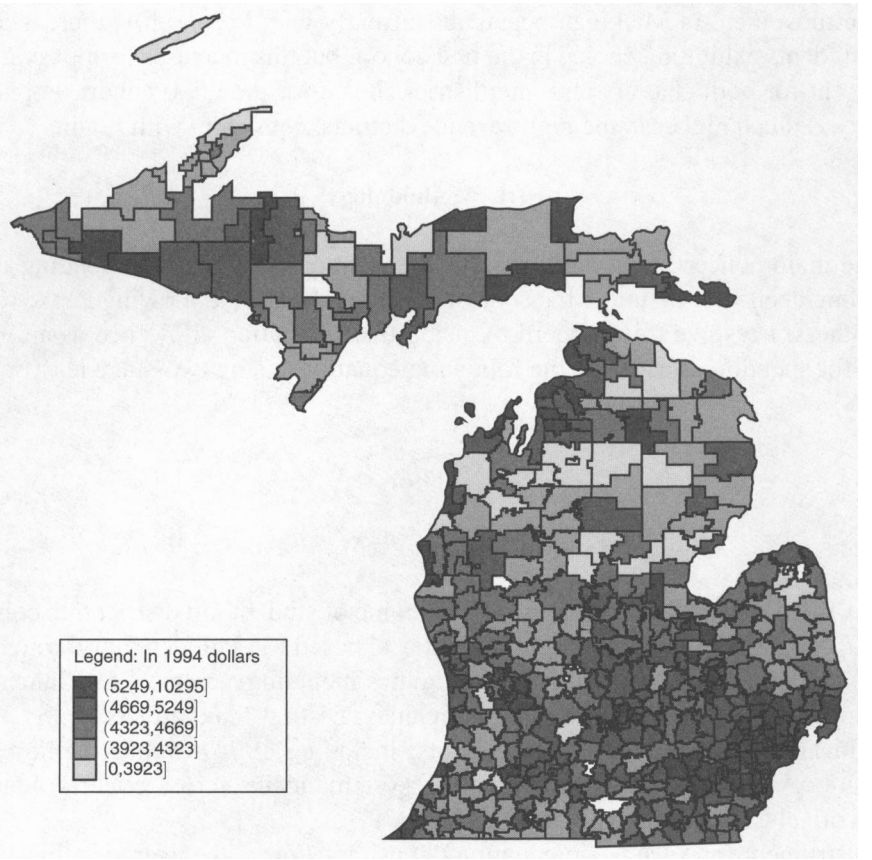


FIGURE 2. 1994 REVENUE BY SCHOOL DISTRICT

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

primarily larger cities. Figure 2, which shows a map of Michigan districts shaded to reflect their 1994 revenue, also illustrates this pattern. The shades reflect the percentile groupings used in Figure 1 (e.g., first through fifth, sixth through twenty-fifth), and the darker shades represent higher 1994 revenue.

The fraction of students on free lunch increased over the sample period, from 15 to 28 percent. Part of this increase was due to out-of-state migration of primarily nonpoor households, a phenomenon also reflected in the data through attrition: only 73 percent of the sample remained as of grade 12. A student who attrits prior to high school graduation—due to out-of-state migration or enrolling in a private school, for example—is indistinguishable from a student who drops out. Fortunately, I submit all students to the NSC, so that the postsecondary outcomes do not suffer from potentially endogenous attrition. Given the imperfect measure of high school graduation, I prefer the postsecondary outcomes, and they are the focus of the analysis.

Table 1 also reports sample means for several district-level school choice, economic, and demographic characteristics during students' fourth grade year. The school

choice movement in Michigan began concurrently with Proposal A; thus, there are zero students exhibiting choice in the first cohort, but this increases to approximately 3 percent for both charters and interdistrict choice for the 2000 cohort. Population density is much higher in the high-revenue districts, consistent with Figure 2.

## III. Methodology

The main concern with a regression of education outcomes on spending is that spending decisions are under the control of school districts. Following Papke (2005) and others, I resolve this concern by using the foundation allowance as an instrument for spending. I estimate the following equations using two-stage least squares (2SLS):

$$(1) \qquad Y_{idc} = \beta_0 + \beta_1 \widehat{Spend}_{idc} + X_i + \alpha_d + \gamma_c + \epsilon_{idc}$$

$$(2) \qquad Spend_{idc} = \delta_0 + \delta_1 Allow_{dc} + X_i + \lambda_d + \pi_c + \mu_{idc},$$

where $Y_{idc}$ is an educational attainment outcome of student $i$ in district $d$ in cohort $c$, $Spend_{idc}$ is the average spending a student is exposed to in grades four through seven,[8] and $X$ is a vector of student demographics including sex, race, free lunch, special education, and limited English proficiency. District fixed effects ($\lambda$, $\alpha$) absorb time-invariant differences across districts, including 1994 revenue. Cohort fixed effects ($\pi$, $\gamma$) absorb any factors varying systematically across cohorts. Standard errors are clustered at the district level.

I instrument for $Spend_{idc}$ in equation (1) using $Allow_{dc}$, the average allowance in students' fourth grade district during fourth grade and the three subsequent years, regardless of whether students change districts. Both the 1995 level and subsequent trajectory of the allowance are beyond districts' control: they are a function of districts' 1994 revenue and growth in the state economy. The identifying assumption is that changes in the allowance are uncorrelated with changes in time-varying, unobserved characteristics related to educational attainment.

I focus on spending and allowance between grades four and seven for two primary reasons. First, I do not observe where a student is enrolled prior to grade four. Second, there is little identifying variation in the allowance after 2003,[9] by which time the most recent fourth grade cohort reaches grade seven. To ensure consistency across cohorts, I restrict to grades seven and below.

Student-level panel data allow me to improve on specifications used in previous studies that rely on district-level data in two ways. First, I can control for student characteristics, $X_i$. Second, by tracking students across grades and districts, I am able to more accurately associate an observed outcome with the spending that affected it. Within-state mobility is high: 45 percent of fourth grade students are

[8] All spending and allowance amounts are expressed in thousands of 2012 dollars. I measure spending in levels, not logs, to avoid the assumption that a dollar of spending has less effect for a high-spending than a low-spending district. The results are similar using logged spending (see online Appendix Table 9).

[9] I show this explicitly in Section IVA.

enrolled in 12th grade outside their fourth grade district. Previous estimates of the effects of school finance reform relied on district-level contemporaneous achievement measures and lagged spending measures. Those estimates assumed that the students who contributed to the achievement measure are the same students exposed to spending in the district several years earlier. This is clearly not so, and the sign of any resulting bias is unclear.[10]

I further control for the rich set of district-level school choice, economic, and demographic characteristics listed in Section II. Education outcomes in districts with low 1994 revenue (i.e., those experiencing larger allowance increases) may have been trending differentially relative to districts with high 1994 revenue. This period witnessed economic improvement, demographic changes, and the rise of school choice in Michigan, all of which may have differentially affected outcomes in districts with low and high 1994 revenue. I collect several locally measured variables that comprehensively control for these specific factors.[11]

I include each of these covariates separately for the years that each cohort was in grade four, five, six, and seven (i.e., four variables for each characteristic).[12] I also include quadratic cohort trends interacted with the 1995 values of these characteristics. These interactions allow for differential trending of the outcome variable by districts with different baseline values of these covariates.[13] Ideally, I would additionally control for each student's achievement level prior to Proposal A. Unfortunately, the first test score I observe is during fourth grade, and for cohorts after Proposal A these scores may have been affected by the policy. Thus, I exclude fourth grade scores from my preferred specification and include them only as a robustness check.

## IV. Results

### A. *First Stage: Effects of the Allowance on Spending*

In this section, I examine to what degree the increases in the foundation allowance increased spending, and whether they led to spending equalization across districts. Figure 3, panel A, shows average per pupil operating expenditures over time in 2012 dollars for districts grouped by 1994 revenue percentile.[14] As in Figure 1, panel B, the lower spending districts experienced complete spending equalization by 2003.

[10] For example, random mobility would result in attenuation bias, but mobility of high-achieving students into districts with increased spending would bias results upward.

[11] Previous studies omit these controls. In online Appendix 2, I replicate Papke (2008) and show that the main results in that study are partially due to these omitted factors.

[12] Theoretically the spending changes could lead to families moving across districts, thus affecting these district characteristics. However, several studies show that there was no major resorting across districts or changes in district demographics in Michigan due to Proposal A (Epple and Ferreyra 2008, Courant and Loeb 1997). Chakrabarti and Roy (2015) find resorting led to changes in district characteristics, but these effects were minor compared to the demographic and economic changes Michigan experienced during this period.

[13] Including a district-specific linear cohort trend vastly reduces first stage power, and nearly triples the size of the standard errors in my main results. This suggests that the variation in the allowance that I exploit is close to a linear trend for each district.

[14] Throughout the paper I use operating as opposed to total expenditure because it is the spending measure directly affected by the allowance and under the purview of Proposal A. As a robustness check in online Appendix Table 9, I report all estimates using total expenditures, revealing a pattern of results identical to that produced using operating expenditures.

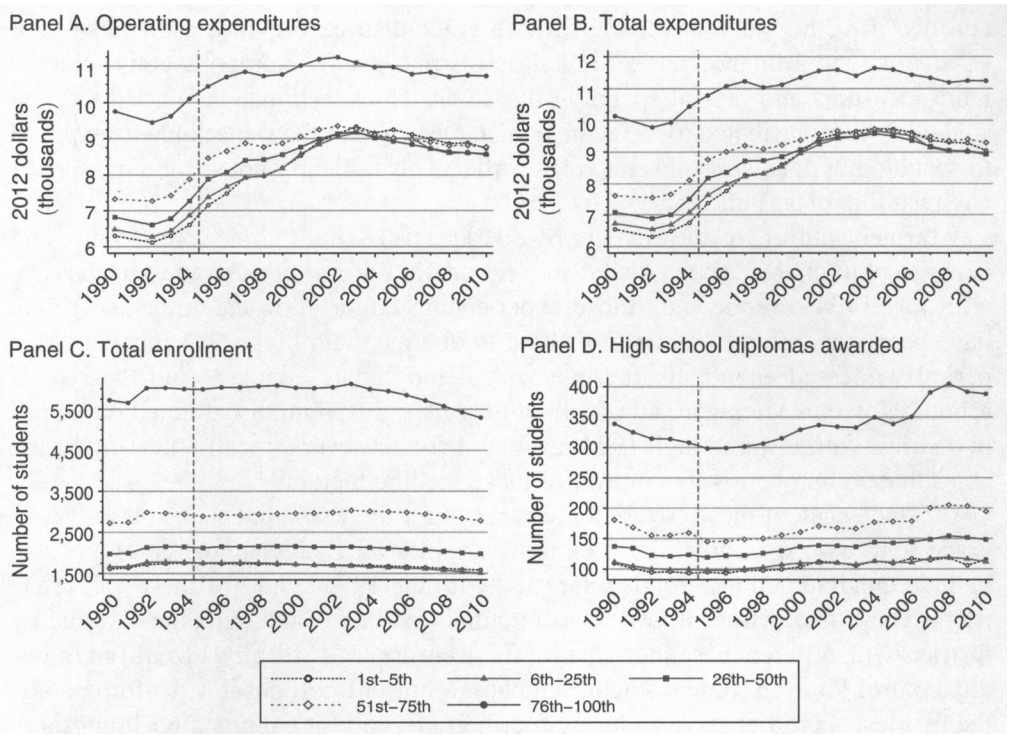


FIGURE 3. PER PUPIL EXPENDITURES, ENROLLMENT, AND HIGH SCHOOL DIPLOMAS BY 1994 REVENUE PERCENTILE

*Notes:* Figure plots district (panel A) average per pupil operating expenditures, (panel B) total expenditures, (panel C) total enrollment, and (panel D) number of high school diplomas awarded over time by 1994 revenue percentiles. Expenditures are in 2012 dollars deflated using the Employment Cost Index for Elementary and Secondary School Employees provided by the Bureau of Labor Statistics. The dashed vertical line marks the passage of Proposal A.

The only stark difference between the evolution of the allowance and of spending is that districts' spending did not decrease as much as their allowance after 2003.

In Table 2, I examine the relationship between the allowance and spending more formally by regressing district-year-level spending on the district-year-level allowance. This represents the first stage relationship for the IV analysis presented in Section IVB. Controlling only for year fixed effects and district 1994 revenue, a dollar increase in the allowance during 1995 to 2010 leads to a 60 cent increase in operating expenditures (column 1). Adding district fixed effects and covariates, and weighting districts by their enrollment to ensure that the estimates reflect a representative sample of the student population in Michigan, the point estimate increases slightly to 66 cents (column 4). These results are on the high end of flypaper effects estimated in previous studies of the effects of state aid to school districts (Hines and Thaler 1995).

Next, I split this panel of districts into two periods: 1995–2003, the period in which the allowance grew at a faster rate for initially low-spending districts, and 2004–2010, the period in which the allowance was no longer equalizing in nominal terms. The relationship between the allowance and operating expenditures was driven by the early period (Table 2, column 5), with a point estimate of 67 cents. The effect

TABLE 2—FIRST STAGE: THE EFFECT OF THE FOUNDATION ALLOWANCE ON OPERATING EXPENDITURES

| | All years, 1995–2010 | | | | 1995–2003 | 2004–2010 |
|---|---|---|---|---|---|---|
| Dependent variable: | (1) | (2) | (3) | (4) | (5) | (6) |
| Operating expenditure | 0.600 | 0.417 | 0.330 | 0.660 | 0.670 | 0.391 |
| | (0.078) | (0.117) | (0.132) | (0.063) | (0.055) | (0.205) |
| Mean dep. var. (2012$) | 9,247 | 9,247 | 9,247 | 9,247 | 9,185 | 9,326 |
| Observations (district-years) | 8,280 | 8,280 | 8,280 | 8,280 | 4,660 | 3,620 |
| Control for 1994 revenue | Yes | No | No | No | No | No |
| District fixed effects | No | Yes | Yes | Yes | Yes | Yes |
| District-year covariates | No | No | Yes | Yes | Yes | Yes |
| Student weighted | No | No | No | Yes | Yes | Yes |

*Notes:* The sample is at the district-year level from 1995 through 2010 and includes the 518 public, non-charter school districts in Michigan that existed in 1994. Each coefficient is from a separate regression of operating expenditures on the foundation allowance, where both are in 2012 dollars (in levels). All specifications include year fixed effects. Standard errors, in parentheses, are clustered at the district level.

of the allowance on expenditures in the later period is smaller (Table 2, column 6), at 39 cents, and less statistically precise. This analysis confirms that the identifying variation in the allowance, and the strong first-stage relationship between the allowance and spending, is driven by the first part of the sample period. Thus, I use this early period to examine the effects of allowance-induced spending in Section IVB.

In addition to examining how much the allowance increased spending for these districts, it is also of interest to examine how each additional dollar was spent. Doing so provides a more thorough understanding of the first-stage relationship between the allowance and spending, and the mechanisms through which changes in the allowance may have led to changes in student outcomes.

Table 3, column 1, row 1, shows the overall effect of the allowance on operating expenditures in the early period (67 cents). Rows 2 and 3 split operating expenditures into instructional and non-instructional expenditures. A dollar increase in the allowance led to a 33 cent increase in instructional expenditures, and a 34 cent increase in noninstructional expenditures. To test whether the marginal allowance dollar was spent differently than the average dollar, I report the fraction of total operating expenditures comprised by each spending category (column 3). I then test whether the percent of the marginal dollar spent on each category (column 1/0.670) is equal to the percent of the average dollar spent on that category (column 3), and report $p$-values from that statistical test in column 4.

I find that 50 percent (= 0.337/0.670) of the marginal dollar was spent on noninstruction compared to 36 percent of the average dollar ($p$-value of 0.006). When I split noninstruction into instructional support (e.g., guidance counselors, curriculum specialists), administration (e.g., superintendent, principals, central business office), operations and maintenance, and transportation, I find that the marginal dollar was spent disproportionally toward administration and toward operations and maintenance.[15]

[15]Further breaking administration into school administration (i.e., principals), district administration (i.e., superintendent), and central business office, reveals increases across all three categories (see online Appendix Table 2).

TABLE 3—FIRST STAGE: HOW DO DISTRICTS SPEND THE ADDITIONAL DOLLAR OF ALLOWANCE?

| Dependent variable: | Absolute effect (1) | Mean (2012$) (2) | Fraction of operating expenditure (3) | $p$-value: (col 1/0.670) = col 3 (4) |
|---|---|---|---|---|
| Operating expenditure | 0.670 | 9,185 | 1.000 | 0.999 |
| | (0.055) | | | |
| Instruction | 0.333 | 5,862 | 0.638 | 0.009 |
| | (0.036) | | | |
| Noninstruction | 0.337 | 3,322 | 0.362 | 0.006 |
| | (0.034) | | | |
| Instructional support | 0.069 | 716 | 0.078 | 0.490 |
| | (0.024) | | | |
| Administration | 0.130 | 1,191 | 0.130 | 0.023 |
| | (0.019) | | | |
| Operations and maintenance | 0.122 | 977 | 0.106 | 0.006 |
| | (0.018) | | | |
| Transportation | 0.016 | 438 | 0.048 | 0.025 |
| | (0.007) | | | |

*Notes:* The sample is at the district-year level as in Table 2, but restricted to years 1995–2003. Each coefficient in column 1 is from a separate regression of the amount spent in the operating expenditure category on the foundation allowance, where both are in 2012 dollars (in levels). The $p$-values in column 4 are from a test of whether the column 1 coefficient divided by 0.670 equals the fraction of operating expenditure accounted for by that category (column 3). All regressions are student-weighted and contain district and year fixed effects and district-year covariaties. Standard errors, in parentheses, are clustered at the district level.

One view of these results is that when districts are constrained, they focus spending on teachers. When their budget constraint is loosened, they can supplement the budgets of their administrators, business office, and facility maintenance—potentially important noninstructional resources.

### B. *Educational Attainment*

In this section, I explore how districts' allowance-induced spending increases affected students' educational attainment.[16]

Table 4 reports results from estimating equations (1) and (2), where the outcome variables are indicators for whether a student enrolls in college (row 1) and earns a degree (row 2). Controlling for district and cohort fixed effects and student demographics, there is a small and statistically insignificant effect of spending on postsecondary enrollment (column 1). Controlling for district-level measures of school choice (column 2) increases the point estimate to 4.0 percentage points (standard error of 0.9 points). This jump in the point estimate is consistent with charter school growth in high-1994-revenue districts causing increased postsecondary enrollment that masked the enrollment gains observed in the low-1994-revenue districts due to the increased spending.[17] Adding the economic and demographic district-level

[16] Effects on student achievement, including a replication of Papke (2008), are presented in online Appendix 2. I report reduced-form effects of the allowance on postsecondary enrollment and degree receipt in online Appendix Table 6.

[17] Consistent with this story, the entire increase in the point estimate moving from column 1 to 2 is driven by the measures of charter school choice, the form of choice that emerged in districts with high 1994 revenue. Including

TABLE 4—THE EFFECT OF SPENDING ON EDUCATIONAL ATTAINMENT

| | Not adjusted for attrition | | | | | Adjusted for attrition | |
|---|---|---|---|---|---|---|---|
| Dependent variable: | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Enroll in postsecondary schooling | 0.014 | 0.040 | 0.031 | 0.030 | 0.033 | 0.036 | 0.041 |
| (*Mean* = 0.448) | (0.018) | (0.009) | (0.011) | (0.014) | (0.015) | (0.017) | (0.018) |
| Earn a postsecondary degree | 0.028 | 0.030 | 0.029 | 0.023 | 0.028 | 0.028 | 0.034 |
| (*Mean* = 0.201) | (0.009) | (0.009) | (0.011) | (0.013) | (0.015) | (0.016) | (0.018) |
| Mean spending (2012$) | | | 9,797 | | | | |
| First stage *F*-statistic | 132 | 164 | 161 | 98 | 98 | 70 | 70 |
| District and cohort fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Student demographics | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *District-cohort covariates:* | | | | | | | |
| School choice | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic and demographic | No | No | Yes | Yes | Yes | Yes | Yes |
| Trend × district-cohort covariates | No | No | No | Yes | Yes | Yes | Yes |
| Student fourth grade scores | No | No | No | No | Yes | No | Yes |

*Notes:* The sample is all first-time fourth-graders in Michigan public (non-charter) schools during 1994–1995 through 1999–2000 ($N = 746{,}834$). See text for attrition adjustment used in columns 6 and 7. Each coefficient is from a separate 2SLS regression of the dependent variable on average real spending during grades four through seven (in thousands of 2012 dollars). The instrument is the average allowance during grades four through seven (also in thousands of 2012 dollars). Standard errors, in parentheses, are clustered at the district level.

School choice district-cohort covariates: percent of students living in the district who attend a charter school; percent of students living in the district who use interdistrict school choice to attend a traditional public school in another district; percent of students attending a traditional public school in the district who live in another district (i.e., gains from interdistrict choice); number of charter schools located in the district; number of charter schools located in the district and adjoining districts.

Economic and demographic district-cohort covariates: population per square mile in the district (i.e., population density); fraction of 5–17-year-olds living in poverty in the district; local median household income (in 2012 dollars); fraction of students attending school in the district who are black; fraction of students attending school in the district who are eligible for free lunch; and local average unemployment rate.

covariates (Table 4, column 3) attenuates the point estimate to 3.1 points (standard error of 1.1). My preferred specification adds the quadratic cohort trend interacted with the covariates (column 4), reducing statistical precision (standard error becomes 1.4 points) but hardly affecting the point estimate, now 3.0.[18]

The interpretation of this 3.0 percentage point effect is that $1,000 of additional spending during each of grades four through seven led to a 3.0 percentage point increase in the probability that a student enrolled in postsecondary school. This represents a 10.2 percent increase in spending during those grades given mean spending of $9,797, and a 6.7 percent increase in enrollment given mean enrollment of 44.8 percent. As a robustness check, I add individual-level fourth grade scores as a control (Table 4, column 5); while they are an endogenous control and the point estimate is thus difficult to interpret, it is worth noting from a statistical perspective that this does little to change the point estimate.

measures of interdistrict choice, the form of choice that emerged in districts with low 1994 revenue, does not affect the point estimate.

[18]The first stage *F*-statistics range between 98 and 164 across the specifications, far surpassing traditional thresholds for weak instruments (Staiger and Stock 1997).

Turning to postsecondary degree receipt, there was a 2.8 percentage point effect of spending on degree receipt (standard error of 0.9 points), estimated with only the fixed effects and student demographics. My preferred specification adding in the district-level covariates and trend interactions attenuates the coefficient to 2.3 points. This effect is statistically significant at the 10 percent level and represents an 11.4 percent increase in degree receipt.

The larger percent increase in degree receipt than enrollment suggests that many of the students induced into college by the additional spending persisted to completion, but also that the additional spending boosted the graduation rate for students who would have enrolled in the absence of the spending increase. I calculate a lower bound on this increased graduation rate. I do so by assuming an upper bound for the graduation rate of the students induced into college by the additional spending equal to the sample mean rate of 0.45.[19] Multiplying this rate by the 3.0 percentage point enrollment effect yields 1.35 percentage points. This is an upper bound for the portion of the 2.3 percentage point degree receipt effect due to new enrollees earning degrees. Subtracting 1.35 from 2.3 yields 0.95 percentage points as a lower bound for the portion of the degree receipt effect due to an increased graduation rate among preexisting enrollees. This represents at least a 4.4 percent increase in the graduation rate among these students.[20]

One possible explanation for this increased graduation rate is that the additional spending appears to have shifted students from two-year to four-year institutions (see online Appendix Table 5), and students have a greater chance of eventually earning a degree if they initially enroll at a four-year institution (Reynolds 2012, Long and Kurlaender 2009).

I present first stage and reduced form versions of my results visually in Figure 4. I regress operating expenditures during grades four through seven (panel A), college enrollment (panel B), and degree receipt (panel C) on all of the controls from column 3 of Table 4, including district and cohort fixed effects. I interact the cohort fixed effects with a dummy for being in a district in the top three quartiles of the 1994 revenue distribution. The coefficients on the cohort fixed effects show how the outcome changes across the cohorts for students in the bottom quartile of districts by 1994 revenue. The coefficients on the interactions show how the outcome changes across the cohorts for students in the high-1994-revenue districts relative to the bottom quartile. I plot these coefficients, adding in the constant and subtracting off the level differences so that the values in 1995 are equal. The figures show that the operating expenditures to which students were exposed increased more for students in the bottom district grouping than for students in the top districts, and that these students experienced a relative increase in their college enrollment and degree receipt.

[19] While presumably the marginal college enrollee should persist to completion at a lower rate than the inframarginal college student, recent studies examining effects of education policies on postsecondary attainment find similar persistence rates across the two groups (Bettinger et al. 2012; Hyman 2017a).

[20] This 4.4 percent is calculated by adding the 0.95 percentage points to the 20.1 percent sample mean for degree receipt, then dividing the resulting 21.05 percent by the sample mean enrollment of 44.8 percent. The result is a graduation rate of 0.47, which is a 2 percentage point or 4.4 percent increase from the sample mean graduation rate of 0.45.

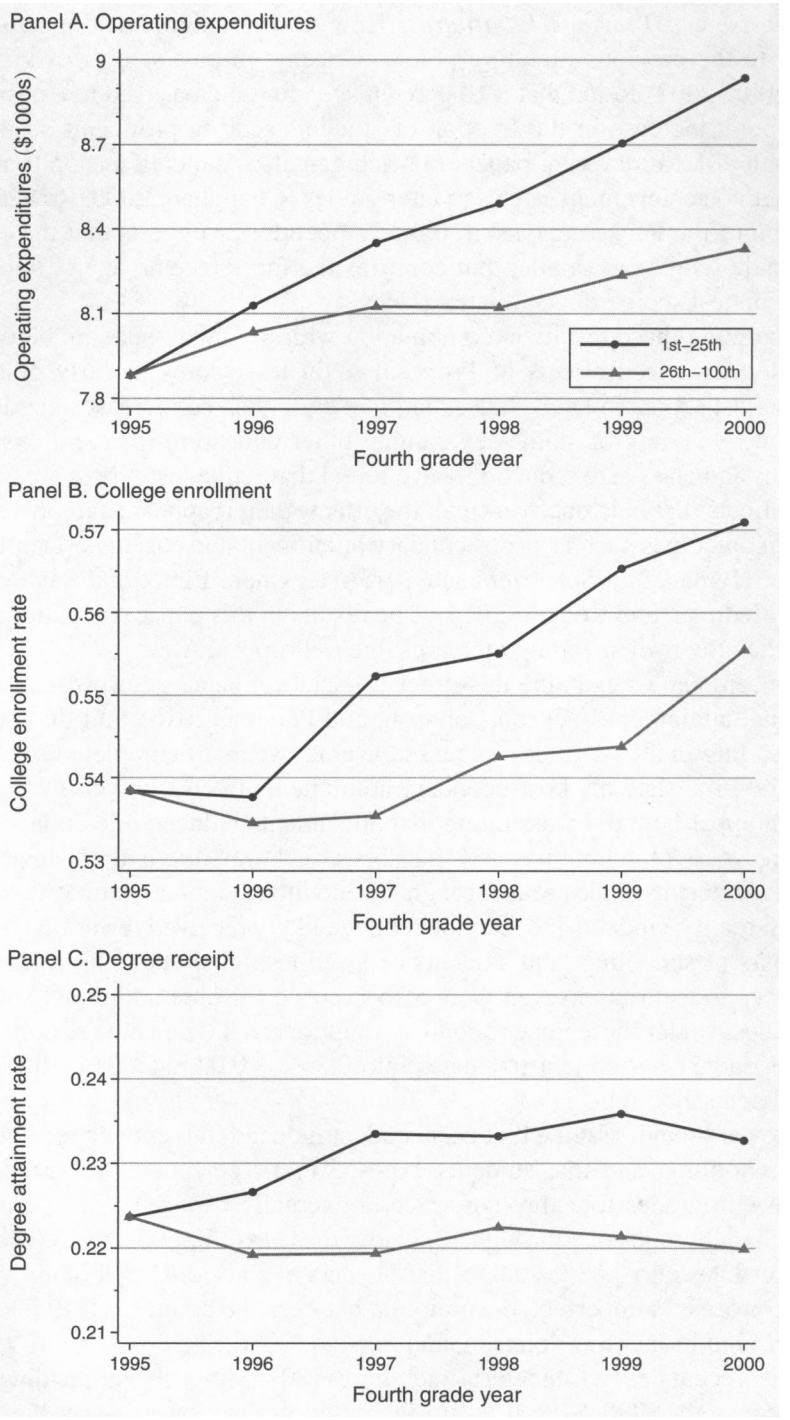


FIGURE 4. PREDICTED SPENDING AND POSTSECONDARY ATTAINMENT BY COHORT

*Notes:* Figure shows predicted (panel A) grade four through seven operating expenditures, (panel B) college enrollment, and (panel C) degree attainment by students' fourth grade cohort year and fourth grade district 1994 revenue percentile grouping. Predictions are from a student-level regression of the outcome on district and cohort fixed effects, the controls from column 3 of Table 4, and interactions of the district groupings and cohort. Figures plot coefficients from these interactions normed relative to the first through twenty-fifth grouping in 1995.

*Comparison to Previous Literature.*—How do the magnitudes of these results compare to the previous literature? First, focusing on past studies of Proposal A, Papke (2005, 2008) found that a 10 percent spending increase led to a nearly 4 percentage point increase in the fraction of students scoring proficient on the fourth grade math test. More recent papers in Michigan also found effects on fourth grade scores, but no achievement effects in later grades (Chaudhary 2009, Roy 2011). My replication of the Papke analysis in online Appendix 2 suggests that the effects on fourth grade scores are smaller, but confirms the more recent papers' findings that there are no test score effects in later grades.

Summarizing these results in combination with my own, there are between zero and moderately sized effects of Proposal A on test scores in early grades, zero effects on test scores in later grades, and moderately sized increases in educational attainment. A variety of studies examining other education interventions, such as Head Start and class-size reduction, have found that while test scores are unaffected or have effects that fade out over time, the effects then reappear as improvements in long-term outcomes such as postsecondary attainment and earnings (Deming 2009; Dynarski, Hyman, and Schanzenbach 2013; Heckman, Pinto, and Savelyev 2013; Chetty, Friedman, and Rockoff 2014). The results in this paper reveal the same pattern for the effects of spending increases due to Proposal A.

Two recent papers examine the effects of school finance reforms nationwide on educational attainment. Jackson, Johnson, and Persico (2016) find that 10 percent more spending in all 12 grades increased average years of completed schooling by 0.31 years. I translate my postsecondary attainment effects into years of schooling, first as an upper bound, by assuming that all students induced into college received four more years of schooling, and that this effect was due entirely to the spending increase during grades four through seven with no increases in lower or higher grades. In reality, students induced into college likely received fewer than four additional years of schooling, and students exposed to higher spending from Proposal A in grades four through seven were also exposed to some additional spending in other grades. Under these upper bound assumptions, a 3.0 percentage point increase in postsecondary enrollment translates into 0.36 ($= 0.03 \times 4 \times 3$) additional years of completed schooling.[21]

As a lower bound, assume that each student induced into college receives 1 extra year of schooling, and that students exposed to 10 percent more spending from Proposal A in grades four through seven are actually exposed to 10 percent more spending in all grades one through 12. Under these assumptions, the 3.0 percentage points translates into 0.03 ($= 0.03 \times 1 \times 1$) years of additional schooling. While this rescaling process is imperfect, the midpoint between the bounds is 0.195, lower than the 0.31 found in Jackson, Johnson, and Persico (2016).

Another recent paper, Candelaria and Shores (2015) finds that a spending increase of approximately \$900–\$1,800 led to increased graduation rates (in high poverty districts) of 5–8 percentage points. While I do not focus on high school graduation

[21] The 0.36 is calculated as follows: multiply the 3 percentage point increase by 4 additional years of schooling, or $0.03 \times 4$, which equals 0.12 years. Then multiply the 0.12 by 3 to get the effect of spending in 12 grades rather than in 4.

as I only observe this outcome for students who stay in Michigan public schools, when I estimate my main analysis with high school graduation as the dependent variable, I find that $1,000 of additional spending leads to a 3–5 percentage point increase in high school graduation rates, which is somewhat lower than the effects in Candelaria and Shores (2015).[22]

*Attrition.*—If students attrit from Michigan public schools (MPS) after grade four, then I do not observe the spending they are exposed to post-attrition. The results I have presented thus far use the average spending that students are exposed to in grades four through seven during the years in which I observe them. If, for example, a student attrits after grade five, then I assign her the average spending that she was exposed to during grades four and five.

This attrition does not bias the results for two reasons: I observe postsecondary outcomes regardless, and I fix the exogenous allowance variable based on students' fourth grade district. This attrition, however, does affect the interpretation of the results. Fourteen percent of students leave MPS prior to grade seven (see online Appendix Table 4 for an analysis of the effects of spending on attrition and mobility). The large majority of these students move out of state.[23] Because the spending that students are exposed to after moving out of state likely differs from the spending that I observed for them prior to moving, my estimates can be interpreted as an intent-to-treat (ITT) effect.

To address attrition, and as an attempt to calculate treatment-on-the-treated (TOT) estimates, I adjust my spending measure for attriters with my best guess of the spending to which they were exposed after leaving MPS. I use migration information from the 2005 American Community Survey to identify individuals who moved out of Michigan in 2004. I observe the Public Use Microdata Area (PUMA) in which the person lived during 2004, and the county in which they live in 2005. For every Michigan PUMA, I create the fraction of the population that moved to each of these non-Michigan counties. I then use school district expenditure data from the CCD during 1995–2010 and a school district to county crosswalk to assign operating expenditures to these non-Michigan counties. I create an average spending measure for each Michigan PUMA and year weighted by the fraction of the population moving to each non-Michigan county. Finally, using a PUMA to school district crosswalk, I assign each Michigan school district and year a measure of predicted spending.

For all attriters, I merge this predicted spending measure onto my student-level data according to the last grade and year in which they were observed. Thus, for a student who attrits after grade five, her new spending variable is the average of her grade four and five observed spending and her grade six and seven predicted spending. Table 4, columns 6 and 7, shows the postsecondary enrollment and degree receipt results adjusted for attrition. The pattern of results is identical, with somewhat larger

[22] I consider students as completing high school if they are observed as graduating in the Michigan microdata, or attrit but appear in the NSC data.

[23] I determine this by examining exit codes that are assigned to students who leave MPS. These codes are only available after 2003, and are missing in many cases, but they show that moving out of state is the largest source of attrition for elementary and middle school students. After grade nine, dropout becomes the leading source.

TABLE 5—EXPLORING MECHANISMS: THE EFFECTS OF SPENDING ON INPUTS TO EDUCATION PRODUCTION

| | | | Pupil/administrator ratio | |
|---|---|---|---|---|
| | Class size (1) | Average teacher salary (2) | School and district admin. (3) | District admin. only (4) |
| Operating expenditure | −1.83 | 1,469 | −9.27 | −92.76 |
| | (0.31) | (1,187) | (9.81) | (47.28) |
| Dependent variable mean | 21.7 | 71,806 | 117 | 468 |
| District and cohort fixed effects | Yes | Yes | Yes | Yes |
| Student demographics | Yes | Yes | Yes | Yes |
| District-cohort covariates | Yes | Yes | Yes | Yes |
| Trend × district-cohort covariates | Yes | Yes | Yes | Yes |
| Student fourth grade scores | No | No | No | No |

*Notes:* The sample is all first-time fourth-graders in Michigan public (non-charter) schools during 1994–1995 through 1999–2000 ($N = 746{,}834$). Each coefficient is from a separate 2SLS regression of the education input on average real spending during grades four through seven (in thousands of 2012 dollars). The instrument is the average allowance during grades four through seven (also in thousands of 2012 dollars). Standard errors, in parentheses, are clustered at the district level.

coefficients. Given the additional assumptions and imperfect nature of the attrition adjustment, I prefer the unadjusted results, keeping in mind their ITT interpretation.

*Mechanisms.*—Given the observed effects of spending on educational attainment, an important and policy-relevant question is whether changes to specific, observed inputs can be credited as the likely source of the postsecondary effects. As observed inputs, I focus on class size, teacher salary, and the ratio of students to school and district administrators (and their staff).

In Table 5, column 1, I find that a $1,000 increase in spending led to a statistically significant 1.8 pupil decrease in the pupil-teacher ratio during grades four through seven (column 1).[24] This effect represents an 8.4 percent decrease in class size relative to the mean pupil-teacher ratio of 21.7. The spending increases led to no statistically significant increase in average teacher salaries, and the magnitude of the coefficient is small (2 percent).

There is a statistically insignificant 8 percent decrease in the ratio of students to school and district administrators (column 3). When I focus on the ratio of pupils to district administrators only, I find a statistically significant 20 percent decrease (column 4). These decreases are consistent with the result in Section IVA that districts spent a large share of the marginal allowance dollar on noninstructional spending and, in particular, on administration.

*Other Robustness.*—While my student microdata and district-level controls are only available beginning in 1994 and 1995, respectively, I plot trends in district-level characteristics available from the CCD prior to Proposal A. Figure 3 plots operating and total expenditures, enrollment, and the number of awarded high school diplomas

[24] All estimates in this table use the preferred specification from Table 4, column 4, controlling for district and cohort fixed effects, student demographics, district-level covariates, and the trend interaction.

over time by 1994 district revenue. Visually, there is little evidence of differential trending by 1994 revenue for any of these characteristics during the five pre-policy years 1990–1994.

I test more formally for pre-trends by regressing the year $t$ minus $t-1$ change in each of these characteristics on 1994 district revenue during 1991 through 1994. I weight by total enrollment and cluster the standard errors by district, as in the district-level analysis presented in Table 3. The coefficient on 1994 revenue is small and statistically insignificant for all four characteristics, providing reassurance that districts' education-related characteristics were not trending differentially by 1994 revenue prior to 1994.

I conduct two additional robustness checks presented in online Appendix Tables 9, 10, and 11. First, to address concerns that the use of operating instead of total expenditures could be driving the results, I report results using total expenditures. The point estimates and standard errors are all similar to those estimated using operating expenditures. Second, I present results using spending during grades four through 12, instead of grades four through seven, in order to explore the sensitivity of the results to the choice of grade span. The point estimates are uniformly larger but not statistically distinguishable from the main estimates.

### C. *Heterogeneity*

It is important to understand whether the effects of spending on educational attainment observed in Section IVB were experienced equally by all types of districts and students. I present results examining heterogeneity by district characteristics in Table 6.[25] Columns 1 and 2 reveal that the postsecondary effects were driven by low-poverty districts (defined as districts with below-median 1995 district-level fraction receiving free lunch), with no effects in high-poverty districts. The point estimates for low-poverty districts are 4.3 percentage points (8 percent) and 3.6 percentages points (13 percent) for enrollment and degree receipt, respectively, compared to 0.003 and −0.005 for high-poverty districts. The $p$-values from tests of equality across groups for the enrollment and degree receipt specifications are 0.161 and 0.081, respectively.

A similar pattern emerges by district prior achievement (defined as above-median 1995 district-level fraction proficient on the grade four exam) in columns 3 and 4 of Table 6, with effects concentrated among high-achieving districts. The differences across groups are less precise, with $p$-values of 0.231 and 0.141 for enrollment and degree receipt, respectively. Although imprecise, these results suggest that while all types of districts received spending increases from Proposal A, the increases translated into changes in educational attainment primarily in the low-poverty and high-achieving districts. It is important to note that this pattern of heterogeneity differs from other studies of school finance reforms that found effects driven by low-income and previously low-achieving districts (e.g., Guryan 2001;

[25] I also examine effects by individual student characteristics (i.e., sex, poverty status, and fourth grade achievement). I do not focus on these results, because they are statistically imprecise, but I present them in online Appendix Table 7.

TABLE 6—THE EFFECT OF SPENDING ON EDUCATIONAL ATTAINMENT, BY DISTRICT CHARACTERISTICS

| | | | | | | | Non-rural | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | High-poverty (1) | Low-poverty (2) | Low grade 4 scores (3) | High grade 4 scores (4) | Rural (5) | Non-rural (6) | Urban (7) | Suburban (8) |
| Enroll in postsecondary schooling | 0.003 | 0.043 | 0.009 | 0.042 | −0.013 | 0.067 | 0.020 | 0.064 |
| | (0.019) | (0.021) | (0.018) | (0.021) | (0.019) | (0.020) | (0.044) | (0.023) |
| | *0.361* | *0.526* | *0.370* | *0.524* | *0.451* | *0.447* | *0.363* | *0.487* |
| Earn a postsecondary degree | −0.005 | 0.036 | 0.001 | 0.035 | −0.016 | 0.051 | 0.034 | 0.056 |
| | (0.014) | (0.018) | (0.013) | (0.020) | (0.014) | (0.018) | (0.018) | (0.019) |
| | *0.119* | *0.273* | *0.128* | *0.272* | *0.205* | *0.199* | *0.116* | *0.239* |
| Mean spending (2012$) | 10,043 | 9,578 | 9,973 | 9,625 | 8,628 | 10,370 | 11,081 | 10,026 |
| Number of districts | 259 | 259 | 259 | 259 | 342 | 177 | 19 | 158 |
| Number of students | 351,913 | 395,069 | 368,550 | 378,284 | 245,471 | 501,176 | 163,465 | 337,711 |
| First stage *F*-statistic | 47 | 79 | 55 | 62 | 108 | 48 | 24 | 45 |
| District and cohort fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Student demographics | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District-cohort covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Trend × district-cohort covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Student fourth grade scores | No | No | No | No | No | No | No | No |

*Notes:* The sample is all first-time fourth-graders in Michigan public (non-charter) schools during 1994–1995 through 1999–2000 ($N = 746{,}834$). Each coefficient is from a separate 2SLS regression of the dependent variable on average real spending during grades four through seven (in thousands of 2012 dollars). The instrument is the average allowance during grades four through seven (also in thousands of 2012 dollars). Standard errors, in parentheses, are clustered at the district level. Means of the dependent variable are in italics below the standard errors.

Jackson, Johnson, and Persico 2016; and Lafortune, Rothstein, and Schanzenbach forthcoming).

I present effects by urbanicity in Table 6, columns 5 and 6. Despite rural areas having received the largest spending increases from Proposal A, those increases did not translate into changes in educational attainment. The effects are concentrated among non-rural (i.e., urban and suburban) districts, with *p*-values from tests of equality across the groups of less than 0.01 for both enrollment and degree receipt. Separating out non-rural districts by urban and suburban status shows somewhat larger effects among suburban districts, but I cannot reject equality across the groups (*p*-values of approximately 0.4).

This heterogeneity by district characteristics could either be mitigated or exacerbated if districts targeted the marginal dollar toward schools serving particular student populations. Education funds are received at the district level but then allocated to individual schools to, for example, hire a new teacher. I use original Michigan school-level expenditure data to provide, to my knowledge, the first plausibly-causal estimates of how school districts allocate a windfall of general purpose funds across schools. In addition to explaining heterogeneity in the effects of spending on educational attainment, this analysis is interesting in its own right because it examines the behavioral responses of districts to increases in revenue from higher levels of government.

The effect of a dollar increase in the allowance on school-level operating expenditures was 69 cents (Table 7, column 1).[26] In columns 2–4, I report results by

[26] Similar to Table 2, I weight by the number of students enrolled in the school-year, and include district and year fixed effects and the vector of district-level covariates.

TABLE 7—DO DISTRICTS SPEND THE ADDITIONAL DOLLAR MORE AT CERTAIN TYPES OF SCHOOLS?

| | | By level | | | Title I status | | Within district poverty | | Within district fraction proficient | |
|---|---|---|---|---|---|---|---|---|---|---|
| Dependent variable: | All schools (1) | Elem. (2) | Middle (3) | High (4) | Yes (5) | No (6) | Poorest quarter (7) | Least poor 3/4 (8) | <Median (9) | >Median (10) |
| School operating expenditure | 0.689 | 0.652 | 0.872 | 0.663 | 0.201 | 0.808 | 0.402 | 0.722 | 0.619 | 0.529 |
| | (0.092) | (0.115) | (0.156) | (0.164) | (0.248) | (0.174) | (0.146) | (0.100) | (0.136) | (0.134) |
| *p*-value of difference | | 0.195 | | | 0.022 | | 0.027 | | 0.488 | |
| Mean dep. var. (2012$) | 6,683 | 6,647 | 6,781 | 6,679 | 6,999 | 6,531 | 6,876 | 6,639 | 6,805 | 6,614 |
| *N* (school-years) | 24,543 | 15,300 | 4,311 | 4,464 | 6,606 | 4,239 | 6,026 | 18,211 | 8,859 | 8,336 |
| District and year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District-year covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Student-weighted | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The sample is at the school-year level and includes years 1995–2003. Each coefficient is from a separate OLS regression of school operating expenditures on the foundation allowance, both in 2012 dollars (in levels). Sample sizes across school types do not sum to the total due to omitted categories, missing data, and other reasons discussed in the text.

school level (i.e., elementary, middle, and high school), and find no statistically significant differences across these groups.

Next, I examine the effect of the allowance on school spending by school poverty status, proxied by whether a school is designated to receive Title I funds. Title I funding is the largest source of federal education funding to districts and is earmarked exclusively for schools serving low-income populations. To ensure that I capture a district's decision of whether to allocate the marginal dollar differentially across Title I and non-Title I schools, I restrict the sample to the 36 percent of districts that contained both types of schools. For each additional dollar of allowance, districts increased spending by 81 cents at non-Title I schools and by 20 cents (not statistically significant) at Title I schools. The *p*-value from a test of equality across the two groups is 0.022.

As another proxy for school poverty status, I split schools by the within-district distribution of school poverty share (proxied using the fraction of the school eligible for free lunch). I divide schools into those in the bottom quarter of the within-district school poverty share and those in the top three quarters of the distribution. As with the Title I split, the coefficient is larger for the low-poverty schools, with the *p*-value of the difference equal to 0.027.[27] These results suggest that districts may be compensating for their inability to use other revenue sources, such as Title I, on these lower poverty schools.

Finally, I examine whether districts spent the marginal dollar disproportionately on lower performing schools (Table 7, columns 9 and 10). I group schools by whether they were below or above the within-district median school fraction proficient on the previous year's state math exam.[28] I find no evidence that districts spent

[27] I split schools into the bottom versus top three quarters to obtain similar sample sizes as are observed in the student-level data when examining effects by student free lunch status (see online Appendix Table 7). If I alternatively divide the sample by schools above and below the median, the pattern is the same.

[28] As 11th grade assessment scores are not available during the earlier years of the sample, I restrict the analysis to elementary and middle schools, using the average proficiency rates in grades four and seven. For schools serving both grades, I use the average of the two.

the marginal dollar differentially by school achievement level. Given the negative correlation between poverty and achievement, this suggests that districts targeted the money toward low-achieving, low-poverty schools.[29]

## V. Conclusion

Given the substantial sums of money spent on public elementary and secondary schooling in the United States, it is important to understand the effects of spending on the later life outcomes of students. This requires both plausibly exogenous variation in spending, and data tracking students from primary school into adulthood. Most previously published studies that isolate plausibly exogenous variation in spending, such as those exploiting school finance reforms, have been limited to examining short-run effects.

This paper examines the long-run effects of school spending on students' educational attainment, exploiting variation in the funding formula imposed by Michigan's 1994 school finance reform, Proposal A. Student-level panel data allow for the examination of effects of spending during primary school on students' college entry and completion later in life.

I find that additional spending led to increases in rates of college entry and completion, by 3.0 and 2.3 percentage points (7 and 11 percent), respectively. These effects were concentrated among non-rural, low-poverty, high-achieving school districts. Districts spent the marginal dollar primarily on schools serving less-poor populations within the district. The spending increases led to smaller class sizes and reductions in the pupil-to-administrator ratio, but little to no change in teacher salaries.

To link my analysis more closely to the debate over whether money matters in education (see, Hanushek 2003, Krueger 2003), I briefly compare the per cost improvement in long-term outcomes to that of other proposed school reforms. I create an index of cost-effectiveness by dividing a policy's cost by the proportion of students it induces into college.[30] For example, assuming a cost in 2012 dollars of $5,000 per student ($1,000 per student per grade over 4 grades plus an additional $1,000 over the remaining grades) for the spending increases, and a 3 percentage point increase in the rate of college entry, the amount of money spent to induce 1 additional child into college is $166,667 (= $5,000/0.03).

This estimated cost-effectiveness is similar to other proposed school reforms. For example, given the effects on college enrollment estimated in Deming (2009), Head Start has a cost per student induced into college of $133,333 (= $8,000/0.06). The cost per student induced into college from the class size decrease in the Tennessee STAR experiment is substantially larger: between $400,000 (= $12,000/0.03) (Dynarski, Hyman, and Schanzenbach 2013) and $666,667 (= $12,000/0.018) (Chetty et al. 2011). Thus, the effects of providing additional general purpose funding to schools is slightly less cost-effective (according to this metric) than enrolling students in Head Start, but more cost-effective than reducing elementary school class size.

[29] See online Appendix Table 8 for correlations between all school characteristics in Table 7.
[30] I focus on college enrollment instead of degree receipt, because very few studies examine degree receipt.

Given the debate surrounding the effects of school resources on student outcomes, and the mixed evidence on the effects of school finance reform on achievement (see Yinger 2004), this paper provides important evidence that increases in school spending improve students' long-run outcomes that are of ultimate concern to policymakers. However, as found in other recent studies (e.g., Cascio, Gordon, and Reber 2013), it also provides evidence that local government responses to education policies imposed on them by higher levels of government can result in benefits accruing to students who may not have been the intended beneficiaries of the policy.

## REFERENCES

**Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu.** 2012. "The Role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA Experiment." *Quarterly Journal of Economics* 127 (3): 1205–42.

**Candelaria, Christopher A., and Kenneth A. Shores.** 2015. "The Sensitivity of Causal Estimates from Court-Ordered Finance Reform on Spending and Graduation Rates." https://cepa.stanford.edu/sites/default/files/shores_candelaria_causal_estimate.pdf.

**Card, David, and Alan B. Krueger.** 1992. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy* 100 (1): 1–40.

**Card, David, and A. Abigail Payne.** 2002. "School finance reform, the distribution of school spending, and the distribution of student test scores." *Journal of Public Economics* 83 (1): 49–82.

**Cascio, Elizabeth U., Nora Gordon, and Sarah Reber.** 2013. "Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South." *American Economic Journal: Economic Policy* 5 (3): 126–59.

**Chakrabarti, Rajashri, and Joydeep Roy.** 2015. "Housing markets and residential segregation: Impacts of the Michigan school finance reform on inter- and intra-district sorting." *Journal of Public Economics* 122 (1): 110–32.

**Chaudhary, Latika.** 2009. "Education inputs, student performance and school finance reform in Michigan." *Economics of Education Review* 28 (1): 90–98.

**Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan.** 2011. "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star." *Quarterly Journal of Economics* 126 (4): 1593–1660.

**Chetty, Raj, John N. Friedman, and Jonah E. Rockoff.** 2014. "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review* 104 (9): 2633–79.

**Courant, Paul N., and Susanna Loeb.** 1997. "Centralization of school finance in Michigan." *Journal of Policy Analysis and Management* 16 (1): 114–36.

**Cullen, Julie Berry, and Susanna Loeb.** 2004. "School Finance Reform in Michigan: Evaluating Proposal A." In *Helping Children Left Behind: State Aid and the Pursuit of Educational Equity*, edited by John Yinger, 215–50. Cambridge: MIT Press.

**Deming, David.** 2009. "Early Childhood Intervention and Life-Cycle Skill Development: Evidence from Head Start." *American Economic Journal: Applied Economics* 1 (3): 111–34.

**Downes, Thomas A.** 1992. "Evaluating the Impact of School Finance Reform on the Provision of Public Education: The California Case." *National Tax Journal* 45 (4): 405–19.

**Dynarski, Susan M., Steven W. Hemelt, and Joshua M. Hyman.** 2015. "The Missing Manual: Using National Student Clearinghouse Data to Track Postsecondary Outcomes." *Educational Evaluation and Policy Analysis* 37 (1S): S53–79.

**Dynarski, Susan, Joshua Hyman, and Diane Whitmore Schanzenbach.** 2013. "Experimental Evidence on the Effect of Childhood Investments on Postsecondary Attainment and Degree Completion." *Journal of Policy Analysis and Management* 32 (4): 692–717.

**Epple, Dennis, and Maria Marta Ferreyra.** 2008. "School finance reform: Assessing general equilibrium effects." *Journal of Public Economics* 92 (5–6): 1326–51.

**Guryan, Jonathan.** 2001. "Does Money Matter? Regression-Discontinuity Estimates from Education Finance Reform in Massachusetts." National Bureau of Economic Research (NBER) Working Paper 8269.

**Hægeland, Torbjørn, Oddbjørn Raaum, and Kjell G. Salvanes.** 2012. "Pennies from heaven? Using exogenous tax variation to identify effects of school resources on pupil achievement." *Economics of Education Review* 31 (5): 601–14.

**Hanushek, Erik A.** 2003. "The Failure of Input-Based Schooling Policies." *Economic Journal* 113 (485): F64–98.

**Heckman, James J., 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.

**Hines, James R., Jr., and Richard H. Thaler.** 1995. "Anomalies: The Flypaper Effect." *Journal of Economic Perspectives* 9 (4): 217–26.

**Hoxby, Caroline M.** 2001. "All School Finance Equalizations Are Not Created Equal." *Quarterly Journal of Economics* 116 (4): 1189–1231.

**Hyman, Joshua.** 2017a. "ACT for All: The Effect of Mandatory College Entrance Exams on Postsecondary Attainment and Choice." *Education Finance and Policy* 12 (3): 281–311.

**Hyman, Joshua.** 2017b. "Does Money Matter in the Long Run? Effects of School Spending on Educational Attainment: Dataset." *American Economic Journal: Economic Policy*. https://doi.org/10.1257/pol.20150249.

**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." *Quarterly Journal of Economics* 131 (1): 157–218.

**Krueger, Alan B.** 2003. "Economic Considerations and Class Size." *Economic Journal* 113 (485): F34–63.

**Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach.** Forthcoming. "School Finance Reform and the Distribution of Student Achievement." *American Economic Journal: Applied Economics*.

**Leuven, Edwin, Mikael Lindahl, Hessel Oosterbeek, and Dinand Webbink.** 2007. "The Effect of Extra Funding for Disadvantaged Pupils on Achievement." *Review of Economics and Statistics* 89 (4): 721–36.

**Long, Bridget Terry, and Michal Kurlaender.** 2009. "Do Community Colleges Provide a Viable Pathway to a Baccalaureate Degree?" *Educational Evaluation and Policy Analysis* 31 (1): 30–53.

**Murray, Sheila E., William N. Evans, and Robert M. Schwab.** 1998. "Education-Finance Reform and the Distribution of Education Resources." *American Economic Review* 88 (4): 789–812.

**National Center for Education Statistics.** 2013. "The Condition of Education 2013." United States Department of Education NCES Compendium 2013037.

**Papke, Leslie E.** 2005. "The effects of spending on test pass rates: Evidence from Michigan." *Journal of Public Economics* 89 (5–6): 821–39.

**Papke, Leslie E.** 2008. "The Effects of Changes in Michigan's School Finance System." *Public Finance Review* 36 (4): 456–74.

**Reynolds, C. Lockwood.** 2012. "Where to attend? Estimating the effects of beginning college at a two-year institution." *Economics of Education Review* 31 (4): 345–62.

**Roy, Joydeep.** 2011. "Impact of School Finance Reform on Resource Equalization and Academic Performance: Evidence from Michigan." *Education Finance and Policy* 6 (2): 137–67.

**Staiger, Douglas, and James H. Stock.** 1997. "Instrumental Variables Regression with Weak Instruments." *Econometrica* 65 (3): 557–86.

**Yinger, John, ed.** 2004. "State Aid and the Pursuit of Educational Equity: An Overview." In *Helping Children Left Behind: State Aid and the Pursuit of Educational Equity*, 3–58. Cambridge: MIT Press.