

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

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February, 2019

## Abstract

We leverage an obscure set of rules in Texas's school funding formula granting districts additional revenue as a function of size and sparsity. We use variation from kinks and discontinuities in the formula to ask how districts spend additional discretionary funds and whether these improve student outcomes. We find that they do. An additional \$1,000 in base funding leads to a 0.1 s.d. increase in ELA scores, a 0.08 s.d. increase in math, a one percentage point decrease in dropout rates (25%), and 6 percentage point increase in college enrollment. We show that gains are concentrated among poorer and minority districts, and accumulate in later grades due to increased exposure. An analysis of budget allocations reveals that additional funding only marginally affects budget shares (e.g. teaching vs. administration) with a very small shift away from direct instruction. Implications for policy are discussed.

JEL: H75; I21; I22; I28

Keywords: School finance; Rural schools; Texas; Achievement; College.

# 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.<sup>1</sup> 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 to 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 close to \$1,000 in additional per-pupil funding 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

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<sup>1</sup>See Card and Krueger (1996); Goldhaber and Brewer (1997); Greenwald et al. (1996); Hanushek (1989, 1997); 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).<sup>2</sup> 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 dominated by studies in Michigan (Chakrabarti and Roy, 2015, 2017; Chaudhary, 2009; Hyman, 2017; Papke, 2005; Roy, 2011). 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, viewing 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 each grade. We find that districts receiving additional funding perform significantly better in

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<sup>2</sup>See Murray et al. (1998) for nationwide finance reforms and effects on the distribution of spending.

both reading and math, and marginally 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 improves reading scores by almost 0.1 standard deviations and math scores by nearly 0.08. 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 1 percentage point, off a base of 4 percent. Again, gains are concentrated in poorer and more Hispanic districts. Still, these gains in achievement and attainment are not sufficient to close the level gap between poor or Hispanic districts and their whiter and 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 in high school leads to a 0.7 standard deviation increase in SAT scores, though estimates are noisy, again with larger and statistically meaningful effects in poorer and more Hispanic districts. We also observe a 6.5 percentage point increase in college enrollment, but do not observe an increase in college graduation rates.

We note that these effects are modest compared with some prior work, for example Guryan (2001) and Lafortune et al. (2018), though are more comparable to others, for example Papke (2005) and Hyman (2017). This is not surprising. A distinguishing characteristic of our contribution is that additional funds here are not targeted toward the poorest districts as is the case in much of the prior literature. Effects of additional funding in the poorest districts in our sample are larger. Since small and rural schools receive more funding on average in Texas, we show that additional funding can benefit high poverty schools even when they are well funded.

When we ask how districts spend additional discretionary funds we find that they keep original spending shares roughly constant, 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 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, these findings suggest that while 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 the Texas Education Agency (TEA). In few cases, measures were unavailable on the TEA website 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 1992 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.<sup>3</sup> This results in a sample of 875 school districts.<sup>4</sup> Summary statistics for relevant measures, which we describe in the next section, can be seen in [Table 1](#).

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-year 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 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. See (United States Department of Education, 1970) for details of the data.

### 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. [Figure 1](#) shows real per-pupil revenue for all districts by source over the years covered by our study.

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 fact that the funding formula does not perfectly capture the true

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<sup>3</sup>Regular ADA is the average number of students in a district measured over a six week period, not including special education students.

<sup>4</sup>There are just over 1,100 non-charter districts in Texas across years 2003-2010.

relationship between size and sparsity and the cost of education. The genesis of the rule dates back nearly 70 years.

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 TTARA (2012).

The primary element of Tier I funding, which accounts for the large majority of district resources, is the district’s adjusted allotment 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:

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

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:

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

We depict this formula graphically [Figure 2](#). 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.<sup>5</sup> 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, the AA is as follows:

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

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 3](#) 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 2](#) 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 clearly for the adjusted allotment. 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 is about \$1,000 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 above 1,373, there is no difference in adjusted allotment by geographic size across ADA. Panels B through D repeat this exercise for reading

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<sup>5</sup>These values, and the number of categories, change over time. We take these examples from the relevant time period from (TTARA, 2012).



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.

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. 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 and discontinuous precisely at 300, 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.<sup>6</sup> This puts our analysis inline with Hyman (2017) who uses changes in the base funding formula in Michigan, which has several notches, as an instrument for funding levels.

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

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<sup>6</sup>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.

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 2](#) is discontinuous and kinked. Let us represent the discontinuous funding formula described above with the function  $f(\cdot)$ :

$$AA \equiv f(ADA, above300) \quad (4)$$

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:

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

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:

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

Identification in this case relies on sufficiently capturing the true cost of size and sparsity in  $g(\cdot)$ , though this function is unknown.

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 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.<sup>7</sup> 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 4](#), 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.

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<sup>7</sup>We calculate this by measuring the total area of all census blocks with any person living in them in the 2010 Census.

We build these factors into our estimation strategy by first estimating the relationship between the adjusted allotment and per-pupil funding, net of smooth controls for the true cost of size and sparsity. We then 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 specification described above, our full specification for funding for district  $i$  in school year  $t$  is:

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

$y_{it}$  is one of several measures of district finance, for example total revenue, or Tier I funds. We put four components into  $g(\cdot)$ .  $D_{it}$  is the number of students in ADA.  $A_i$  is district area in square miles.  $S_{it}$  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 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, an indicator for the few districts that do not offer all grades K-12, and an indicator if the district is considered consolidated.<sup>8</sup> 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 then test for endogenous responses on the part of districts, 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

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<sup>8</sup>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.

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.<sup>9</sup> We then do the same for average ADA, sparsity, and CEI. This full model for (3rd grade) cohort  $c$  in district  $i$  at grade  $g^*$  (which defines year  $t$ ) is as follows:

$$y_{icg} = \alpha + \sum_{g=3}^{11} \beta_g \overline{AA}_{icg} + \Gamma g(\bar{D}_{icg}, A_i, \bar{S}_{chg}, \bar{C}_{icg}) + X'_{it} \Pi + \rho_g + \tau_{r*} + \varepsilon_{igt} \quad (8)$$

This is analogous to our funding equation with exception for taking the cumulative average funding students were exposed to.<sup>10</sup> We define the cumulative average AA for cohort  $c$  in district  $i$  at grade  $g^*$  as,

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

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.<sup>11</sup> 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.

Thus we interpret  $\beta_g$  as 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. 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, or 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 four years to approximate our cumulative exposure measure during high school.<sup>12</sup>

<sup>9</sup>Some districts do not have kindergarten, though results are similar if we include funding at that age too.

<sup>10</sup>This is similar to average cumulative exposure measures in Aaronson and Mazumder (2011).

<sup>11</sup>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.

<sup>12</sup>Results from a 3 year average are similar, as are longer lags.

We next 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 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 identically to those for high school dropout and graduation rates, though here we use a three-year average adjusted allotment since students are taking exams in the 11th grade.<sup>13</sup>

#### 4.4 Effects on Input Shares

Finally, we conclude with estimates where the left hand side of a district-by-year equation, as in our funding regressions in Equation 7, are spending shares 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.

## 5 Results

### 5.1 Residual Variation in Funding

We begin by estimating our base model from Equation 7 on various components of district revenue and expenditures. In the top panel (A) of Table 2, we include only region-year fixed effects, a quadratic in ADA, and the cost of education index. 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 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. 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 for our full set of covariates, total revenue increases by \$3,400. Columns 2 and 3 show that without accounting for size, sparsity, or other factors, this additional funding is largely derived from local funding. Columns 3 and 4 show that federal funding districts receive are not impacted by variation in the adjusted allotment, even in this simple specification.

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<sup>13</sup>Again results are very similar using a four-year average.

In columns 6 and 7 we show that indeed the residual funding increase operates entirely through additional Tier I funding.<sup>14</sup> Finally, in column 8 we show that per-pupil expenditures increase by about \$2,700 for each \$1,000 in the adjusted allotment. This figure is less than total revenue because we use total operating expenditures, which does not include capital outlays or debt service. Total expenditures by object, which does include these, matches total revenue.

In the second panel (B) we specify the full model, which includes our smooth measures of size and sparsity and proxies in commute and bus miles to match our outcome equations. If we have specified the true cost of size and sparsity correctly here with smooth measures and proxies, then the residual variation is additional funding districts receive over and above the true cost of these factors.

Column 1 suggests that for each additional \$1,000 in the adjusted allotment, districts receive an additional \$1,600 in total revenue. Approximately \$1,000 of which comes from state funds. Again, the entire pass-through works through Tier I funds, as should be the case.

## 5.2 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 some 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 3 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.

In column 3 we show differences in per-pupil residential housing value (in \$1,000's). We find little difference, noting that these are not home prices but the average taxable property value as determined by the district. In columns 4-7 we show differences in teacher salaries, experience, and student-teacher ratios. We find no difference in salaries, starting or average. We also find no difference in the average number of years of experience teachers have. We do find that districts receiving additional funding have 0.6 fewer students per teacher. While small, districts in the sample have an average student to teacher ratio of 12, so the increase is not trivial. Lastly, we show that there is no difference in the likelihood that the district is K-12 or not.

## 5.3 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

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<sup>14</sup>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.

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 4](#) shows results.

Columns 1 and 2 start with a pooled model with no grade interactions. 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 A1](#) in the Appendix we show very similar results from a simple pooled cross-section with one observation per district over the entire sample period.

In columns 3 and 4 we observe how these gains manifest across grades. Column 3 suggests that by 3rd grade, an additional \$1,000 per year over grades 1-3 yields a 0.07 standard deviation advantage in reading (3rd grade is the omitted category; then the main effect for AA in columns 3 and 4 is the effect of funding in grade 3). We then find that test score gains from additional funding accumulate in later grades. The effect in grades 4-5 are no different than in grade 3, but effects in grades 6-10 suggest cumulative effects of exposure. We find no difference in grade 11, though scores there are unique. Since grade 11 is an exit exam year, students get multiple attempts. Turning to math, we find no benefits in grade 3, but that beginning in grade 5 math scores increase as well. Here the effect is large in grade 11, but with the same caveat.

## 5.4 Achievement Effects, Robustness

[Tables A2](#) and [A3](#) in the Appendix show a series of robustness checks following our simple cross-sectional specification in [Table A1](#). In column 1 we include only a quadratic in ADA, the CEI, region-year and grade fixed effects. This omits area, sparsity, their squared terms, commute times, bus miles and indicators for K-12 for whether the district is consolidated. We find that estimates are comparable to the full model though slightly larger, which we show here in column 5 for reference.

In columns 2-4 we sequentially include these elements, demonstrating that results are largely unaffected by whether or how we specify these. In column 6 we replace ADA, the CEI, and sparsity with contemporaneous measures as opposed to cumulative averages. Effects are slightly larger. In column 7 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 column 8 we include the earliest measure of district demographic measures we observe, in 1997, these include the share of students who are black, Hispanic, other race, and the share who are economically disadvantaged. We include main effects and interact these with a linear time trend. In column 9 we include contemporaneous versions of these measures. In both cases we observe a level shift downward in estimated effects, but that the gradient as students age is unchanged. In column 10 we limit only to K-12 districts with marginally smaller results from the main specification in column 5, again with the same grade pattern intact.

Finally we ask whether changes in ADA, which affect funding, are driving results. First, in column 11 we ask whether effects are driven by districts with large annual changes in ADA, which would affect funding levels. 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 virtually identical to column 5, confirming that these changes are not driving results. Second, in column 12 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. Hence we ask to what degree these drive results. Column 12 confirms that results are attenuated when we omit these districts entirely from the sample, but that they only contributed to a small fraction of the overall effect.

## 5.5 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. Hence, the impacts we observe may be the result of funds being targeted not at poor communities, but rather at sparse communities, net of smooth controls for the cost of size and sparsity. Many of these are in fact poor, but are not funded inadequately. 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 1,400 students that are larger than 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 5 and 6 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. We then 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 column 9 of our robustness tables. The purpose here is to observe whether effects of additional funding are larger for poorer or minority districts, or whether additional funding affects students in all districts equally. This has policy implications insofar as where additional dollars are best spent.

We find that the impact of additional funding is meaningfully and statistically larger in poorer and more Hispanic districts. For example, in Table 5 we find that effects, 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 6](#) a similar story emerges, with a few distinctions. 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. How these effects accumulate over schooling years differs depending on math or reading. Reading score gains are largely concentrated among districts with the largest Hispanic populations, while gains in math are prevalent across all terciles, with some advantage in later grades for the most Hispanic districts. These comparative results shed light on how additional resources translate to student outcomes differentially across student groups (by poverty or Hispanic), disciplines (math versus reading), and at different grades, suggesting that simply looking for average effects pooled across all students and grades might mask meaningful differences.

While these results suggest that targeting additional funds to poorer and/or minority districts can achieve test score and retention gains, they also highlight the fact that disparities 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 or largely minority districts, we also conclude that even meaningful resource shifts toward poor districts are not sufficient to close what are very large initial achievement disparities.

## 5.6 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-year level where the adjusted allotment is the average over the past four years to approximate our cumulative exposure measure during high school.

[Table 7](#) 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. We find that the four-year high school dropout rate declines by 1 percentage point across all districts, but that this effect is driven entirely by the poorest and most Hispanic districts, each of which see a 1.4 percentage point decrease in the dropout rate. The average four-year dropout rate across all districts in our sample is 4 percent, and is just under 6 percent for the poorest and most Hispanic districts. In this sense, the \$1,000 increase in base funding decreases the dropout rate by a measurable percentage. Turning to four-year graduation rates we see a similar pattern. Graduation rates increase by just less than 3 percentage points for the poorest and most Hispanic districts, who have an average four-year graduation rate of 88 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, though using a three-year average adjusted allotment since students are taking exams in the 11th grade. [Table 8](#) shows results from these specifications.

In columns 1-3 we repeat our exercises from the previous table now on the share of students who took the SAT. We find little effect of an increase in test taking, even across different types of

districts. We only detect a 3 percentage point increase in test taking for the most Hispanic districts. When we estimate performance effects of additional funding, we find that gains are again largely concentrated in the poorest and most Hispanic districts, who see about a 0.07 standard deviation increase in SAT score, with little evidence of gains for less wealthy and whiter districts.

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 7 percentage points across all districts. Here we see little gradient with respect to the share of students who are poor, but evidence that gains are concentrated among primarily Hispanic districts. When we turn to degree completion, we find no 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. Again, we do find small impacts in more Hispanic districts. We discuss these results relative to the existing literature in Section 7.

## 6 Budget share allocations

Our data also allow us to ask how additional funds are spent. Economic theory suggests that firms allocate resources to the most productive inputs. Although school districts are far from profit maximizing firms it is not unreasonable to expect them to devote additional resources in the most productive manner available, given the available technology and prevailing input costs. While we are neither in a position to determine what the most productive resources are, nor to determine whether districts are allocating resources effectively, we are in a position to compare how districts with similar characteristics but different budgets allocate resources. To do so we estimate the relationship between additional funding and spending allocations across various input categories. The regression specifications here are the same as the funding models described in [Equation 7](#), which are at the district-year level.

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. [Table A7](#) in the Appendix 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), and by whether they are larger or smaller than 300 square miles in area. While this rough breakout does not contain the all the subtleties 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 5](#), 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. Moving to panel B, we show the same breakdown by spending shares. The lack of difference across groups is clear. The share of expenditures dedicated to each category is virtually identical across district types, despite the fact that low ADA districts larger than 300 square miles receive about 10% more in funding.

To move beyond these rough approximations we re-estimate our main model, which controls flexibly for the true cost of size and sparsity as in our regressions on revenues, now with each budget share as the dependent variable in Table 9. We show that from each additional \$1,000 in allotments, net of our full set of controls, districts spend a smaller share, by 1.4 percentage points, on direct instruction. Likewise, we find these are marginally shifted toward administration and other factors. A regression where data are pooled at the district level show similar results. In the final four columns we conduct the same exercise for total expenditures including non-operating expenditures.<sup>15</sup> We find no impact on capital outlays, but a small decrease in debt service.

We take these very modest shifts in budget shares as evidence that districts receiving additional discretionary funds allocate these roughly proportionally across spending categories, contrasting somewhat with Chakrabarti and Roy (2017). There are several possible explanations for this. First, it may be the case that educational resources are best allocated in fixed proportions, and that if we double district revenue, the most efficient thing for districts to do in terms of student achievement is to double expenditures on every input – Cobb-Douglas technology in education. Second, it may be that districts are constrained in their ability to set their own budget items, or that markets for administrators, teachers, and other school inputs are not flexible. Determining which of these factors has led to these results is beyond the scope of the our analysis and we leave this to future research.

## 7 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, but less so graduation. These gains are largely concentrated in later grades (for test scores) and among poorer or more minority districts across the board with exception for college enrollment. Here we situate these in the literature and discuss their meaningfulness.

### 7.1 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

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<sup>15</sup>The TEA has two breakdowns of expenditures, total operating expenditures are shown in 1-5 and total expenditures are in columns 6-9.

(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). 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.

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.

## 7.2 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 3](#) we find few characteristic differences between schools getting additional (residual) 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 the 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 A4](#) 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.

In the first panel (A) of [Figure 6](#), 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 the 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 those receiving more or less in the adjusted allotment, net of smooth controls for size and sparsity, saw 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. We confirm this showing regression results in [Table A5](#) in the Appendix.

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 this we limit this 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 7](#).

We find that counties where all districts 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, we find that rates declined less rapidly for those not receiving additional funding from the policy, suggesting negative selection if any. After 1980 we find that gaps in poverty between these counties remains roughly stable. We believe this provides little evidence of positively selected migration, though 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.

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 time. We show this in [Table A6](#) in the Appendix. The odd numbered columns re-estimates our main specifications from [Table 4](#) 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.

### 7.3 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 8](#) shows the share of districts in each state that are remote, rural, or distant, according to the NCES. 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 9](#). 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.

## 8 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 yields a 0.10 standard deviation increase in reading and a 0.08 standard deviation increase in math. 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 increases in college enrollment, but not completion. Again, these benefits largely accrue in minority and high poverty districts.

We show that increasing discretionary funding for districts does little to alter how they allocate resources across input categories. Districts receiving more money keep spending shares roughly constant, or if anything devote marginally smaller shares to direct instruction and slightly more to administration. Student-teacher ratios are also smaller.

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 almost entirely estimates effects from school finance reforms that targeted schools with either inadequate or inequitable funding levels. The variation we exploit here 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 add value by introducing evidence from a new state, and 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 or minority 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.

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## 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.

Panel A:	(1) Total	(2) State	(3) Local	(4) Federal	(5) Other	(6) Tier I	(7) Tier II	(8) Expend
AA/1000	3451.4*** (461.4)	636.5*** (221.1)	2404.9*** (425.0)	75.7 (114.2)	334.3** (132.6)	1539.8*** (94.3)	-35.9 (32.2)	2723.1*** (254.4)
ADA, ADA <sup>2</sup>	×	×	×	×	×	×	×	×
CEI	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×
Panel B:	(1) Total	(2) State	(3) Local	(4) Federal	(5) Other	(6) Tier I	(7) Tier II	(8) Expend
AA/1000	1686.7*** (466.0)	941.7*** (231.7)	592.3 (393.5)	-5.9 (139.3)	158.6 (174.2)	1282.4*** (81.4)	24.4 (34.0)	1146.5*** (242.6)
$X_{i(t)}$	×	×	×	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×
Dep. Mean	12,164	5,472	4,687	1,207	796	5,556	605	10,366
N	6,958	6,958	6,958	6,958	6,958	4,341	4,341	6,958
Obs.	875	875	875	875	875	870	870	875

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. Expenditures are total operating expenditures, which is less than Total Revenue which includes debt and capital.  $g(\cdot)$  includes ADA, ADA<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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.  $Region * Year$  are region-year fixed effects. Standard errors clustered on districts in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).

**Table 3:** District level characteristics as a function of residual variation in funding.

	(1) % Poor	(2) % Hisp	(3) Land Val.	(4) Start. Sal.	(5) Avg. Sal.	(6) Exper.	(7) S/T Ratio	(8) K12
AA/1000	-0.011 (0.016)	-0.049** (0.019)	-22.818 (15.402)	147.485 (408.774)	-275.626 (312.473)	0.046 (0.251)	-0.557*** (0.147)	0.007 (0.005)
$X_{i(t)}$	×	×	×	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×
Dep. Mean	0.530	0.301	115.218	33,594	43,547	12.61	11.90	0.932
R <sup>2</sup>	0.321	0.686	0.124	0.272	0.381	0.130	0.622	0.910
N	6,958	6,958	6,958	6,084	6,957	6,957	6,957	6,958
Districts	875	875	875	873	875	875	875	875

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. Residential 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<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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. *Region \* Year* are region-year fixed effects. Standard errors clustered on districts in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).

**Table 4:** Effects of additional funding on test scores by grade.

	(1) Reading (Z)	(2) Reading (Z)	(3) Math (Z)	(4) Math (Z)
$\bar{A}A/1000$	0.097*** (0.029)	0.071*** (0.025)	0.077** (0.032)	0.037 (0.027)
$\bar{A}A$ *grade 4		0.005 (0.009)		-0.008 (0.012)
$\bar{A}A$ *grade 5		0.012 (0.012)		0.030** (0.014)
$\bar{A}A$ *grade 6		0.065*** (0.013)		0.071*** (0.016)
$\bar{A}A$ *grade 7		0.057*** (0.014)		0.054*** (0.017)
$\bar{A}A$ *grade 8		0.052*** (0.013)		0.061*** (0.017)
$\bar{A}A$ *grade 9		0.082*** (0.014)		0.115*** (0.017)
$\bar{A}A$ *grade 10		0.048*** (0.017)		0.096*** (0.018)
$\bar{A}A$ *grade 11		0.022 (0.017)		0.102*** (0.018)
$X_{i(t)}$	×	×	×	×
$g(\cdot)$	×	×	×	×
Grade	×	×	×	×
Region*Year	×	×	×	×
R <sup>2</sup>	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).  $\bar{A}A$  is the average adjusted allotment (in real '000s) cohort  $c$  in district  $i$  received through grade  $g$ .  $\bar{g}(\cdot)$  measures ADA, and students per square mile (ADA/Area) analogously to the average  $\bar{A}A$  and includes quadratics. Area, Area<sup>2</sup> 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.  $Region * Year$  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 5:** Effects of additional funding on test scores by grade and % poor.

	(1) Reading (Z)		(2) Reading (Z)		(3) Math (Z)		(4) Math (Z)	
$\bar{AA}/1000$	0.042*	(0.025)	0.044*	(0.024)	0.019	(0.029)	0.020	(0.028)
AA*grade 4			0.014	(0.015)			-0.012	(0.018)
AA*grade 5			-0.000	(0.018)			0.025	(0.022)
AA*grade 6			0.031	(0.020)			0.031	(0.025)
AA*grade 7			0.018	(0.018)			-0.022	(0.025)
AA*grade 8			0.008	(0.018)			-0.008	(0.024)
AA*grade 9			0.014	(0.021)			0.036	(0.024)
AA*grade 10			-0.005	(0.022)			0.027	(0.024)
AA*grade 11			-0.030	(0.021)			0.029	(0.024)
$\bar{AA}$ *Mid tercile poor	0.053**	(0.022)	0.035	(0.023)	0.064**	(0.027)	0.028	(0.028)
AA*Mid Poor, gr 4			-0.003	(0.019)			-0.009	(0.024)
AA*Mid Poor, gr 5			0.010	(0.023)			-0.012	(0.029)
AA*Mid Poor, gr 6			0.038	(0.025)			0.047	(0.034)
AA*Mid Poor, gr 7			0.011	(0.024)			0.059*	(0.034)
AA*Mid Poor, gr 8			0.011	(0.024)			0.052	(0.033)
AA*Mid Poor, gr 9			0.045	(0.029)			0.066*	(0.035)
AA*Mid Poor, gr 10			0.030	(0.031)			0.064*	(0.037)
AA*Mid Poor, gr 11			0.030	(0.031)			0.076**	(0.036)
$\bar{AA}$ *Top tercile poor	0.073***	(0.023)	0.025	(0.023)	0.067**	(0.029)	-0.001	(0.029)
AA*Top poor, gr 4			-0.026	(0.020)			0.012	(0.026)
AA*Top poor, gr 5			0.007	(0.023)			0.004	(0.030)
AA*Top poor, gr 6			0.041	(0.026)			0.049	(0.035)
AA*Top poor, gr 7			0.073***	(0.026)			0.134***	(0.035)
AA*Top poor, gr 8			0.080***	(0.025)			0.117***	(0.034)
AA*Top poor, gr 9			0.113***	(0.028)			0.133***	(0.033)
AA*Top poor, gr 10			0.088***	(0.033)			0.105***	(0.034)
AA*Top poor, gr 11			0.081**	(0.033)			0.100***	(0.035)
$X_{i(t)}$	×		×		×		×	
$g(\cdot)$	×		×		×		×	
Mid, Top poor*Grade	×		×		×		×	
Mid, Top poor	×		×		×		×	
Grade	×		×		×		×	
Region*Year	×		×		×		×	
R <sup>2</sup>	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  $\bar{g}(\cdot)$  are included.  $\bar{AA}$  is the average adjusted allotment (in real '000s) cohort  $c$  in district  $i$  received through grade  $g$ .  $\bar{g}(\cdot)$  measures ADA, and students per square mile (ADA/Area) analogously to the average  $\bar{AA}$  and includes quadratics. Area, Area<sup>2</sup> 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.  $Region * Year$  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 % Hispanic.

	(1) Reading (Z)		(2) Reading (Z)		(3) Math (Z)		(4) Math (Z)	
$\bar{A}A/1000$	0.000	(0.031)	0.009	(0.027)	-0.015	(0.036)	-0.034	(0.031)
AA*grade 4			-0.009	(0.017)			-0.031	(0.022)
AA*grade 5			-0.017	(0.018)			0.018	(0.025)
AA*grade 6			0.046**	(0.022)			0.093***	(0.035)
AA*grade 7			0.016	(0.023)			0.023	(0.032)
AA*grade 8			0.014	(0.020)			0.009	(0.031)
AA*grade 9			0.011	(0.025)			0.076**	(0.032)
AA*grade 10			-0.023	(0.029)			0.074**	(0.031)
AA*grade 11			-0.052*	(0.030)			0.062**	(0.031)
$\bar{A}A$ *Mid tercile Hisp.	0.057**	(0.027)	0.029	(0.026)	0.044	(0.033)	0.042	(0.031)
AA*Mid Hisp., gr 4			0.009	(0.020)			0.025	(0.028)
AA*Mid Hisp., gr 5			0.023	(0.023)			0.008	(0.031)
AA*Mid Hisp., gr 6			0.002	(0.026)			-0.058	(0.041)
AA*Mid Hisp., gr 7			0.023	(0.027)			-0.015	(0.040)
AA*Mid Hisp., gr 8			0.019	(0.025)			0.012	(0.038)
AA*Mid Hisp., gr 9			0.062**	(0.030)			0.006	(0.039)
AA*Mid Hisp., gr 10			0.060	(0.036)			-0.007	(0.040)
AA*Mid Hisp., gr 11			0.071*	(0.037)			0.015	(0.040)
$\bar{A}A$ *Top tercile Hisp	0.121***	(0.028)	0.074***	(0.028)	0.127***	(0.035)	0.089***	(0.033)
AA*Top Hisp., gr 4			0.013	(0.021)			0.022	(0.027)
AA*Top Hisp., gr 5			0.030	(0.023)			-0.001	(0.030)
AA*Top Hisp., gr 6			0.022	(0.027)			-0.019	(0.040)
AA*Top Hisp., gr 7			0.056**	(0.028)			0.071*	(0.038)
AA*Top Hisp., gr 8			0.050*	(0.026)			0.099***	(0.036)
AA*Top Hisp., gr 9			0.091***	(0.030)			0.075*	(0.038)
AA*Top Hisp., gr 10			0.092***	(0.034)			0.043	(0.038)
AA*Top Hisp., gr 11			0.086**	(0.037)			0.062	(0.038)
$X_{i(t)}$	×		×		×		×	
$g(\cdot)$	×		×		×		×	
Mid, Top Hisp.*Grade	×		×		×		×	
Mid, Top Hisp.	×		×		×		×	
Grade	×		×		×		×	
Region*Year	×		×		×		×	
$R^2$	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  $g$ . ADA, ADA/Area and CEI are constructed analogously.  $g(\cdot)$  measures ADA, and students per square mile (ADA/Area) analogously to the average  $\bar{A}A$  and includes quadratics. Area, Area<sup>2</sup> and sparsity proxies, including measures of commute times for workers and average miles based 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.  $Region * Year$  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:** Additional funding and four-year dropout and graduation rates.

	(1) Dropout	(2) Dropout	(3) Dropout	(4) Graduation	(5) Graduation	(6) Graduation
$\bar{AA}_4/1000$	-0.009** (0.004)	-0.002 (0.004)	0.002 (0.005)	0.010 (0.008)	-0.004 (0.007)	-0.008 (0.009)
Mid 3rd Poor		0.027* (0.014)			-0.062** (0.028)	
Top 3rd Poor		0.103*** (0.016)			-0.200*** (0.025)	
Mid 3rd Poor x $\bar{AA}_4$		-0.003 (0.003)			0.007 (0.005)	
Top 3rd Poor x $\bar{AA}_4$		-0.014*** (0.003)			0.028*** (0.005)	
Mid 3rd Hisp.			0.023 (0.018)			-0.043 (0.029)
Top 3rd Hisp.			0.092*** (0.021)			-0.165*** (0.034)
Mid 3rd Hisp x $\bar{AA}_4$			-0.004 (0.003)			0.008 (0.005)
Top 3rd Hisp x $\bar{AA}_4$			-0.014*** (0.004)			0.026*** (0.006)
$g(\cdot)$	×	×	×	×	×	×
$X_{i(t)}$	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×
R <sup>2</sup>	0.198	0.249	0.214	0.203	0.261	0.220
Obs.	6,439	6,439	6,439	6,439	6,439	6,439
Districts	815	815	815	815	815	815

Notes:  $\bar{AA}_4$  is the (real) average adjusted allotment for district  $i$  in year  $t$  for the past four 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.  $\bar{AA}_4$  is the average adjusted allotment (in real '000s) in district  $i$  over the past four years. 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<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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.  $Region * Year$  are region-year fixed effects. Standard errors clustered on districts in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).



**Table 8:** Long run educational outcomes.

	Took SAT			SAT (z)		Enrolled in College			College Degree			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\overline{AA}_3/1000$	-0.006 (0.025)	-0.011 (0.028)	-0.028 (0.026)	0.067 (0.057)	0.031 (0.057)	-0.002 (0.061)	0.065** (0.023)	0.065** (0.023)	0.024 (0.024)	0.023 (0.013)	0.011 (0.012)	-0.004 (0.014)
Mid Tercile Poor		-0.101 (0.071)			-0.237 (0.131)			-0.116* (0.058)			-0.142*** (0.035)	
Top Tercile Poor		-0.173* (0.072)			-0.550*** (0.142)			-0.140* (0.058)			-0.166*** (0.034)	
$\overline{AA}_3$ *Mid Poor		0.003 (0.018)			0.038 (0.033)			0.005 (0.014)			0.019* (0.009)	
$\overline{AA}_3$ *Top Poor		0.014 (0.018)			0.072* (0.036)			0.001 (0.015)			0.014 (0.008)	
Mid Tercile Hisp.			0.054 (0.064)			-0.285* (0.139)			-0.0777 (0.065)			-0.06 (0.039)
Top Tercile Hisp.			-0.229** (0.071)			-0.450** (0.151)			-0.242*** (0.067)			-0.194*** (0.040)
$\overline{AA}_3$ *Mid Hisp.			-0.015 (0.016)			0.059 (0.035)			0.0205 (0.016)			0.012 (0.009)
$\overline{AA}_3$ *Top Hisp.			0.033* (0.016)			0.075* (0.037)			0.040* (0.016)			0.030*** (0.009)
$X_{i(t)}$	×	×	×	×	×	×	×	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×	×	×	×	×
N	5,535	5,535	5,535	5,023	5,023	5,023	5,535	5,535	5,535	5,535	5,535	5,535
Districts	815	815	815	815	815	815	815	815	815	815	815	815

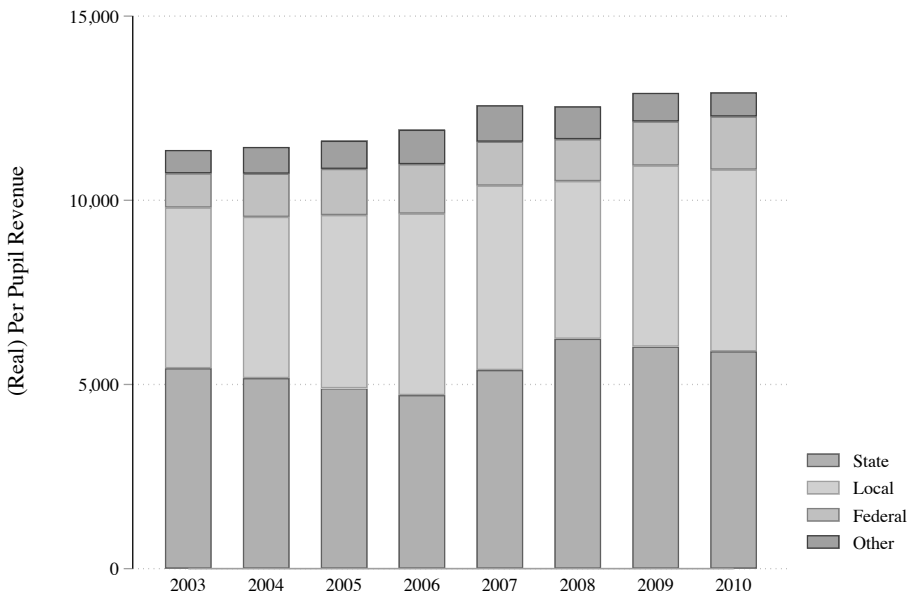
Notes: Outcomes are district averages from College Board and NSC records.  $\overline{AA}_3$  is the (real) average adjusted allotment for district  $i$  in year  $t$  for the past three years in \$'000s.  $g(\cdot)$  includes ADA, ADA<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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. *Region \* Year* are region-year fixed effects. Standard errors clustered on districts in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).

**Table 9:** Increased funding and expenditures by category.

	(1) Instruction	(2) Leadership	(3) Svcs./Suppt.	(4) Admin	(5) Plant/Opr.	(6) Capital	(7) Debt	(8) Payroll	(9) Other
AA/1000	-0.014*** (0.004)	-0.002 (0.002)	-0.000 (0.003)	0.010*** (0.002)	0.004 (0.003)	-0.003 (0.008)	-0.009** (0.004)	-0.003 (0.010)	0.015*** (0.005)
$X_{i(t)}$	×	×	×	×	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×	×
Dep. Mean	0.561	0.062	0.099	0.077	0.169	0.087	0.055	0.642	0.217
R <sup>2</sup>	0.177	0.123	0.358	0.549	0.173	0.115	0.164	0.114	0.254
N	6,958	6,958	6,958	6,958	6,958	6,957	6,958	6,958	6,958
Districts	875	875	875	875	875	875	875	875	875

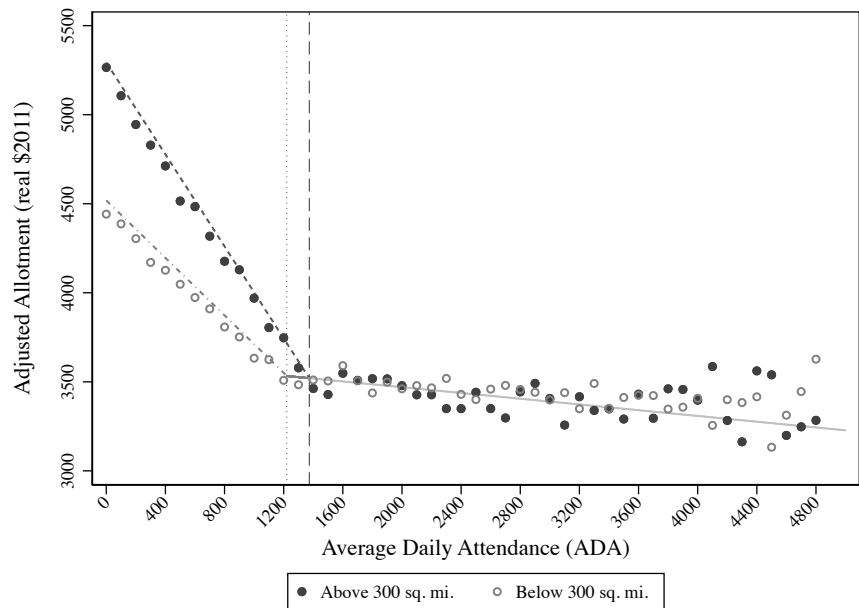
Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Dependent variables in columns 1-5 are shares of total operating expenditures measured at the district-year level. Descriptions for each can be found in [Table A7](#). Outcomes in columns 6-9 are total expenditures by category.  $g(\cdot)$  includes ADA, ADA<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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.  $Region * Year$  are region-year fixed effects. Standard errors clustered on districts in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).

**Figure 1: Real Per-Pupil Revenue.**



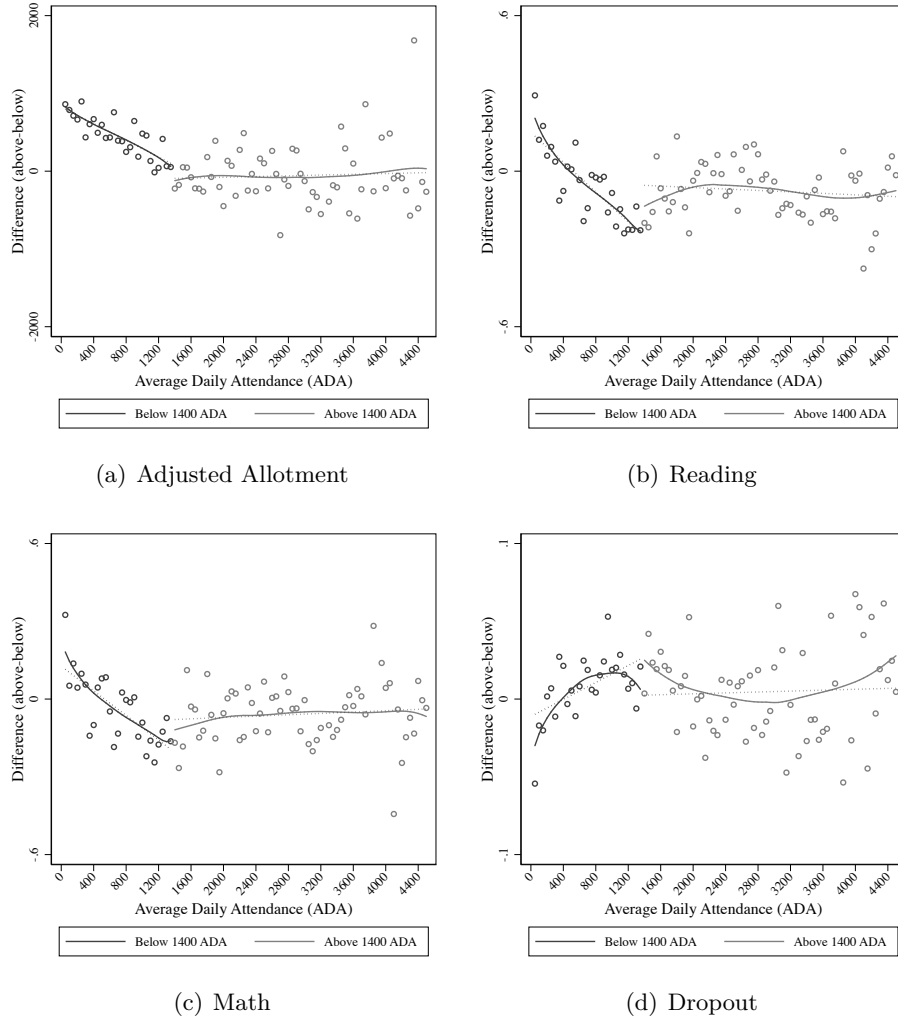
Notes: Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010. Figure charts total revenue by category in real \$2011.

**Figure 2:** Expected and true adjusted allotment funding.



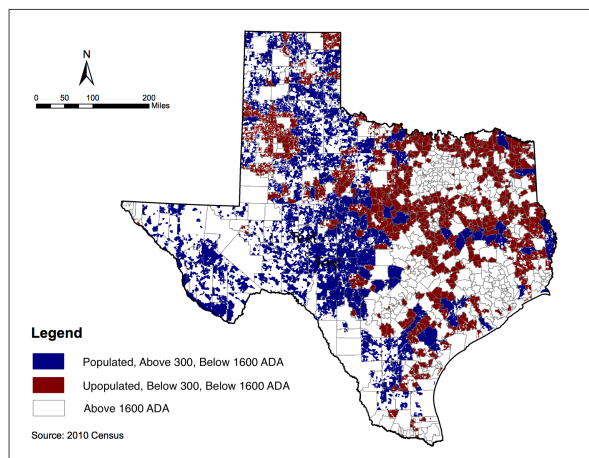
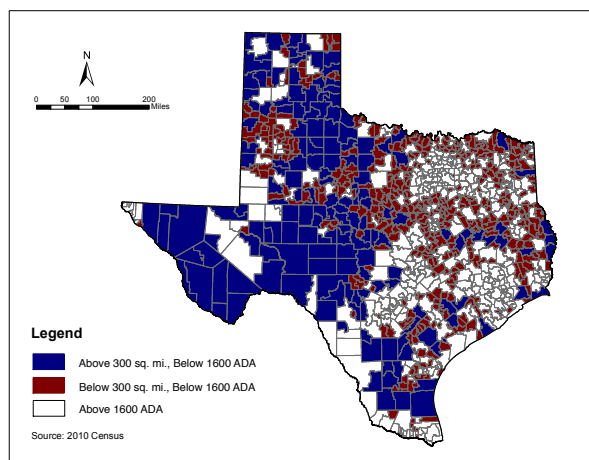
Notes: Lines plots the adjusted allotment as calculated from Texas’s funding formula using the average BA over all years as projected by Equation 1. Points plot the true adjusted allotment in bins of 100 ADA, pooled over all years. Sample consists of non-charter districts with fewer than 5,000 students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

**Figure 3:** Differences in funding and outcomes (above-below 300 sq. mi.) by ADA



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.

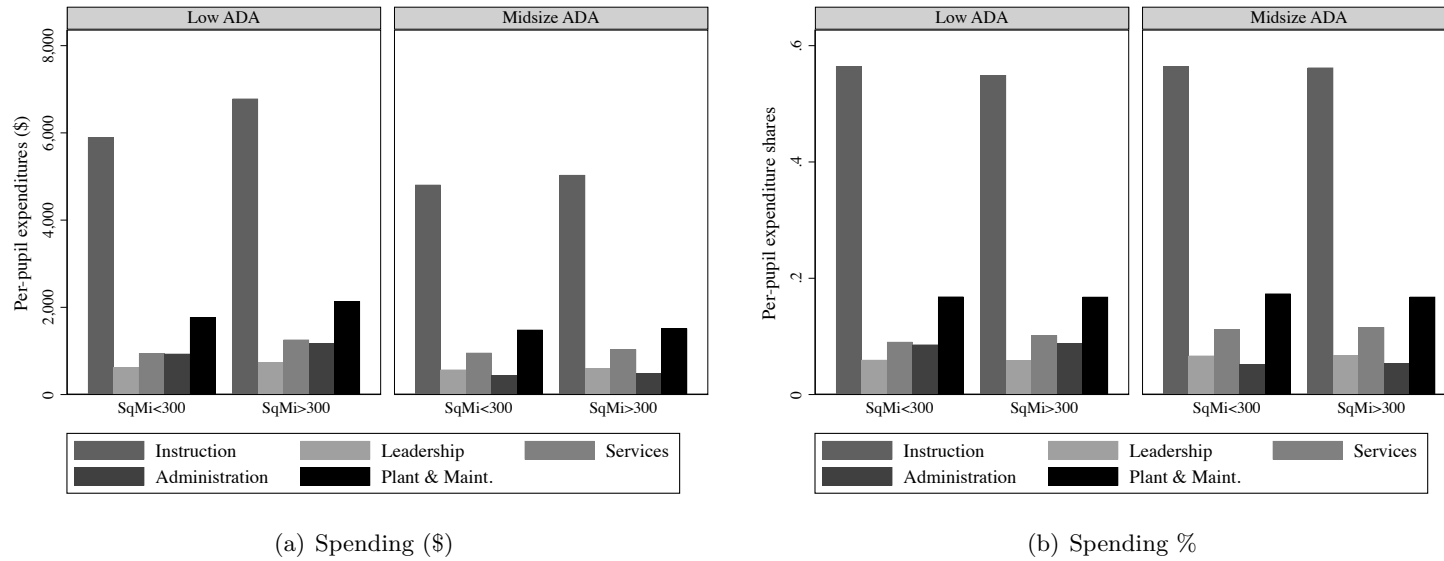
**Figure 4:** Map of (populated) district area.



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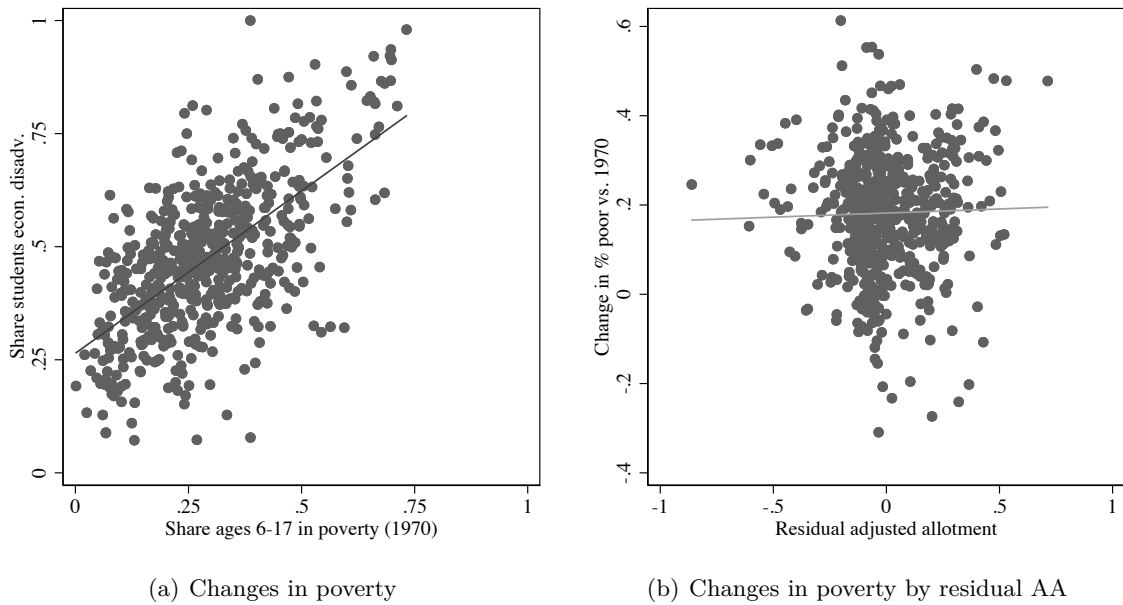
Notes: Populated census blocks are determined by those with  $> 0$  residents in the 2000 Census.

**Figure 5:** Increased funding and budget shares.



Notes: Figures split districts into low ( $< 1,373$ ) and midsize ( $> 1,373$  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 A7](#). Sample consists of non-charter districts with fewer than 5,000 students in ADA in years 2003-2010.

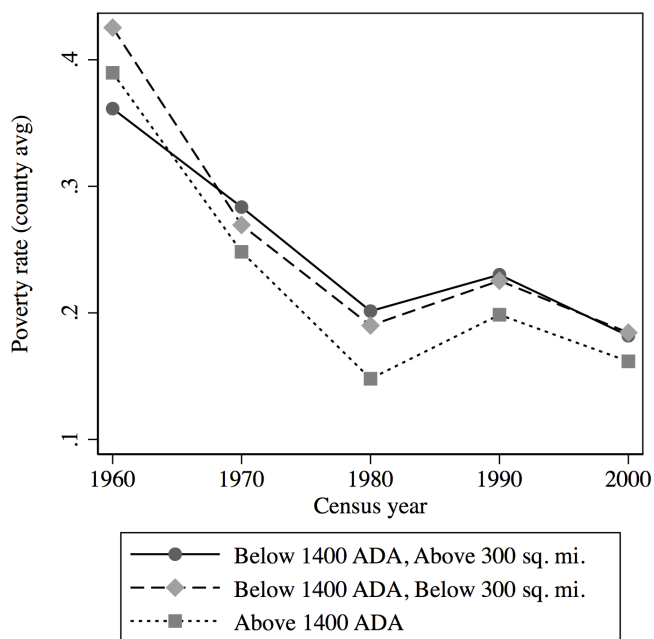
**Figure 6:** Changes in poverty since 1970.



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.

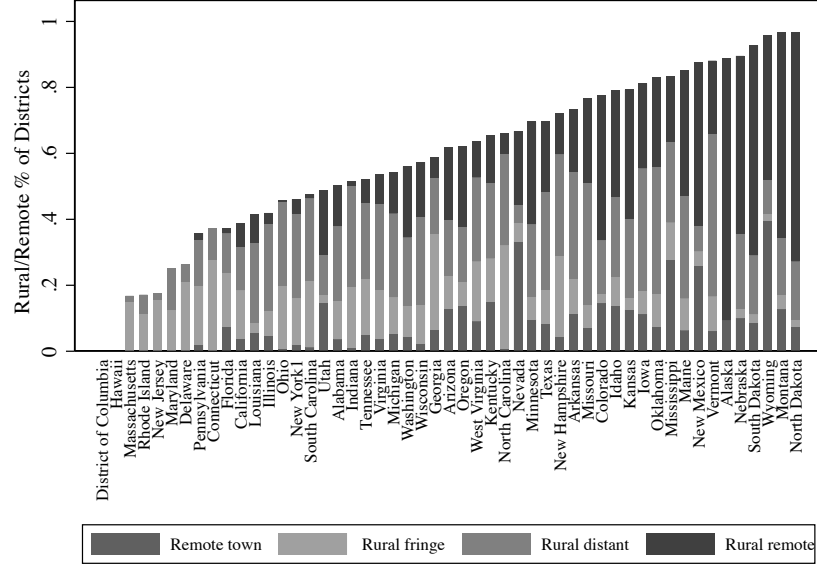


**Figure 7:** Historical poverty rates.



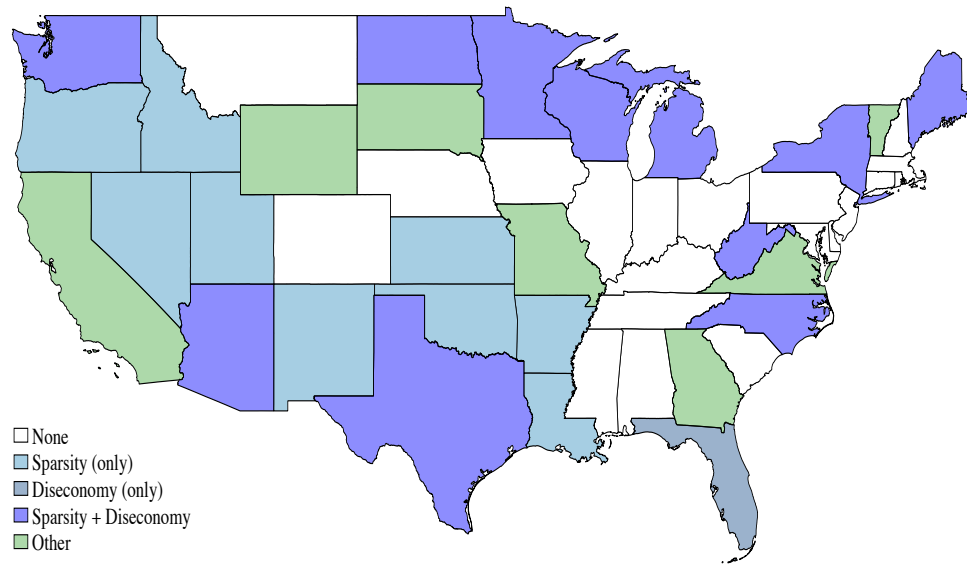
Notes: Figure plots county average poverty rates from Decennial Census. Treatment counties are those where all districts in our sample are below 1,373 ADA and larger than 300 sq. mi. Same is true for other two plots.

**Figure 8:** 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 9:** 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

**Table A1:** Pooled adjustment allotment, revenue, and outcomes.

	(1) Revenue	(2) Reading (z)	(3) Math (z)	(4) Dropout	(5) Graduation
$AA/1000$	1954.5*** (647.51)	0.092** (0.039)	0.078* (0.044)	-0.009 (0.006)	0.010 (0.011)
$\bar{X}_i$	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×
Region	×	×	×	×	×
R <sup>2</sup>	0.488	0.255	0.179	0.233	0.305
Obs.	875	875	875	815	815

Notes: Regression is on averages for all covariates and dependent variables over all years (2003-2010).  $g(\cdot)$  includes district averages of ADA, ADA<sup>2</sup>, Area, Area<sup>2</sup>, students per square mile (ADA/Area) and (ADA/Area)<sup>2</sup>, 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](#).  $\bar{X}_i$  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 A2:** Grade evolution reading, Robustness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\bar{A}\bar{A}$	0.072*** (0.023)	0.074*** (0.023)	0.086*** (0.023)	0.068*** (0.025)	0.071*** (0.025)	0.093*** (0.024)	0.077*** (0.024)	0.032* (0.018)	0.042** (0.017)	0.059** (0.024)	0.071*** (0.025)	0.066** (0.025)
$\bar{A}\bar{A}$ *grade 4	0.005 (0.009)	0.005 (0.009)	0.007 (0.009)	0.005 (0.009)	0.005 (0.009)	0.009 (0.009)	0.006 (0.009)	-0.002 (0.008)	-0.001 (0.008)	0.003 (0.009)	0.001 (0.009)	0.005 (0.009)
$\bar{A}\bar{A}$ *grade 5	0.013 (0.011)	0.013 (0.011)	0.016 (0.011)	0.012 (0.012)	0.012 (0.012)	0.018 (0.012)	0.013 (0.011)	-0.002 (0.009)	-0.001 (0.009)	0.005 (0.011)	0.010 (0.012)	0.010 (0.012)
$\bar{A}\bar{A}$ *grade 6	0.065*** (0.012)	0.066*** (0.012)	0.069*** (0.012)	0.064*** (0.013)	0.065*** (0.013)	0.071*** (0.013)	0.066*** (0.012)	0.049*** (0.011)	0.050*** (0.011)	0.051*** (0.012)	0.060*** (0.013)	0.062*** (0.013)
$\bar{A}\bar{A}$ *grade 7	0.057*** (0.013)	0.058*** (0.013)	0.061*** (0.013)	0.056*** (0.014)	0.057*** (0.014)	0.064*** (0.014)	0.058*** (0.013)	0.036*** (0.012)	0.037*** (0.011)	0.039*** (0.013)	0.050*** (0.014)	0.061*** (0.014)
$\bar{A}\bar{A}$ *grade 8	0.052*** (0.013)	0.053*** (0.013)	0.056*** (0.013)	0.051*** (0.013)	0.052*** (0.013)	0.059*** (0.013)	0.053*** (0.013)	0.030*** (0.012)	0.031*** (0.011)	0.035*** (0.013)	0.045*** (0.014)	0.054*** (0.014)
$\bar{A}\bar{A}$ *grade 9	0.083*** (0.014)	0.083*** (0.014)	0.086*** (0.014)	0.082*** (0.014)	0.082*** (0.014)	0.089*** (0.014)	0.084*** (0.014)	0.058*** (0.013)	0.061*** (0.013)	0.068*** (0.014)	0.074*** (0.015)	0.084*** (0.015)
$\bar{A}\bar{A}$ *grade 10	0.050*** (0.016)	0.050*** (0.016)	0.053*** (0.016)	0.048*** (0.017)	0.048*** (0.017)	0.056*** (0.017)	0.050*** (0.016)	0.025* (0.015)	0.029** (0.014)	0.033** (0.015)	0.041** (0.017)	0.048*** (0.017)
$\bar{A}\bar{A}$ *grade 11	0.023 (0.016)	0.023 (0.016)	0.026 (0.016)	0.022 (0.017)	0.022 (0.017)	0.029* (0.017)	0.024 (0.017)	-0.002 (0.015)	0.001 (0.014)	0.005 (0.015)	0.011 (0.016)	0.021 (0.017)
Donut hole												×
Drop ADA changes											×	
K-12 Only										×		
Demographics <sub>t</sub>									×			
Demog. <sub>1997</sub> *trend								×				
Density, Oil							×					
Contemporaneous						×						
Commute					×	×	×		×	×	×	×
Area <sup>2</sup>				×	×	×	×	×	×	×	×	×
ADA/Area <sup>2</sup>				×	×	×	×	×	×	×	×	×
Area			×	×	×	×	×	×	×	×	×	×
ADA/Area		×	×	×	×	×	×	×	×	×	×	×
ADA <sup>2</sup>	×	×	×	×	×	×	×	×	×	×	×	×
ADA	×	×	×	×	×	×	×	×	×	×	×	×
CEI	×	×	×	×	×	×	×	×	×	×	×	×
Grade	×	×	×	×	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×	×	×	×	×
R <sup>2</sup>	0.102	0.103	0.103	0.106	0.109	0.112	0.116	0.275	0.322	0.119	0.114	0.098
Obs.	60,103	60,103	60,103	60,103	60,103	60,103	60,103	60,103	60,103	57,767	49,763	55,236
Districts	875	875	875	875	875	875	875	875	875	816	875	807

Column 5 is full model from column 2 of [Table 4](#).  $\bar{A}\bar{A}$  is average AA up to current grade for cohort  $c$  in district  $i$  in grade  $g$ . ADA, students per square mile (ADA/Area), and CEI are defined analogously to the average  $\bar{A}\bar{A}$ . In specification 6, contemporaneous controls are current measures of ADA, ADA/Area and CEI. PopDensity is population per square mile in 2010. Oil is a quadratic in per-pupil taxable land value of oil, gas and minerals. Demog<sub>1997</sub> are main effects of share Black, Hispanic, other race, and Poor in 1997 interacted with a linear time trend. Demographics are contemporaneous measures of these. 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 A3:** Grade evolution math, Robustness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\bar{AA}$	0.038 (0.026)	0.039 (0.026)	0.055** (0.026)	0.035 (0.027)	0.037 (0.027)	0.064** (0.027)	0.045* (0.027)	0.007 (0.023)	0.015 (0.021)	0.021 (0.027)	0.038 (0.028)	0.033 (0.029)
$\bar{AA}$ *grade 4	-0.007 (0.011)	-0.007 (0.011)	-0.005 (0.012)	-0.008 (0.012)	-0.008 (0.012)	-0.003 (0.012)	-0.007 (0.011)	-0.014 (0.011)	-0.014 (0.011)	-0.012 (0.012)	-0.014 (0.012)	-0.006 (0.012)
$\bar{AA}$ *grade 5	0.031** (0.014)	0.031** (0.014)	0.035** (0.014)	0.030** (0.014)	0.030** (0.014)	0.037*** (0.014)	0.032** (0.014)	0.019 (0.013)	0.019 (0.012)	0.025* (0.014)	0.028* (0.014)	0.029** (0.015)
$\bar{AA}$ *grade 6	0.071*** (0.016)	0.071*** (0.016)	0.075*** (0.016)	0.070*** (0.016)	0.071*** (0.016)	0.079*** (0.016)	0.072*** (0.016)	0.058*** (0.015)	0.058*** (0.015)	0.057*** (0.016)	0.071*** (0.017)	0.068*** (0.017)
$\bar{AA}$ *grade 7	0.054*** (0.017)	0.055*** (0.017)	0.059*** (0.017)	0.053*** (0.017)	0.054*** (0.017)	0.062*** (0.017)	0.056*** (0.017)	0.036** (0.015)	0.037** (0.015)	0.032* (0.017)	0.052*** (0.018)	0.057*** (0.018)
$\bar{AA}$ *grade 8	0.062*** (0.016)	0.062*** (0.016)	0.066*** (0.016)	0.061*** (0.017)	0.061*** (0.017)	0.070*** (0.016)	0.063*** (0.016)	0.043*** (0.015)	0.043*** (0.015)	0.036** (0.016)	0.055*** (0.017)	0.064*** (0.017)
$\bar{AA}$ *grade 9	0.116*** (0.016)	0.116*** (0.016)	0.120*** (0.017)	0.115*** (0.017)	0.115*** (0.017)	0.124*** (0.017)	0.118*** (0.017)	0.092*** (0.015)	0.097*** (0.015)	0.101*** (0.016)	0.103*** (0.017)	0.113*** (0.017)
$\bar{AA}$ *grade 10	0.097*** (0.017)	0.097*** (0.017)	0.101*** (0.017)	0.096*** (0.018)	0.096*** (0.018)	0.105*** (0.018)	0.099*** (0.017)	0.074*** (0.016)	0.079*** (0.016)	0.082*** (0.017)	0.083*** (0.018)	0.091*** (0.018)
$\bar{AA}$ *grade 11	0.103*** (0.017)	0.103*** (0.017)	0.107*** (0.018)	0.102*** (0.018)	0.102*** (0.018)	0.111*** (0.018)	0.105*** (0.018)	0.079*** (0.016)	0.083*** (0.016)	0.087*** (0.017)	0.087*** (0.018)	0.096*** (0.018)
Donut hole												×
Drop ADA changes											×	
K-12 Only										×		
Demographics <sub>t</sub>									×			
Demog. <sub>1997</sub> *trend								×				
Density, Oil							×					
Contemporaneous						×						
Commute					×	×	×		×	×	×	×
Area <sup>2</sup>				×	×	×	×	×	×	×	×	×
ADA/Area <sup>2</sup>				×	×	×	×	×	×	×	×	×
Area			×	×	×	×	×	×	×	×	×	×
ADA/Area		×	×	×	×	×	×	×	×	×	×	×
ADA <sup>2</sup>	×	×	×	×	×	×	×	×	×	×	×	×
ADA	×	×	×	×	×	×	×	×	×	×	×	×
CEI	×	×	×	×	×	×	×	×	×	×	×	×
Grade	×	×	×	×	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×	×	×	×	×
N	0.073	0.073	0.074	0.077	0.079	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	60,107	60,107	60,107	57,776	49,770	55,240
Districts	875	875	875	875	875	875	875	875	875	815	875	807

Column 5 is full model from column 4 of [Table 4](#).  $\bar{AA}$  is average AA up to current grade for cohort  $c$  in district  $i$  in grade  $g$ . ADA, students per square mile (ADA/Area), and CEI are defined analogously to the average  $\bar{AA}$ . In specification 6, contemporaneous controls are current measures of ADA, ADA/Area and CEI. PopDensity is population per square mile in 2010. Oil is a quadratic in per-pupil taxable land value of oil, gas and minerals. Demog<sub>1997</sub> are main effects of share Black, Hispanic, other race, and Poor in 1997 interacted with a linear time trend. Demographics are contemporaneous measures of these. 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:** Summary statistics by availability in 1970 SSDT.

	Not missing	Missing
ADA	1301.27 (1087.89)	430.66 (592.32)
Area	295.32 (373.04)	217.51 (319.44)
ADA/Area	15.53 (42.87)	11.94 (49.03)
AA	4,165.5 (899.2)	4,609.6 (929.8)
% Poor	53.99 (16.78)	51.19 (19.12)
Reading (z)	0.020 (0.218)	0.061 (0.262)
Math (z)	0.026 (0.241)	0.036 (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 SSDT 1970 tables.

**Table A5:** Residual Adjusted Allotment funding and poverty in 1970.

	(1) % Poor	(2) Missing	(3) % Hisp.	(4) Missing
AA/1000	0.007 (0.025)	0.027 (0.073)	-0.013 (0.036)	0.037 (0.076)
ADA, ADA <sup>2</sup>	×	×	×	×
Area, Area <sup>2</sup>	×	×	×	×
ADA/Area, ADA/Area <sup>2</sup>	×	×	×	×
CEI	×	×	×	×
Region*Year	×	×	×	×
R <sup>2</sup>	0.491	0.340	0.769	0.366
N	570	875	459	875

Notes: Data are pooled % Poor is share of children (ages 6-17) in poverty (official Census definition) in the 1970 SSDT. Missing is a binary indicator if poverty in 1970 is missing. % Hisp. is share of children “Spanish” in 1970 SSDT. Robust standard errors in parentheses. (\* $p < 0.10$  \*\* $p < 0.05$  \*\*\* $p < 0.01$ ).



**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)
AA	0.097*** (0.029)	0.078*** (0.026)	0.071*** (0.025)	0.055** (0.022)	0.077** (0.032)	0.059** (0.029)	0.037 (0.027)	0.022 (0.025)
Share poor (1970)		-0.677*** (0.068)		-0.676*** (0.067)		-0.701*** (0.080)		-0.700*** (0.080)
Share poor (missing)		-0.177*** (0.028)		-0.177*** (0.028)		-0.210*** (0.033)		-0.211*** (0.033)
AA*grade 4			0.005 (0.009)	0.002 (0.009)			-0.008 (0.012)	-0.010 (0.011)
AA*grade 5			0.012 (0.012)	0.007 (0.011)			0.030** (0.014)	0.026+ (0.013)
AA*grade 6			0.065*** (0.013)	0.059*** (0.012)			0.071*** (0.016)	0.066*** (0.016)
AA*grade 7			0.057*** (0.014)	0.051*** (0.013)			0.054*** (0.017)	0.049*** (0.016)
AA*grade 8			0.052*** (0.013)	0.046*** (0.012)			0.061*** (0.017)	0.056*** (0.016)
AA*grade 9			0.082*** (0.014)	0.076*** (0.014)			0.115*** (0.017)	0.111*** (0.016)
AA*grade 10			0.048*** (0.017)	0.043*** (0.016)			0.096*** (0.018)	0.092*** (0.017)
AA*grade 11			0.022 (0.017)	0.017 (0.016)			0.102*** (0.018)	0.098*** (0.017)
Poor Missing*Year	×	×	×	×	×	×	×	×
Share Poor*Year	×	×	×	×	×	×	×	×
$g(\cdot)$	×	×	×	×	×	×	×	×
Grade	×	×	×	×	×	×	×	×
Region*Year	×	×	×	×	×	×	×	×
R <sup>2</sup>	0.106	0.149	0.109	0.151	0.075	0.106	0.079	0.109
N	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 4. 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 A7:** 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.