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

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Abstract

This paper examines the long-run effects of increases in education expenditures on educational attainment. Using student-level panel data, I exploit variation in the school funding formula imposed by Michigan's 1994 school finance reform, Proposal A. Students exposed to additional funding were more likely to enroll in college and earn a postsecondary degree. The increases in spending lowered class sizes, raised teacher salaries, and substantially reduced the ratio of pupils to administrators. School districts targeted the additional dollar toward schools serving less poor populations within the district, and consistent with this finding, the postsecondary effects appear concentrated among non-poor students.

JEL: H4, H7, I2, J2

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Government spending on public elementary and secondary education accounts for 4.3% of GDP in the United States (National Center for Education Statistics, 2013). Despite this large government investment, the effect of education spending on the long-run well-being of students remains an open question. One challenge in answering this question is that it is difficult to find plausibly exogenous variation in spending to estimate a causal effect. Further, to examine the effects of spending during childhood on students' long-run outcomes, it is necessary to track individual students into adulthood, and, even now, such data are rare.

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 equalizing spending across school districts and subsequently reducing inequalities in education. These reforms were generally effective in at least partially equalizing spending between poor and rich school districts (Downes, 1992; Murray et al., 1998; Hoxby, 2001; Guryan, 2001; Card & Payne, 2002; Papke, 2005, 2008; Roy, Forthcoming). However it is less clear whether the changes in spending affected student achievement, with some studies finding positive effects and others finding no effects.

At least twenty years have passed since the majority of school finance reforms were implemented, and a natural question is whether the increases in spending due to these reforms improved the long-term outcomes of students. Educational attainment, earnings, and other important long-run measures that determine quality of life are the litmus test for whether increases in spending improve the welfare of students. Whether there are effects of spending on standardized test scores is only partially informative because districts could spend their additional dollars on inputs that may increase test scores, but have little

or no effect on long-term outcomes, or vice-versa. Yet, no previously published study has examined the effects of increased spending due to school finance reform on these long run measures of students' well being.[1]

In this paper, I examine the long-run effects of primary school spending on students' educational attainment. To plausibly identify the causal effects of spending, I instrument for spending by exploiting variation across districts and over time in the funding formula implemented as part of Michigan's 1994 school finance reform, Proposal A. While previous studies of school finance reform are limited to examining effects on achievement, student-level panel data allow me to examine the effects of spending during primary school on a student's college entry and degree completion behavior much later in life. Such data also allow for improved identification over previous studies in two key ways: 1) I control for a student's standardized test scores just prior to the spending increases, alleviating concerns about omitted confounding variables; and 2) unlike previous studies that have examined the effects of lagged school- or district-level spending on average achievement in that school or district, I track individual students across schools and grades, and observe the spending that they are exposed to in each year, reducing measurement error in the spending variable.

To help understand the effects on postsecondary attainment, I examine the underlying mechanisms by estimating the behavioral responses of school districts to the additional funding. I examine whether changes in spending lead to changes in specific inputs to education production (e.g., class size, teacher quality). I identify the types of expenditures for which districts allocated the additional dollar (e.g., instruction, administration). Finally, I determine if

[1]One current working paper, Jackson et al. (2014), examines the long-run effects of school finance reforms. I discuss this working paper further in Sections 1.2 and 5.1.

school districts strategically allocated additional spending toward certain types of schools (e.g., low-income, low-achieving). Detailed school- and district-level expenditure data throughout the sample period allow me to identify the behavioral responses of districts.

I find that students who were exposed to $1,000, or 12%, more spending per year during grades four through seven experienced a 3.9 percentage point (9%) increase in the probability of enrolling in college, and a 2.5 percentage point (16%) increase in the probability of earning a degree. The larger percent increase in degree completion suggests some combination of the following: 1) students induced into college by the additional spending persisted to degree completion at a higher rate than the inframarginal student; and 2) the additional spending boosted the probability of degree receipt for students who would have enrolled and dropped out in the absence of the spending increase. Districts spent slightly more of each additional dollar on instruction than non-instruction, but the proportional increase relative to base spending levels was larger for non-instruction—particularly administration. Consequently, the increases in spending reduced class sizes and boosted teacher salaries only slightly, but substantially reduced the ratio of pupils to administrators. The postsecondary effects of spending appear to have been concentrated among non-poor students. I find suggestive evidence that districts allocated the extra dollar primarily toward schools serving less poor populations within the district, providing one explanation for the heterogeneity of effects by student poverty status.

I show that the boost to postsecondary attainment from an extra dollar of school spending is similar to that from a dollar spent on other programs aimed at altering students' human capital, such as the federally funded preschool program Head Start (Deming, 2009). Given the debate surrounding the effect of

school resources on student outcomes (see Hanushek, 2003 and Krueger, 2003), and the mixed evidence on the effect of school finance reform on achievement (see Yinger, 2004), this paper provides important new evidence that increases in education expenditures improve the later life outcomes of students.

The remainder of this paper is organized as follows. In the next section, I describe Proposal A and summarize the previous literature. In Section 2, I describe the data, and in Section 3, the methodology. In Section 4, I present the results, first examining how districts spent the additional dollar, then examining the effects on postsecondary outcomes as well as heterogeneity in the results. Section 5 places the results in context relative to past studies, and compares the cost/benefit of the effects on postsecondary attainment. Section 6 concludes the paper.

1 Background

1.1 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 the public outcry over an increasing property tax burden, in July 1993 the Michigan state legislature abolished local school property taxes to begin in the 1994–95 school year. In March 1994 voters passed Proposal A, which relied on state rather than local sources of revenue to finance education funding in Michigan.[2]

Proposal A not only substantially changed the source of education funding

[2]In addition to other minor changes, the sales tax increased from 4% to 6%, and cigarette taxes increased from 25 to 75 cents per pack. The fraction of education funding coming from state sources increased from 37% in 1993–94 to 80% in 1994–95. For a thorough review of Michigan education finance and Proposal A, please see Courant & Loeb (1997).

in the state, but also the funding mechanism. 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 were not allowed to raise funds locally to spend more.[3] Proposal A was equalizing 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 that was inversely related to the 1994 revenue.[4,5]

Proposal A also set into motion a future time path of allowances that resulted in further equalization and provides the plausibly exogenous variation in spending that I exploit in the present paper. Figure Ia illustrates how the time path of the allowance varied by a district's 1994 revenue. The allowance is plotted over time (in nominal dollars), grouping districts by percentiles of the 1994 revenue distribution. The bottom line shows the average allowance for districts in the bottom 5% of 1994 revenue. These districts received the minimum allowance of $4,200 in 1995, which rose quickly through 2002 and more slowly after that.[6] The allowance for the remaining districts in the bottom half of the 1994 revenue distribution also rose through the late 1990s, although at a slower rate than the lowest districts. The allowance rose even more slowly and in parallel across all districts in the top half of the 1994 revenue distri-

[3]An important exception is that Proposal A only restricts spending on operating expenditures. Districts can raise money locally to pay for capital outlays, major repairs, and technology purchases. Any observed spending response by a district to the change in allowance that is less than one-for-one could be due to districts substituting between operating and capital expenditures, as well as districts seeking additional local, state, or federal revenue sources.

[4]Here and throughout the paper, I refer to a school year by its spring year, i.e., 1994 refers to the 1993–94 school year.

[5]If a district had lower per-pupil revenue in 1994 than in 1993, then the base revenue was calculated as the average revenue across the two years. Appendix Figure Ia shows how the allowance in 1995 varied by 1994 revenue.

[6]Appendix Table 1 shows the minimum foundation allowance by year and the fraction of districts at the minimum. Appendix Figure Ib shows how the allowance in 2000 varied by 1994 revenue.

bution. In sum, Figure Ia shows how the allowance was designed to equalize school funding in nominal dollars through the early 2000s without reducing the funding of initially high spending districts.

Adjusting for inflation reveals a different story. Figure Ib shows the time path of allowances by 1994 revenue, deflating current dollars using the Employment Cost Index (ECI) for elementary and secondary school employees.[7] Through 2002, districts in the bottom half of the 1994 revenue distribution experienced substantial annual real increases in the allowance while districts in the top half saw very small increases, or even decreases for the highest districts. Beginning in 2003, as the economy worsened and allowance growth stalled, all districts experienced real declines that were sharpest for the highest 1994 revenue districts.

1.2 Previous Literature

An extensive literature using observational data finds contrasting evidence on the effects of spending on achievement (see Hanushek, 2003 and Krueger, 2003). Most of these studies focus on the effects of specific inputs, such as class size or teacher quality, and some examine effects on long-term outcomes using crude, publicly available measures of aggregate earnings and educational attainment (e.g., Card & 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 and Guryan, 2001), nationally (Hoxby, 2001; Card & Payne, 2002), or by exploiting other sources of plausibly exogenous variation (e.g., Leuven et al., 2007 and Hægeland et al., 2012). Most of these studies find positive effects of spending, and the remainder find no

[7] 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, given that over 80% of their operating expenses are employee compensation. The time path of the allowance deflated using the CPI looks similar and is shown in Appendix Figure II.

effect.

Most relevant to the present paper are a handful of studies that examine the short- and medium- run effects of Proposal A in Michigan, and one current working paper that examines the long-run effects of school finance reforms nationwide. The papers exploiting Proposal A find positive effects on fourth grade test scores, but no effects on seventh grade scores or on school-level ACT- or SAT-taking rates or mean scores (Papke, 2005, 2008; Chaudhary, 2009; Roy, Forthcoming). Jackson et al. (2014) uses data on 15,353 individuals from the Panel Survey of Income Dynamics (PSID) and exploits the timing of statewide school finance reforms. The authors find that higher per-pupil spending during childhood leads to more years of completed schooling and higher earnings during adulthood. I compare the magnitude of these effects to those estimated in the present paper in Section 5.1.

2 Data

This paper uses an original dataset containing six cohorts of first-time fourth grade students in Michigan public, non-charter schools between 1994–95 and 1999–2000.[8] The data contain time-invariant student demographic information such as sex and race, as well as time-varying student characteristics such as free and reduced-price lunch status, limited English proficiency (LEP) status, and special education (SPED) status.[9] The data also contain student

[8]The first available year of data is 1993–94, allowing for identification of first-time fourth graders in 1994–95 (i.e., students in fourth grade during 1994–95 who are not observed in 1993–94). I assembled the data from individual test-taking records, and so only students who took the fourth grade state test are in the sample. All students are required to take the fourth grade test during these years, and a comparison of these microdata with publicly available aggregate fourth grade head count data shows very similar total numbers. In 1995, the total number of students in fourth grade and the total number of test-takers were 123,946 and 120,785, respectively, for a difference of 2.5%. From 1996 to 2000, the difference was always less than 1%.

[9]Because the time-varying characteristics are first available in 2003, the year in which the first cohort is in twelfth grade given on-time grade progression, I measure these characteristics

scores on state assessments by subject during grades four, five, seven, eight, and eleven, as well as information on whether a student graduated high school. Student-level postsecondary enrollment and degree receipt information was obtained by matching students to the National Student Clearinghouse (NSC).[10]

Based on where and when students were enrolled in school, I merged in school- and district-level expenditure information, the district-year level foundation allowance, and 1994 district revenue information.[11] Finally, I obtained several variables available at the district-year level, which measure 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 I include as covariates in the model.[12]

Table 1 reports sample means for the 746,834 students and 518 school districts in the sample. Eighteen percent of the sample is black (column 1), although this percentage differs dramatically for districts in the bottom half versus the top half of the 1994 revenue distribution: the low 1994 revenue districts are only 3% black (column 2), whereas the top half of districts are 24% black (column 3). This heterogeneity reflects the fact that the low 1994 revenue districts are in primarily rural areas, towns, and smaller cities, whereas the high 1994 revenue districts are primarily in larger cities.[13] Figure II, which shows a map of Michigan school districts shaded to reflect their 1994 revenue,

during grade twelve.

[10]The NSC is a non-profit organization that houses postsecondary enrollment and degree receipt information on over ninety percent of undergraduate students nationwide. Colleges not in the NSC tend to be for-profit institutions. See Dynarski et al. (2013a) for a detailed discussion of the NSC matching process and coverage rates.

[11]The expenditure data contain information on spending by category, such as, instruction, administration, and operations and maintenance.

[12]The full list of covariates and all data sources are available in Appendix A.1.

[13]The 259 districts in the bottom half of 1994 revenue contain only 220,720 students, or 30% of the sample.

also illustrates this pattern.[14]

The fraction of students on free lunch dramatically increased over the sample period, from 15% to 27%. Part of these increases is due to out-of-state migration during this period, with white, non-poor households leaving the state. Attrition in the data is high: only 73% of the sample of fourth grade students was still in Michigan public schools as of grade twelve. Attrition from the sample has implications for the measured outcome variables in the present paper. Test scores, in grade eleven for example, are missing for students who leave the sample prior to eleventh grade. Similarly, a student who leaves the sample 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 observed during fourth grade to the NSC regardless of attrition, 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 my analysis.

Table 1 also reports sample means for several district-year level school choice, economic, and demographic characteristics during the year that a student is in fourth grade. The school choice movement in Michigan began concurrently with the implementation of Proposal A; thus there are zero students exhibiting choice in the first cohort, but this increases to approximately 3% for both charters and inter-district choice in the 2000 cohort. Population density is much higher in the high revenue districts, consistent with Figure II. Economic conditions during students' fourth grade year improved between the 1995 and

[14]The different shades portraying 1994 revenue reflect the same percentile groupings used in Figure I (e.g., 1^{st}–5^{th}, 6^{th}–25^{th}, etc.). The darker shaded districts are those with the highest 1994 revenue, and tend to appear in the largest urban areas of Michigan, whereas the lighter shaded districts (those with low 1994 revenue) tend to appear in the more rural areas, towns, and smaller cities.

2000 cohort, reflected in changes in the local unemployment rate and median household income. However, these conditions deteriorated dramatically during the mid-2000s.

3 Methodology

One of the main concerns with a regression of education outcomes on education spending is that spending is under the control of the school district, and the types of districts that have high or low spending are different in unobserved ways that may be correlated with the outcome of interest. Year and district fixed effects help to control for this omitted variables bias. However, the time path of spending within a district is still typically within a district's control.[15] Instrumenting for spending with the foundation allowance solves this concern because the time path of the allowance is not within the control of the district, and is a function only of a district's 1994 revenue (absorbed by the district fixed effect) and the growth in the state economy over time (arguably absorbed by the year fixed effect). The identifying assumption is that, conditional on the fixed effects and any covariates, changes in the allowance will not be correlated with changes in time-varying unobserved characteristics related to the outcome of interest.

Several papers beginning with Papke's 2005 study of the effects of spending on district-level fourth grade test proficiency rates instrument for spending using the foundation allowance. While this specification is certainly an improvement over a fixed-effects OLS regression without the IV, the identifying assumption is strong. There are a number of reasons to suspect that educational outcomes in districts that experienced larger increases in the allowance

[15]Even in post-Proposal A Michigan, the time path of total operating expenditures is still somewhat within the control of the district, because districts may seek additional funding over and above the allowance (e.g., local fundraising, federal sources, etc.).

over time (i.e., the lowest 1994 revenue districts) could have been trending differentially relative to districts with smaller increases in the allowance over time.[16] In Appendix A.2, I replicate Papke (2008) and show that the main results in that study are due largely to these omitted factors.[17]

In the present paper, I examine the effect of spending on long-term educational attainment using student-level data and several strategies to alleviate the aforementioned concerns. I estimate the following equations using two-stage least squares (2SLS):

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

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

where Y_{isdc} is a long-run educational attainment outcome of student i in school s in district d in cohort c, $Spend_{idc}$ is the average real spending in thousands of 2012 dollars that a student is exposed to in grades four through seven (in levels),[18] X is a vector of student demographics including a student's sex, race, free lunch, special education, and limited English proficiency status, $Allow_{dc}$ is the average real allowance in thousands of 2012 dollars during a student's fourth through seventh grade in the student's fourth grade district, regardless

[16]First, the economy improved in Michigan during this time, and welfare reform was implemented. Second, at the same time that Proposal A was passed, charter schools and inter-district choice began to appear in Michigan. Finally, accountability was ramping up during this period, especially with the implementation of No Child Left Behind in 2001. All of these factors could have had differential effects on education outcomes in low and high 1994 revenue districts, and the direction of any bias due to these omitted factors is unclear ex-ante.

[17]Papke (2008) is a follow-up to Papke (2005). I replicate Papke (2008) because it uses more years of data and a longer lag structure of spending.

[18]I measure spending in levels instead of logs for two primary reasons: 1) To avoid the ex-ante assumption that a dollar of spending has less effect for a high spending district than a low spending district; and 2) to maintain consistency in interpretation across my IV and first stage analyses. The first stage analysis of how districts spend the additional allowance dollar in Section 4.1 is more sensible in levels because a 1% increase in the allowance should not lead to a 1% increase in spending, given that the level of the allowance is smaller than spending. Appendix Table 2 shows the main results using logged spending, which are similar to the results in levels.

of whether the student changes districts,[19] λ and α are district fixed effects, and π and γ are year fixed effects. The standard errors are clustered at the district level.

I measure the average of spending across grades four through seven for a number of reasons. There is little identifying variation in the foundation allowance after 2003, and the relationship between the allowance and spending breaks down during this period.[20] The final cohort of students is in grade seven during 2003, so I restrict the spending measure to grade seven and below in order to have a consistently measured spending variable across cohorts. I do not use spending (or the allowance) prior to grade four because: 1) I do not observe where a student was enrolled prior to grade four; 2) as I discuss later, I control for students' fourth grade test scores in my regressions, and so prefer to use a spending measure that occurs after this control variable is measured; and 3) the earlier cohorts of students attended grades lower than four during the years before Proposal A, in which there was no foundation allowance to use as an instrument for spending.[21]

The use of student-level panel data allows me to improve on specifications from previous studies in a number of ways. First, 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: only 55% of fourth grade students were observed in grade twelve in their fourth grade district. Previous estimates of the effects of school finance reform rely on school- or district-level contemporaneous achievement measures and lagged

[19] Thus, while the spending variable measures the actual dollars the student was exposed to, and is subject to potential endogenous mobility, the instrument is not.

[20] I show this explicitly and discuss explanations in Section 4.1.

[21] I use the average allowance taken over the grades during which spending is measured, as opposed to using the allowance only in a single grade, because the first stage is stronger and results more precisely estimated. The point estimates are nearly identical if I instrument for average spending using the allowance in a single grade.

spending measures. These estimates assume that students who contributed to the contemporaneous achievement measure were the same students exposed to spending in the district several years earlier. This is clearly not the case.

Second, I control for an individual student's fourth grade achievement to help control for time-varying unobserved district characteristics that vary by 1994 district revenue and are correlated with educational attainment.[22] Doing so absorbs the effects of any of the confounders or differential trending by 1994 revenue that are reflected in students' achievement during fourth grade.

In a further attempt to control for other factors that could affect districts over this time period differentially by 1994 revenue, I control for a rich set of district-year level school choice, economic, and demographic characteristics.[23,24] I also include quadratic cohort trends interacted with the average of each of these characteristics over the years in which the first cohort was in grades four through seven.[25]

Finally, after presenting results for all districts, I focus the analysis on the bottom half of districts by 1994 revenue. Doing so restricts the sample to a more homogenous group of districts and therefore alleviates concern that districts are trending differentially by their 1994 revenue. Low and high 1994

[22]To flexibly control for achievement, I include a cubic in students' fourth grade math score and in their fourth grade reading score. Using a square or quartic instead of a cubic, or using a polynomial in math and reading percentiles instead of scores, yields virtually identical results.

[23]To capture as much variation as possible, I include each of these district-year covariates separately for the years that each cohort was in grade four, five, six, and seven (i.e., four variables for each characteristic).

[24]Theoretically the changes in spending could lead to families moving across districts, thus affecting these district demographic and economic characteristics. However, several studies have shown that there was no major resorting across districts or changes in district demographics in Michigan in response to Proposal A (Chakrabarti & Roy, 2012; Epple & Ferreyra, 2008; Courant & Loeb, 1997).

[25]These interactions allow for differential trending of the outcome variable by districts with different baseline values of these covariates. I do not report results including a district-specific cohort trend, because doing so attenuates the first stage F-statistic well below ten, the rule-of-thumb threshold for weak instruments (Staiger & Stock, 1997).

revenue districts are very different in racial composition and school choice use, mostly stemming from the urban/rural difference. It is more plausible that educational attainment would be trending similarly over time in the absence of the spending increases within this more homogenous group of primarily non-urban districts. There is little reason not to restrict the analysis to this sample, given that, as shown in the following section, the first stage variation in allowance growth is driven nearly entirely by the bottom half of districts.[26]

4 Results

4.1 First Stage: Effects of the Allowance on Spending

As described in Section 1.1, Proposal A introduced a foundation allowance that grew at a faster rate for previously low spending districts, with the goal of equalizing spending across districts. In this section, I examine to what degree the increases in the allowance actually increased spending, and whether they led to equalization. Figure III shows average per-pupil operating expenditures over time in 2012 dollars for districts grouped by 1994 revenue percentile.[27] As in Figure Ib, the lower spending districts experienced complete spending equalization by 2003. The only stark difference between the evolution of the allowance and of spending is that the top districts' spending did not decrease as did their allowance. By 2010, there was more inequality across districts than in 2003, due to decreased spending in the low districts and relatively stable spending in the higher districts.

The relationship between the allowance and spending is examined more

[26] Also, the high 1994 revenue districts, especially those observing real decreases in the allowance during 1995–2003, are those that were under the most pressure to break the relationship between the allowance and spending by strategically reallocating expenditures or seeking other funding sources.

[27] Appendix Figure IIb shows operating expenditures over time deflated using the CPI instead of ECI.

formally by regressing district-year level spending on the district-year level allowance, both in 2012 dollars (in levels). This is the first stage relationship for the IV analysis presented in Section 4.2. 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 (Table 2, column 1). Adding district fixed effects and weighting districts by their enrollment to ensure that the estimates reflect a representative sample of the student population in Michigan, the point estimate decreases slightly to 58 cents (column 3). These results are on the high end of flypaper effects estimated in previous studies of the effects of state aid to school districts (Hines & Thaler, 1995).[28]

Next, I split the sample 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 this early period (column 4), with a point estimate of 61 cents. The effect of the allowance on expenditures in the later period is small (column 5), at less than 20 cents, and is statistically imprecise. 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 the early period to examine the effects of allowance-induced spending in Section 4.2.

Because most of the variation in changes in the allowance over time was in the bottom half of districts by 1994 revenue, and because of the concerns discussed in Section 3 regarding the differences in demographics between the bottom and top half of districts, I narrow the analysis to examine the effects of

[28]This is unsurprising given that unlike in the class flypaper case, Proposal A explicitly requires districts to spend the amount of the allowance by forbidding districts to change local property taxes.

the allowance on spending only in the bottom half of districts. The relationship between the allowance and spending was substantially stronger during the early period for the low 1994 revenue districts—a dollar increase in the allowance led to a nearly 90 cent increase in operating expenditures (column 6). This is my preferred sample throughout the majority of the analysis.

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 for low 1994 revenue districts.[29] Rows 2 and 3 split operating expenditures into instructional and non-instructional expenditures.[30] A dollar increase in the allowance led to a 49 cent increase in instructional expenditures, and a 38 cent increase in non-instructional expenditures. However, given that instructional spending comprises 65% of operating expenditures while non-instructional makes up only 35% (column 3), the proportional effect—calculated as the point estimate (column 1) divided by the proportion of total expenditures (column 3)—of an additional dollar of the allowance on non-instruction was substantially greater than the proportional effect on instruction (column 4). To explore what was driving this proportionally larger effect on non-instruction, I break the non-instruction category into (a) instructional support (e.g., speech therapists, guidance counselors,

[29] All results in Table 3 control for district and year fixed effects and are student weighted.

[30] Instructional spending represents the cost of activities dealing directly with teaching students in a classroom, such as instructional salaries and benefits as well as supplies for general, special, and adult education. Non-instructional spending includes the cost of activities providing administrative, technical, and logistical support to facilitate and enhance instruction.

school nurses, curriculum specialists), (b) administration (e.g., superintendent, principals, central business office), (c) operations and maintenance, and (d) transportation. The largest proportional effect of an additional dollar of allowance was on administrative expenditures, followed by transportation, and then operations and maintenance. Further breaking administration into school administration (e.g., principals), district administration (e.g., superintendent), and central business office, reveals that the bulk of the increased spending on administration goes toward district administration.

An optimistic view of these results is that when districts are constrained, they focus spending on teachers. When their budget constraint is loosened, they are able to supplement the budgets of their administrators. A more pessimistic view is that low 1994 revenue district administrators are reacting to forced increases in education spending by essentially pocketing the cash.

4.2 Educational Attainment

In the previous section, I examined how school district spending responds to the imposed foundation allowance. In this section, I explore how these spending responses affect students' educational attainment.[31] 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 for whether a student earns a degree (row 2). Controlling only for district and cohort fixed effects and student demographics, there is a small and statistically insignificant effect of spending on postsecondary enrollment (column 1). Adding fourth grade test scores, the vector of district-cohort level covariates, and the quadratic cohort

[31]Effects on student achievement are presented in Appendix A.2. I first use district-level data to replicate Papke (2008), showing that the estimated effects on fourth grade achievement are sensitive to the inclusion of additional controls. I then use student-level data to estimate effects on achievement during seventh and eleventh grade, controlling for fourth grade achievement. I find some evidence of positive effects on eleventh grade achievement, but I present results demonstrating that selective attrition calls these results into question.

trend interacted with the covariates boosts the point estimate to 3.3 percentage points (standard error of 1.5 points).[32] The interpretation of this effect is that $1,000 of additional spending during each of grades four through seven led to a 3.3 percentage point increase in the probability that a student enrolled in postsecondary school. This represents a nearly 10% increase in spending during those grades (given mean spending of almost $10,000) and an approximately 7% increase in enrollment (given mean enrollment of 44.8%).

Turning to postsecondary degree receipt, there was a statistically significant 2.1 percentage point effect of spending on the probability of degree receipt, estimated without fourth grade scores or covariates. The effect drops slightly and loses statistical precision with the inclusion of fourth grade scores, the district-cohort covariates, and covariate-trend interactions.[33]

As discussed in Section 3, in an attempt to mitigate concerns about time-varying omitted confounding variables, columns 4 through 8 in Table 4 present results from estimating the effect of spending on these postsecondary outcomes for the bottom half of districts by 1994 revenue. For both college enrollment and degree receipt, the point estimates are more stable across specifications in this sample, suggesting that restricting to the more homogeneous group of districts does reduce concerns about the time-varying omitted factors. Given that adding the covariate-trend interactions in this sample (column 8) does not change the point estimates but substantially reduces the statistical precision, my preferred specification moving forward excludes them, showing a 3.9 per-

[32]The first stage F-statistics, ranging between 94 and 160 across the specifications, are all far greater than the rule-of-thumb threshold for weak instruments of 10 (Staiger & Stock, 1997).

[33]In results not presented here, I find that the effects on enrollment and degree receipt are driven by similarly sized increases at two-year and four-year postsecondary institutions. I also examine effects on high school graduation, finding that a $1,000 increase in spending during grades 4–7 increased the high school graduation rate by 3 to 5 percentage points depending on the specification.

centage point (9%) effect on enrollment and a 2.5 percentage point (16%) effect on degree receipt (column 7). The larger percent increase in postsecondary degree completion than enrollment suggests some combination of the following: 1) students induced into college by the additional spending persisted to degree completion at a higher rate than the inframarginal student; and 2) the additional spending boosted the probability of degree receipt for students who would have enrolled and dropped out in the absence of the spending increase.

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 quality (crudely measured using teacher salary),[34] 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 during grades four, five, six, and seven led to a 1.2 pupil decrease in the pupil-teacher ratio during these grades (column 1).[35] This effect represents a 6% decrease in class size relative to the mean pupil-teacher ratio of 22. The spending increases also led to a $2,632, or 4%, increase in the average teacher salary from a mean over the sample period of $65,972 (column 2).[36] The small size of these effects suggest that changes in class size and teacher salaries are likely not responsible for the majority of observed effects on postsecondary attainment.[37]

[34]While most studies find only a weak positive relationship between teacher salary and student outcomes (Hanushek & Rivkin, 2006), it is the best measure available.

[35]All estimates in this and subsequent tables use the preferred specification from Table 4, column 7, controlling for district and cohort fixed effects, student demographics, fourth grade test scores, and district-cohort covariates. As in Table 4, the point estimates are nearly identical when I include the interaction of the covariates with the quadratic trend, but less statistically precise.

[36]This figure is reported in 2012 dollars. While this average salary may appear high, it is comparable to other relatively high salary states such as California and Massachusetts and to the current average reported by the Michigan Department of Education of $63,000.

[37]Also, when the identifying equation is estimated controlling for either or both of these

Finally, I examine the effect of the spending increases on the ratio of pupils to school and district administrators (column 3). There is a statistically insignificant decrease in the number of students per administrator. When I focus on the ratio of pupils to district administrators only, I find a stronger, and marginally statistically significant decrease of 53 students (column 4). This represents a 13% decrease in the number of students per district administrator. This substantial effect is consistent with the result found in Section 4.1 that districts spend the additional dollar of allowance proportionally more on administration, and in particular, on district administration. It seems possible that this increase in administrators could explain some of the postsecondary effects, perhaps through better organization and focusing of district resources, particularly if a goal of district administrators is to boost postsecondary attainment.

4.3 Heterogeneity

It is important to understand whether the effects of spending on educational attainment observed in the previous section were experienced equally by all students, or were concentrated among certain types of students. Table 6 examines how the postsecondary effects vary by sex, poverty status, and fourth grade achievement.

Effects on both college enrollment and degree receipt are similar across genders (columns 1 and 2). While the enrollment results are similar for low- and high-achieving students, the effects on degree receipt are concentrated nearly entirely within the students with high fourth grade scores. These results suggest some combination of the following: 1) high-achieving students induced into college by the additional spending persisted to graduation at a much higher

inputs, the treatment effect does not diminish as would be expected if the effects were operating through these inputs.

rate than their low-achieving counterparts; and 2) high-achieving students who would have dropped out of college in the absence of the spending increase persisted at a higher rate due to the additional spending. Given that the welfare increase to students will be smaller (and potentially even negative) if they are induced to enroll but not complete postsecondary school,[38] these results have implications for which types of students experienced larger welfare gains as a result of Proposal A.

Finally, I examine effects by student poverty status (as proxied by free lunch status). While the standard errors preclude any firm conclusions, the pattern suggests that the effects of spending are concentrated among non-poor students. This result suggests that Proposal A may not have actually increased the long-run outcomes of the students it aimed to help.[39]

Heterogeneity of effects by student characteristics can either be due to differential responses of students to an identical treatment, or alternatively, to differences in treatment dosage: certain types of students could have been exposed to different increases in spending if districts targeted the additional dollar toward schools serving particular student populations. Education funds are received at the district level but then are allocated toward operating expenditures at individual schools, such as hiring a new teacher or purchasing supplies. I use Michigan school-level expenditure data during my sample period to provide, to my knowledge, the first plausibly causal estimates of how school districts spend an additional dollar of general purpose funds across types

[38] Substantial diploma, or "sheepskin," effects have been found in a number of studies (e.g., Kane & Rouse, 1995; Jaeger & Page, 1996).

[39] A possible alternative explanation is that the marginal poor student is not on the margin of enrolling or graduating from college, but may experience effects on earlier educational outcomes. I estimate the specifications from Table 6 with high school graduation and eleventh grade math score percentile as dependent variables. The pattern of results by free lunch status is the same as for the postsecondary outcomes.

of schools.[40] In addition to potentially helping to explain 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 overall effect of a dollar increase in the allowance on school-level operating expenditures was 79 cents (Table 7, column 1).[41,42] In columns 2–4, I report results from estimating separate OLS regressions by school level (elementary, middle, and high school). A dollar increase in the district allowance leads to the largest spending increases in high schools and the smallest spending increases in elementary schools.[43] Next I examine the effect of the allowance on spending by school poverty status, proxied by whether a school is designated to receive Title I funds.[44] The point estimates, though imprecise, suggest that districts used the additional allowance money primarily on non-Title I schools within their district.

To further investigate whether districts strategically allocated money more toward schools serving less poor families, I split schools by the within-district

[40]Van der Klaaw (2008) examines responses of school-level expenditures in New York City to increased Title I funding. Title I funds, the largest source of federal funding for primary and secondary schooling, are allocated to districts with the requirement that they spend the money on the poorest schools in the district. Cascio et al. (2013) showed that the implementation of Title I in the south reduced high school dropout rates of white students, but not black students, providing indirect evidence that the funds were not used as the federal government intended.

[41]Spending and the allowance are in 2012 dollars (in levels). Consistent with the majority of the analysis, I restrict the sample to include the bottom 1994 revenue districts from 1995 to 2003, and include the vector of district-year level covariates. I weight by the number of students enrolled in the school-year.

[42]This point estimate is slightly smaller than the 87 cents estimated for this sample using district-level expenditures (Table 2, column 6), but it is important to note that some district spending is not reported at the school level because it is not school-specific (e.g., the superintendent's salary).

[43]This does not simply reflect differences in spending levels across school type, as mean spending in elementary, middle, and high schools is similar.

[44]To ensure that I capture a district's decision of whether to allocate funds differentially across Title I and non-Title I schools, I restrict the sample to districts that contained both types of schools. These represent 36% of school-years in the sample.

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. While the difference in the effect of the allowance on spending is less stark than with the Title I split, there is again evidence that districts strategically allocated the additional dollar toward the low poverty schools.[45,46] Thus, it seems that the heterogeneity by student poverty status in the effects of spending on postsecondary attainment was at least partially due to the relatively lower share of increased spending that reached schools serving primarily poor students.

Finally, I examine whether districts spent the additional dollar disproportionately on recently low-performing schools (columns 9 and 10). I group schools by whether they are below or above the within-district median school fraction proficient on the state math exam in the previous year.[47] Districts spent the additional dollar increase in the allowance more on the schools that were relatively low performing within the district in the previous year. While this may seem at odds with the spending being allocated more to relatively low poverty schools within the district, note that poverty and proficiency status are not perfectly correlated. The results suggest that districts targeted money to-

[45]I split schools into the bottom quarter and top three quarters in order to obtain a similar sample size split as is observed in the student-level data when examining effects by student free lunch status. If I alternatively divide the sample into those schools above and below the median to approximate the Title I/non-Title I split, the pattern is the same, though slightly attenuated (0.636 and 0.757 for the high and low poverty schools, respectively).

[46]One concern could be that this pattern of results is due to the previous result that high schools experience larger effects on spending combined with the fact that high schools typically have a lower fraction free lunch than elementary schools. This is not the case; when I estimate results for elementary schools only, the patterns by Title I status and school poverty remain.

[47]As eleventh grade assessment scores are not available during the earlier years of the sample, I restrict the analysis to elementary and middle schools. I use the average proficiency rate across grades four and seven (the two tested grades in math) for schools serving both grades.

ward schools serving less poor populations and that had room for improvement in test scores.

5 Comparison to Previous Literature

5.1 Magnitude of the Effects

Both Papke (2005) and (2008) found that a 10% increase in spending led to nearly a 4 percentage point increase in the fraction of students scoring proficient on the fourth grade math test. More recent papers in Michigan found even larger positive effects on fourth grade scores, but no achievement effects in later grades (Chaudhary, 2009; Roy, Forthcoming). The present paper shows that the early effects on achievement reappear as improvements in long-run measures of educational attainment. A variety of studies examining other educational 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 et al., 2013b; Chetty et al., 2013; Heckman et al., 2013). The results in the present paper reveal the same pattern for the effects of spending increases due to Proposal A.

Jackson et al. (2014) shows that a 20% increase in per-pupil funding in all twelve grades of public school leads to 0.9 additional years of completed spending. The increases in postsecondary enrollment and degree receipt seen in the present paper are almost certainly larger in magnitude given that they are from a 12% increase in spending over four grades.

While no other studies examine the long-run effects of school spending, several recent studies have examined the educational attainment effects of specific inputs to the education production function, such as class size and teacher

quality (Dynarski et al., 2013b; Fredriksson et al., 2013; Chetty et al., 2013). Taken together, these studies show that changes to specific inputs, estimated on similar grade ranges as the present paper, result in postsecondary effects that are in the same ballpark as the effects estimated in the present paper. However, to truly compare the magnitude of these effects, the most relevant benchmark would compare effects on postsecondary attainment, as well as the costs of the treatment that produced these effects.

5.2 Cost-Benefit Comparison

To examine the relative cost-effectiveness of the gains in postsecondary attainment due to increased school spending observed in the present paper, I compare the costs and benefits to those from other education policies that have been shown to increase educational attainment. I create an index of cost-effectiveness by dividing a policy's cost by the proportion of students it induces into college.[48] For example, assuming a cost in 2012 dollars of $4,000 ($1,000 per student over four grades) for the spending increases estimated in the present paper, and a 4 percentage point increase in the rate of college entry, the amount of money spent to induce one additional child into college is $100,000 (=$4,000/0.04).[49]

Several other policies aim to alter the human capital of students, and similarly cost in the hundreds of thousands of dollars to induce one additional student into college. For example, given the effects on college enrollment estimated in Deming (2009), Head Start has a cost per student induced into

[48] I focus on the college enrollment effects instead of the degree receipt effects, because only a small number of studies have examined effects on degree receipt. I exclude Jackson et al. (2014) from this analysis because the authors do not estimate effects on the probability of college enrollment.

[49] One way to think of this calculation is as follows: if 100 students are treated with the additional $4,000 of spending, four of them will be induced to attend college at a total cost of $400,000 (=$4,000 x 100). Thus, the cost per student induced into college is $100,000 (=$400,000/4).

college of $133,000 (=$8,000/0.06). The cost per student induced into college from the class size decrease in the Tennessee STAR experiment is substantially larger: $400,000 (=$12,000/0.03) (Dynarski et al., 2013b).

Other policies aim specifically at boosting college enrollment by either reducing the price of college or dismantling administrative barriers to enrollment. For example, Dynarski (2003) showed that it takes approximately $21,000 of aid to induce a single student into college, including the aid spent on students who would have enrolled regardless. Bettinger et al. (2012) randomly offered families at H&R Block assistance filling out the FAFSA, finding a cost per student induced into college of $1,100 (=$88/0.08). Hyman (2013) evaluated the effect of Michigan's recent requirement that all students take the ACT college entrance exam, a policy that is being operated at scale in a dozen states, finding a cost of $8,333 (=$50/0.006) to induce an additional student into college.[50]

Given that these estimated costs per student induced into college do not reflect the statistical precision of the enrollment effects, and that the interventions earlier in students' lives may have impacts beyond those on postsecondary attainment, these comparisons are best viewed as rough approximations. Nonetheless, these back-of-the-envelope calculations suggest that the postsecondary attainment effects estimated in this paper are within the same general magnitude of cost/benefit as other policies targeting the accumulation of students' human capital during childhood.

6 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

[50] One caveat to the analyses in this section is that the marginal student may vary across studies: the student who is induced to attend college through assistance filling out the FAFSA may differ from the student who is induced in from smaller classes. The above differences in cost/benefit could reflect that it is more difficult to induce certain students.

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. 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 a student's college entry and degree completion behavior much later in life. Further, such data also allow for methodological improvements over previous studies by controlling for a student's standardized test scores prior to the spending increases, and by tracking individual students across schools and grades, reducing measurement error in the spending variable.

I find that additional spending led to increases in rates of college entry and degree completion, by 3.9 and 2.5 percentage points, respectively. To determine the mechanisms through which spending affected long run attainment, I examine the behavioral responses of districts to the imposed spending increases. I find that districts spent proportionally more of the additional dollar on administration than instruction relative to the base spending levels in these categories. Thus, the spending increases lowered class sizes and raised teacher salaries slightly, but substantially decreased the ratio of pupils to administrators. The effects of spending on postsecondary outcomes appear to have been stronger for non-poor students. I find that one possible explanation for this result is that districts spent the additional dollar primarily on schools serving less poor populations within the district. The cost/benefit of the spending increases at boosting postsecondary attainment is similar to other policies, such

as Head Start, which aim to improve students' human capital.

Given the debate surrounding the effects of school resources on student outcomes (see Hanushek, 2003; Krueger, 2003), 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 the long-run outcomes of students that are of ultimate concern to policy-makers. However, as found in other recent studies (e.g., Cascio et al., 2013) it also provides suggestive 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.

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Table 1. Sample Means of Michigan Fourth Grade Cohorts

	All Districts and Cohorts	All Cohorts, by 1994 District Revenue		All Districts	
		Bottom Half	Top Half	1995 Cohort	2000 Cohort
	(1)	(2)	(3)	(4)	(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 Twelve					
Observed	0.725	0.757	0.711	0.707	0.739
Observed 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	0.162	0.153	0.166	0.168	0.157
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
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 1994-95 through 1999-2000. Free lunch, special education, and limited English proficiency status are measured during grade twelve. College enrollment and degree receipt include any postsecondary institution and are measured within two and five years, respectively, after scheduled on-time high school graduation based on fourth grade cohort year.

Table 2. First Stage: The Effect of the Foundation Allowance on Operating Expenditures

	All Districts					Bottom Half of Districts by 1994 Revenue	
	1995-2010	1995-2010	1995-2010	1995-2003	2004-2010	1995-2003	2004-2010
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Operating Expenditure	0.600***	0.417***	0.575***	0.610***	0.178	0.872***	-0.163
	(0.078)	(0.117)	(0.071)	(0.062)	(0.309)	(0.086)	(0.335)
Mean Dep. Var. (2012 $)	9,247	9,247	9,247	9,185	9,326	8,455	8,799
N (District-Years)	8,280	8,280	8,280	4,660	3,620	2,329	1,813
Control for 1994 Revenue	Y	N	N	N	N	N	N
District Fixed Effects	N	Y	Y	Y	Y	Y	Y
Student Weighted	N	N	Y	Y	Y	Y	Y

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

*** = significant at the 10% level, ** = 5% level, * = 1% level.

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)	Proportional Effect (1) / (3) (4)
Operating Expenditure	0.872***	8,455	1.000	0.872***
	(0.086)			(0.086)
Instruction	0.490***	5,502	0.651	0.753***
	(0.069)			(0.106)
Total Non-Instruction	0.382***	2,953	0.349	1.095***
	(0.050)			(0.143)
Instructional Support	0.064**	632	0.075	0.858**
	(0.030)			(0.408)
Administration	0.168***	1,027	0.121	1.382***
	(0.032)			(0.264)
Operations and Maintenance	0.095***	854	0.101	0.937***
	(0.023)			(0.228)
Transportation	0.056***	440	0.052	1.071***
	(0.015)			(0.285)

Notes: The sample is at the district-year level and includes only districts in the bottom half of the 1994 revenue distribution during 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). Each point estimate in column 4 is the column 1 coefficient divided by the fraction of operating expenditure accounted for by that category (column 3). All regressions contain district and year fixed effects, and are student-weighted. Standard errors, in parentheses, are clustered at the district level.
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Table 4. The Effect of Spending on Educational Attainment

	All Districts				Bottom Half 1994 Revenue Districts			
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll in Postsecondary Schooling	0.015	0.003	0.025**	0.033**	0.026*	0.036**	0.039**	0.039*
	(0.018)	(0.019)	(0.011)	(0.015)	(0.015)	(0.016)	(0.018)	(0.022)
		0.448				*0.431*		
Earn a Postsecondary Degree	0.021***	0.019**	0.019*	0.016	0.021*	0.027**	0.025*	0.025
	(0.008)	(0.009)	(0.011)	(0.014)	(0.011)	(0.012)	(0.014)	(0.016)
		0.162				*0.153*		
Sample Size		746,834				220,720		
Mean Spending (2012 $)		9,797				8,418		
First Stage F-Statistic	132	132	162	95	109	109	102	83
District & Cohort Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Student Demographics	Y	Y	Y	Y	Y	Y	Y	Y
Student Fourth Grade Scores	N	Y	Y	Y	N	Y	Y	Y
District-Cohort Covariates	N	N	Y	Y	N	N	Y	Y
Trend * District-Cohort Covariates	N	N	N	Y	N	N	N	Y

Notes: The sample is all first-time fourth graders in Michigan public (non-charter) schools during 1994-95 through 1999-2000. Columns 5-8 restrict the sample to students in the bottom half of districts by 1994 district revenue. Each coefficient is from a separate 2SLS regression of the dependent variable on average real spending during grades 4-7 (in thousands of 2012 dollars). The instrument is the average allowance during those grades (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.
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Table 5. Exploring Mechanisms: The Effects of Spending on Inputs to Education Production

			Pupil / Administrator Ratio	
	Class Size	Average Teacher Salary	School and District Admin.	District Admin. Only
	(1)	(2)	(3)	(4)
Operating Expenditure	-1.215***	2,632***	-8.12	-52.59*
	(0.270)	(965)	(7.17)	(31.22)
Dependent Variable Mean	21.6	65,972	114	420
District & Cohort Fixed Effects	Y	Y	Y	Y
Student Demographics	Y	Y	Y	Y
Student Fourth Grade Scores	Y	Y	Y	Y
District-Cohort Covariates	Y	Y	Y	Y

Notes: The sample is all first-time fourth graders graders in Michigan public (non-charter) schools during 1994-95 through 1999-2000 in the bottom half of districts by 1994 district revenue. Each coefficient is from a separate 2SLS regression of the education input on average real spending during grades 4-7 (in thousands of 2012 dollars). The instrument is the average allowance during those grades (also in thousands of 2012 dollars). Standard errors, in parentheses, are clustered at the district level.

*** = significant at the 10% level, ** = 5% level, * = 1% level.

Table 6. Heterogeneity of Effects by Student Characteristics

			Grade 4 Math Score			
	Male	Female	<Median	>Median	Free Lunch	Non Free Lunch
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Enroll in Postsecondary Schooling	0.039*	0.036	0.047**	0.036	-0.024	0.040
	(0.022)	(0.023)	(0.022)	(0.025)	(0.039)	(0.028)
	0.388	*0.477*	0.322	0.566	*0.358*	*0.556*
Earn a Postsecondary Degree	0.024*	0.027	0.009	0.046**	-0.018	0.026
	(0.014)	(0.018)	(0.013)	(0.021)	(0.023)	(0.023)
	0.126	*0.182*	0.082	0.231	*0.081*	*0.213*
First Stage F-Statistic	97	105	91	106	92	83
N (Students)	112,860	107,357	108,981	106,664	35,211	125,173
District & Cohort Fixed Effects	Y	Y	Y	Y	Y	Y
Student Demographics	Y	Y	Y	Y	Y	Y
Student Fourth Grade Scores	Y	Y	Y	Y	Y	Y
District-Cohort Covariates	Y	Y	Y	Y	Y	Y

Notes: The sample and estimating equation are the same as in column 7 of Table 4. Thus, only districts in the bottom half of the 1994 revenue distribution are included. Standard errors, in parentheses, are clustered at the district level. Means of the dependent variable are in italics below the standard errors. The sum of the sample sizes across groups does not equal 220,720 due to missing demographic and test score data.
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Table 7. Do Districts Spend the Additional Dollar More on Certain Types of Schools?

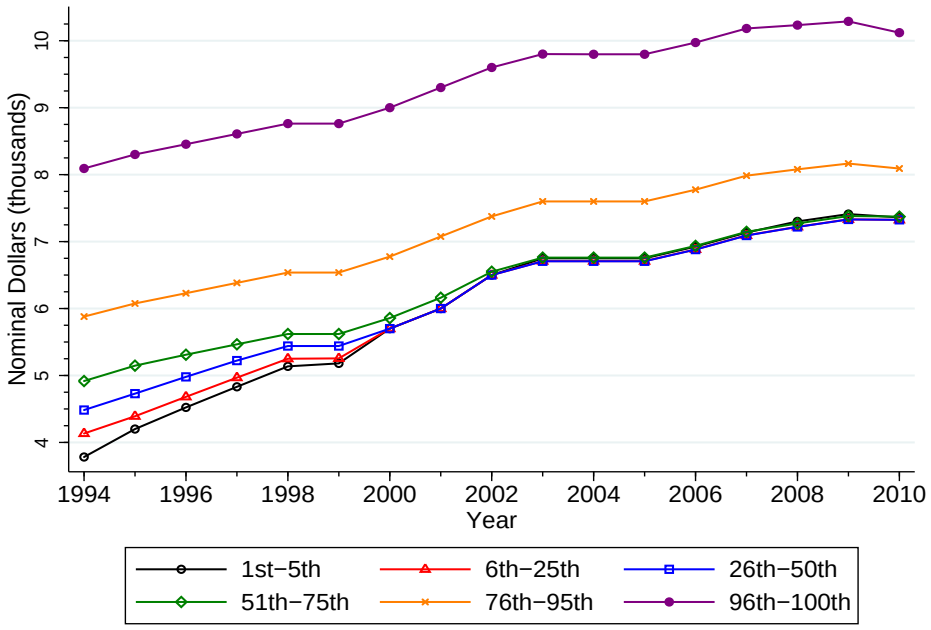
		By Level			Title I Status		Within District Poverty		Within District Fraction Proficient	
	All Schools	Elem.	Middle	High	Yes	No	Poorest Quarter	Least Poor 3/4	<Median	>Median
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
School Operating Expenditure	0.792***	0.634***	0.890***	1.025***	0.240	0.571**	0.558**	0.822***	0.718***	0.387
	(0.137)	(0.205)	(0.284)	(0.238)	(0.345)	(0.230)	(0.264)	(0.156)	(0.276)	(0.240)
Mean Dep. Var. (2012 $)	6,085	6,087	6,045	6,092	6,277	6,127	6,187	6,053	6,140	6,006
N (School-Years)	8,055	4,473	1,404	1,935	1,449	1,467	2,158	5,726	2,364	2,224
District & Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District-Year Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Student-Weighted	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The sample is at the school-year level. It includes years 1995-2003 and only schools in districts in the bottom half of the 1994 revenue distribution. 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.

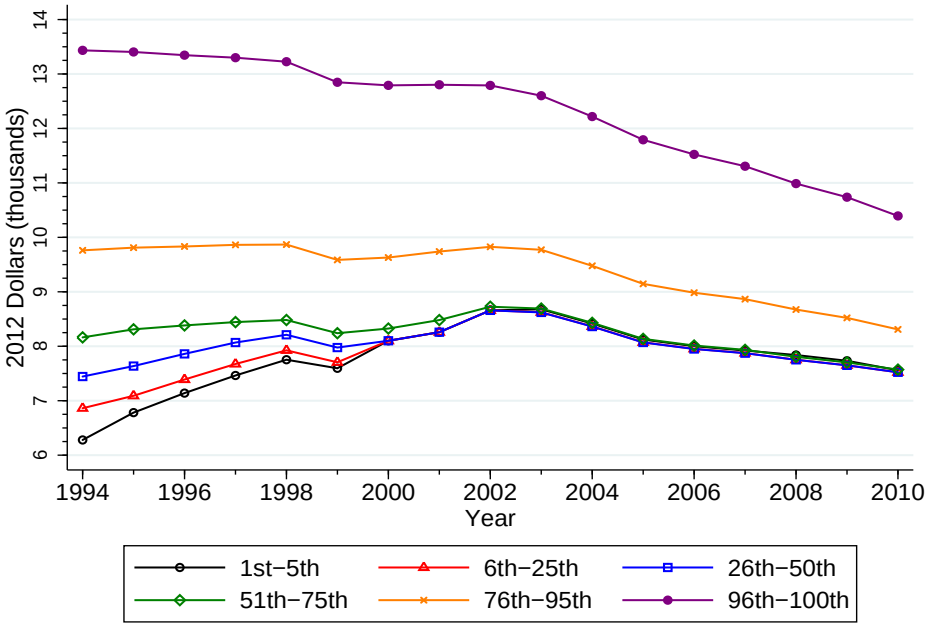
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Figure I: Foundation Allowance Over Time by 1994 Revenue Percentile

(a) Nominal Dollars

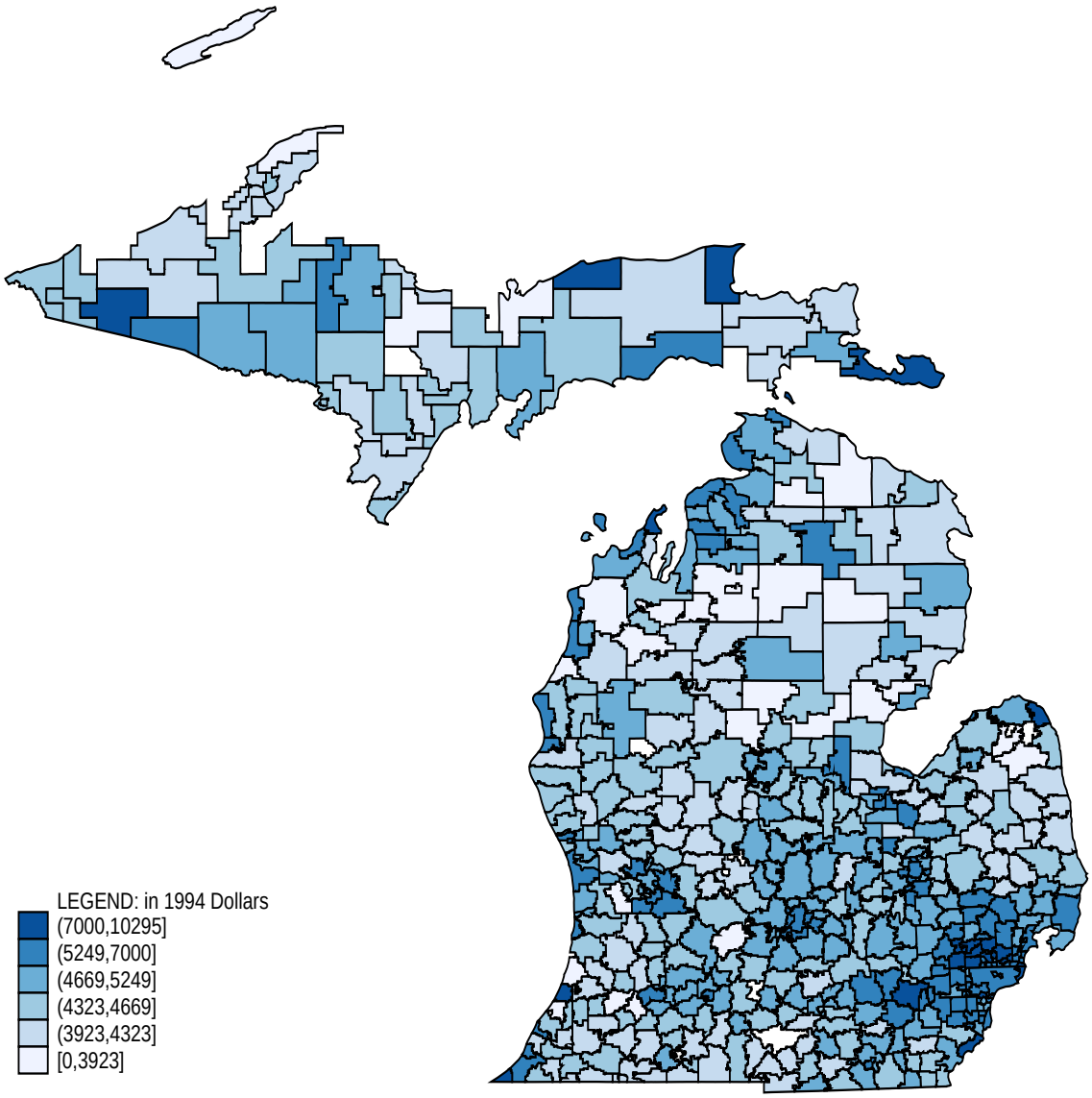


(b) 2012 Dollars



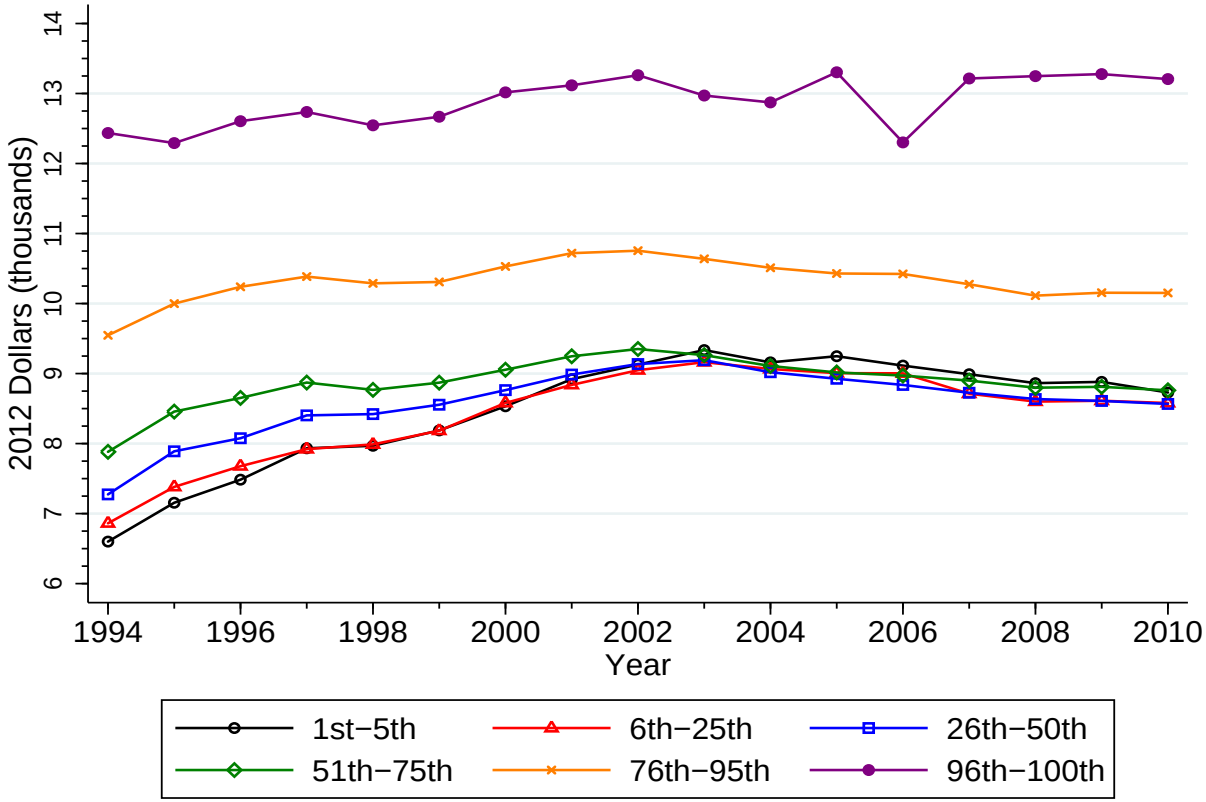
Notes: Figures show the average foundation allowance over time for districts grouped by 1994 revenue percentiles. Figure (a) uses current nominal dollars and Figure (b) uses real 2012 dollars deflated using the Employment Cost Index for elementary and secondary school employees provided by the Bureau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

Figure II: 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 I (e.g., 1st-5th, 6th-25th, etc.).

Figure III: Per-Pupil Operating Expenditures Over Time by 1994 Revenue Percentile



Notes: Figure plots average per-pupil operating expenditures over time for districts grouped by 1994 revenue percentiles. Expenditure is in 2012 dollars deflated using the Employment Cost Index for Elementary and Secondary School Employees provided by the Bureau of Labor Statistics.

A Appendix

A.1 Data Sources

All student-level microdata and school- and district-level school finance data used in this paper were provided by the Michigan Department of Education (MDE), Center for Educational Performance and Information (CEPI), Michigan Consortium for Educational Research (MCER), and Michigan Senate Fiscal Agency.

In addition to the microdata and school finance information, I assembled a longitudinal, district-level dataset including several variables characterizing local school choice, demographic, and economic conditions. The school choice variables include: a) percent of students living in the district who attend a charter school; b) percent of students living in the district who use inter-district school choice to attend a traditional public school in another district; c) percent of students attending a traditional public school in the district who live in another district (i.e., gains from inter-district choice); d) number of charter schools located in the district; and e) number of charter schools located in the district and adjoining districts. The first three variables are constructed using information from CEPI's Public Student Headcount Data and CEPI's Nonresident Student Research Tool. The last two variables are constructed using charter school addresses and school district geographic boundaries.[51]

The district-level variables characterizing demographic and economic conditions are: a) population per square mile in the district (i.e., population density); b) fraction of 5–17 year olds living in poverty in the district; c) local median household income (in 2012 dollars); d) fraction of students attending school in the district who are black; e) fraction of students attending school in the district

[51]Thank you to Francie Streich, Brian Jacob, and Tamara Wilder Linkow for providing the school choice variables for use in the present paper.

that are eligible for free lunch; and f) local average unemployment rate. The fraction of a students in the district who are black and fraction eligible for free lunch come from the National Center for Education Statistics (NCES) Common Core of Data (CCD). School district population and poverty counts are from the Census Small Area Income and Population Estimates (SAIPE). Median income information is also from SAIPE, but only available at the county level (there are 83 counties as opposed to the 518 districts in my sample). School district square mileage used to calculate population density is from CEPI. 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 for August through July. If more than half of the students in a district attend school in a city for which the rate is available, then I used 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 I used the county unemployment rate.

A.2 Replication of Papke (2008)

As a first step toward estimating the long-run effects of school spending in Michigan using student-level data, I replicate previous work examining the short-run effects on achievement using group-level (i.e., school- or district-level) data. I then examine the sensitivity of the estimates to concerns regarding omitted factors that were changing over this time period in Michigan and could have affected districts differentially by 1994 revenue. Finally, using my student-level data and several strategies to alleviate these concerns, I examine effects on student attrition, mobility, and achievement in later grades.

I begin my analysis of the causal impact of spending on educational outcomes by replicating Papke's 2008 study of the effects of spending increases due

to Proposal A on district-level fourth grade test proficiency rates.[52] Her main specification estimates the following equations using two-stage least squares (2SLS):

$$Y_{dy} = \beta_0 + \beta_1 ln(\widehat{Spend})_{dy} + X_{dy} + \alpha_d + \gamma_y + \epsilon_{dy} \tag{3}$$

$$\ln(Spend)_{dy} = \delta_0 + \delta_1 Allow_{dy} + X_{dy} + \lambda_d + \pi_y + \mu_{dy} \tag{4}$$

where Y_{dy} is the fourth grade test pass rate of district d in year y, *Spend* is average real spending in district d in years $y, y-1, y-2$, and $y-3$, X is a vector of district-year level characteristics that includes enrollment and fraction free lunch, *Allow* is the foundation allowance in district d and year y, λ and α are district fixed effects in the first and second stage, respectively, and π and γ are year fixed effects in the first and second stage, respectively.[53] Following Papke (2008), I cluster the standard errors at the district level.

I insert the main results from that study in row 1 of Appendix Table 3 (these results are Papke's estimation of Equations 3 and 4). This is a level-log regression of the fraction passing the fourth grade test on logged spending. The interpretation of the coefficient is that a 10% increase in spending leads to a 3.7 percentage point increase in the fraction passing the fourth grade test (column 1). When I attempt to replicate this analysis, I estimate an effect of 4.0 percentage points.[54] The first stage coefficients (column 3) are also very

[52]Papke (2008) is a follow-up to Papke (2005). Both examine the effects of spending on fourth grade Michigan test scores, but I replicate the former because it uses more years of data (1995–2004) and a longer lag structure of spending.

[53]Papke also reports results from the fixed effects OLS regressions without the instrument. I focus on the IV results for the sake of brevity, but the results for the OLS estimation and their sensitivity to omitted variables are similar.

[54]The 0.3 percentage point difference is likely due to a few minor differences between our data. First, our sample of districts is slightly different. Papke uses the 500 districts that have non-missing covariates in her data, while I have 518 districts with non-missing covariates. I do not know which specific districts are included in her analysis, so I cannot exactly replicate her sample. Second, Papke's data come largely from older data sources (e.g., "Michigan School Reports") that have since been deleted from the Michigan Department of

similar across the two analyses.

The use of the allowance as an IV mitigates some key concerns with identification. However, there are a number of factors changing in Michigan over the sample period that could confound this estimation strategy. In order to examine whether this is the case, I run a handful of falsification checks in which I re-estimate Equations 3 and 4, replacing the achievement dependent variable with a district-year level covariate. Ideally, the effect of spending on these characteristics would be zero or small, given that it is relatively unlikely that an increase in school spending would cause large changes in school choice, demographic, and economic characteristics of the district.

I find large and precisely estimated point estimates, suggesting that the specification is flawed. There are large negative "effects" of spending on the percentage of students living in the district attending a charter school. This suggests that districts experiencing the largest relative increases in the allowance are those experiencing the smallest relative increases in charter school attendance. This is consistent with increases in charter schools over the period occurring among the urban, high 1994 revenue districts. Spending is related positively to district density, which is consistent with high 1994 revenue districts experiencing population declines during this period. Finally, the fraction of children in the district living in poverty, and the unemployment rate, are both precisely and negatively associated with spending, suggesting that the low 1994 revenue districts were gaining economically relative to the median district concurrent with the relative growth in their allowance.

Education website. The data may have been changed or corrected over the years, and may be slightly different than the sources from which I obtained my data. For spending, I used total current operating expenditures from the Bulletin 1014 Form (http://www.michigan.gov/mde/0,1607,7-140-6530_6605-21514--,00.html). Similarly, instead of using district-level test proficiency rates, which have since become unavailable for those years, I used individual test scores aggregated up to the district level, and so this could cause slight differences if the state used different scores in its aggregate reports.

To examine the sensitivity of the results to these omitted variables, I add them to the specification. Because the school choice variables could arguably be affected by spending, I first include the demographic and economic characteristics listed in Appendix A.1. The inclusion of these variables reduces the point estimate from 3.97 to 3.14 percentage points. When I additionally include the school choice variables, the point estimate drops very slightly to 2.95 points. Finally, I include the demographic and economic characteristics in 1995 interacted with a quadratic time trend, to allow for differential trending of the outcome variable by districts with different baseline values of these covariates.[55] The point estimate is attenuated further to 2.18 percentage points (bottom row).[56] The effect is no longer statistically significant at conventional levels, and the drop by nearly half after the inclusion of these controls is concerning, suggesting that further omitted variables could be continuing to produce an upward biased estimate.

As a next step, I use my student-level data to examine the effect of spending on achievement. Appendix Table 4 reports results from estimating Equations 1 and 2. Because I want to control for fourth grade test scores as a measure of prior achievement, I examine effects on seventh grade and eleventh grade scores. However, a substantial fraction of the sample leaves Michigan public schools prior to these grades. Before examining the effects on test scores, I examine whether the increases in allowance-induced spending are associated with student attrition and mobility. Row 1, column 1, shows that there is zero relation between the spending increases and the probability of being observed in seventh grade. Adding fourth grade test scores, district-cohort level covariates,

[55] I do not interact the 1995 school choice variables because they were all zero during 1995.

[56] In the penultimate row, for the sake of completeness I report the coefficient from including a district-specific linear time trend given that this is a sensible specification to attempt. However, the district-specific trends completely eliminate the first stage.

and the interaction of the covariates with the quadratic cohort trend do little to affect the point estimate.

The probability of being observed in grade eleven, on the other hand, does have a statistically significant relationship with spending. A $1,000 increase in spending during grades four, five, six, and seven is associated with a 2.8 percentage point increased probability of being observed in eleventh grade using the preferred specification (column 4). There is a similarly sized point estimate for the probability that a student is observed in grade eleven in his or her fourth grade district. The association between spending increases and student attrition and mobility suggests another reason why the results from previous studies examining the effects of lagged spending in a district on contemporaneous achievement in the district may be biased.

The bottom rows of Appendix Table 4 provide the estimated effects of spending on achievement for the sample of students who have non-missing test scores during grades seven and eleven. The results are quite sensitive to which controls are included; however, the preferred specification shows that there is near zero effect of spending on seventh grade scores, but a marginally statistically significant effect of 3.2 percentage points on eleventh grade scores.

Appendix Table 1. Foundation Allowances, 1995-2010

	Minimum	% at Minimum	Target	% Below Target
1995	4,200	5.7	5,000	55.5
1996	4,506	5.7	5,153	51.1
1997	4,816	5.7	5,308	46.4
1998	5,124	5.7	5,462	36.1
1999	5,170	8.4	5,462	36.1
2000	5,700	55.7	5,700	0
2001	6,000	55.5	6,000	0
2002	6,300	55.5	6,300	0
2003	6,700	64.9	6,700	0
2004	6,700	64.9	6,700	0
2005	6,700	64.7	6,700	0
2006	6,875	64.7	6,875	0
2007	7,085	64.7	7,085	0
2008	7,204	62.6	7,204	0
2009	7,316	62.6	7,316	0
2010	7,316	62.4	7,316	0

Notes: Allowance is in nominal dollars.

Appendix Table 2. The Effect of Log Spending on Postsecondary Attainment

	All Districts				Bottom Half 1994 Revenue Districts			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll in Postsecondary Schooling	0.053	-0.016	0.166**	0.188*	0.203*	0.280**	0.305**	0.307*
	(0.156)	(0.157)	(0.078)	(0.107)	(0.120)	(0.125)	(0.142)	(0.164)
		0.448				*0.431*		
Earn a Postsecondary Degree	0.129**	0.093*	0.083	0.040	0.162*	0.197**	0.192*	0.197*
	(0.051)	(0.055)	(0.069)	(0.087)	(0.083)	(0.091)	(0.105)	(0.119)
		0.162				*0.153*		
F-Statistic	287	288	334	186	132	132	117	96
District & Cohort Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Student Demographics	Y	Y	Y	Y	Y	Y	Y	Y
Student Fourth Grade Scores	N	Y	Y	Y	N	Y	Y	Y
District-Cohort Covariates	N	N	Y	Y	N	N	Y	Y
Trend * District-Cohort Covariates	N	N	N	Y	N	N	N	Y

Notes: The sample is all first-time fourth graders in Michigan public (non-charter) schools during 1994-95 through 1999-2000. Columns 5-8 restrict the sample to students in the bottom half of districts by district 1994 revenue. Each coefficient is from a separate 2SLS regression of the dependent variable on average real spending (in logs) during grades 4-7. The instrument is the average logged allowance during those grades. As an example of how to interpret the coefficients, row 1 column 8 shows that a 10% increase in spending leads to a 3.07 percentage point increase in the probability of enrolling in postsecondary school. Standard errors, in parentheses, are clustered at the district level. Dependent variable means are in italics below the standard errors.
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Appendix Table 3. Replication of Papke (2008) with Falsification and Sensitivity Checks

		Dep. Var. Mean	First Stage	
			Coef.	F-Stat.
	(1)	(2)	(3)	(4)
Dep. Var. = Frac. Pass 4th Grade Math Test				
Papke (2008)	0.368***	NA	0.768	369
	(0.078)			
Replication	0.397***	0.678	0.758	419
	(0.090)			
Replication Specification, Dep. Var. =				
Percent of Students Living in District Attending Charter School	-9.536***	0.943	0.758	419
	(2.412)			
Population per Square Mile	63.788***	81.624	0.758	419
	(14.592)			
Fraction Black in School	-0.218***	0.055	0.758	419
	(0.036)			
Fraction of 5 - 17 Year Olds in Poverty	-0.063**	0.119	0.758	419
	(0.026)			
Unemployment Rate	-5.800***	5.746	0.758	419
	(1.210)			
Dep. Var. = Frac. Pass 4th Grade Math Test, Replication Specification Plus:				
District-Year Demographic and Economic Covariates	0.314***	0.678	0.745	397
	(0.102)			
District-Year Demographic, Economic, and School Choice Covariates	0.295***	0.678	0.758	373
	(0.100)			
All Covariates Plus District-Specific Linear Time Trends	3.764	0.678	-0.038	0.35
	(6.41)			
All Covariates Plus Interactions with Quadratic Time Trends	0.218	0.678	0.692	254
	(0.135)			

Notes: Sample is at the district-year level and includes 518 districts in 1995 through 2004 (5,180 observations). Each point estimate is from a separate two-stage-least-squares (2SLS) regression of the fourth grade pass rate on the average of the contemporaneous, one, two, and three year lagged logged spending, covariates (logged enrollment and fraction free lunch), and year and district fixed effects. The average spending variable is instrumented for by the log of the foundation allowance in that district-year. Standard errors, in parentheses, are clustered at the district level.

*** = significant at the 10% level, ** = 5% level, * = 1% level.

Appendix Table 4. The Effects of Spending on Attrition, Mobility, and Achievement Using Student-Level Data

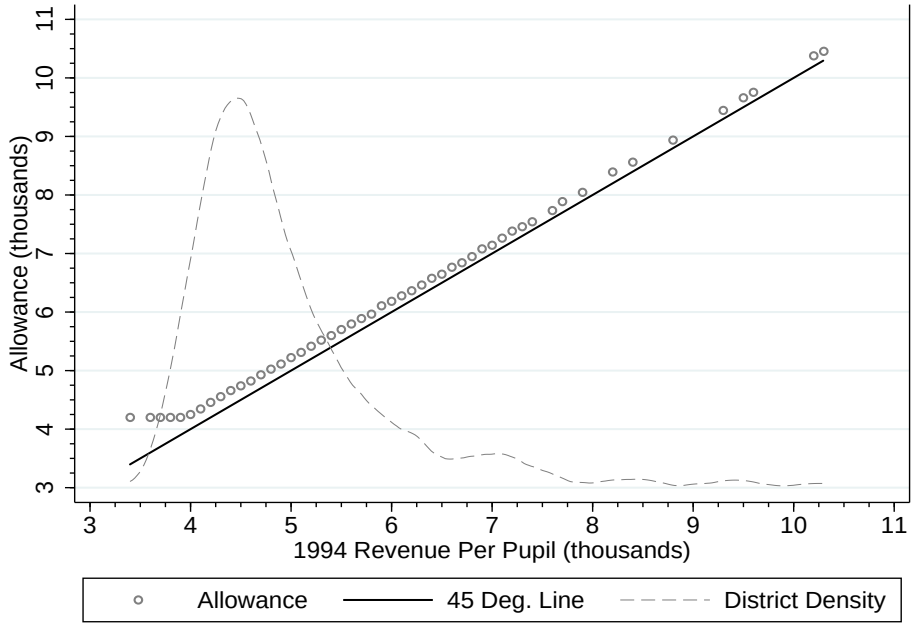
					Dep. Var. Mean	Sample Size
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Attrition						
Observed in Grade Seven	0.005	0.003	0.006	-0.003	0.861	746,834
	(0.009)	(0.009)	(0.011)	(0.015)		
Observed in Grade Eleven	0.065***	0.061***	0.040***	0.028*	0.742	746,834
	(0.012)	(0.012)	(0.012)	(0.016)		
Mobility (Observed in Fourth Grade District:)						
In Grade Seven	0.029**	0.026*	0.006	-0.003	0.733	746,834
	(0.014)	(0.014)	(0.013)	(0.017)		
In Grade Eleven	0.078***	0.074***	0.023*	0.025	0.572	746,834
	(0.019)	(0.019)	(0.013)	(0.018)		
Achievement						
Seventh Grade Math Percentile	2.860**	1.507	0.324	0.242	50.0	613,104
	(1.294)	(1.465)	(1.387)	(1.986)		
Eleventh Grade Math Percentile	3.145***	1.137	1.346	3.236*	49.7	472,822
	(0.988)	(0.954)	(1.214)	(1.721)		
District & Cohort Fixed Effects	Y	Y	Y	Y		
Student Demographics	Y	Y	Y	Y		
Student Fourth Grade Scores	N	Y	Y	Y		
District-Cohort Covariates	N	N	Y	Y		
Trend * District-Cohort Covariates	N	N	N	Y		

Notes: The sample is all first-time fourth graders in Michigan public (non-charter) schools during 1994-95 through 1999-2000. Each coefficient is from a separate 2SLS regression of the dependent variable on average real spending during grades 4-7 (in thousands of 2012 dollars). The instrument is the average allowance during those grades (also in thousands of 2012 dollars). Mean spending during grades 4-7 for all samples in this table is approximately $9,800. Standard errors, in parentheses, are clustered at the district level. First stage F-statistics are between 84 and 162 depending on the sample and specification.

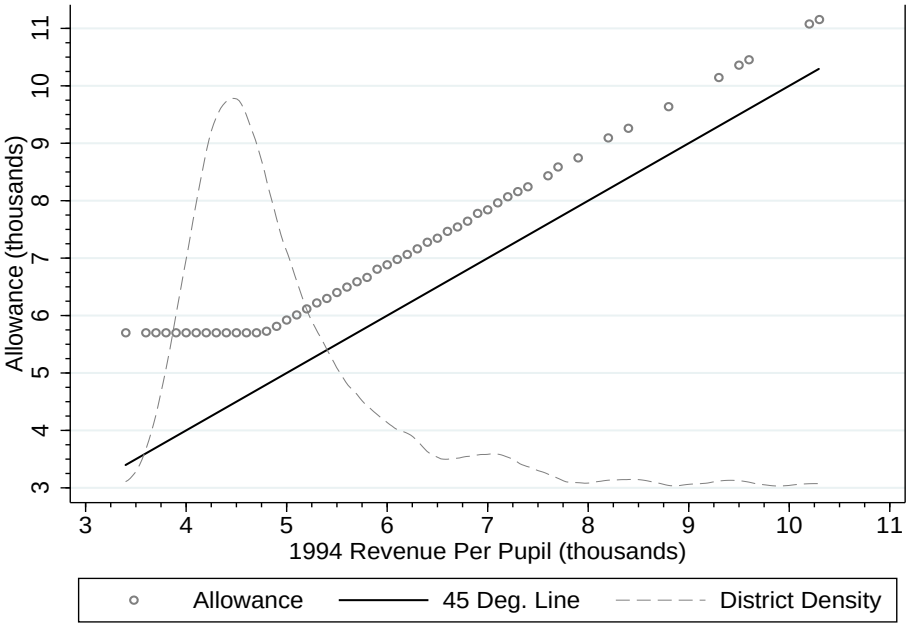
*** = significant at the 10% level, ** = 5% level, * = 1% level.

Appendix Figure I: Foundation Allowance in 1995 and 2000, by 1994 District Revenue

(a) 1995



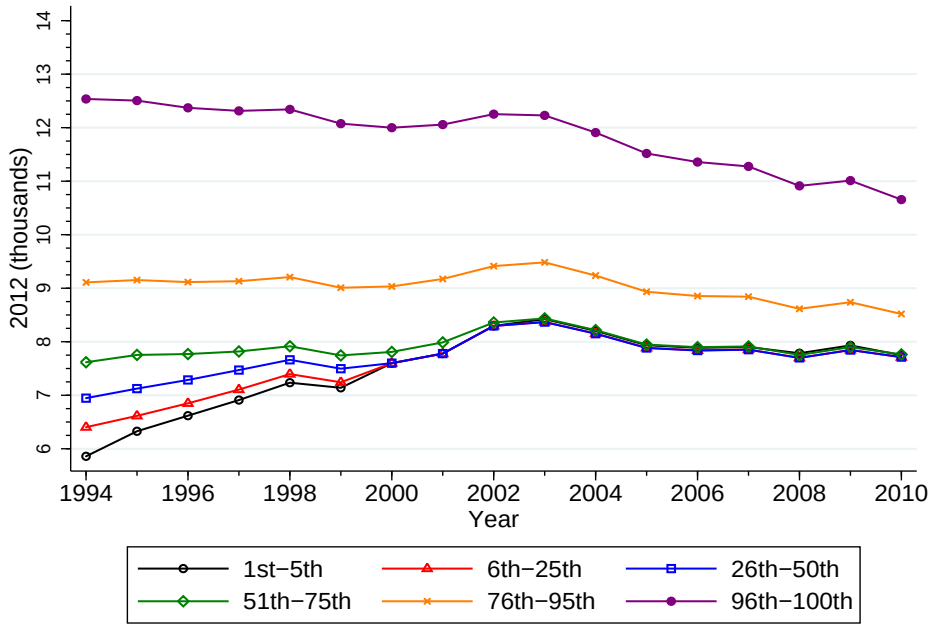
(b) 2000



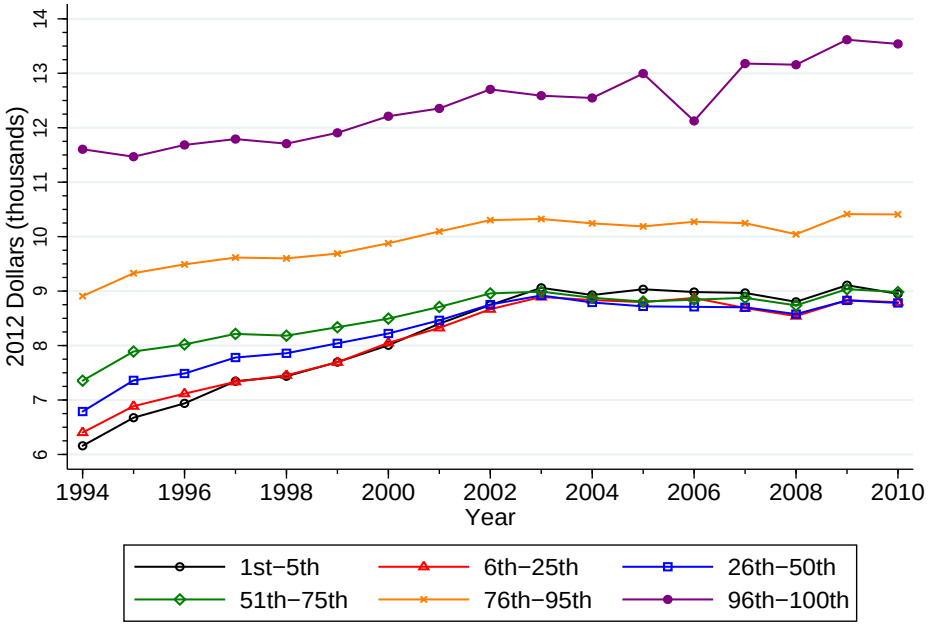
Notes: Figures show the average foundation allowance for districts in $100 bins of 1994 revenue. The dashed line gives the density of the number of districts in each bin. Figure (a) shows that the allowance was equalizing in its first year mostly through boosting revenue for the lowest districts. Figure (b) shows that the allowance was further equalizing over time, by bringing more districts into the flat portion of the allowance curve. All dollars are in nominal dollars.

Appendix Figure II: CPI-Adjusted Allowance and Expenditures Over Time by 1994 Revenue

(a) Foundation Allowance



(b) Operating Expenditures



Notes: Figures show the average foundation allowance (a) and average per-pupil operating expenditures (b) over time for districts grouped by 1994 revenue percentiles. Both figures use real 2012 dollars deflated using the Consumer Price Index (CPI). The 1994 value (pre-Proposal A) in Figure (a) is the district's 1994 revenue.