ANNENBERG
BROWN UNIVERSITY

EdWorkingPaper No. 19-58

# The Effect of Increased Funding on Student Achievement: Evidence From Texas's Small District Adjustment

Daniel Kreisman
Georgia State University

Matthew P. Steinberg
George Mason University

We leverage an obscure set of rules in Texas's school funding formula granting some districts additional revenue as a function of size and sparsity. We use variation from kinks and discontinuities in this formula to ask how districts spend additional discretionary funds, and whether these improve student outcomes. A $1,000 annual increase in foundation funding, or 10% increase in expenditures, yields a 0.1 s.d. increase in reading scores and a near 0.08 increase in math. In addition, dropout rates decline, graduation rates marginally increase, as does college enrollment and to a smaller degree graduation. These gains accrue in later grades and largely among poorer districts. An analysis of budget allocations reveals that additional funding only marginally affects budget shares.

VERSION: May 2019

# The Effect of Increased Funding on Student Achievement: Evidence From Texas's Small District Adjustment

Daniel Kreisman*
Georgia State University
dkreisman@gsu.edu

Matthew P. Steinberg
George Mason University
msteinb6@gmu.edu

May 16, 2019

**Abstract**

We leverage an obscure set of rules in Texas's school funding formula granting some districts additional revenue as a function of size and sparsity. We use variation from kinks and discontinuities in this formula to ask how districts spend additional discretionary funds, and whether these improve student outcomes. A $1,000 annual increase in foundation funding, or 10% increase in expenditures, yields a 0.1 s.d. increase in reading scores and a near 0.08 increase in math. In addition dropout rates decline, graduation rates marginally increase, as does college enrollment and to a smaller degree graduation. These gains accrue in later grades and largely among poorer districts. An analysis of budget allocations reveals that additional funding only marginally affects budget shares.

JEL: H75; I21; I22; I28
Keywords: School finance; Foundation funding; Rural schools; Texas; Sparsity adjustment.

*Corresponding author. We thank Jonathan Smith for accessing College Board data, and Cameron Anglum for research assistance. We also thank two anonymous referees, Jonathan Smith, Ben Keys, and Brian Jacob for comments on the manuscript. Views do not represent those of the College Board.

# 1 Introduction

The extent to which financial resources lead to improvements in educational outcomes is a long-standing area of debate among education policy scholars as well as an issue of pressing importance in the current economic environment of limited state and local support for public schooling. While in general one would expect that increasing educational expenditures should yield improvements in student outcomes, disentangling the close relationship between school spending and district wealth, among other factors, is a difficult task.[1] In the following we bring new evidence to bear on the issue of (i) how school districts spend additional discretionary funding, and (ii) whether the provision of additional funding through state formulae impacts academic achievement and attainment.

To do so we leverage a long-standing rule in the state funding formula for Texas's public schools that grants additional per-pupil allotments to geographically large districts with few students. We exploit the fact that the formula is discontinuous in size, at 300 square miles, and is kinked with respect the number of students, at 1,373. Since the true relationship between size and sparsity and the cost of educating students is in all likelihood smooth, we can exploit the difference between the true smooth relationship and the kinked and discontinuous formula as a source of variation in per-pupil funding. Because this element of the formula determines in large part base per-pupil funding for districts, this variation is meaningful in determining per-pupil revenue and expenditures. Our data allow us to observe districts receiving more than $1,600, or 13%, in additional per-pupil revenue that is arguably unrelated to the true cost of educating students. This is integral to understanding the relationship between funding and achievement.

The 10th Amendment to the U.S. Constitution places plenary authority for education with state governments. However, states have delegated significant responsibility for finance and operations to their local school districts. As a consequence, much of the within-state variation in local education spending reflects the tastes and preferences local communities have for education, as well as a community's resource endowments which reflect, among other things, local property tax wealth and labor market conditions. For the purposes of identifying the causal effect of discretionary funds on educational outcomes, concerns exist that the level of (and changes over time in) school resources are likely correlated with these and other factors at the district level that also affect student achievement. One approach to overcome the endogeneity of district resources is to exploit changes in state funding policies over time, often in response to court orders, or discontinuities in state spending formulas, which attempt to equalize funding across districts that vary in their ability to raise local resources for educational spending. While we refer readers to Jackson (2018) for a full accounting of the literature, a few specific examples are relevant here.

Guryan (2001), for example, exploits one such discontinuity in Massachusetts' education finance equalization scheme, which provided additional resources to low property wealth districts. His findings suggest that a one standard deviation increase in per-pupil spending increases test scores in math, reading, science and social studies from about one-third to about one-half of a standard

[1] See Card and Krueger (1996); Goldhaber and Brewer (1997); Greenwald et al. (1996); Hanushek (1997, 1989); Hoxby (2001).

deviation in 4th grade, although he finds no effects on 8th grade scores. Papke (2005) estimates effects from a discontinuity in Michigan's school finance equalization program designed to increase spending in the least funded districts. She finds that a 10% increase in spending increases the share of students passing a state exam in mathematics by roughly one to three percentage points. Another set of papers estimates effects of school finance reforms across the country on attainment and earnings, finding robust gains in both with effects driven by poorer districts (see Card and Payne, 2002; Jackson et al., 2016; Lafortune et al., 2018).[2] Hyman (2017) bridges these by exploiting changes in Michigan's funding formula and estimating effects on long run outcomes, concluding that a 10% increase in funding led to a 7% increase in college enrollment and 11% increase in college completion, though in this case gains were concentrated among higher achieving and less-poor districts at baseline.

While these previous studies offer a wealth of insight, they are largely limited to evaluating effects of increases in funding in response to inadequate or unequal conditions. Moreover, we are in a unique context of sparse and rural schools, which are overlooked in much of the literature. Hence, while our study is similar to prior work in spirit, it differs in context, allowing us to provide new insights. We highlight four here.

First, we uniquely observe districts that receive additional funding not due to low wealth, but rather due to size and sparsity, irrespective of wealth. Thus we observe both poor and wealthy districts receiving additional funding that is plausibly exogenous, allowing us to ask whether the effect of an additional dollar is the same across these margins. Second, we add evidence from a new state. Prior work has either used survey data from nationwide reforms (e.g. Jackson et al., 2016; Lafortune et al., 2018), or state level data in large part focusing on reforms in Michigan (Chakrabarti and Roy, 2015, 2017; Chaudhary, 2009; Hyman, 2017; Papke, 2005; Roy, 2011), though examples exist in other states as well.[3] Thus, evidence from a new state diversifies our knowledge base in a meaningful way. Third, our study is in the context of small and rural districts, a topic that has received very little attention in the literature (one might see Monk, 1990, or Andrews et al., 2002 for example). In fact, more than half of all school districts in the U.S. are classified as either "distant" or "remote". As populations in the U.S. and elsewhere migrate toward urban centers, policy concerns for rural and sparse school districts become more salient. The literature has not provided much guidance. Fourth, our study observes the long run equilibrium effects of additional funding on district performance and characteristics, driven by a policy change many years ago. Given that policy levers can affect funding but not responses, observing how districts fare in the long run across not only outcomes but also composition provides valuable insight.

Our empirical strategy leverages Texas's funding formula by controlling for smooth functions of size and sparsity in regression models, and observing differences across districts attributable to residual variation in funding resulting from kinks and discontinuities in the formula. To allow for cumulative effects of exposure, we measure the average additional funding students experienced by

[2] See Murray et al. (1998) for nationwide finance reforms and effects on the distribution of spending.

[3] For example, Clark (2003) in Kentucky, Gigliotti and Sorensen (2018) in New York, Guryan (2001) in Massachusetts, Deke (2003) in Kentucky, and Steinberg et al. (2016) in Pennsylvania.

each grade. We find that districts receiving additional funding perform significantly better in both reading and math, and better in terms of high school graduation and dropout rates. We estimate that exposure to an additional $1,000 per year in the base funding level over students' schooling years, or equivalently a 10% increase in expenditures, improves reading scores by almost 0.1 standard deviation and math scores by more than 0.07. Yet, these average gains mask heterogeneity.

When we break out gains by grade, we find that benefits are largely concentrated among later grades, after students have had longer exposure to increased funding. Likewise, when we observe effects by the share of students in a district who are poor or Hispanic, we find that gains are largely concentrated among the poorest and most minority districts (and likewise in later grades for them). We also show an annual $1,000 increase in funding decreases high school dropout rates by almost 2 percentage points, off a base of 4 percent. Again, gains are concentrated in poorer districts. Still, these gains in achievement and attainment are not sufficient to close the level gap between poor districts and their wealthier counterparts. Hence, while increased funding can narrow gaps, it is unlikely that disparities can be entirely eliminated through additional school resources alone.

We also observe long-run student outcomes through access to National Student Clearinghouse records for students in our districts who interacted with the College Board, through the PSAT, SAT, or AP exams. We find little increase in the share of students taking the SAT, but among those who did we find that SAT score gains approximate our results for state standardized tests. An additional $1,000 per year since 3rd grade leads to a 0.1 standard deviation increase in SAT scores, though estimates are noisy, again with larger and statistically meaningful effects in poorer districts. We also observe a 9 percentage point increase in college enrollment, though only a 4 percentage point increase in college graduation.

When we ask how districts spend additional discretionary funds we find that they keep original spending shares largely in tact, with a marginal decrease in the share of funding dedicated to direct instruction in favor of a slight increase in the share of funds allocated toward administration. Thus increasing funding increases levels but does not dramatically change shares. We also find a small but meaningful decrease in the student-teacher ratio, between 5 and 10 percent, which likely contributes to gains among other potential factors. Using historical Census records, we find little evidence of selective migration to districts receiving additional funding, as measured by the share of students who are poor. We do find a slight decrease in the share of students who are Hispanic.

Taken together, while these findings suggest that increased discretionary funds yield meaningful academic gains, supporting the "money matters" camp (Greenwald et al., 1996; Guryan, 2001; Hyman, 2017; Jackson et al., 2016; Lafortune et al., 2018; Papke, 2005; Roy, 2011), they also suggest that additional funding does not generate gains uniformly to all schools, and that districts may be somewhat constrained in how they can shift resources across inputs, especially when faced with high fixed costs, lending some credence to the limited capacity argument (Hanushek, 1986, 2003).

## 2 Data

The majority of data for this project come from publicly available data from the Texas Education Agency (TEA). In few cases, measures were unavailable and had to be requested via email correspondence with the TEA. District revenue and expenditure data come from Texas's Public Education Information Management System (PEIMS) which date back to 1997 with some exceptions. These provide annual district level measures of revenue, by source (Local, State and Federal), including the taxable value of property by category; expenditures, by spending category; district characteristics; and district ADA. Measures of the adjusted allotment, the key determinant of student funding described in detail below, the cost of education index used to adjust for local price variation, and district area were secured through requests to the TEA's reporting division (definitions for each are described in the next section). For our measure of district area, which is a key determinant in the funding formula, we use the first observation made available to us, in 1996, to account for very small changes in district area over time. While in virtually all cases these changes are less than 1 mile in area, likely the result of variability in cartographic measurements by the state, 8 districts crossed the 300 mile threshold since 1996 through consolidation, and a small number of others saw larger changes. We omit the districts crossing the threshold from our sample and instrument for area using the 1996 measure, the earliest available to us. Results are not sensitive to these decisions. Because our test score data described below are limited to 2003-2010, we limit our funding data to the same years though use the full range to calculate funding students received in years before we observe their test scores (for example students who were in 11th grade in 2003).

Outcome and student demographic data come from the Texas Assessment of essential Knowledge and Skills (TAKS) taken from Texas's Academic Excellence Indicator System (AEIS). Outcome data exist for academic years 2003-2010 and provide average scale scores by district and grade for achievement tests in grades 3-11. We focus on Math and Reading tests which are offered consistently to all grades throughout, and limit analysis to academic years 2003-2010 because Texas used different tests before and after these years, and because student-level standard deviations were only available from the Texas Department of Education for these tests in these years. These are necessary to handle variation in the scale and variance of the test over time. We also observe four-year high school dropout and graduation rates for these years as well. Student demographic characteristics are available beginning in 1997.

Because Texas's funding system works differently for charter districts, we omit these from the analysis. We also restrict the sample to the majority of districts with fewer than 5,000 regular ADA, which are not affected by the relevant sparsity and size policies.[4] This results in a sample of 875 school districts.[5] Our analyses on high school graduation and long-run educational outcomes is limited to the 814 of these districts that are K-12 and have their own high schools. Summary statistics for relevant measures are in Table 1.

[4]Regular ADA is the average number of students in a district measured over a six week period, not including special education students.

[5]There are just over 1,100 non-charter districts in Texas across years 2003-2010.

For long-run outcomes we use data from the National Student Clearinghouse for students who interacted with any one of three College Board programs – the PSAT (PSAT/NMSQT), the SAT, and Advanced Placement (AP) exams. These data are available beginning in 2004 and to match our state standardized test data we limit the panel to end with students who take these exams in 2010, which also allows us to observe college graduation rates after 6 years, where these data end in 2016. We likewise collapse this data to the district-cohort level. On average, two-thirds of students in our districts are in the College Board sample through interaction with one of these products.

Last, to address how districts may have changed over time in response to these policies, we take data from two sources. First, we use pre-policy data from the National Institute of Education Special Tabulations and 1970 Census Fifth Count Data File. These Census created tables provide the share of the population ages 6-17 who are poor in each district, according to the official Census definition. Coverage is not complete, and the districts with the fewest number of students in our sample do not have statistics reported by Census due to small sample sizes.[6] Second, we use data from Decennial Censuses 1970-2000 to observe county level changes is poverty status.

## 3 School funding in Texas

Texas funds education through what is commonly referred to as a foundation program. This model of education finance is meant to guarantee similar districts equal per-pupil revenue from equal effort (tax rates) regardless of property wealth. To do so the state first determines the cost of educating each student in a district, which is a function of both student "type", for example regular or special education, and district characteristics, such as sparsity and the cost of living. Then the share of these costs born by the district and the share born by the state are calculated, with the property poorest districts paying the smallest share of their own costs. Finally, districts can raise additional funds by taxing above a set base rate, though these are capped. Educational grants and federal funds are then added on. This system does not apply to capital outlays, which are determined by local bond elections, nor does it apply to transportation costs, which are a separate line item and is of note here as we compare across geographically large and small districts.

Within this formula, students in small and sparsely populated districts are effectively deemed more "expensive" to educate than identical students in otherwise comparable districts. This rule thus *entitles* these small and sparse districts to more funding, per-pupil, than districts with otherwise identical students. We are able to use this rule to better understand the relationship between funding and achievement by exploiting the idea that the funding formula is unlikely to capture perfectly the true relationship between size and sparsity and the cost of education. The genesis of the rule dates back nearly 70 years.

[6] See United States Department of Education (1970) for details of the data.

### 3.1 The small district adjustment

In 1949 Texas instituted a small district adjustment to its funding formula to compensate for what the state calls "diseconomies of scale". The rule provided an increase in the base funding formula for districts with fewer than 1,000 regular program students in average daily attendance (ADA). The formula was later updated to include districts with fewer than 1,600 ADA, where it now stands, providing additional *allotted* funding in inverse proportion to the number of students in ADA. In a push by the state legislature to encourage many of Texas's districts with small student counts to consolidate, in 1975 the legislature formalized a distinction among these low student districts. Those that were smaller than 300 square miles in area were deemed "small by choice" and penalized in the funding formula, while their larger "sparse" counterparts were not. In 1984 the diseconomies of scale provision was extended to offer a less generous funding increase to districts between 1,600 and 5,000 ADA with no distinction between those larger and smaller than 300 square miles in area.

Today, the majority of funding districts receive comes through the state's two-tiered Foundation School Program, covering over 80% of total resources. This tiered designation began in 2006, though the elements of the adjusted allotment are consistent throughout our data. Within this, Tier I determines the non-federal entitlements a district receives, and the share of this funding borne by the state and by the local district as described above. Tier II allows districts to generate supplemental funding above their required contribution although the ability of districts to do so is capped, a feature that is the currently the subject of several Supreme Court cases. Additional revenue comes from local intermediary sources (transfers across districts) and from the Federal government (mostly grants), although these account for a small share of total revenue. We detail the relevant elements of these funding formulas below and refer readers interested in the myriad details of Texas's school finance system to Alexander et al. (2000), Reschovsky and Imazeki (1999), and the Texas Taxpayers and Research Association (TTARA, 2012).

### 3.2 Components of district funding

The primary element of Tier I funding, which accounts for the majority of district resources, is the district's adjusted allotment (AA) which is created from two basic building blocks: the basic allotment (BA), a base per-pupil level of funding guaranteed to all districts; and the adjusted basic allotment (ABA), which is the BA adjusted by a cost of education index (CEI). The adjusted allotment is defined by the following:

$$\begin{aligned} AA =& ABA * (1 + \mathbf{SizeAdj}) \\ =& \underbrace{BA * [(SalCost * CEI) + (1 - SalCost)]}_{\text{ABA}} * (1 + \mathbf{SizeAdj}), \end{aligned} \tag{1}$$

where $SalCost$ is the share of a district's costs that are salary, and the $CEI$ is an adjustment for differential prices of labor across districts. Then, the small and mid-sized district adjustment, depending on the number of students and geographic size, is determined as follows:

$$\textbf{SizeAdj} = \begin{cases} 0.0004 * (1,600 - ADA), & \text{if } 0 < \text{ADA} \leq 1,373, \ \& \text{ Area} \geq 300 \\ 0.000025 * (5,000 - ADA), & \text{if 1,373} < \text{ADA} < 5,000, \ \& \text{ Area} \geq 300 \\ 0.00025 * (1,600 - ADA), & \text{if } 0 < \text{ADA} \leq 1,220, \ \& \text{ Area} < 300 \\ 0.000025 * (5,000 - ADA), & \text{if 1,220} < \text{ADA} < 5,000, \ \& \text{ Area} < 300 \\ 0, & \text{if ADA} \geq 5,000 \end{cases} \tag{2}$$

We plot the formula and the actual AA received in Figure 1. The lines plot what the formula dictate while the data points plot what districts received separated by those above and below 300 square miles.

Tier I funding is then determined by multiplying the adjusted allotment by a series of weights for each of several student population types. Regular program students are the base factor with a weight of approximately 1. Weights are larger for other student groups. For example, bilingual students are weighted at 1.1, gifted students at 1.2, and some special education students are weighted up to a factor of 5.[7] Then for any district $i$ across student types $j$, where $\omega_j$ are weights for each of the student types and $N_{ij}$ is the number of that class of student in the district, Tier I funding is as follows:

$$TierI_i = AA_i\left[\sum_{j=1}^{J} N_{ij} * \omega_j\right] \tag{3}$$

Total district funding is then Tier I, plus addition funds raised by the district that are subject to recapture in Tier II, plus Federal and other funds, capital outlays, and debt service, each of which are not directly affected by the formula above.

Figure 2 presents a visual representation of the raw pattern of funding and achievement differences across district size and student population for all years in our data. Each of the panels shows the mean difference between districts larger and smaller than 300 square miles by district ADA overlaid with a non-parametric and linear fit. These figures simply plot Figure 1 but show differences across districts above and below 300 square miles on either side of the 1,373 ADA cutoff. These plots do not capture the subtleties of size and sparsity, but rather use the categorical distinctions according to the formula to provide intuition and graphical evidence. Our regression models use smooth functions of these parameters to account for the full relationship.

We expect a downward sloping line for low ADA districts (below 1,373) and a flat line for mid-sized ADA districts (between 1,373 and 5,000). The top left panel (A) demonstrates this total per-pupil expenditures. Districts larger than 300 square miles with fewer than 1,373 students in ADA receive more funding on average than those below 300 miles, and this benefit can in fact be several thousand dollars for the districts with the fewest students and approximates $0 as ADA approaches the small/mid-sized district threshold above the kink point. For districts with more

[7] These values, and the number of categories, change over time. We take these examples from the relevant time period from (TTARA, 2012).

than 1,373 students, there is no difference in expenditures by geographic size across ADA. Panels B through D repeat this exercise for reading and math achievement scores and the district dropout rate respectively. The patterns for reading and math mirror the adjusted allotment remarkably well, while the pattern for dropout rates are consistent, though less precise, which is in part a function of low dropout rates among these smaller districts.

It is important to note that we do not observe student outcomes before and after the implementation of this rule, beginning in 1975 and in 1984, implying that the impacts we estimate are the long-run effects of increased district funding on district level outcomes. That is, we are in no position to rule out endogenous responses on the part of families living in, or migrating to, districts according to the way funding is allotted as a result of this specific element of the formula. In fact, we take these as important outcomes in themselves and test both for differences in our sample, and for changes dating back to the origination of the policy using historical census data following our main results. That said, while one might expect endogenous sorting to occur within, say, an MSA in response to a shift in school funding, moving to a sparsely populated remote district is much more costly. Moreover, each additional family that migrates to a district for additional funding will receive marginally less due to the formula's decreasing funding in ADA. In this sense, a massive shift to any district would negate the per-pupil funding benefits of moving there in the first place.

Regardless, we make the case that medium/long-run impacts are the policy relevant outcome, as districts are likely constrained in the changes they can make in the very short-run to additional resources. This puts our analysis in line with Lafortune et al. (2018) who estimate gains 10 years out. Moreover, while states can reallocate funding through formulas, they cannot control how families respond. Observing these differences in the long run are thus useful policy exercises.

Lastly, we frame the narrative in terms of increased per-pupil allotments through the district foundation as opposed to direct increases in revenue or, equivalently, expenditures, though we do estimate effects as a function of expenditures as well. We believe this is appropriate as policy-makers cannot helicopter drop funding on students, but rather have leverage over funding formulas of which base allotments are the fundamental element.

## 4 Estimation Strategy

Our empirical strategy is based on the assumption that the formula providing additional funding to small and sparse districts does not perfectly capture the true relationship between size and sparsity and educational costs. If it is the case that the cost of educating students is kinked exactly at 1,373 students and discontinuous precisely at 300 square miles, as described in Equation 2, we cannot exploit any variation. If not, we can exploit the difference between the true and formulaic relationships as a source of variation in funding.[8]

[8] One could also exploit only the 300 square mile discontinuity, but at the district level our data are underpowered for this. In addition, the allocation is an interaction between being above 300 square miles and ADA, not the average difference at the 300 square mile cutoff. Recent work by Calonico et al. (2018) demonstrates that estimators relying on interactions at the threshold can be inconsistent.

The adjusted allotment is determined by a formula which is a function of the number of students in the district (ADA) and geographic size (a binary classification larger than 300 sq. mi.). The true cost of educating students might also be a function of these, but likely not the same. Rather, the true relationship between the number of students, sparsity, and the cost of education is in all likelihood smooth, while the formula described above in Equation 2 and shown in Figure 1 is discontinuous and kinked. Let us represent the discontinuous funding formula described above with the function $f(\cdot)$:

$$\text{AA} = f(ADA, above300) \tag{4}$$

And let us define the true cost of schooling with respect to these factors as some smooth function of size, sparsity, and ADA in $g(\cdot)$ as:

$$\text{True cost} = g(ADA, size, sparsity) \tag{5}$$

As long as $f(\cdot)$ and $g(\cdot)$ are not the same, and there exists sufficient residual variation in revenue after controlling for $g(\cdot)$, then the residual variation in the AA is a valid instrument for per-pupil spending. The reduced form estimating equation is then as follows:

$$y = \alpha + \beta AA + \Gamma g(ADA, size, sparsity) + \epsilon \tag{6}$$

One can think of this in terms of partial regression. This would involve first regressing $y$ on $g(\cdot)$ and taking residuals (call these $\tilde{y}$), which is residual variation in test scores net of smooth controls for size and sparsity. Then we would regress $AA$ on $g(\cdot)$, again taking residuals to get $\tilde{AA}$. This is residual variation in the adjusted allotment, resulting from kinks and discontinuities, net of smooth controls for size and sparsity. Then, $\beta$ results from regressing $\tilde{y}$ on $\tilde{AA}$. Identification in this case relies on sufficiently capturing the true cost of size and sparsity in $g(\cdot)$, though this function is unknown, and assuming that other factors correlated with outcomes is not related to the residual difference between the two.

Our solution to this is two part. First, we define three elements of geographic size and student population that might affect the cost of schooling: the number of students; geographic size; and sparsity, defined by the number of students per square mile, and include these in the function $g(\cdot)$. We then assume that that the relationship between these and the true cost of schooling is smooth but unknown. Hence, we estimate $g(\cdot)$ flexibly by including smooth polynomial functions of each of student population (ADA), district size (area), and sparsity (students per square mile). We also include several robustness checks to show that results are not reliant on one particular parameterization.

Second, we include additional proxies for size and sparsity that are not in the funding formula. These include the average number of miles bused by students in the district, and average commute times among workers in the district. These measures account for the fact that simply knowing the number of students and the total area of the district may not accurately represent the true costs associated with size and sparsity. The reason for this is that population is not uniformly distributed across these large districts; in fact much of the area is unpopulated. Our summary statistics confirm this. The average district is 270 square miles in area in total, but only 148 square miles of these districts are populated.[9] Similarly, while on average districts have about 14 students per square mile, measured by populated area this increases to 24. We show this in Figure 3, where the first panel shows populated area for districts above and below 300 square miles that have low ADA counts. The second figure is identical with only populated area shaded in.

Table A1 shows summary statistics for the sample broken out by small (<1,373 ADA) and mid-sized districts (1,373 < ADA < 5,000), and by whether a district is larger or smaller than 300 square miles. The table demonstrates that geographically larger and smaller districts are equivalent in student population, though geographically larger districts have more Hispanic and slightly more economically disadvantaged students, though this is true for those receiving additional funding through the formula (with fewer than 1,373 in ADA) as well as those not. The key difference is funding, with a roughly $2,000 difference across the 300 square mile threshold for small districts and no difference across the threshold for mid-sized districts.

We also find that in raw means geographically large districts (> 300 sq. mi.) have similar educational outcomes among low ADA districts (<1,373 in ADA) as do geographically small districts (< 300 sq. mi.). But, for mid-sized districts, for whom the 300 square mile distinction has no impact on funding, geographically large districts perform significantly worse in math and reading.

We build the factors described above into our estimation strategy by first estimating the relationship between the adjusted allotment and per-pupil funding and expenditures, net of smooth controls for the true cost of size and sparsity. We then estimate the relationship between additional funding and student and district characteristics. Finally, we estimate whether these funding increases, net of size and sparsity, improve student outcomes. For outcome specifications, we want to take into account that effects of additional funding may accumulate as students age. To capture this we estimate effects at the district-cohort-grade level, using the average residual funding students received by each grade. We describe our empirical specifications below.

### 4.1 Effects on Funding and District Level Characteristics

Building on the simplified model described above, our full specification for funding for district $i$ in school year $t$ is:

$$y_{it} = \alpha + \beta AA_{it} + g(D_{it}, A_i, S_{it}, C_{i(t)})'\Gamma + X'_{i(t)}\Pi + \tau_{r*t} + \varepsilon_{it} \quad (7)$$

$y_{it}$ is one of several measures of district finance, for example total revenue, Tier I funds, or expenditures on instruction. We put four components into $g(\cdot)$. $D_{it}$ is the number of students in ADA, $A_i$ is district area in square miles, and $S_{it}$ is sparsity, measured by students per square mile. We enter each of these into the model with second order polynomials. Lastly, $C_{i(t)}$ is a measure of

[9] We calculate this by measuring the total area of all census blocks with any person living in them in the 2010 Census.

commuting variables to proxy for other costs of sparsity. These include average miles bused per student, which varies each year, and average travel times for workers in the county measured in the ACS 5-year sample.

In $X_{i(t)}$ we include the cost of education adjustment (CEI), an indicator for the few districts that do not offer all grades K-12, and an indicator if the district is considered consolidated.[10] To account for regional variation and year effects, $\tau_{r*t}$ is a set of 159 region-by-year fixed effects for Texas's 20 academic regions less one for a reference group to ensure that we are not comparing districts across vastly different geographic conditions and to absorb local economic shocks. In all specifications we divide the adjusted allotment, $AA_{it}$, by 1,000. $\beta$ then tells us the effect of a \$1,000 increase in residual AA, after controlling for smooth functions of size and sparsity, on per-pupil expenditures, state, local, and federal revenue, as well as by Tier I and Tier II. This is important not only to get a sense of the pass-through rate, but also to ensure that there is sufficient residual variation in revenue and expenditures after controlling for our measures of the cost of size and sparsity.

We also use this specification to ask how funds are spent by observing how districts with larger or smaller funding allotments distribute revenue across different educational inputs, such as instruction, administration, and student support. This is intended to provide context to the relationship we find between additional funding and achievement as the funding differentials we observe are discretionary.

Following this we test for endogenous responses on the part of districts, or whether additional funding is correlated with district factors that might also impact outcomes, for example whether housing values are affected or if more or fewer poor students are in districts receiving additional funds, using the same specification. In all cases throughout we cluster standard errors on districts.

### 4.2 Effects on Achievement

We next turn to the relationship between funding and achievement. We do this in a manner that allows effects of additional funding to vary by grade, and that captures the cumulative exposure students have experienced over schooling. To do this, for each district-cohort we construct the average additional funding students have received by each grade since entering school at grade 1.[11] We then do the same for average ADA, sparsity, and CEI. This full model for (3rd grade) cohort $c$ in district $i$ at grade $g$ is as follows:

$$y_{icg} = \alpha + \sum_{g=3}^{11} \beta_g \overline{AA}_{icg} + \Gamma g(\bar{D}_{icg}, A_i, \bar{S}_{chg}, \bar{C}_{icg}) + X'_{it}\Pi + \rho_g + \tau_{r*t} + \varepsilon_{igt} \tag{8}$$

This is analogous to our funding equation with exception for taking the cumulative average funding students were exposed to.[12] We define the cumulative average $AA$ for cohort $c$ in district $i$ at grade

[10] Results are similar if we limit only to K-12 districts, shown in the appendix. Dropout and graduation rates are naturally only available for K-12 districts.

[11] Some districts do not have kindergarten, though results are similar if we include funding at that age too.

[12] This is similar to average cumulative exposure measures in Aaronson and Mazumder (2011) and Kreisman (2017).

$g^*$ as,

$$\overline{AA}_{icg^*} = \frac{1}{g^*}\sum_{g=1}^{g^*} AA_{icg} \tag{9}$$

For example, in 4th grade ($g = 4$), $\overline{AA}$ for cohort $c$ in district $i$ is the average $AA$ students received over grades 1-4.[13] We do the same for ADA, sparsity, and the CEI, though results using contemporaneous versions of these are similar, which we show in robustness checks in addition to a simple specification pooled across all years and grades. In addition, $\rho_g$ are a set of main effects for grade.

In this case, $\beta_g$ is the effect of an additional \$1,000 on average at each grade, up to grade $g$. Results can then tell us whether effects of additional residual funding are absorbed by 3rd grade, the earliest we can observe, and whether they accumulate as students progress, potentially resulting from dynamic complementarities where the impact of additional funding in later grades is enhanced by additional funding in earlier grades. It does not allow us to identify the effect of an additional one-time influx of funding, which we do not observe as funding levels are strongly correlated over time. In addition, we ask whether effects are heterogeneous across poorer and wealthier districts, or for districts with higher or lower shares of Hispanic students.

### 4.3 Long run outcomes

In our final set of analyses on educational outcomes we measure differences in medium and long-run educational attainment. The first set of these are four-year high school graduation and dropout rates, where the former measures the share who graduated with a traditional diploma in four years, and the latter measures the share who neither graduate, earn a GED, nor remain in high school after four years. For this specification, since we are not measuring by grade but rather cohort, we aggregate data to the district-cohort level and specify the adjusted allotment as the average over the past nine years to approximate our cumulative exposure since third grade. This is similar to our regressions on district level revenue and expenditures.

Finally, we turn to long-run educational outcomes using data from the College Board and National Student Clearinghouse. We begin by measuring the share of students in the district taking the SAT as an outcome and to get a sense of selection into the sample. We calculate this by dividing the number of students in the district who took the SAT by the number of students in the cohort. We then estimate effects on average SAT (math + verbal) scores for test-takers, and college enrollment and graduation rates for those who are in the sample. We specify these models similar to those for high school dropout and graduation rates.

[13] Note that our historical funding data only dates back to 1992, thus averages for 10th and 11th grade we drop 1st and 2nd grade as these are unobserved for some cohorts. We cannot add in the current year's funding and include lagged funding as there is a high degree of correlation, particularly in early grade, between current and average lagged funding. In third grade the correlation is well over 0.9 leading to full rank problems.

# 5 Results

## 5.1 Residual variation in funding

We begin by estimating our main model from Equation 7 on various components of district revenue in Table 2. These specifications control for smooth functions of size and sparsity and proxies in commute and bus miles, $g(\cdot)$, the cost of education adjustment and whether the district is k-12 or consolidated, $X_{i(t)}$, and for region-year fixed effects, $\tau_{r*t}$, to match outcome equations that follow.

Beginning with columns 1 through 5 we show the impact of an additional \$1,000 on total revenue, and then broken down by state, local, federal, and other. Because the adjusted allotment is multiplied by a series of weighted student counts, as in Equation 3, each additional dollar in adjusted allotment yields more than one dollar in total revenue (the average adjusted allotment is \$4,318 and average per-pupil revenue is \$12,164). The share of this funding that comes from the local district, and the share that comes from the state, is determined by whether the district is property rich or property poor, with the wealthiest districts paying the largest share from local funds, and the poorer districts receiving a larger share from state funding. Regardless of this, the adjusted allotment determines how much in local + state funding the district is entitled to, but does not alone determine local versus state shares. Because the allotment does not affect federal or Tier II funding, we expect no relationship there.

Column 1 shows that for each additional \$1,000 in the AA, after including our full set of covariates, total revenue increases by approximately \$1,600. In other words, districts receiving an additional \$1,000 in AA, over and above controls for size and sparsity, receive an additional \$1,600 in total revenue. If we have specified the true cost of size and sparsity correctly with smooth measures and proxies, then this residual variation is additional funding districts receive over and above the true cost of these factors. Controlling for district demographic characteristics, such as the share of students who are poor or bilingual, for example, marginally lowers this result to \$1,540, confirming that conditional on our controls, other district factors are not generating the relationship between residual adjusted allotment and revenue. Columns 2 and 3 show that roughly 60 percent of this additional residual funding comes from state funds, implying that districts receiving more residual funding are property poorer on average.[14]

Columns 4 and 5 confirm that additional funding has no impact on either federal funds districts receive, nor on "other" categorical revenue, such as transfers across districts. Finally, in columns 6 and 7, we show that indeed the residual funding increase operates entirely through additional Tier I funds as we expect.[15]

[14] We cannot rule out causal effects of funding on property value, which we explore in our analysis on funding and district characteristics. Regardless, it is unlikely that additional funding would lower property values, hence this relationship reflects the fact that sparser districts have lower property values. We note that prior work finds effects on effective tax rates from increased state funding, for example Steinberg et al. (2016).

[15] These estimates are from years 2006-2010 as the Tier I/II designation as funding is only broken out in this way for these years.

## 5.2 Expenditures and budget allocations

Our data allow us to ask how these additional funds are spent. We group spending into five main categories: direct instructional spending, which is largely teacher salaries; instructional and school leadership, which includes principals and staff; instructional related services, co-curricular activities and student support, which includes resources such as libraries, guidance counselors, health services and after-school activities, as well as staff development; central administration and data processing; and security, food services, plant maintenance and operations. Appendix B lists the items covered by each in full.

We begin by depicting differences in total spending and shares across each category by breaking out small and mid-sized districts (those below and above 1,373 in ADA on average in our sample), and by whether they are larger or smaller than 300 square miles in area. While this rough breakout does not contain the all the subtitles of size and sparsity, it rather serves to contrast how large revenue differences are across those receiving additional sparsity funding and those not. Shown in Figure 4, Panel A confirms expected differences in per-pupil spending levels. Low ADA districts (those below 1,373) spend more per-pupil in every category than midsize ADA districts, and geographically large districts (above 300 square miles) spend more in every category than geographically small districts, but only among low ADA districts who receive additional funds across this threshold. For mid-sized ADA districts, for whom the 300 square mile distinction does not apply, total spending differences across that threshold are minor. Moving to panel B, we show the same breakdown by spending shares. The share of expenditures dedicated to each category is similar across districts above and below 300 square miles, and in fact across districts with more or fewer students, despite the fact that low ADA districts larger than 300 square miles receive about 10% more in funding on average.

To move beyond these categorical approximations we re-estimate the same model as in our regressions on revenue, now with each expenditure category in levels (panel A) and in log expenditures (panel B), as the dependent variable in Table 3. We show that from each additional $1,000 in allotments, net of our full set of controls, districts spend an additional $1,600. Of this $1,600, districts spend about $400 on instruction, $100 on support and services, $260 on administration and $280 on plaint and operations. To think about these as percents, in panel B we show log expenditure differences. Here we find that the largest shares are dedicated to administration and plant and operations, which are small shares in the overall budget. This suggests that additional funding is dedicated proportionally more to administration and less toward direct instruction. Given that these are small districts and the additional funding is meant to compensate for diseconomies of scale, this result is in line with the intended use of the funds. A regression where data are pooled at the district level shows similar results.

In the final four columns we conduct the same exercise for total expenditures including non-operating expenditures, for example capital outlays and debt service.[16] We find that funds are not used toward capital expenditures or debt service, but rather toward payroll and other funds.

[16] The TEA has two breakdowns of expenditures, total operating expenditures are shown in 1-5 and total expenditures are in columns 6-9.

## 5.3 District characteristics

We do not include district demographics in our main outcome specifications to follow, such as the share of students who are poor or Hispanic for example, as these might themselves be products of the funding formula (though we do include them in robustness specifications for completeness). To explore this, we next run a separate series of regressions with several of these potentially endogenous characteristics as outcomes themselves.

In Table 4 we show results. Beginning in columns 1 and 2 we show differences in the share of students who are economically disadvantaged and who are Hispanic. We find no difference in the share who are poor, but do find districts receiving additional funding through the allotment have fewer Hispanic students, by about 5 percentage points per additional $1,000. Likewise, in column 3 we show that each additional $1,000 is associated with a 2 percentage point increase in students who have limited English proficiency (we find the same result for bilingual students). We cannot determine if these differences are the result of selective migration, or increasing growth among the Hispanic community in smaller and more rural districts receiving additional funding. We find no differences in the share of students who are gifted or special education, but do find a 3 percentage point increase in the share who are classified as vocational per $1,000 in the AA.

In columns 7-10 we test for differences in characteristics of teachers. We find that on average teachers are not paid more (starting salaries or average salary), nor are they more experienced on average (this is true for total experience or experience within the district). We do find that student-teacher ratios are lower in districts receiving additional funding, by about 0.6 fewer students per class. While small, districts in the sample have an average student to teacher ratio of 12, so the increase is not trivial. There is no difference in the likelihood that a district is K-12, which in nearly all cases would result from a district having its own high school or not. Finally, we show that assessed property values (the average taxable property value used to determine school funding) are lower in districts receiving additional allotted resources.

## 5.4 Effects on achievement

In our main specification we ask how additional funding affects achievement, and whether this varies as students progress through school. Our strategy for this exploits the panel nature of our data. We disaggregate our data to the district-cohort-grade level and include interactions between grade and the average allotment difference students experienced up to that grade as in Equation 8, where the cumulative average adjusted allotment is calculated as in Equation 9. Table 5 shows results.

We find that an additional $1,000 in the adjusted allotment per year increases reading scores by about 0.1 standard deviations, and math scores by 0.077. These are net of our full set of controls for size and sparsity, main effects for grade, and region-by-year fixed effects. In Table A2 in the Appendix we show very similar results from a simple pooled cross-section with one observation per district over the entire sample period.

We find that these results accumulate in later grades. Column 2 suggests that by 3rd grade, an additional \$1,000 per year over grades 1-3 yields a 0.071 standard deviation advantage in reading. The effect in grades 4-5 are no different than in grade 3, but effects in grades 6-10 suggest cumulative effects of exposure. That is, receiving an additional \$1,000 per year on average over grades 1-6 (i.e. \$6,000 in total over 6 years) leads to a 0.135 standard deviation increase in reading test scores. This advantage narrows somewhat in grade 11. Since grade 11 is an exit exam year, students get multiple attempts which might explain the reduction.

Turning to math, we find a smaller impact in grade 3 that is positive though not statistically different from zero. Statistically meaningful differences manifest by grade 5 and accumulate as students progress. By 11th grade, an additional \$1,000 in allotted funding per-year increases math achievement by 0.14 standard deviations.

### 5.5 Achievement effects, robustness

Tables A3 and A4 in the Appendix show a series of robustness checks following our simple cross-sectional specification in Table A2.

In column 1 we replicate our main specification for reference. In column 2 we drop area, sparsity (ADA/area), and our proxies for sparsity costs in commute times and bus miles traveled entirely from the model and find nearly identical results. This suggests that beyond their contribution to the funding formula, these factors in fact have little or no direct effect on academic performance, noting that we are controlling for ADA, region-by-year effects, and all factors in $X_{i(t)}$.

In column 3 we replace average measures of ADA, sparsity and the CEI with contemporary versions finding a slightly stronger relationship. In column 4 we add the per-pupil taxable value of oil in the districts, and a squared term, and a measure of population density in the district (not just of students) to test whether additional proxies for the costs (or benefits) of size and sparsity change conclusions. We find no change to results.

In columns 5 and 6 we include district demographic characteristics, bearing in mind that these might be endogenous to funding. These include the share of students who are black, Hispanic, other race, and the share who are economically disadvantaged. In column 5 we include the earliest measures of these we observe, in 1997, and interact these with a linear time trend. In column 6 we include contemporaneous versions of these measures including interactions with grade. In both cases we observe a level shift downward in estimated effects, but that the gradient as students age is unchanged. In column 7 we limit only to K-12 districts with little change to results.

In column 8 we ask whether effects are driven by districts with large annual changes in ADA, which would affect funding levels through the formula. To do so, in each year we drop the 10 percent of districts with the largest positive ADA change (as a percent of previous year), and the 10 percent with the largest negative year-on-year change. These correspond to districts with either a 9 percent increase in ADA or an 8 percent decrease. Results are similar to column 1, confirming that these changes are not driving results though may contribute to them. In column 9 we create a “doughnut hole” regression by dropping districts that ever cross the 1,200 to 1,373 kink point. We should expect that some of the effects are driven by this, because small, smooth changes in ADA over the threshold would lead to non-smooth changes in funding. Results confirm that impacts are attenuated when we omit these districts entirely from the sample, but that districts crossing this boundary are not themselves driving results.

### 5.6 Heterogeneous achievement effects

Prior work estimating effects of funding on achievement largely rely on changes targeting school districts that were inadequately funded, in most cases these are poorer communities. The variation we exploit does not explicitly target poor districts. In fact, on average sparse districts in Texas are well funded compared with other Texas schools and other schools in general. To take an example, the average adjusted allotment for districts with fewer than 1,373 students that are larger then 300 square miles (those districts receiving the largest size adjustment) is \$4,958. The average adjusted allotment for the remaining districts is \$4,069. Yet, 55% of students are economically disadvantaged in the sparser districts compared with 52% in the remainder. Thus we are observing effects of additional funding among poorer districts with comparatively high funding levels. Similarly, these districts are disproportionately more Hispanic. 39% of sparser districts are Hispanic compared with 31% in less sparse districts not benefiting from the size adjustment. In order to reconcile our results with previous work, we turn here to heterogeneity in effects.

In Tables 6 and 7 we break districts into terciles of the share of students who are economically disadvantaged, and terciles of the share who are Hispanic, averaged over all years in our sample. Figure A1 in the Appendix shows variation in the share of students who fall into these categories by ADA and whether the district is above or below 300 square miles, confirming that across the ADA and the geographic distribution there exists considerable variation in the share of students who are poor or minority. Our analysis on Hispanic students is in part a proxy for the many students for whom English is not a primary language in Texas, which may in part explain differences in impacts on math versus reading scores. We can conduct these analyses by ESL or bilingual status, but note that better funded schools may have effects on English language learning leading to selection effects on which students are ESL or bilingual, particularly in later grades. Since Hispanic status does not change over time within student, we use this as a proxy. Conclusions are substantively similar if we use ESL or bilingual status.

For this analysis, we re-estimate our main specification with these shares as controls and include interactions with additional funding. Since these regressions include main effects for poverty and Hispanic, we will see smaller effects, as in columns 5 and 6 of our robustness tables.

In Table 6 we find that gains, both for reading and math, are nearly entirely driven by districts in the top two terciles of the share of students who are poor. These have 53 and 72 percent of students who are economically disadvantaged respectively, compared with 35 percent for the bottom tercile. Moreover, we find that effects are in fact driven largely by the poorest districts in later grades. In Table 7 a similar story emerges. For the third of districts with the highest Hispanic populations, 61 percent compared with 6 percent for the lowest and 22 percent for the median tercile, effects of additional funding are prevalent by the third grade.

While these results suggest that targeting additional funds can achieve test score gains and lower dropout rates, they also highlight the fact that disparities between poor and minority districts and wealthier and whiter districts remain large. For example, in raw differences the poorest and most Hispanic districts in our model score nearly 0.4 standard deviations worse on math and reading compared with the wealthiest and least Hispanic. In this sense, while we can argue that increased spending is most effective when targeted toward high poverty districts, we also conclude that even meaningful resource shifts toward poor districts are not sufficient to close what are very large initial achievement disparities.

### 5.7 Long-run educational outcomes

We now turn to medium and long-run educational outcomes. We begin by measuring four-year high school graduation and dropout rates at the district-cohort level where the adjusted allotment is the average over schooling years (beginning in 3rd grade) to approximate our cumulative exposure in prior specifications.

Table 8 shows results from this specification, including a pooled specification in columns 1 and 3 for dropout and graduation rates, and then broken out by terciles of poor and Hispanic students. For each additional $1,000 per year, four-year high school dropout rates decline by 1.6 percentage points. This effect is driven entirely by the poorest and most Hispanic districts, each of which see a 2 percentage point decrease in the dropout rate across the sample. The average four-year dropout rate across all districts in our sample is 4 percent, which is lower than the average for all districts which includes many urban centers, and is just under 6 percent for the poorest and most Hispanic districts. Turning to four-year graduation rates we see a similar pattern. Graduation rates increase by just more than 4 percentage points for the poorest and 3 for the most Hispanic districts, who have an average four-year graduation rate of 90 percent, among those in our sample.

We next turn to long-run educational outcomes using data from the College Board and National Student Clearinghouse. These models are the same as those for high school dropout and graduation rates using the average adjusted allotment since 3rd grade. Table 9 shows results from these specifications. The sample is limited to the 814 districts that are K-12 in our sample. For statistics on SAT scores and college-going and completion, the sample is limited to districts with any students who took the SAT (column 4-6), or those with students who interacted with any College Board exam (7-12).

In columns 1-3 we repeat our exercises from the previous table now on the share of students who took the SAT, finding little impact. Among test-takers, we detect no average effect, but do find evidence that students in the poorest districts perform meaningfully better, on the order of 0.12 standard deviations, which is very close to estimated effects on reading and math scores for the same set of districts in 11th grade.

Finally, we estimate college enrollment and college degree attainment for those in the College Board sample. We find meaningful enrollment gains, on the order of about 10 percentage points across all districts from an average $1,000 increase in base funding during schooling. Here we find that gains are not concentrated among poor or wealthy districts, though some evidence that more Hispanic districts see larger enrollment effects. When we turn to degree completion, we find a 4 percentage point effect on average, which is consistent with a story where additional funding pushes marginal students into college, but has less effect on performance of inframarginal students. We discuss these results relative to the existing literature in the next section.

# 6 Interpretation and Policy

Our estimates suggest that a $1,000 increase in base funding yields a 0.1 s.d. increase in reading scores, and a near 0.08 increase in math. In addition, dropout rates decline, graduation rates marginally increase, as does college enrollment and to a smaller degree graduation. These gains are largely concentrated in later grades (for test scores) and among poorer districts. Here we situate these in the literature.

## 6.1 Magnitudes relative to the literature

Estimating effects of school finance reforms across the country between 1990 and 2011 Lafortune et al. (2018) estimate 0.12 to 0.24 standard deviation effects from $1,000 in additional per-pupil spending. Papke (2005) finds a 1-3 percentage point increase in passing state exams from a 10% increase in funding. Similarly, Chaudhary (2009) finds a 60% increase in spending in Michigan increased the share achieving satisfactory on the state exam by one standard deviation. Hyman (2017) finds a 7 percent increase in college enrollment and an 11 percent increase in graduation from a 10% ( about $1,000) increase in funding in his study in Michigan. Our results are in fact not far from these, in particular if we focus on effects in poorer schools, where funds were largely targeted in these other papers. Focusing on effects in the poorest tercile of districts, we find a 0.11 standard deviation gain in reading and more than 0.08 standard deviation gain in math, near the lower bound of Lafortune et al. (2018). Though this is from a $1,000 increase in base funding, which is equivalent to about $1,400-$1,600 in expenditures. Our college enrollment gains are also similar to those in Hyman (2017), and like him we also find that gains were not concentrated among the poorest districts.

To further situate our results, in Table 10 we show main results estimated using two-stage least squares with the adjusted allotment as an instrument for expenditures in our panel setting. Column 1 shows results from the first stage regression where a $1,000 residual increase in the adjusted allotment generates a 9.5% increase in total expenditures. We then show that a 10% increase in expenditures, due only to the increase in the adjusted allotment over and above our controls for the cost of schooling due to sparsity, leads to a 0.09 standard deviation increase in reading scores and 0.07 standard deviation increase in math. Effects are slightly smaller if we include district demographic characteristics, as in our robustness checks.

It is important to note a few distinctions between ours and existing research. First, we interpret impacts as contemporaneous effects of long-term increases in per-pupil allotted revenue, as the size adjustment was instituted decades ago. Second, treatment is restricted to districts that are small in population, many of which are low income but have relatively high per-pupil spending. The average student-teacher ratio across the low-ADA districts in this sample is 12, roughly 4 fewer than the "small" classes in the Tennessee Star experiment. We do find that funding decreased student-teacher ratios, but find no difference in salaries.

### 6.2 Mechanisms and equilibrium effects

While we interpret results as the effect of increased budget allocations on student achievement, we cannot rule out general equilibrium effects from a longstanding policy. Along these lines one explanation for the patterns we observe could be that given increased funding families might selectively migrate to recipient districts. For example, Chakrabarti and Roy (2015) find that funding equalization in Michigan led to a decline in neighborhood sorting. Testing for differences in contemporaneous data in Table 4 we find some characteristic differences between schools getting additional funding and those not, with the key difference being a smaller share of Hispanic students.

We first point out that migration is in part self-defeating. The allocation benefit to districts is linearly declining in enrollment, thus each additional family migrating to a district would lower the benefit for all families residing there. District consolidation poses the same dilemma, and additionally requires a neighboring district to participate. We take the former concern seriously and ask whether we see evidence of selection into these districts over time.

We begin by using the National Institute of Education (NIE) Special Tabulations and 1970 Census Fifth Count Data File. This provides school district level statistics (counts) for several populations. We focus on poverty rates among school age children (ages 6-17). The Census/NIE did not report statistics for very small school districts, which are a large part of our sample. As a result, one-third of our sample districts are not in the data. Table A5 in the Appendix shows differences across districts in our sample with data and those without, indicating that our smallest districts, who have larger adjusted allotments, are not represented. Similarly, Figure A2 shows the share of districts missing by bins of ADA.

In the first panel (A) of Figure 5, we plot the share poor in the 1970 sample by the share economically disadvantaged in our contemporaneous sample, pooled over all years for each district in both datasets. We find a high degree of correlation with a level shift due to differing definitions of poverty (in 1970) and economically disadvantaged (in 2003-2010). In the right panel (B), we plot changes in poverty rates between 1970 and the average over 2003-2010 by residual variation in the adjusted allotment. To do this, we collapse data to the district level over our sample and regress the average AA on quadratics in average ADA, area, sparsity, and a linear term in the CEI. We then take residuals and plot the percentage point change in poverty (y-axis) on residual variation in the AA. In this we are asking whether districts receiving more or less in the adjusted allotment in our sample, net of smooth controls for size and sparsity, saw larger or smaller changes in poverty since the policy was enacted. We find no relationship between the residual variation in funding we exploit in our empirical models and changes in poverty among children across these time points.

To get a fuller picture over multiple decades, we take data from decennial Censuses 1960-2000 to observe changes beginning prior to the formula change. We cannot identify districts in full censuses prior to 2000, with exception for the Census/NIE tables described above, rather we only observe counties. One problem then is that many counties are comprised of districts from all combinations of ADA and geographic size, making it difficult to identify what share of the county received additional funding from the allotment or which districts families might have migrated to if any within the county. To address these limitations we restrict the analysis to counties where either all districts in a county are larger than 300 square miles and below 1,373 ADA during our sample period, or those with no districts that would have received the full size adjustment as a rough approximation. We then observe the share of residents who were poor over time across these groups. Results are shown in Figure 6.

Counties where all districts were eligible for the size adjustment in our panel had lower poverty rates prior to the 1974 legislation, by about 4 percentage points. Between 1960 and 1980, when poverty rates were declining generally across the country, rates declined less rapidly for those not receiving additional funding from the policy, suggesting negative selection if any. After 1980 gaps in poverty between these counties remains roughly stable. We believe this provides little evidence of positively selected migration, thought we cannot rule it out. Counties receiving the additional funding from the policy do appear to close the gap between 1980 and 2000, though we cannot determine if this is due to the policy or not and changes are quite small. Though it is unlikely that selective migration would occur in the second decade after the policy change as opposed to the first, we cannot rule this out either.

As a final check against sorting driving results, we re-estimate our robustness checks using the 1970 poverty rate from the NIE/Census sample as a control and include interactions with time. We show this in Table A6 in the Appendix. The odd numbered columns re-estimate our main specifications from Table 5 for reading and math. The even columns repeat this exercise adding the share poor in 1970 as a control, an indicator for missing 1970 poverty, and both of these interacted with year fixed effects. Results are attenuated but still statistically meaningful leading to similar conclusions. Taken together we find little evidence that selective migration has driven test score differences across districts we observe receiving additional funding or not in our specific context.

### 6.3 Small and Sparse Schools

Last, we note that while the size and sparsity adjustments we exploit here are unique in the literature, they are in fact quite common across the country. This is not surprising. The majority of schools, though a minority of students, are in small, rural, or isolated districts. Figure 7 shows the share of districts in each state that are remote, rural, or distant, according to the NCES. 8,030 of 13,491 districts fit one of these designations; 847 are remote towns, 1,582 are rural fringe, 3,145 are rural distant, and 2,429 districts are remote and rural. Texas is not an outlier in this sense at all. It then might not be surprising that we find most states have some accommodation for either diseconomies of scale, size, or sparsity in their funding formulas.

To document this we check current formulas for each state. We discovered that 30 states have at least one of these funding accommodations. We map these in Figure 8. Some are similar to Texas's. For example in Arizona districts with less than 600 students are considered small. If they are also isolated, they receive additional weights in order to provide more funding. Kansas has a similar diseconomy of scale as Texas, with a kink at 1,622 students, but no sparsity adjustment. Many, like Michigan, have provisions for declining enrollment as well. The most common accommodation is for schools or districts with very low enrollment, often below 100. Even in New York sparsity is a factor in K-12 districts with fewer than 25 pupils per square mile. Another good example is Wisconsin, where districts with 745 or fewer students and whose membership is less than 10 per square mile will receive $300 per pupil. Despite the plurality of these types of adjustments, Texas's is unusually generous. Possibly too generous. We conclude by noting that the Texas legislature has begun 5 year phase out of the 300 square mile differential beginning with the 2019 school-year.

## 7 Conclusion

We address two questions: (i) how do school districts receiving additional discretionary funds allocate these resources; and (ii) does the provision of additional funding through state formulas impact academic achievement and attainment? We find that students in districts receiving additional discretionary funds due to a size and sparsity allotment perform better on a host of academic measures, net of our controls for the true costs of size and sparsity.

Results suggest that a $1,000 increase in allotted formulaic funding during schooling years yields a 0.10 standard deviation increase in reading and a 0.08 standard deviation increase in math. In terms of expenditures, we find that a 10% increase in per-pupil spending leads to 0.9 and 0.7 standard deviation increases in reading and math achievement respectively. Effects are largely concentrated in districts with more poor and Hispanic students, and in later grades, when students have had more exposure to additional resources.

We also document modest long-run educational benefits. Four-year dropout rates are lower, and on-time graduation rates are higher. Following this we find meaningful increases in college enrollment and smaller effects on completion. Again, these benefits largely accrue in poorer districts.

We also show that additional discretionary funding marginally increases the share of all funds spent on administration and support services, and slightly decreases the share spent on direct instruction, though levels increase across all categories. Student-teacher ratios decrease as well.

Our estimated effects from additional funding are smaller than previous estimates from nationwide studies, though modestly so, and are on par with other studies focusing on state-level changes. This is not entirely surprising. Previous work largely estimates effects from school finance reforms that targeted schools or districts with either inadequate or inequitable funding levels. The variation we exploit allots additional funding to districts with few students, often in sparse places. On average these districts in fact have high per-pupil funding rates, though are poorer than the average district. While the generalizability to the universe of schools is therefore somewhat limited, we argue that estimating effects of additional funding not targeted to low funded districts provides novel insight. In particular, we contend that additional funding to well funded districts with few poor students yields little in terms of test score gains. Yet, additional funding yields significant and meaningful gains for districts with high proportions of poor students, even if their initial funding levels were high.

Importantly, we also contribute to a very sparse literature on small, rural, and sparse schools. We argue that while these districts face equally pressing policy concerns as their more urban counterparts, research has failed to provide adequate guidance.

It is important to note that the effects here are long-run outcomes, as the law in question was implemented in 1975, and the available data permit an analysis only recently. Thus, results here demonstrate differences across districts that have been receiving (or not) additional discretionary funds for over three decades. Yet, we find little evidence of selective migration, suggesting that what we are observing is the long run district level effect of increased allotments. In the current environment of limited funding for education and debates over how scarce resources can best be allocated, evaluations of the long-term impacts of per-pupil funding levels take on a renewed importance.

## References

Aaronson, D. and Mazumder, B. (2011). The impact of rosenwald schools on black achievement. *Journal of Political Economy*, 119(5):821–888.

Alexander, C. D., Gronberg, T. J., Jansen, D. W., Keller, H., Taylor, L. L., and Treisman, P. (2000). A study of uncontrollable variations in the costs of texas public education. *The Charles A. Dana Cente, University of Texas at Austin.*

Andrews, M., Duncombe, W., and Yinger, J. (2002). Revisiting economies of size in american education: Are we any closer to a consensus? *Economics of Education Review*, 21(3):245–262.

Calonico, S., Cattaneo, M. D., Farrell, M. H., and Titiunik, R. (2018). Regression discontinuity designs using covariates. *Under Review*.

Card, D. and Krueger, A. (1996). School resources and student outcomes: An overview of the literature and new evidence from north and south carolina. *The Journal of Economic Perspectives*, 10(4):31–50.

Card, D. and Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of public economics*, 83(1):49–82.

Chakrabarti, R. and Roy, J. (2015). Housing markets and residential segregation: Impacts of the michigan school finance reform on inter-and intra-district sorting. *Journal of Public Economics*, 122:110–132.

Chakrabarti, R. and Roy, J. (2017). Effect of constraints on tiebout competition: evidence from a school finance reform. *Regional Studies*, 51(5):765–785.

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

Clark, M. A. (2003). Education reform, redistribution, and student achievement: Evidence from the kentucky education reform act. Technical report, Mathematica Policy Research.

Deke, J. (2003). A study of the impact of public school spending on postsecondary educational attainment using statewide school district refinancing in kansas. *Economics of Education Review*, 22(3):275–284.

Gigliotti, P. and Sorensen, L. C. (2018). Educational resources and student achievement: Evidence from the save harmless provision in new york state. *Economics of Education Review*, 66:167–182.

Goldhaber, D. and Brewer, D. (1997). Why Don't Schools and Teachers Seem to Matter? Assessing the Impact of Unobservables on Educational Productivity. *Journal of Human Resources*, 32(3):505–523.

Greenwald, R., Hedges, L. V., and Laine, R. D. (1996). The Effect of School Resources on Student Achievement. *Review of Educational Research*, 66(3):361–396.

Guryan, J. (2001). Does Money Matter? Regression-Discontinuity Estimates from Education Finance Reform in Massachusetts. *NBER Working Paper Number 8269*, pages 1–54.

Hanushek, E. (1986). The Economics of Schooling: Production and Efficiency in Public Schools. *Journal of Economic Literature*, 24(3):1141–1177.

Hanushek, E. (1997). Assessing the Effects of School Resources on Student Performance: An Update. *Educational Evaluation and Policy Analysis*, 19(2):141–164.

Hanushek, E. A. (1989). The Impact of Differential Expenditures on School Performance. *Educational Researcher*, 18(4):45–62.

Hanushek, E. A. (2003). The failure of input-based schooling policies. *The Economic Journal*, 113(485):F64–F98.

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

Hyman, J. (2017). Does money matter in the long run? effects of school spending on educational attainment. *American Economic Journal: Economic Policy*, 9(4):256–280.

Jackson, C. K. (2018). Does school spending matter? the new literature on an old question. Technical report, National Bureau of Economic Research.

Jackson, C. K., Johnson, R. C., and Persico, C. (2016). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1):157–218.

Kreisman, D. (2017). The next needed thing the impact of the jeanes fund on black schooling in the south, 1900–1930. *Journal of Human Resources*, 52(2):573–620.

Lafortune, J., Rothstein, J., and Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2):1–26.

Monk, D. H. (1990). Educational costs and small rural schools. *Journal of Education Finance*, 16(2):213–225.

Murray, S. E., Evans, W. N., and Schwab, R. M. (1998). Education-finance reform and the distribution of education resources. *American Economic Review*, pages 789–812.

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

Reschovsky, A. and Imazeki, J. (1999). Does the School Finance System in Texas Provide Students with an Adequate Education? *Paper prepared for presentation at the Annual Meeting of the American Education Finance Association.*, pages 1–28.

Roy, J. (2011). Impact of school finance reform on resource equalization and academic performance: Evidence from michigan. *Education Finance and Policy*, 6(2):137–167.

Steinberg, M. P., Quinn, R., Kreisman, D., and Anglum, J. C. (2016). Did pennsylvania's statewide school finance reform increase education spending or provide tax relief? *National Tax Journal*, 69(3):545.

TTARA (2012). An introduction to school finance in texas. Technical report, Texas Taxpayers and Research Association.

United States Department of Education (1970). National Institute of Education (NIE) Special Tabulations and 1970 Census Fifth Count Data File.

# Tables

**Table 1:** Summary Measures.

| | Mean | S.D. |
|---|---|---|
| Geography | | |
| ADA | 1,001.2 | (1,033.3) |
| Area | 270.9 | (371.92) |
| ADA/Sq. Mi. | 14.3 | (45.12) |
| Populated area | 148.7 | (139.1) |
| ADA/Populated Sq. Mi. | 23.98 | (131.14) |
| Fundng | | |
| CEI | 1.07 | (0.03) |
| Basic Allotment | 3,304.1 | (631.42) |
| Adjusted Basic Allotment | 3,449.0 | (624.0) |
| Adjusted Allotment | 4,318.5 | (934.0) |
| Total Revenue | 12,164.3 | (3,996.2) |
| Expenditures (Total) | 12,622.0 | (5,145.2) |
| Expenditures (Operating) | 10,366.9 | (2897.8) |
| Demographics | | |
| Black | 0.07 | (0.11) |
| Hispanic | 0.30 | (0.26) |
| Poor | 0.53 | (0.18) |
| LEP/Bilingual | 0.06 | (0.08) |
| Sparsity | | |
| K12 | 0.93 | (0.25) |
| CSD | 0.04 | (0.20) |
| Avg. Miles Bused | 2.54 | (3.17) |
| Commute Time (mins) | 26.36 | (6.94) |
| Outcomes | | |
| Reading (z) | 0.03 | (0.23) |
| Math (z) | 0.03 | (0.26) |
| Dropout rate* | 0.04 | (0.05) |
| Graduation rate* | 0.90 | (0.08) |
| College Board/NSC* | | |
| % Students in CB sample | 0.67 | (0.17) |
| % taking SAT | 0.26 | (0.17) |
| SAT | -0.01 | (0.37) |
| Enroll college | 0.48 | (0.15) |
| College Degree | 0.21 | (0.08) |
| 1970 SSDT | | |
| Not missing | 0.66 | (0.48) |
| % poor | 0.29 | (0.15) |
| % Hisp. | 0.23 | (0.25) |
| Districts | 875 | |
| District*Year Obs. | 6,985 | |

Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. *High school graduation and dropout, and College Board statistics only calculated for K-12 districts.

**Table 2:** Adjusted allotment and revenue (real $2011), by source.

| | (1) Total | (2) State | (3) Local | (4) Federal | (5) Other | (6) Tier I | (7) Tier II |
|---|---|---|---|---|---|---|---|
| AA/1000 | 1608.7*** | 932.4*** | 522.6 | -5.5 | 159.2 | 1271.0*** | 42.2 |
| | (476.6) | (247.7) | (403.9) | (147.8) | (171.5) | (86.9) | (35.7) |
| $X_{i(t)}$ | × | × | × | × | × | × | × |
| $g(\cdot)$ | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × |
| Dep. Mean | 12,164.3 | 5,472.3 | 4,687.4 | 1,207.6 | 796.8 | 5,556.9 | 605.070 |
| $R^2$ | 0.407 | 0.258 | 0.298 | 0.190 | 0.153 | 0.738 | 0.575 |
| N | 6,958 | 6,958 | 6,958 | 6,958 | 6,958 | 4,341 | 4,341 |
| Obs. | 875 | 875 | 875 | 875 | 875 | 870 | 870 |

Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Dependent variables are measured at the district-year level. Total is total revenue. State, Local, Federal and Other sum to equal Total Revenue. Tier I and Tier II are only beginning in 2006. $g(\cdot)$ includes ADA, ADA$^2$, Area, Area$^2$, students per square mile (ADA/Area) and (ADA/Area)$^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $X_{i(t)}$ includes the cost of education index measured at the year level, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 3:** Increased funding and expenditures by category.

Panel A: Expenditures, by category

| | (1) Total | (2) Instruction | (3) Leadership | (4) Svcs./Suppt. | (5) Admin | (6) Plant/Opr. | (7) Capital | (8) Debt | (9) Payroll | (10) Other |
|---|---|---|---|---|---|---|---|---|---|---|
| AA/1000 | 1423.55** | 410.86*** | 35.22 | 101.22** | 261.26*** | 281.56*** | 210.27 | -78.55 | 662.96*** | 628.85*** |
| | (610.98) | (131.46) | (24.56) | (41.71) | (49.33) | (74.47) | (351.27) | (116.72) | (167.44) | (218.95) |
| $g(\cdot)$ | × | × | × | × | × | × | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × | × |
| Dep. Mean | 12,622 | 5,777 | 635 | 1,016 | 846 | 1,762 | 1,444 | 728 | 7,737 | 2,712 |
| $R^2$ | 0.259 | 0.461 | 0.175 | 0.209 | 0.470 | 0.340 | 0.096 | 0.078 | 0.440 | 0.314 |
| N | 6,958 | 6,958 | 6,958 | 6,958 | 6,958 | 6,958 | 6,957 | 6,958 | 6,958 | 6,958 |
| Obs. | 875 | 875 | 875 | 875 | 875 | 875 | 875 | 875 | 875 | 875 |

Panel B: Log expenditures, by category

| | (1) Total | (2) Instruction | (3) Leadership | (4) Svcs./Suppt. | (5) Admin | (6) Plant/Opr. | (7) Capital | (8) Debt | (9) Payroll | (10) Other |
|---|---|---|---|---|---|---|---|---|---|---|
| AA/1000 | 0.075*** | 0.058*** | 0.022 | 0.097** | 0.184*** | 0.112*** | 0.078 | -0.163 | 0.068*** | 0.138*** |
| | (0.028) | (0.018) | (0.041) | (0.039) | (0.035) | (0.028) | (0.107) | (0.109) | (0.018) | (0.032) |
| $g(\cdot)$ | × | × | × | × | × | × | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × | × |
| Dep. Mean | 9.39 | 8.64 | 6.40 | 6.84 | 6.58 | 7.42 | 6.21 | 6.35 | 8.93 | 7.83 |
| $R^2$ | 0.317 | 0.510 | 0.122 | 0.308 | 0.690 | 0.420 | 0.121 | 0.148 | 0.472 | 0.479 |
| N | 6,958 | 6,958 | 6,939 | 6,958 | 6,958 | 6,958 | 6,660 | 5,963 | 6,958 | 6,958 |
| Obs. | 875 | 875 | 874 | 875 | 875 | 875 | 874 | 820 | 875 | 875 |

Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Dependent variable in column 1 is total expenditures. Variables in columns 2-6 categorical operating expenditures in levels; descriptions for each can be found in Table B. Outcomes in columns 7-10 are total expenditures by category, which equal total expenditures. Panel B repeats outcomes using log values of the dependent variable. $g(\cdot)$ includes ADA, ADA$^2$, Area, Area$^2$, students per square mile (ADA/Area) and (ADA/Area)$^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $X_{i(t)}$ includes the cost of education index measured at the year level, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Standard errors clustered on districts in parentheses. ($^*p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 4:** District level characteristics as a function of residual variation in funding.

| | % Poor | % Hisp | % LEP | % Sp.Ed. | % Gift | % Voc | Start Sal. | Avg. Sal. | Exper. | S/T Ratio | K12 | Prop. val |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| AA/1000 | -0.007 | -0.051** | -0.021** | -0.000 | -0.003 | 0.028*** | 188.888 | -269.771 | -0.000 | -0.590*** | 0.007 | -0.178+ |
| | (0.016) | (0.021) | (0.008) | (0.003) | (0.003) | (0.008) | (427.162) | (331.552) | (0.265) | (0.155) | (0.006) | (0.108) |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × | × | × | × |
| $g(\cdot)$ | × | × | × | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × | × | × | × |
| Dep. Mean | 0.530 | 0.301 | 0.063 | 0.127 | 0.067 | 0.266 | 33594.991 | 43547.618 | 12.615 | 11.902 | 0.932 | 17.817 |
| $R^2$ | 0.326 | 0.686 | 0.413 | 0.326 | 0.080 | 0.250 | 0.287 | 0.395 | 0.142 | 0.625 | 0.911 | 0.769 |
| N | 6,958 | 6,958 | 6,958 | 6,958 | 6,958 | 6,485 | 6,084 | 6,957 | 6,957 | 6,957 | 6,958 | 6,892 |
| Obs. | 875 | 875 | 875 | 875 | 875 | 816 | 873 | 875 | 875 | 875 | 875 | 868 |

Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Dependent variables are measured at the district-year level. Prop. val is per-pupil taxable property value for residential land in real $1,000. S/T ratio is student-teacher ration. K12 is whether the district is K-12 (this is removed as a right hand side variable in that regression). $g(\cdot)$ includes ADA, ADA$^2$, Area, Area$^2$, students per square mile (ADA/Area) and (ADA/Area)$^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $X_{i(t)}$ includes the cost of education index measured at the year level, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 5:** Effects of additional funding on test scores by grade.

| | (1) Reading (z) | (2) Reading (z) | (3) Math (z) | (4) Math (z) |
|---|---|---|---|---|
| $\overline{AA}/1000$ | 0.097*** | | 0.077** | |
| | (0.029) | | (0.032) | |
| $\overline{AA}$*grade 3 | | 0.071*** | | 0.037 |
| | | (0.025) | | (0.027) |
| $\overline{AA}$*grade 4 | | 0.075*** | | 0.029 |
| | | (0.029) | | (0.033) |
| $\overline{AA}$*grade 5 | | 0.083** | | 0.067* |
| | | (0.032) | | (0.035) |
| $\overline{AA}$*grade 6 | | 0.135*** | | 0.107*** |
| | | (0.033) | | (0.036) |
| $\overline{AA}$*grade 7 | | 0.127*** | | 0.090** |
| | | (0.034) | | (0.038) |
| $\overline{AA}$*grade 8 | | 0.122*** | | 0.098*** |
| | | (0.033) | | (0.037) |
| $\overline{AA}$*grade 9 | | 0.152*** | | 0.152*** |
| | | (0.033) | | (0.036) |
| $\overline{AA}$*grade 10 | | 0.119*** | | 0.133*** |
| | | (0.034) | | (0.037) |
| $\overline{AA}$*grade 11 | | 0.093*** | | 0.139*** |
| | | (0.035) | | (0.037) |
| $\overline{g(\cdot)}$ | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × |
| grade | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × |
| $R^2$ | 0.106 | 0.109 | 0.075 | 0.079 |
| Obs. | 60,103 | 60,103 | 60,107 | 60,107 |
| Districts | 875 | 875 | 875 | 875 |

Notes: Dependent variable is measured at the district-cohort-grade level (cohort-grade defines a year). $\bar{AA}$ is the average adjusted allotment (in real '000s) cohort $c$ in district $i$ received through grade $g$. $\overline{g(\cdot)}$ measures ADA, and students per square mile (ADA/Area) analogously to the average $\bar{AA}$ and includes quadratics. Area, Area$^2$ and sparsity proxies, including measures of commute times for workers and average miles bused for students, are included as normal. $X_{i(t)}$ includes the cost of education index, measured as the average over schooling years as is $\bar{AA}$, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Grade are grade fixed effects with 3rd grade as the omitted category. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 6:** Effects of additional funding on test scores by grade and % poor.

| | (1) Reading (z) | | (2) Reading (z) | | (3) Math (z) | | (4) Math (z) | |
|---|---|---|---|---|---|---|---|---|
| $\overline{AA}$/1000 | 0.041 | (0.025) | | | 0.018 | (0.029) | | |
| $\overline{AA}$*grade 3 | | | 0.043* | (0.024) | | | 0.019 | (0.028) |
| $\overline{AA}$*grade 4 | | | 0.057** | (0.027) | | | 0.007 | (0.033) |
| $\overline{AA}$*grade 5 | | | 0.043 | (0.029) | | | 0.044 | (0.033) |
| $\overline{AA}$*grade 6 | | | 0.074** | (0.030) | | | 0.050 | (0.037) |
| $\overline{AA}$*grade 7 | | | 0.061** | (0.029) | | | -0.003 | (0.037) |
| $\overline{AA}$*grade 8 | | | 0.051* | (0.028) | | | 0.011 | (0.036) |
| $\overline{AA}$*grade 9 | | | 0.057* | (0.030) | | | 0.055 | (0.033) |
| $\overline{AA}$*grade 10 | | | 0.038 | (0.031) | | | 0.046 | (0.034) |
| $\overline{AA}$*grade 11 | | | 0.013 | (0.031) | | | 0.048 | (0.034) |
| $\overline{AA}$Mid 3rd poor | 0.053** | (0.022) | | | 0.063** | (0.027) | | |
| $\overline{AA}$*Mid 3rd poor, gr 3 | | | 0.034 | (0.023) | | | 0.028 | (0.028) |
| $\overline{AA}$*Mid 3rd poor, gr 4 | | | 0.032 | (0.026) | | | 0.019 | (0.032) |
| $\overline{AA}$*Mid 3rd poor, gr 5 | | | 0.045 | (0.028) | | | 0.016 | (0.031) |
| $\overline{AA}$*Mid 3rd poor, gr 6 | | | 0.072** | (0.028) | | | 0.074** | (0.038) |
| $\overline{AA}$*Mid 3rd poor, gr 7 | | | 0.045 | (0.028) | | | 0.087** | (0.039) |
| $\overline{AA}$*Mid 3rd poor, gr 8 | | | 0.045* | (0.025) | | | 0.080** | (0.037) |
| $\overline{AA}$*Mid 3rd poor, gr 9 | | | 0.080*** | (0.029) | | | 0.094*** | (0.036) |
| $\overline{AA}$*Mid 3rd poor, gr 10 | | | 0.064** | (0.032) | | | 0.092** | (0.038) |
| $\overline{AA}$*Mid 3rd poor, gr 11 | | | 0.064** | (0.032) | | | 0.104*** | (0.037) |
| $\overline{AA}$Top 3rd poor | 0.072*** | (0.024) | | | 0.066** | (0.029) | | |
| $\overline{AA}$*Top 3rd poor, gr 3 | | | 0.025 | (0.023) | | | -0.002 | (0.029) |
| $\overline{AA}$*Top 3rd poor, gr 4 | | | -0.001 | (0.028) | | | 0.010 | (0.036) |
| $\overline{AA}$*Top 3rd poor, gr 5 | | | 0.031 | (0.028) | | | 0.002 | (0.034) |
| $\overline{AA}$*Top 3rd poor, gr 6 | | | 0.066** | (0.030) | | | 0.048 | (0.040) |
| $\overline{AA}$*Top 3rd poor, gr 7 | | | 0.098*** | (0.030) | | | 0.132*** | (0.039) |
| $\overline{AA}$*Top 3rd poor, gr 8 | | | 0.104*** | (0.028) | | | 0.115*** | (0.038) |
| $\overline{AA}$*Top 3rd poor, gr 9 | | | 0.138*** | (0.029) | | | 0.131*** | (0.034) |
| $\overline{AA}$*Top 3rd poor, gr 10 | | | 0.112*** | (0.035) | | | 0.103*** | (0.034) |
| $\overline{AA}$*Top 3rd poor, gr 11 | | | 0.106*** | (0.033) | | | 0.098*** | (0.034) |
| $\overline{X_{i(t)}}$ | × | | × | | × | | × | |
| $\overline{g(\cdot)}$ | × | | × | | × | | × | |
| Mid, Top poor. | × | | × | | × | | × | |
| Mid, Top poor.*grade | × | | × | | × | | × | |
| grade | × | | × | | × | | × | |
| $\tau_{r*t}$ | × | | × | | × | | × | |
| $R^2$ | 0.255 | | 0.259 | | 0.194 | | 0.199 | |
| Obs. | 60,103 | | 60,103 | | 60,107 | | 60,107 | |
| Districts | 875 | | 875 | | 875 | | 875 | |

Notes: Dependent variable is measured at the cohort-grade-year level. Terciles of poverty (economically disadvantaged) are taken from district means over the entire sample. Main effects for terciles of poverty and interactions with grade are included. $\bar{AA}$ is the average adjusted allotment (in real '000s) cohort $c$ in district $i$ received through grade $g$. $\overline{g(\cdot)}$ measures ADA, and students per square mile (ADA/Area) analogously to the average $\bar{AA}$ and includes quadratics. Area, $\text{Area}^2$ and sparsity proxies, including measures of commute times for workers and average miles bused for students, are included as normal. $X_{i(t)}$ includes the cost of education index, measured as the average over schooling years as is $\bar{AA}$, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Grade are grade fixed effects with 3rd grade as the omitted category. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 7:** Effects of additional funding on test scores by grade and % Hispanic.

| | (1) Reading (z) | | (2) Reading (z) | | (3) Math (z) | | (4) Math (z) | |
|---|---|---|---|---|---|---|---|---|
| $\overline{AA}$/1000 | 0.000 | (0.031) | | | -0.015 | (0.036) | | |
| $\overline{AA}$*grade 3 | | | 0.009 | (0.027) | | | -0.033 | (0.031) |
| $\overline{AA}$*grade 4 | | | -0.000 | (0.033) | | | -0.065* | (0.039) |
| $\overline{AA}$*grade 5 | | | -0.009 | (0.035) | | | -0.016 | (0.041) |
| $\overline{AA}$*grade 6 | | | 0.055 | (0.037) | | | 0.059 | (0.048) |
| $\overline{AA}$*grade 7 | | | 0.024 | (0.036) | | | -0.010 | (0.046) |
| $\overline{AA}$*grade 8 | | | 0.023 | (0.035) | | | -0.024 | (0.045) |
| $\overline{AA}$*grade 9 | | | 0.020 | (0.036) | | | 0.043 | (0.043) |
| $\overline{AA}$*grade 10 | | | -0.014 | (0.040) | | | 0.040 | (0.043) |
| $\overline{AA}$*grade 11 | | | -0.044 | (0.042) | | | 0.029 | (0.044) |
| $\overline{AA}$Mid 3rd Hisp. | 0.058** | (0.027) | | | 0.044 | (0.033) | | |
| $\overline{AA}$*Mid 3rd Hisp, gr 3 | | | 0.029 | (0.026) | | | 0.042 | (0.031) |
| $\overline{AA}$*Mid 3rd Hisp, gr 4 | | | 0.038 | (0.032) | | | 0.068* | (0.037) |
| $\overline{AA}$*Mid 3rd Hisp, gr 5 | | | 0.052 | (0.032) | | | 0.051 | (0.037) |
| $\overline{AA}$*Mid 3rd Hisp, gr 6 | | | 0.031 | (0.033) | | | -0.015 | (0.047) |
| $\overline{AA}$*Mid 3rd Hisp, gr 7 | | | 0.052 | (0.033) | | | 0.028 | (0.045) |
| $\overline{AA}$*Mid 3rd Hisp, gr 8 | | | 0.048 | (0.030) | | | 0.054 | (0.043) |
| $\overline{AA}$*Mid 3rd Hisp, gr 9 | | | 0.091*** | (0.033) | | | 0.049 | (0.042) |
| $\overline{AA}$*Mid 3rd Hisp, gr 10 | | | 0.089** | (0.040) | | | 0.035 | (0.042) |
| $\overline{AA}$*Mid 3rd Hisp, gr 11 | | | 0.100** | (0.040) | | | 0.058 | (0.042) |
| $\overline{AA}$Top 3rd Hisp. | 0.122*** | (0.028) | | | 0.128*** | (0.035) | | |
| $\overline{AA}$*Top 3rd Hisp, gr 3 | | | 0.075*** | (0.028) | | | 0.089*** | (0.033) |
| $\overline{AA}$*Top 3rd Hisp, gr 4 | | | 0.088*** | (0.033) | | | 0.111*** | (0.040) |
| $\overline{AA}$*Top 3rd Hisp, gr 5 | | | 0.105*** | (0.033) | | | 0.089** | (0.039) |
| $\overline{AA}$*Top 3rd Hisp, gr 6 | | | 0.097*** | (0.036) | | | 0.070 | (0.048) |
| $\overline{AA}$*Top 3rd Hisp, gr 7 | | | 0.131*** | (0.036) | | | 0.160*** | (0.048) |
| $\overline{AA}$*Top 3rd Hisp, gr 8 | | | 0.125*** | (0.033) | | | 0.188*** | (0.045) |
| $\overline{AA}$*Top 3rd Hisp, gr 9 | | | 0.166*** | (0.034) | | | 0.164*** | (0.043) |
| $\overline{AA}$*Top 3rd Hisp, gr 10 | | | 0.167*** | (0.038) | | | 0.132*** | (0.042) |
| $\overline{AA}$*Top 3rd Hisp, gr 11 | | | 0.161*** | (0.041) | | | 0.151*** | (0.043) |
| $\underline{X_{i(t)}}$ | × | | × | | × | | × | |
| $g(\cdot)$ | × | | × | | × | | × | |
| Mid, Top Hisp. | × | | × | | × | | × | |
| Mid, Top Hisp.*grade | × | | × | | × | | × | |
| grade | × | | × | | × | | × | |
| $\tau_{r*t}$ | × | | × | | × | | × | |
| $R^2$ | 0.159 | | 0.162 | | 0.107 | | 0.113 | |
| Obs. | 60,103 | | 60,103 | | 60,107 | | 60,107 | |
| Districts | 875 | | 875 | | 875 | | 875 | |

Notes: Dependent variable is measured at the cohort-grade-year level. Terciles of Hispanic are taken from district means over the entire sample. Main effects for terciles of Hispanic and interactions with grade are included. $\bar{A}A$ is the average adjusted allotment (in real '000s) cohort $c$ in district $i$ received through grade $g$. $\overline{g(\cdot)}$ measures ADA, and students per square mile (ADA/Area) analogously to the average $\bar{A}A$ and includes quadratics. Area, Area$^2$ and sparsity proxies, including measures of commute times for workers and average miles bused for students, are included as normal. $X_{i(t)}$ includes the cost of education index, measured as the average over schooling years as is $\bar{A}A$, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Grade are grade fixed effects with 3rd grade as the omitted category. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 8:** Additional funding and four-year dropout and graduation rates.

| | (1) Dropout | (2) Dropout | (3) Dropout | (4) Graduation | (5) Graduation | (6) Graduation |
|---|---|---|---|---|---|---|
| $AA_9$ | -0.016*** | -0.005 | -0.001 | 0.021+ | -0.004 | -0.005 |
| | (0.006) | (0.006) | (0.007) | (0.011) | (0.010) | (0.013) |
| Mid 3rd Poor | | 0.023 | | | -0.060+ | |
| | | (0.017) | | | (0.034) | |
| Top 3rd Poor | | 0.112*** | | | -0.227*** | |
| | | (0.018) | | | (0.030) | |
| $AA_9$ x Mid 3rd Poor | | -0.003 | | | 0.009 | |
| | | (0.004) | | | (0.009) | |
| $AA_9$ x Top 3rd Poor | | -0.020*** | | | 0.043*** | |
| | | (0.004) | | | (0.007) | |
| Mid 3rd Hisp | | | 0.030 | | | -0.043 |
| | | | (0.021) | | | (0.035) |
| Top 3rd Hisp | | | 0.096*** | | | -0.175*** |
| | | | (0.024) | | | (0.039) |
| $AA_9$ x Mid 3rd Hisp | | | -0.008 | | | 0.010 |
| | | | (0.005) | | | (0.008) |
| $AA_9$ x Top 3rd Hisp | | | -0.020*** | | | 0.036*** |
| | | | (0.006) | | | (0.009) |
| $g(.)$ | × | × | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × |
| Dep. Mean | 0.040 | 0.040 | 0.040 | 0.904 | 0.904 | 0.904 |
| $R^2$ | 0.200 | 0.250 | 0.214 | 0.204 | 0.264 | 0.221 |
| Obs. | 6,437 | 6,437 | 6,437 | 6,437 | 6,437 | 6,437 |
| Districts | 814 | 814 | 814 | 814 | 814 | 814 |

Notes: $\overline{AA}_9$ is the (real) average adjusted allotment for district $i$ in year $t$ for the past 9 years in $'000s. Dropout is the 4-year dropout rate (those not graduating, receiving a diploma or GED, or persisting in high school, for the cohort scheduled to graduate on time in year $t$. Graduation is the share scheduled to graduate on time in year $t$ who did. Terciles of poverty (economically disadvantaged) and Hispanic are taken from district means over the entire sample. Main effects for terciles of poverty and interactions with grade are included. Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010, and that are K-12 districts. $g(\cdot)$ includes ADA, $ADA^2$, Area, $Area^2$, students per square mile (ADA/Area) and $(ADA/Area)^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $X_{i(t)}$ includes the cost of education index measured at the year level, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 9:** Long run educational outcomes

| | Share took SAT | | | SAT (Z) | | | Enrolled in College | | | Graduated College | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| $AA_9$ | 0.018 | -0.006 | -0.019 | 0.098 | 0.008 | 0.012 | 0.103*** | 0.090*** | 0.027 | 0.039*** | 0.018 | -0.011 |
| | (0.029) | (0.034) | (0.036) | (0.061) | (0.064) | (0.075) | (0.026) | (0.027) | (0.031) | (0.014) | (0.015) | (0.017) |
| Mid 3rd poor | | -0.122 | | | -0.356* | | | -0.101 | | | -0.149*** | |
| | | (0.108) | | | (0.194) | | | (0.089) | | | (0.0521) | |
| Top 3rd poor | | -0.153 | | | -0.744*** | | | -0.124 | | | -0.126** | |
| | | (0.108) | | | (0.209) | | | (0.087) | | | (0.0491) | |
| $AA_9$ x Mid 3rd poor | | 0.009 | | | 0.067 | | | 0.002 | | | 0.020 | |
| | | (0.026) | | | (0.049) | | | (0.022) | | | (0.013) | |
| $AA_9$ x Top 3rd poor | | 0.010 | | | 0.121** | | | -0.002 | | | 0.004 | |
| | | (0.026) | | | (0.052) | | | (0.022) | | | (0.012) | |
| Mid 3rd Hisp. | | | 0.084 | | | -0.330 | | | -0.170 | | | -0.139** |
| | | | (0.109) | | | (0.224) | | | (0.106) | | | (0.058) |
| Top 3rd Hisp. | | | -0.330*** | | | -0.544** | | | -0.433*** | | | -0.303*** |
| | | | (0.123) | | | (0.242) | | | (0.106) | | | (0.060) |
| $AA_9$ x Mid 3rd Hisp. | | | -0.023 | | | 0.069 | | | 0.042 | | | 0.031** |
| | | | (0.026) | | | (0.056) | | | (0.026) | | | (0.014) |
| $AA_9$ x Top 3rd Hisp. | | | 0.055* | | | 0.095 | | | 0.084*** | | | 0.056*** |
| | | | (0.029) | | | (0.060) | | | (0.025) | | | (0.014) |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × | × | × | × |
| $g(\cdot)$ | × | × | × | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × | × | × | × |
| Obs. | 5,688 | 5,688 | 5,688 | 5,010 | 5,010 | 5,010 | 5,520 | 5,520 | 5,520 | 5,520 | 5,520 | 5,520 |
| Districts | 814 | 814 | 814 | 798 | 798 | 798 | 805 | 805 | 805 | 805 | 805 | 805 |

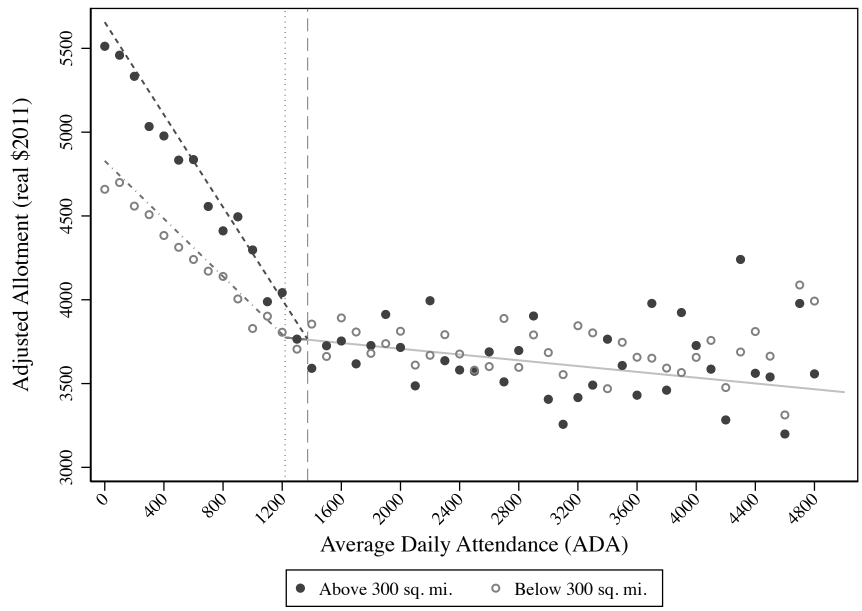
Notes: Outcomes are district averages from College Board and NSC records. $\overline{AA}_9$ is the (real) average adjusted allotment for district $i$ in year $t$ for the past nine years in \$'000s. $g(\cdot)$ includes ADA, ADA$^2$, Area, Area$^2$, students per square mile (ADA/Area) and (ADA/Area)$^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $X_{i(t)}$ includes the cost of education index measured at the year level, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Table 10:** Instrumental variables.

| | (1) ln(Expend) | (2) Read (z) | (3) Math (z) |
|---|---|---|---|
| AA/1000 | 0.095*** | | |
| | (0.022) | | |
| $ln(Expend)$ | | 0.916** | 0.702* |
| | | (0.362) | (0.373) |
| $\underline{X_{i(t)}}$ | × | × | × |
| $\overline{g(\cdot)}$ | × | × | × |
| grade | × | × | × |
| $\tau_{r*t}$ | × | × | × |
| F-stat | 19.20 | | |
| Obs. | 54,430 | 54,430 | 54,427 |
| Districts | 875 | 875 | 875 |

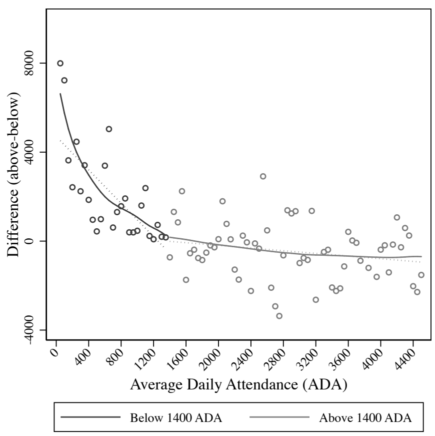
Notes: Dependent variable is measured at the cohort-grade-year level. AA is the average adjusted allotment (in real '000s) cohort $c$ in district $i$ received through grade $g$. Expenditures are calculated in the same fashion. $\overline{g(\cdot)}$ measures ADA, and students per square mile (ADA/Area) analogously to the average $\bar{A}A$ and includes quadratics. Area, Area$^2$ and sparsity proxies, including measures of commute times for workers and average miles bused for students, are included as normal. $X_{i(t)}$ includes the cost of education index, measured as the average over schooling years as is $\bar{A}A$, whether the district is consolidated, and whether the district is K-12. $\tau_{r*t}$ are region-year fixed effects. Grade are grade fixed effects with 3rd grade as the omitted category. Standard errors clustered on districts in parentheses. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$).

**Figure 1:** Expected and true adjusted allotment funding.

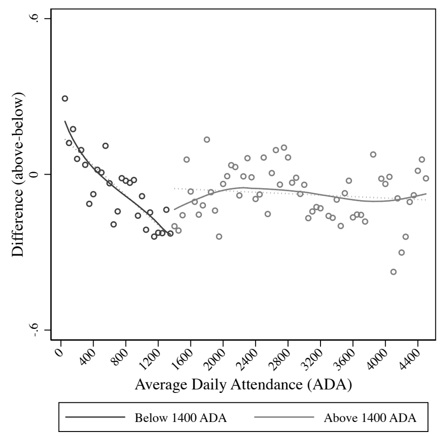



Notes: Lines plots the adjusted allotment as calculated from Texas's funding formula using the average BA over all years as projected by Equation 1. Points plot the true adjusted allotment in bins of 100 ADA, pooled over all years. Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010.

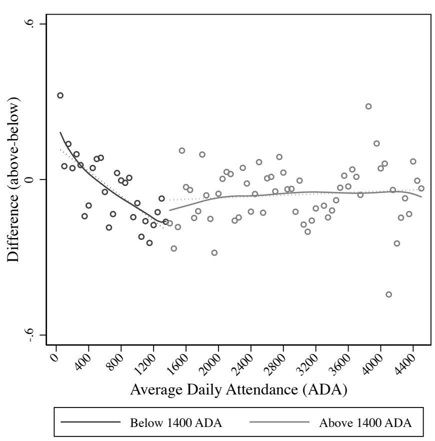
**Figure 2:** Differences in funding and outcomes (above-below 300 sq. mi.) by ADA



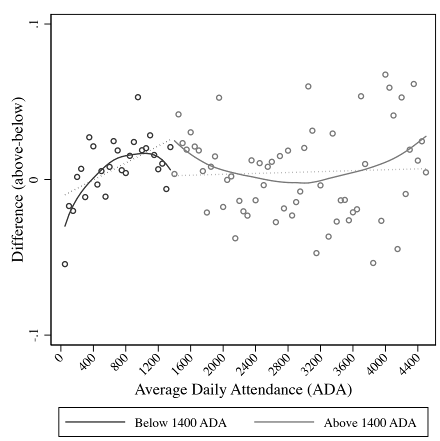

(a) Expenditures




(b) Reading



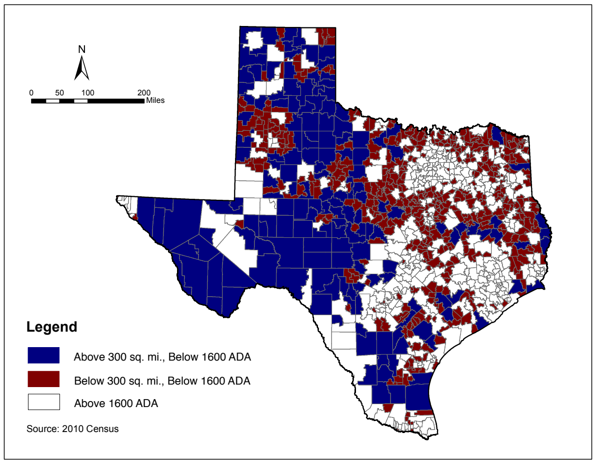

(c) Math

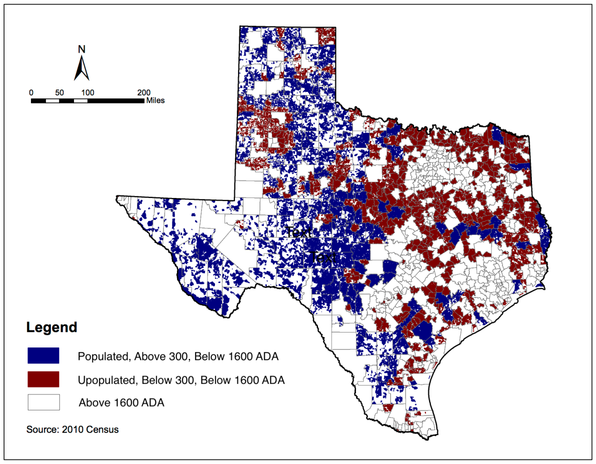



(d) Dropout

Notes: Data points plot differences (above 300 sq. mi. - below 300 sq. mi.) by ADA in bins of 50. Solid lines are locally weighted plots, light dotted lines are linear fits, both estimated separately above and below the 1400 ADA bin. Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010.

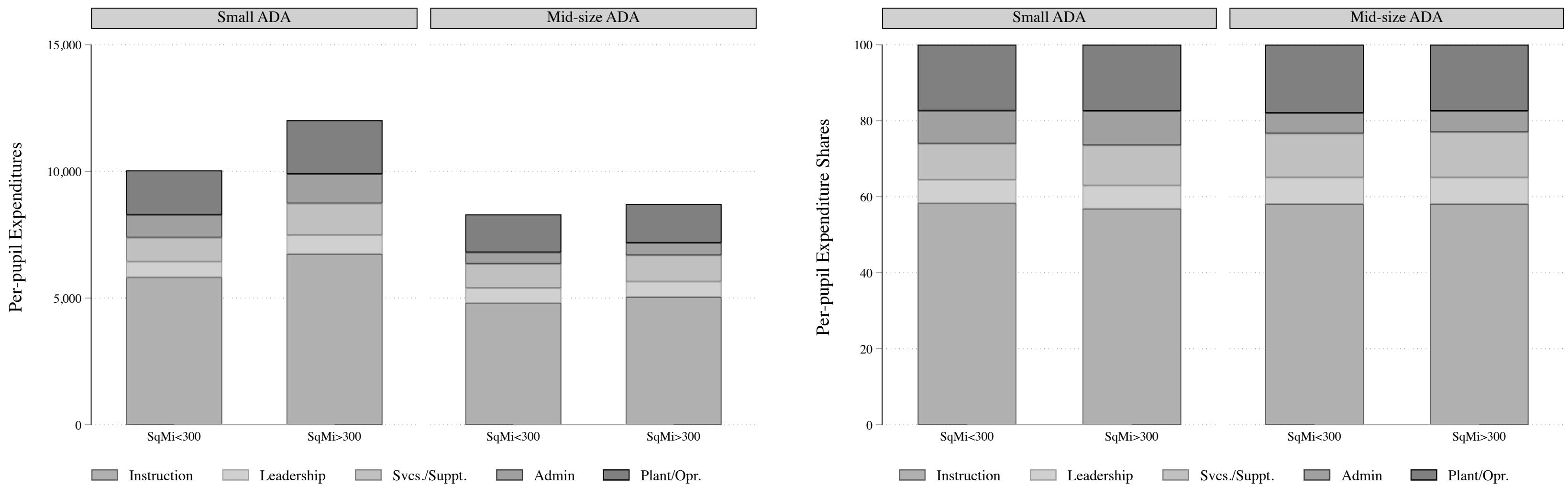
**Figure 3:** Map of (populated) district area.




Notes: Populated census blocks are determined by those with > 0 residents in the 2000 Census.

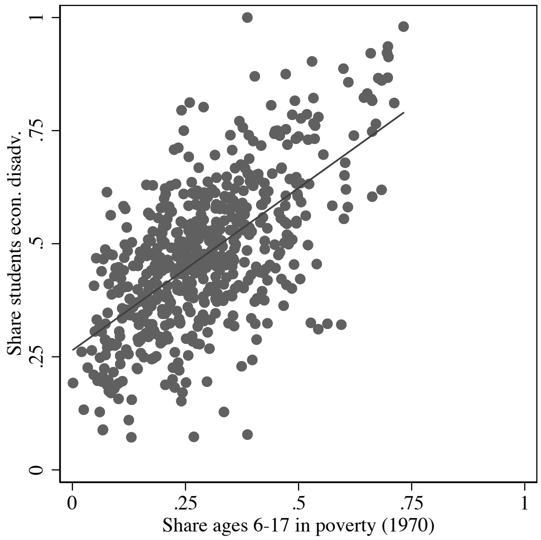
**Figure 4:** Increased funding and budget shares.



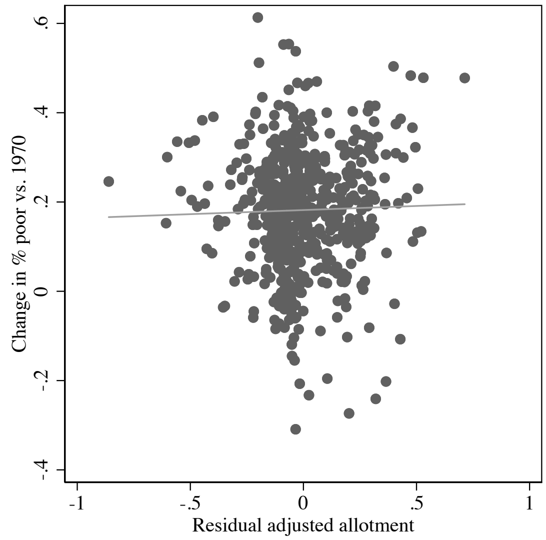

(a) Expenditures (\$) (b) Expenditures %

Notes: Figures split districts into low (< 1,373) and midsized (> 1,373 and < 5,000), and above or below 300 sq. mi. Bars plot average per-pupil expenditures or expenditure shares in real \$2011. Definitions for each category can be found in Table B. Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010.

**Figure 5:** Changes in poverty since 1970.



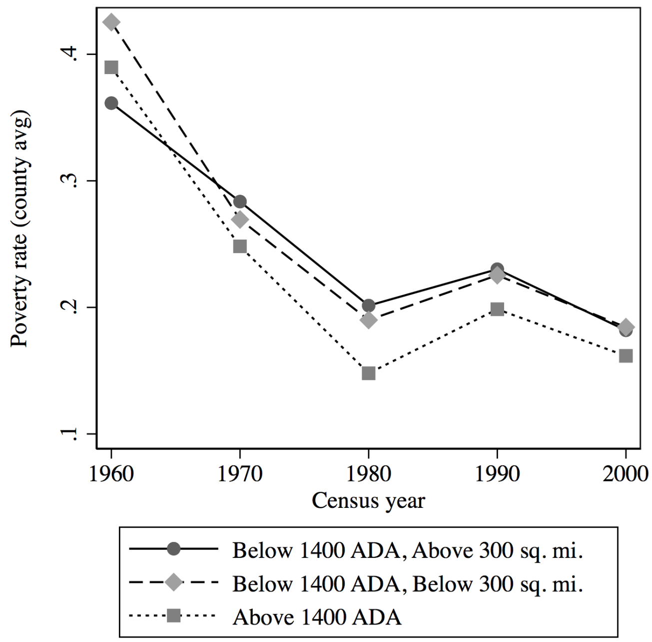

(a) Changes in poverty




(b) Changes in poverty by residual AA

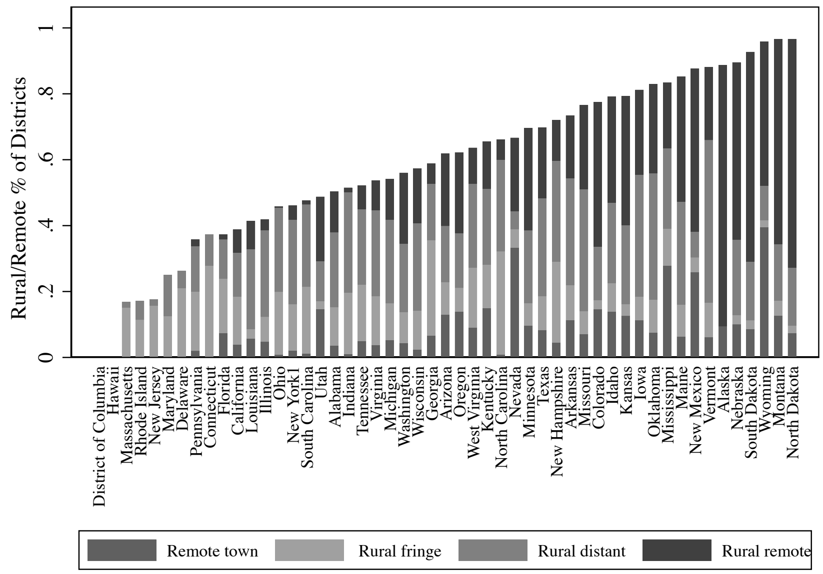
Notes: Left plot shows share of children (6-17) in poverty (Census definition) in 1970 SSDT. Share students economically disadvantaged is average district level share economically disadvantaged for districts in our sample (not missing in 1970). Change in % poor (panel B) is share in poverty (1970) – share economically disadvantaged in our sample. Residual adjusted allotment are residuals from a pooled cross-sectional regression of adjusted allotment on quadratics in ADA, area, sparsity, and the CEI.

**Figure 6:** Historical poverty rates.



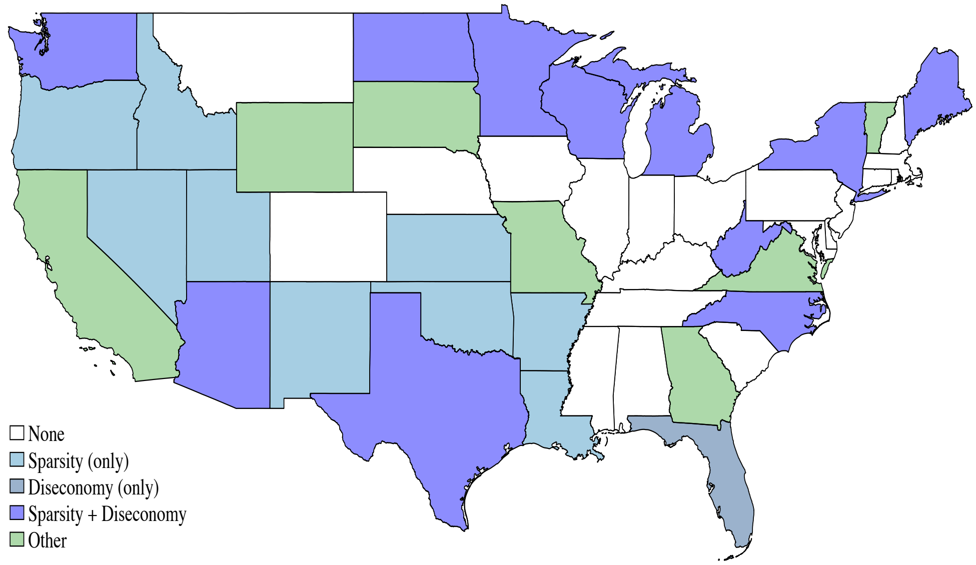

Notes: Figure plots county average poverty rates from Decennial Census.

**Figure 7:** Rural and Remote School Districts in the US




Notes: Data from U.S. Department of Education, National Center for Education Statistics, Common Core of Data (CCD), "Local Education Agency Universe Survey," 2013–14 (version 1a)." Definitions - Remote Town: Territory inside an urban cluster that is more than 35 miles from an urbanized area. Rural Fringe: Census-defined rural territory that is less than or equal to 5 miles from an urbanized area, as well as rural territory that is less than or equal to 2.5 miles from an urban cluster Rural distant: Census-defined rural territory that is more than 5 miles but less than or equal to 25 miles from an urbanized area, as well as rural territory that is more than 2.5 miles but less than or equal to 10 miles from an urban cluster. Rural remote: Census-defined rural territory that is more than 25 miles from an urbanized area and is also more than 10 miles from an urban cluster.

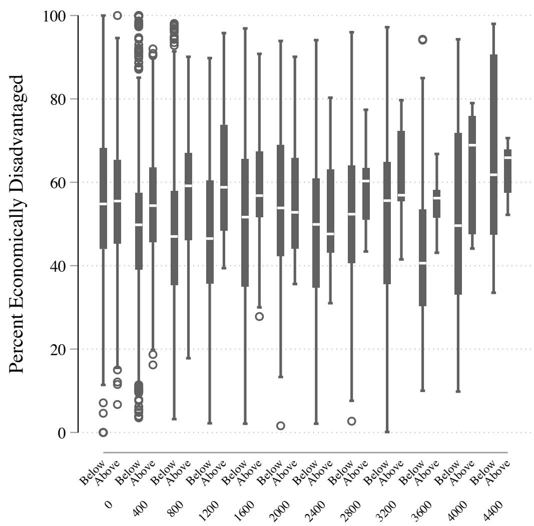
**Figure 8:** States with Size or Sparsity Adjustments.



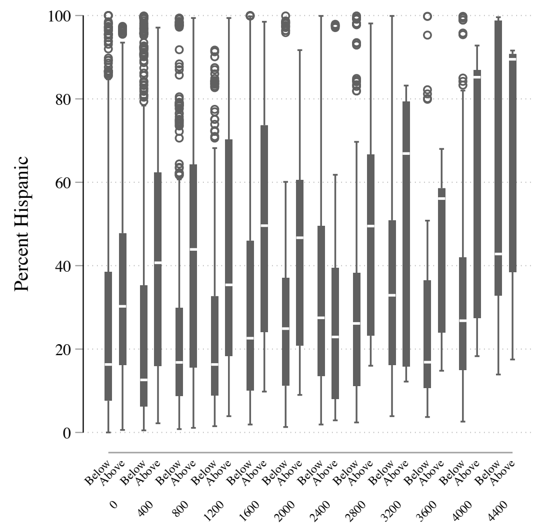

Shaded states are those with funding adjustments for geographic size, diseconomy of scale in students, adjustment for low/declining enrollment, or sparsity. Authors own calculations.

# Appendix

**Figure A1:** Distribution of Economically Disadvantage and Hispanic students.




(a) Percent poor




(b) Percent Hispanic

Figures plots the share of students who are economically disadvantaged or Hispanic by ADA in bins of 400, and by whether the district is above or below 300 square miles. Each unit of observation is a district-year.

**Figure A2:** Districts found in SSDT, by ADA.

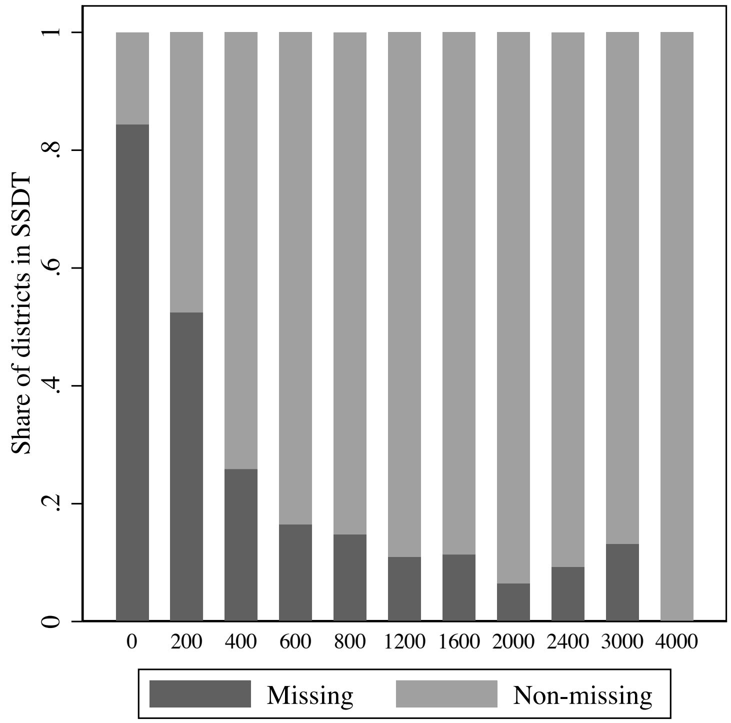



Figure plots the share of districts, binned by average ADA from 2003-2010, found or not in the Census/NIE (1970).

**Table A1:** Summary statistics.

| | Small ADA | | Midsize ADA | |
|---|---|---|---|---|
| | Below 300 | Above 300 | Below 300 | Above 300 |
| ADA | 506.76 | 528.45 | 2604.43 | 2485.52 |
| | (356.52) | (365.53) | (938.07) | (859.05) |
| Area | 128.37 | 693.44 | 126.37 | 645.87 |
| | (71.54) | (557.10) | (80.22) | (484.04) |
| ADA/Area | 8.78 | 1.08 | 49.92 | 5.17 |
| | (32.32) | (0.93) | (81.28) | (3.19) |
| Populated area | 91.97 | 297.05 | 101.76 | 329.82 |
| | (54.20) | (187.26) | (70.41) | (147.70) |
| Unpopulated area | 18.93 | 2.27 | 69.15 | 10.44 |
| | (161.13) | (1.95) | (107.81) | (9.68) |
| CEI | 1.06 | 1.07 | 1.09 | 1.09 |
| | (0.02) | (0.02) | (0.03) | (0.02) |
| Basic Allotment | 3303.76 | 3304.47 | 3304.59 | 3304.99 |
| | (631.19) | (631.83) | (631.97) | (632.90) |
| Adjusted Basic Allotment | 3427.35 | 3437.64 | 3509.64 | 3512.20 |
| | (617.41) | (603.71) | (656.07) | (641.08) |
| Adjusted Allotment | 4371.28 | 4910.61 | 3704.85 | 3688.82 |
| | (838.56) | (1002.15) | (697.25) | (701.92) |
| Revenue | 12069.14 | 14860.54 | 10023.37 | 10520.54 |
| | (3070.57) | (6293.46) | (1241.85) | (1590.29) |
| Expenditures | 10357.50 | 12425.18 | 8568.69 | 8997.63 |
| | (2563.72) | (3862.03) | (955.32) | (1011.19) |
| Expenditures (total) | 12377.23 | 15177.35 | 11137.50 | 10924.77 |
| | (4282.71) | (7762.61) | (3336.95) | (2515.96) |
| Share black | 0.07 | 0.05 | 0.10 | 0.09 |
| | (0.11) | (0.09) | (0.12) | (0.10) |
| Share Hispanic | 0.24 | 0.39 | 0.33 | 0.46 |
| | (0.23) | (0.26) | (0.27) | (0.28) |
| Share poor | 0.52 | 0.56 | 0.51 | 0.58 |
| | (0.17) | (0.16) | (0.21) | (0.13) |
| Share LEP | 0.05 | 0.06 | 0.09 | 0.09 |
| | (0.07) | (0.08) | (0.10) | (0.07) |
| K12 | 0.89 | 0.98 | 0.99 | 1.00 |
| | (0.31) | (0.16) | (0.08) | (0.00) |
| CSD | 0.03 | 0.10 | 0.02 | 0.07 |
| | (0.16) | (0.30) | (0.13) | (0.26) |
| Miles bused | 2.24 | 5.14 | 0.95 | 1.67 |
| | (2.66) | (4.59) | (0.38) | (0.75) |
| Travel time | 28.10 | 22.09 | 27.15 | 23.15 |
| | (6.69) | (5.70) | (6.81) | (6.00) |
| Read (z) | 0.04 | 0.05 | 0.02 | -0.05 |
| | (0.24) | (0.24) | (0.24) | (0.17) |
| Math (z) | 0.03 | 0.03 | 0.05 | -0.01 |
| | (0.27) | (0.27) | (0.25) | (0.19) |
| Dropout rate | 0.03 | 0.04 | 0.05 | 0.06 |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| Graduation rate | 0.92 | 0.91 | 0.87 | 0.86 |
| | (0.07) | (0.09) | (0.07) | (0.07) |
| Obs. | 3,937 | 1,371 | 1,226 | 424 |

Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Small ADA districts are less than 1,373 in average ADA over the sample. Mid-sized are those between 1,373 and 5,000.

**Table A2:** Pooled adjustment allotment, revenue, and outcomes.

| | (1) Revenue | (2) Reading (z) | (3) Math (z) | (4) Dropout | (5) Graduation |
|---|---|---|---|---|---|
| $\overline{AA}/1000$ | 1954.499*** | 0.092** | 0.078+ | -0.009 | 0.010 |
| | (647.516) | (0.039) | (0.044) | (0.006) | (0.011) |
| $\overline{g(\cdot)}$ | × | × | × | × | × |
| $\overline{X_{i(t)}}$ | × | × | × | × | × |
| Region | × | × | × | × | × |
| $R^2$ | 0.488 | 0.255 | 0.179 | 0.233 | 0.305 |
| N | 875 | 875 | 875 | 815 | 815 |

Notes: Regression is on averages for all covariates and dependent variables over all years (2003-2010). $\overline{g(\cdot)}$ includes district averages of ADA, $ADA^2$, Area, $Area^2$, students per square mile (ADA/Area) and $(ADA/Area)^2$, and sparsity proxies, which include measures of commute times for workers and average miles bused for students, as shown in Equation 7 and Table 1. $\overline{X_i}$ includes district average cost of education index and whether the district is consolidated and whether the district is K-12. ($^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$). Robust standard errors in parentheses.

**Table A3:** Grade evolution reading, Robustness.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| $\overline{AA}$*grade 3 | 0.071*** | 0.072*** | 0.093*** | 0.077*** | 0.032* | 0.041** | 0.059** | 0.071*** | 0.066** |
| | (0.025) | (0.023) | (0.024) | (0.024) | (0.018) | (0.017) | (0.024) | (0.025) | (0.025) |
| $\overline{AA}$*grade 4 | 0.075*** | 0.078*** | 0.102*** | 0.083*** | 0.030 | 0.046** | 0.063** | 0.072** | 0.070** |
| | (0.029) | (0.027) | (0.029) | (0.028) | (0.022) | (0.020) | (0.028) | (0.029) | (0.030) |
| $\overline{AA}$*grade 5 | 0.083** | 0.085*** | 0.112*** | 0.090*** | 0.030 | 0.042** | 0.064** | 0.081** | 0.076** |
| | (0.032) | (0.029) | (0.032) | (0.031) | (0.022) | (0.020) | (0.030) | (0.032) | (0.033) |
| $\overline{AA}$*grade 6 | 0.135*** | 0.137*** | 0.165*** | 0.143*** | 0.081*** | 0.098*** | 0.110*** | 0.131*** | 0.128*** |
| | (0.033) | (0.030) | (0.032) | (0.031) | (0.023) | (0.021) | (0.031) | (0.033) | (0.033) |
| $\overline{AA}$*grade 7 | 0.127*** | 0.130*** | 0.157*** | 0.135*** | 0.068*** | 0.083*** | 0.098*** | 0.121*** | 0.126*** |
| | (0.034) | (0.030) | (0.033) | (0.032) | (0.024) | (0.022) | (0.032) | (0.034) | (0.035) |
| $\overline{AA}$*grade 8 | 0.122*** | 0.125*** | 0.152*** | 0.130*** | 0.062*** | 0.076*** | 0.094*** | 0.116*** | 0.120*** |
| | (0.033) | (0.029) | (0.033) | (0.032) | (0.024) | (0.022) | (0.032) | (0.033) | (0.034) |
| $\overline{AA}$*grade 9 | 0.152*** | 0.155*** | 0.182*** | 0.161*** | 0.090*** | 0.101*** | 0.128*** | 0.144*** | 0.150*** |
| | (0.033) | (0.029) | (0.033) | (0.032) | (0.024) | (0.022) | (0.031) | (0.033) | (0.034) |
| $\overline{AA}$*grade 10 | 0.119*** | 0.122*** | 0.149*** | 0.127*** | 0.057** | 0.076*** | 0.092*** | 0.112*** | 0.113*** |
| | (0.034) | (0.031) | (0.034) | (0.033) | (0.025) | (0.023) | (0.032) | (0.035) | (0.035) |
| $\overline{AA}$*grade 11 | 0.093*** | 0.095*** | 0.123*** | 0.100*** | 0.030 | 0.049** | 0.065** | 0.082** | 0.086** |
| | (0.035) | (0.031) | (0.035) | (0.034) | (0.025) | (0.023) | (0.032) | (0.034) | (0.036) |
| Donut hole | | | | | | | | | × |
| Drop ADA changes | | | | | | | | × | |
| K-12 only | | | | | | | × | | |
| $Demog_t$*grade | | | | | | × | | | |
| $Demog_{1997}$*trend | | | | | × | | | | |
| Density, Oil | | | | × | | | | | |
| Contemp. | | | × | | | | | | |
| Commute | × | | × | × | | × | × | × | × |
| $g(.)$ | × | | × | × | × | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × |
| grade | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × |
| $R^2$ | 0.109 | 0.102 | 0.112 | 0.116 | 0.275 | 0.324 | 0.119 | 0.114 | 0.098 |
| N | 60,103 | 60,103 | 60,103 | 60,103 | 60,103 | 60,103 | 57,767 | 49,763 | 55,236 |
| Obs. | 875 | 875 | 875 | 875 | 875 | 875 | 816 | 875 | 807 |

Column 1 is full model from column 2 of Table 5. $\overline{AA}$ is average AA up to current grade for cohort $c$ in district $i$ in grade $g$. Column 2 drops area, ADA/area, and travel times. Contemp (col. 3) uses current measures of ADA, ADA/Area and CEI. Density is population per square mile in 2010 and Oil is a quadratic in per-pupil taxable land value of oil, gas and minerals. $Demog_{1997} * trend$ are main effects of share Black, Hispanic, other race, and Poor in 1997 interacted with a linear time trend. $Demog_t$ are contemporaneous measures of these plus interactions with grade dummies. K-12 only limits the sample to K-12 districts. Drop ADA changes drops districts with 10% largest positive and 10% largest negative year-on-year ADA changes. Donut hole drops districts that ever crossed the 1,220 to 1,373 ADA threshold in our sample. ($^*p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$). Robust standard errors clustered on districts in parentheses.

**Table A4:** Grade evolution math, Robustness.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| $\overline{AA}$*grade 3 | 0.037 | 0.038 | 0.064** | 0.045* | 0.007 | 0.010 | 0.021 | 0.038 | 0.033 |
| | (0.027) | (0.026) | (0.027) | (0.027) | (0.023) | (0.021) | (0.027) | (0.028) | (0.029) |
| $\overline{AA}$*grade 4 | 0.029 | 0.030 | 0.061* | 0.039 | -0.008 | 0.000 | 0.009 | 0.024 | 0.027 |
| | (0.033) | (0.031) | (0.032) | (0.032) | (0.027) | (0.025) | (0.032) | (0.034) | (0.034) |
| $\overline{AA}$*grade 5 | 0.067* | 0.068** | 0.102*** | 0.077** | 0.026 | 0.025 | 0.046 | 0.066* | 0.062* |
| | (0.035) | (0.033) | (0.034) | (0.034) | (0.028) | (0.026) | (0.034) | (0.035) | (0.036) |
| $\overline{AA}$*grade 6 | 0.107*** | 0.109*** | 0.143*** | 0.118*** | 0.065** | 0.080*** | 0.078** | 0.109*** | 0.101*** |
| | (0.036) | (0.035) | (0.036) | (0.036) | (0.029) | (0.028) | (0.035) | (0.037) | (0.038) |
| $\overline{AA}$*grade 7 | 0.090** | 0.092*** | 0.127*** | 0.101*** | 0.043 | 0.056* | 0.053 | 0.090** | 0.090** |
| | (0.038) | (0.036) | (0.037) | (0.037) | (0.031) | (0.029) | (0.037) | (0.039) | (0.039) |
| $\overline{AA}$*grade 8 | 0.098*** | 0.100*** | 0.134*** | 0.108*** | 0.049 | 0.062** | 0.057 | 0.093** | 0.097** |
| | (0.037) | (0.035) | (0.037) | (0.037) | (0.030) | (0.028) | (0.036) | (0.038) | (0.039) |
| $\overline{AA}$*grade 9 | 0.152*** | 0.154*** | 0.188*** | 0.163*** | 0.099*** | 0.115*** | 0.122*** | 0.141*** | 0.146*** |
| | (0.036) | (0.034) | (0.036) | (0.036) | (0.029) | (0.027) | (0.035) | (0.037) | (0.038) |
| $\overline{AA}$*grade 10 | 0.133*** | 0.135*** | 0.169*** | 0.144*** | 0.080*** | 0.100*** | 0.103*** | 0.121*** | 0.124*** |
| | (0.037) | (0.035) | (0.037) | (0.037) | (0.030) | (0.028) | (0.036) | (0.038) | (0.039) |
| $\overline{AA}$*grade 11 | 0.139*** | 0.141*** | 0.175*** | 0.150*** | 0.085*** | 0.099*** | 0.108*** | 0.126*** | 0.129*** |
| | (0.037) | (0.035) | (0.037) | (0.037) | (0.030) | (0.028) | (0.036) | (0.038) | (0.039) |
| Donut hole | | | | | | | | | × |
| Drop ADA changes | | | | | | | | × | |
| K-12 only | | | | | | | × | | |
| $\text{Demog}_t$*grade | | | | | | × | | | |
| $\text{Demog}_{1997}$*trend | | | | | × | | | | |
| Density, Oil | | | | × | | | | | |
| Contemp. | | | × | | | | | | |
| Commute | × | | × | × | | × | × | × | × |
| $g(.)$ | × | | × | × | × | × | × | × | × |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × | × |
| grade | × | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × | × |
| $R^2$ | 0.079 | 0.073 | 0.083 | 0.085 | 0.200 | 0.252 | 0.087 | 0.077 | 0.070 |
| Obs. | 60,107 | 60,107 | 60,107 | 60,107 | 60,107 | 60,107 | 57,776 | 49,770 | 55,240 |
| Districts | 875 | 875 | 875 | 875 | 875 | 875 | 816 | 875 | 807 |

Column 1 is full model from column 2 of Table 5. $\overline{AA}$ is average AA up to current grade for cohort $c$ in district $i$ in grade $g$. Column 2 drops area, ADA/area, and travel times. Contemp (col. 3) uses current measures of ADA, ADA/Area and CEI. Density is population per square mile in 2010 and Oil is a quadratic in per-pupil taxable land value of oil, gas and minerals. $\text{Demog}_{1997} * trend$ are main effects of share Black, Hispanic, other race, and Poor in 1997 interacted with a linear time trend. $Demog_t$ are contemporaneous measures of these plus interactions with grade dummies. K-12 only limits the sample to K-12 districts. Drop ADA changes drops districts with 10% largest positive and 10% largest negative year-on-year ADA changes. Donut hole drops districts that ever crossed the 1,220 to 1,373 ADA threshold in our sample. ($*p < 0.10$ $**p < 0.05$ $***p < 0.01$). Robust standard errors clustered on districts in parentheses.

**Table A5:** Summary statistics by availability in 1970 Census/NIE.

| | Not missing | Missing |
|---|---|---|
| ADA | 1301.27 | 430.66 |
| | (1087.89) | (592.32) |
| Area | 295.32 | 217.51 |
| | (373.04) | (319.44) |
| ADA/Area | 15.53 | 11.94 |
| | (42.87) | (49.03) |
| AA | 4,165.5 | 4,609.6 |
| | (899.2) | (929.8) |
| % Poor | 53.99 | 51.19 |
| | (16.78) | (19.12) |
| Reading (z) | 0.020 | 0.061 |
| | (0.218) | (0.262) |
| Math (z) | 0.026 | 0.036 |
| | (0.241) | (0.302) |
| N | 4,560 | 2,398 |
| n | 570 | 305 |

Table shows means (and standard deviations) over entire sample (2003-2010) by whether district statistics are available in Census/NIE 1970 tables.

**Table A6:** Additional robustness controlling for poverty in 1970.

| | (1) Read (z) | (2) Read (z) | (3) Read (z) | (4) Read (z) | (5) Math (z) | (6) Math (z) | (7) Math (z) | (8) Math (z) |
|---|---|---|---|---|---|---|---|---|
| $\overline{AA}$ | 0.097*** | 0.078*** | | | 0.077** | 0.059** | | |
| | (0.029) | (0.026) | | | (0.032) | (0.029) | | |
| Share poor (1970) | | -0.677*** | | -0.678*** | | -0.701*** | | -0.701*** |
| | | (0.068) | | (0.068) | | (0.080) | | (0.080) |
| Share poor missing | | -0.177*** | | -0.177*** | | -0.210*** | | -0.211*** |
| | | (0.028) | | (0.028) | | (0.033) | | (0.033) |
| $\overline{AA}$ *grade 4 | | | 0.043** | 0.032+ | | | 0.012 | 0.002 |
| | | | (0.018) | (0.016) | | | (0.021) | (0.019) |
| $\overline{AA}$ *grade 5 | | | 0.048** | 0.035+ | | | 0.049** | 0.037+ |
| | | | (0.021) | (0.018) | | | (0.023) | (0.021) |
| $\overline{AA}$ *grade 6 | | | 0.100*** | 0.086*** | | | 0.089*** | 0.076*** |
| | | | (0.021) | (0.019) | | | (0.024) | (0.022) |
| $\overline{AA}$ *grade 7 | | | 0.092*** | 0.078*** | | | 0.072*** | 0.060** |
| | | | (0.023) | (0.020) | | | (0.026) | (0.024) |
| $\overline{AA}$ *grade 8 | | | 0.087*** | 0.073*** | | | 0.080*** | 0.067*** |
| | | | (0.022) | (0.019) | | | (0.025) | (0.023) |
| $\overline{AA}$ *grade 9 | | | 0.118*** | 0.104*** | | | 0.134*** | 0.122*** |
| | | | (0.023) | (0.020) | | | (0.025) | (0.023) |
| $\overline{AA}$ *grade 10 | | | 0.084*** | 0.071*** | | | 0.115*** | 0.103*** |
| | | | (0.024) | (0.022) | | | (0.026) | (0.024) |
| $\overline{AA}$ *grade 11 | | | 0.058** | 0.044** | | | 0.121*** | 0.109*** |
| | | | (0.025) | (0.022) | | | (0.026) | (0.024) |
| Poor missing x year | | × | | × | | × | | × |
| Share poor x year | | × | | × | | × | | × |
| $X_{i(t)}$ | × | × | × | × | × | × | × | × |
| $g(.)$ | × | × | × | × | × | × | × | × |
| grade | × | × | × | × | × | × | × | × |
| $\tau_{r*t}$ | × | × | × | × | × | × | × | × |
| $R^2$ | 0.106 | 0.149 | 0.107 | 0.150 | 0.075 | 0.106 | 0.079 | 0.109 |
| Obs. | 60,103 | 60,103 | 60,103 | 60,103 | 60,107 | 60,107 | 60,107 | 60,107 |
| Districts | 875 | 875 | 875 | 875 | 875 | 875 | 875 | 875 |

Columns 1, 3, 5 and 7 replicate main specifications in Table 5. Share poor (1970) is share of residents ages 7-16 who are in poverty according to the Census definition in the 1970 SSDT. Missing is an indicator for missing poverty measure. Share poor*Year and Missing*Year are Interactions between those measures and year fixed effects. ($*p < 0.10$ $**p < 0.05$ $***p < 0.01$). Robust standard errors clustered on districts in parentheses.

**Table B:** Definition of budget allocation categories

| | |
|---|---|
| Instruction | All activities dealing directly with the interaction between teachers and students, including instruction aided with computers. |
| Instructional Leadership & School Leadership | Managing, directing, supervising, and providing leadership for staff who provide instructional services, and directing and managing a school. |
| Instructional Related Services, Co-curricular Activities, & Student Support Services | Expenditures for educational resources and media, such as resource centers and libraries; and, curriculum development and instructional staff development; school-sponsored activities during or after the school day that are not essential to the delivery of instructional services; guidance, counseling, and evaluation services; social work services ; and, health services. |
| Central Administration & Data Processing Services | Managing or governing the school district as an overall entity; costs associated with the purchase or sale of attendance credits either from the state or from other school district(s); data processing services, whether in-house or contracted. |
| Plant Maintenance and Operations, Security and Monitoring Services & Food Services | Transporting students to and from school; keeping the physical plant and grounds in effective working condition; food service operation, including cost of food and labor; keeping student and staff surroundings safe. |

Definitions taken from TEA AEIS glossary.