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EdWorkingPaper No. 22-593

Variation in the Relationship between School Spending and Achievement: Progressive Spending Is Efficient

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The equity-efficiency tradeoff and cumulative return theories predict larger returns to school spending in areas with higher previous investment in children. Equity – not efficiency – is therefore used to justify progressive school funding: spending more in communities with fewer financial resources. Yet it remains unclear how returns to school spending vary across areas by previous investment. Using county-level panel data 2009-2018 from the Stanford Education Data Archive, the F-33 finance survey, and National Vital Statistics, we estimate achievement returns to school spending and test whether returns vary between counties with low and high levels of initial human capital (measured as birth weight), child poverty, and previous spending. Spending returns are higher among counties with low previous investment (counties that also have a high percent of Black students). Evidence of diminishing returns by previous investment documents another way that schools increase equality and establishes another argument for progressive school funding: efficiency.

VERSION: June 2022

Suggested citation: Rauscher, Emily, and Yifan Shen. (2022). Variation in the Relationship between School Spending and Achievement: Progressive Spending Is Efficient. (EdWorkingPaper: 22-593). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/sdch-ga28

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Abstract

The equity-efficiency tradeoff and cumulative return theories predict larger returns to school spending in areas with higher previous investment in children. Equity – not efficiency – is therefore used to justify progressive school funding: spending more in communities with fewer financial resources. Yet it remains unclear how returns to school spending vary across areas by previous investment. Using county-level panel data 2009-2018 from the Stanford Education Data Archive, the F-33 finance survey, and National Vital Statistics, we estimate achievement returns to school spending and test whether returns vary between counties with low and high levels of initial human capital (measured as birth weight), child poverty, and previous spending. Spending returns are higher among counties with low previous investment (counties that also have a high percent of Black students). Evidence of diminishing returns by previous investment documents another way that schools increase equality and establishes another argument for progressive school funding: efficiency.

Acknowledgements

The authors are grateful for feedback from Susanna Loeb and other colleagues at the Annenberg Institute for School Reform and from anonymous reviewers. This research was supported by PSTC, which receives funding from the NIH (P2C HD041020), and by grants from the Spencer Foundation and the American Educational Research Association which receives funds for its "AERA Grants Program" from the National Science Foundation under NSF Grant #DRL-1749275. Opinions reflect those of the authors and do not necessarily reflect those of the granting agencies.

Introduction

Progressive school funding – spending more in communities with fewer financial resources – is typically justified for equity, not efficiency. The equity rationale is that these communities invest less in children and it therefore costs more for schools to help children reach a given level of academic achievement (e.g., Baker 2017). This rationale matches the equity-efficiency tradeoff (Okun 1975), which suggests that promoting equity requires sacrificing efficiency: spending more in communities with lower previous investment and fewer resources (higher equity) predicts lower returns (lower efficiency). Scholars have questioned this tradeoff conceptually in education and healthcare (Le Grand 1990; Reidpath et al. 2012; Brighouse et al. 2018), but little work has tested it empirically.

How do benefits of school spending vary by previous investment? Public K-12 education spending in the U.S. totaled $739 billion in 2017 (NCES 2020). If that spending yields larger benefits in areas that already invest more in children, then an equal increase in school spending across communities would increase inequality of achievement. In addition to the equity-efficiency tradeoff argument, the economic theories of cumulative returns and dynamic complementarities also predict larger returns in areas with higher previous investment, because those with more initial human capital benefit more from later investments (Heckman 2000; Cunha and Heckman 2007; Johnson and Jackson 2019). In contrast, growing evidence suggests school spending yields larger benefits in areas with fewer resources (Biddle and Berliner 2002; Lafortune et al. 2018; Rauscher 2020a, 2020b), but variation by initial human capital has not been explicitly examined.

A strength of research in sociology of education and stratification is identifying how schools reproduce inequality (DiPrete and Fox-Williams 2021). Less is known about how schools reduce inequality. This study examines whether and how the relationship between school spending and achievement varies by previous investments, including initial human capital (at birth), child poverty rate, and previous school spending. We link annual county-level panel data on educational achievement and school spending in years 2009-2018 from the Stanford Education Data Archive 4.0 and Census Finance Survey (F-33) to county infant health data from National Vital Statistics to estimate achievement returns to school spending and to assess how returns vary between counties with low and high levels of initial human capital (measured as birth weight), child poverty, and previous spending. Recent school finance court case decisions help address potential endogeneity. To adjudicate among potential theories, we also examine variation by family income and whether spending distribution in school districts and in states changes with spending increases.

To preview our results, we find a stronger relationship between spending and achievement among counties with low previous investment. Results contribute to evidence that equality and efficiency are not necessarily a tradeoff (Okun 1975) and that progressive investments in children can be both equalizing and efficient. Higher returns in areas with lower investment establish another way that schools reduce inequality and provide another powerful argument for progressive school funding: efficiency. Higher returns to school spending in counties with low investment – counties which also have a high concentration of Black residents – suggest school funding could be one pathway for community reparations (Kaiman 2016).

Theoretical and Empirical Background

Schools and Inequality

Scholars have debated whether schools reduce or reproduce inequality at least since Horace Mann called schools "the great equalizer of the conditions of men" (1849:59). Evidence suggests schools reduce inequality in achievement by providing more equal academic input and contexts than students would receive at home (Downey et al. 2004). Schools benefit disadvantaged students more because the counterfactual learning opportunities they would have access to outside of school are substantially worse than those provided in school (Raudenbush and Eschmann 2015). The greater equality of access to skills at school – than students would otherwise experience outside of school – overcomes unequal aspects of school on average, allowing schools to increase equality of achievement (Downey et al. 2004; Raudenbush and Eschmann 2015).

Although schools may increase equality of achievement on average, the unequal distribution of school spending limits this equalizing potential. School finance reforms increase achievement by allowing districts (especially low-income districts) to invest more in resources that improve learning, such as small class sizes and quality teachers (Jackson et al. 2016; Lafortune et al. 2018; Boyd-Zaharias 1999; Ferguson 1991). In many states, funding formulae distribute funds progressively by poverty/wealth (Corcoran and Evans 2015). However, funding inequality remains both between and within districts (Roza 2010; Ejdemyr and Shores 2017; Condron and Roscigno 2003). School funding is unequally distributed by student characteristics, including race and income (Sosina and Weathers 2019; Condron 2009; Darling-Hammond 1998; Kozol 1991). Uneven local tax revenue between districts and unequal distribution of funds across schools within districts result in unequal resources both within and between districts (Roza 2010). Unequal educational outcomes including achievement may therefore reflect unequal spending.

<u>School Resources and Inequality</u>

Even if school spending could be equalized across districts, equal resources may have limited potential to increase equality of student achievement for two reasons. First, advantaged communities and students may benefit more from the same amount of school spending. In that case, even equal spending on each student would still generate inequality. The theory of dynamic complementarity in child development and the classic economic argument that "skill begets skill" (Heckman 2000; Cunha and Heckman 2007; Johnson and Jackson 2019) suggest that those with more initial human capital (e.g., higher birth weight) benefit more from later investments, such as school spending. Some evidence supports this and suggests that investments are complementary (Aizer and Cunha 2012; Cunha et al. 2010; Heckman et al. 2010; Gilraine 2016). Johnson and Jackson (2019) provide a particularly relevant example, examining variation in the effects of school finance reforms, and find evidence of complementary effects of school spending with Head Start access.

In addition to variation in school investments, family investments in child development are also unequal. High-income families spend more on lessons, school, and childcare than families in poverty (Schneider et al. 2018), contributing to unequal academic preparation. If students with higher previous investments benefit more from later investments, then school spending increases should improve achievement more among high-income students in areas with initial advantages, increasing inequality of achievement.

Hypothesis 1a: Higher school spending improves achievement more among students in counties with high initial human capital than in counties with low initial human capital.

Hypothesis 1b: Within high-investment counties, higher school spending improves achievement among students from high-income but not low-income families.

Why School Resources May Reduce Inequality

Sociological arguments offer multiple reasons that schools could increase inequality, including unequal school resources (Ladson-Billings 2006; Condron and Roscigno 2003; Kozol 1991). Compared to literature on how schools reproduce inequality, less is known about how schools reduce inequality (Downey 2020). Evidence that schools increase equality of achievement overall (Downey et al. 2004; Raudenbush and Eschmann 2015), despite substantial inequalities, suggests that schools increase equality by providing more equal contexts than students would experience out of school. Another explanation for how schools reduce inequality is that school spending benefits students with initial disadvantages more than those with advantages. If school resources have larger benefits among disadvantaged students, then spending increases should improve achievement more in areas with low initial human capital and among low-income students (Hypothesis 2). This theory suggests that schools improve not only equality of context, but also equality of outcomes (equity). That is, once children are in the more equal context of school, spending more has progressive effects on achievement and schools improve outcomes more among those with disadvantages.

Growing evidence supports the hypothesis that disadvantaged students benefit more from school funding (Cellini et al. 2010; Jackson et al. 2016; Lafortune et al. 2016; Rauscher 2020a, 2020b; Jackson and Mackevicius 2021). Studies that take advantage of state finance reforms find that greater equality of school funding narrows SAT score gaps by parental education (Card and Payne 2002) and increases student test scores in low-income districts (Lafortune et al. 2018; Roy 2011; Guryan 2001; Papke 2005; Downes et al. 2009). Studies that examine heterogeneous effects of school spending (rather than effects of greater spending equality) also find larger benefits among disadvantaged students (Jackson et al. 2016; Candelaria and Shores 2019; Rauscher 2020a).

Less is known about why school funding may benefit disadvantaged students more than others. One potential explanation is the theory of diminishing returns, which suggests the benefits of investment decline at higher levels (Knight 1944). Originally applied to agriculture and investments in land, the theory has been applied more broadly to a variety of topics, including technology, exports, economic development, and social capital (Shephard and Fare 1974; Kohli and Singh 1989; McFadyen and Cannella 2004; Potter and Watts 2011). Evidence of diminishing returns to time investment in child academic achievement (Walberg and Tsai 1984) suggests the theory could help account for higher returns to school spending among disadvantaged students. Recent research comparing estimates from multiple studies finds little evidence of diminishing achievement returns to school spending over baseline per pupil spending (Jackson and Mackevicius 2021). However, estimates using the same measures and methods would be more directly comparable. Furthermore, in addition to variation by previous school spending, benefits of spending may also vary by earlier measures of previous investments before children start school. Children come to school with a history of previous investments, producing substantial inequality at school entry (Downey et al. 2004; Hart and Risley 1995), and returns to school spending may differ by early investments.

Children have limited time and attention spans and increasing academic investment may yield little test score benefit in areas with already high investment. If we use a sponge analogy, children soak up a great deal like sponges, but once a sponge is saturated, additional water cannot be retained and will run off. Similarly, child academic achievement improves from educational investments, but additional investments among children with already high levels of investment may yield lower benefit (analogous to a saturated sponge). Diminishing returns theory suggests larger benefits to school spending among students in areas with lower levels of previous investment:

Hypothesis 2: Higher school spending improves achievement more among students in counties with:

a. low initial human capital than high initial human capital.
b. high child poverty than low child poverty.
c. low levels of previous school spending than high previous spending.

If spending benefits are indeed larger in counties with lower previous investment, diminishing returns further suggests that, within those counties, spending benefits should only accrue to low-income students. The theory predicts spending benefits among low-income but not high-income students in areas with lower levels of previous investment because high-income students receive greater investment from families outside of school.

Hypothesis 2d: Within low-investment counties, higher school spending improves achievement among students from low-income but not high-income families.

The implications of school investments for inequality remain unclear due to potential heterogeneity by previous investments (Rossin-Slater and Wust 2020; Gilraine 2016). We test the hypotheses above to assess whether the relationship between school spending and achievement differs by previous investments, including human capital, family income, and school spending.

Methods

Data and Sample

We use county-level achievement data from the Stanford Education Data Archive (SEDA 4.0; Reardon et al. 2021). These data include county mean achievement test scores for each year 2009-2018 and each grade 3-8. Within each county, SEDA also provides achievement separately for low-income students (economically disadvantaged) and for higher-income students (not economically disadvantaged), based on each state's definition of "economic disadvantage". Year refers to spring of the academic year throughout the paper. We link SEDA data using FIPS county code to Public Elementary-Secondary Education Finance Data 2009-2018 from the Census Finance Survey (called F-33 data) and to administrative birth data from the National Vital Statistics System (NVSS). F-33 data include annual expenditure details for each school district, which we use to calculate total county-level spending and enrollment among all districts in each county and year. The NVSS data provide inclusive and reliable infant health information for all live births in the United States. We calculate annual county-level measures of infant health for each cohort born 1995-2009. We include NVSS birth data for birth years 1995-2009 because children born during this period correspond to the academic cohorts covered in the SEDA data. The oldest cohort in SEDA, the "8th-grade-in-2009" cohort, roughly corresponds to children born in 1995, while the youngest cohort in SEDA, the "3rd-grade-in-2018" cohort, roughly corresponds to children born in 2009.

The publicly available NVSS birth microdata identify mother's county of residence, but only for counties with a total population of 100,000 or more (~500 counties). We use restricted NVSS birth data, which provide geographic information for all births and allow inclusion of 3,025 counties with achievement, spending, and infant health information (99% of all counties and 96% of all counties or statistical equivalencies; U.S. Department of Commerce 1994:4-9). Therefore, our sample includes most counties in the U.S.

Measures

Achievement: Our dependent variable is math achievement test score for each county, grade, and year measured in standard deviation units relative to the distribution in the national reference cohorts (Fahle et al. 2021:30). A value of 0.2 in our analyses indicates that average student achievement in the county-grade is one fifth of a standard deviation above the mean score of the national reference cohorts in the same grade (Fahle et al. 2021:30). Main analyses use annual county-level achievement, aggregated across grades weighted by grade-level enrollment in each year. We examine county-grade achievement measures in sensitivity analyses to allow more precise estimates.

We focus on math achievement because it is more strongly related to educational attainment and is less likely to be influenced by the proportion of English language learners (Jencks and Phillips 1999). SEDA data suppress observations with fewer than 20 students for privacy reasons and remove observations based on over 20% alternate assessments, low participation rate (<95%), low reliability (<0.7), or with standard errors over 2 (see Fahle et al. 2021). After linking achievement data to spending and infant health data, we have 22,864 county-year observations (based on 133,438 county-grade-year observations) with complete information (representing 96% of all counties or statistical equivalencies). The number of observations is smaller for specific subgroups, such as achievement among low-income students.

SEDA documentation suggests that achievement in large districts is slightly overestimated (by 0.05 standard deviations, on average; Reardon et al. 2019:11). We examine county-level achievement and we control for enrollment to help address this. In addition, the SEDA data translate achievement measures into a common national scale based on overall state performance and on area performance on a state exam relative to other areas in the state (see Fahle et al. 2021 for details). This process raises concern about between-state comparisons of county achievement measures. Our estimates rely on variation in achievement and spending within the same county over time to avoid concern about between-state achievement comparisons.

Spending: Our key independent variable is per pupil spending at the county level in each year 2009-2018. The F-33 data contain annual total K-12 spending and enrollment for all school districts. We calculate the total spending of all districts within the same county to obtain total annual spending at the county level. All currency is adjusted for inflation to 2018 dollars using monthly consumer price index data based on the fiscal rather than calendar year to match the school calendar (Candelaria and Shores 2020). We sum the total number of students enrolled in K-12 education across all districts in the same county to obtain the county-level student enrollment. Annual county-level per pupil spending is county total spending (in 2018 dollars) divided by the county total student enrollment (logged to reduce skewness).

Initial Human Capital: We examine variation in the association between educational spending and academic achievement by initial human capital. Birth weight is frequently used as a measure of initial or early human capital (Datar et al. 2010; Hsin 2012) and has important implications for later human capital and earnings (Conley et al. 2003; Black et al. 2007; Royer 2009). Other studies have examined variation by later measures of human capital (e.g., Head Start access; Johnson and Jackson 2019). We use birth weight (in grams) because it is one of the earliest measures possible, it is reliable, and parents cannot selectively invest in birth weight based on earlier measures of human capital. For example, parental decisions about whether to enroll their child in Head Start could differ depending on child health. Healthy birth weight has important benefits for health and labor market outcomes through adulthood and low birth weight (<2,500 grams) has both short- and long-term negative implications (Conley et al. 2003; Black et al. 2007; Royer 2009; Johnson and Schoeni 2011). High birth weight can also be associated with health problems (Johnsson et al. 2015; Koifman and Pombo-de-Oliveira 2008), but this is less of an issue here because we use the county rate of low birth weight.

Ideally, we would like to have precise birth weight measures for each academic cohort included in the SEDA data. We calculate academic cohort based on grade and year ($cohort_{jt} =$

$Year_t - (grade_j + 5) - 1$) so that cohorts include children who are age 6 by spring of their kindergarten year. However, in reality not all children born in the same academic year attend primary school at the same age. Families with children have relatively low (4%) between-county migration rates during the time period examined (see Appendix Table A2), but students may also move across counties in a selective manner. Therefore, we calculate the rate of low birth weight across all cohorts born 1995-2009 (roughly the birth years of the academic cohorts included in the SEDA data) for births to mothers residing in each county. Our time-constant county birth weight measure avoids over-stating precision and is justified by the data. In our sample, the majority of the variation in cohort birth weight is between counties rather than within counties. Regressing birth weight measured for each county and cohort (birth year) on county identification indicators yields an R-squared of 0.67, meaning that two-thirds of the county-level birth weight variation in our panel data is accounted for by variation between counties and only a third of the variation lies in variation over time within the same county. Thus, cohorts within the same county tend to share similar birth weights over time. Because county rate of low birth weight is calculated across all cohorts examined and does not vary over time, it may therefore partially proxy for the various local socioeconomic and demographic circumstances that influence infant birth weight (Morenoff 2003; Gorman 1999).

High and Low Investments: In addition to human capital, we also examine variation by county-level measures of child poverty and previous school spending. We divide counties into terciles based on: county rate of low birth weight across all cohorts examined (1995-2009); 2008 child poverty rate (ages 0-17, from Census Small Area Income and Poverty Estimates program); and 2008 per pupil spending (from F-33 data).

Control Variables: We include the following time-varying control variables, measured at the county-level, in all analyses: total number of students enrolled in K-12 schools (from F-33; logged to reduce skewness); number of schools (from F-33); percent of schools that are in a town, suburban, and urban location (rural is omitted; from SEDA); percent of students who are Black, Hispanic, Asian, and American Indian (white omitted; from SEDA); information index measures of income segregation and Black-white segregation between schools (from SEDA); percent of students with an individual education plan and learning English (from NCES); and county socioeconomic status measures from SEDA: proportion of families with a bachelor's degree or higher, poverty rate, unemployment rate, SNAP receipt rate, single mother household rate, median income (log), and student free/reduced-price lunch eligibility rate.

Analytic Strategy

We use two approaches to examine the relationship between spending and achievement. The strengths of each approach address limitations in the other. First, we estimate the extent to which achievement changes with per pupil spending in models that include county and year fixed effects. This approach addresses several challenges. Regardless of spending, achievement may be higher than average in counties with highly educated families and effective teachers. At the same time, locations with higher achievement often have higher property values and can spend more on education due to higher local property tax revenues (Hoxby 2001). We account for those stable differences between counties by including county fixed effects. We include year fixed effects to account for changes over time that could affect achievement in all counties (e.g., the 2008 recession). Estimates rely on variation in achievement and spending within the same county across years.

All models are fit in Stata 16 using reghdfe (Correia 2017), which allows multiple levels of fixed effects and eliminates singleton observations (e.g., counties with only one observation).

Equation 1 predicts achievement in a given county (i) and year (t) with county and year fixed effects, county-level per pupil spending (PPS), and time-varying controls (X_{it}) (identified above). Robust standard errors are adjusted for county-level clustering in all models.

$$\text{Achievement}_{it} = \alpha + \beta_1 \text{PPS}_{it} + \beta_k X_{it} + \text{County}_i + \text{Year}_t + \epsilon_{it} \quad (1)$$

In Equation 1, β_1 estimates the extent to which achievement changes with per pupil spending over time within a county. After controlling for county fixed effects, β_1 is no longer confounded by county characteristics that remain stable over time during the period under study. We add time-varying control variables to account for changes in enrollment, number of schools, and local economic conditions that could potentially influence achievement and vary across counties.

To examine the variation in β_1 by initial human capital, we stratify the sample and fit separate models using samples limited to counties in the bottom and top terciles of county rate of low birth weight (0-6.8%, 8.2-25%). We compare β_1 coefficients from the two samples to determine whether the relationship between spending and achievement differs significantly by initial human capital (Clogg et al. 1995; Paternoster et al. 1998). Specifically, we calculate $z = \frac{\beta_{Low} - \beta_{High}}{\sqrt{SE_{Low}^2 + SE_{High}^2}}$, where β_{Low} indicates β_1 from Equation 1 when predicting achievement in counties with low birth weight (top tercile of low birth weight) and β_{High} indicates β_1 when predicting achievement in counties with high birth weight (bottom tercile of low birth weight). We use the same approach to test for differences by child poverty and previous spending.

Despite strengths, this approach does not address unobserved time-varying county characteristics that could influence both spending and achievement. For example, a stronger economic downturn in certain counties could reduce both school spending and achievement.

Because our focus is on comparing the spending-achievement relationship by birth weight, those unobserved characteristics would have to alter the relationship differently in the top and bottom terciles of low birth weight to invalidate our findings.

A second approach addresses these potential endogeneity concerns. We use difference-in-differences (DID) to compare results before and after state supreme court decisions on school finance cases that introduced exogenous changes in school spending. Specifically, we limit analyses to counties in the 19 states that had a school finance court case decision between 2009 and 2018 (Rebell 2017; Baker et al. 2021). Figure 1 shows states that had a school finance court case decision in the time period and whether the decision supported higher funding (dark-colored) or not (light-colored); states without a decision are white. We use the first court case in the time period in each state, with adequacy or equity cases taking precedence over compliance cases (those that sought to make states compliant with pre-recession policy). Appendix Table A1 lists these court cases and whether the decision supported higher funding.

Using these cases, we create indicators for counties in a state where the court case decision was in support of more funding (*Treat* in Equation 2) and for observations after the court decision in each state (*Post*). We limit analyses to observations two years before and two years after each court decision to prevent counties in states with decisions early or late in the period examined from driving results.

$$\text{Achievement}_{it+1} = \alpha + \beta_1 \text{Post}_t + \beta_{DiD} \text{Post}_t * \text{Treat}_i + \beta_k X_{it} + \text{County}_i + \text{Year}_t + \epsilon_{it+1} \quad (2)$$

Holding constant the main effect of being observed after the court decision and county fixed effects, the DID model tests the interaction between Treat and Post to estimate whether achievement (in year *t*+1) in treated counties differs significantly after the decision compared to before the decision and to untreated counties. The main effect for Treat drops out of the model due to inclusion of county fixed effects. Achievement should have a delayed response to court decisions (because it takes time for states to implement funding changes and for districts to make curriculum or staff changes based on those funding changes). DID estimates therefore predict one-year lead achievement (t+1). DID models include the same fixed effects and time-varying controls (X_{it}) as in the fixed effects approach. Robust standard errors are adjusted for county-level clustering in all models. As in the main analyses, we fit the DID regressions separately on subsamples by low birth weight tercile and test whether β_{DiD} coefficients differ significantly.

The main assumptions in DID analyses are: 1) parallel trends – treatment and control observations were on similar trajectories over time before treatment; 2) another change did not occur at the same time as the treatment. Figure 2 shows parallel achievement trends in low-birth weight counties before the court decision. (Figure 2 includes more years before the court decision than regression analyses to illustrate trends.) Trends are comparable but not parallel among high birth-weight counties. The court decisions occurred at different times across states, so other changes would also have had to vary over time across states to bias results. Appendix Table A3 shows DID estimates predicting per pupil spending. A pro-funding decision increases spending and coefficients predicting spending do not differ significantly by birth weight, poverty, or previous spending.

Sensitivity Analyses

We conduct a series of sensitivity analyses to assess robustness and address concerns. First, we test the robustness of our results using other model specifications. We predict annual county-grade achievement observations in models with county-cohort and county-grade fixed effects to address potential differences within counties. For example, certain cohorts within a county may have higher achievement, or student test-taking ability may improve at higher grade levels, particularly in counties with higher income or property values. We address these potential differences by including fixed effects for county-specific grades and cohorts.

Second, results are potentially subject to the influence of outlying observations. For instance, Figure 3 reveals some observations with exceptionally high per pupil spending. Therefore, we repeat analyses when excluding observations with outlying values of per pupil spending: observations 3 times the interquartile range above the 75th percentile (excludes 0.49% or 766 observations with per pupil spending over $37,550; there are no low-spending outliers).

Third, we assess robustness to alternative specifications of initial human capital. The main analyses examine variation by tercile of county mean low birth weight across all cohorts. All county-year observations are assigned to the same tercile. This approach avoids overstating the precision of our cohort infant health measures, but ignores potentially important within-county variation. We repeat analyses by county-cohort tercile of low birth weight, assigning each observation based on cohort-specific low birth weight regardless of whether other observations in the county are in the same tercile. We conduct these analyses using three cohort specifications: **A)** cohort is calculated based on grade and year ($cohort_{jt} = Year_t - (grade_j + 5) - 1$); **B)** cohort is modified to account for the age cutoff required to start kindergarten in most states. Birth quarters 1-3 remain in the same cohort calculated in A above. Those born in quarter 4 are assigned to one cohort higher than the Cohort A calculation ($cohort_{jt} = Year_t - (grade_j + 5)$); **C)** cohort varies by the state-specific age cutoff required to start kindergarten (Education Commission of the States 2011). In most states (with age 5 cutoff dates August 15-November 31 or determined by district), Cohort B is assigned. In states with cutoff dates December 1-January 1 (CA, CT, DC, MI, VT), Cohort A is assigned. In states with cutoff dates July 1-August 14 (AK, IN, MO), cohort B is assigned but birth quarter 3 is assigned one cohort higher.

Fourth, we use alternative measures of initial human capital, including preterm birth rate (gestational length <37 weeks) and a combination of low birth weight and preterm birth rates. Specifically, we examine variation by tercile of county mean preterm birth rate and by tercile of the sum of county mean rates of preterm birth and low birth weight.

Fifth, variation in school spending returns could reflect different relationships between enrollment and achievement over time. All models control for time-varying county enrollment, but high spending and low poverty counties often attract families and new students may have lower achievement. In this scenario, enrollment may be increasingly important for achievement over time, particularly in financially advantaged counties. To address potential variation in the confounding effect of enrollment over time, we repeat the main analyses when including: 1) log county enrollment interacted with year; and 2) county-specific enrollment trends (log county enrollment interacted with year and with an indicator for each county).

Sixth, we examine potential lag and lead effects (Angrist and Pischke 2009:237-241) to check that effects on achievement emerge after the change in spending. In fixed effects models, spending in future years should have no effect on current achievement. In DID analyses, a pro-funding decision should have no effect on achievement in years before the court decision year.

Results

Descriptive Statistics

Table A2 provides descriptive statistics for county-year observations from 2009 to 2018, along with mean values among counties in the top and bottom terciles of birth weight, poverty, and spending. Students in high-birth weight counties have better average academic achievement and receive higher educational investments in the form of per pupil spending than students in low-birth weight counties. High-birth weight counties also have higher median household income and lower poverty rates, on average. Similar differences are observed by poverty and spending, with higher achievement and income in low-poverty and high-spending counties.

Figure 3 provides scatterplots and fitted lines of achievement by spending in counties with high and low birth weight. It shows a clear pattern: a positive association between per pupil spending and achievement among low-birth weight counties. Among high-birth weight counties, the relationship is relatively flat. This different relationship by birth weight is also observed when examining achievement in a single grade (Figures S1-S2 in the Supplemental Online Appendix).

Regression Analyses

Results of regression analyses also suggest the relationship between educational spending and achievement differs by birth weight. The first two models in Table 1 provide estimates of the relationship between per pupil spending and mean academic achievement in samples stratified by county birth weight. Model 2 reveals that among low-birth weight counties, a ten percent increase in per pupil spending is associated with about a 0.01 standard deviation increase in math achievement ($p<0.01$) within the same county (log(1.1) x 0.07 = 0.01). At mean spending (~$13,000), the model suggests a 0.01 standard deviation increase in achievement with a $1,300 increase in per pupil spending. This is a small-moderate cost effectiveness ratio in education: small effect size at a moderate cost (Kraft 2020). In contrast, Model 1 finds no association between spending and achievement among high-birth weight counties. The difference between spending coefficients in high and low birth weight counties is statistically significant (z=3.28, $p<0.01$). Models 3-6 show a similar pattern by county child poverty rate and previous school spending. Models 4 and 5 reveal a relationship between spending and achievement in high-poverty and low-spending counties that is similar in magnitude to that in low-birth weight counties. In contrast, the spending-achievement relationship is null in low-poverty and high-spending counties and spending coefficients differ significantly by poverty and spending (*z*=2.86 and 2.54, $p<0.05$).

DID estimates in Table 2 also find variation by birth weight. Achievement increased by 0.06 standard deviations more in a county after a court decision supported higher funding, compared to counties where the state supreme court did not rule in favor of greater funding. This increase in achievement occurred only in low-birth weight counties; the change is null in other counties and spending coefficients differ significantly in high- and low-birth weight counties (*z*=3.03, $p<0.01$).

Results in Tables 1 and 2 support *Hypotheses 2a-c*: the relationship between spending and achievement is higher in areas with lower previous investments, including initial human capital, poverty, and spending. To test *Hypotheses 1b and 2d*, Table 3 shows models predicting achievement separately among low-income and high-income students in the same county. Consistent with results in Tables 1-2, the relationship between spending and achievement is larger in low-birth weight, high-poverty, and low-spending counties. Spending coefficients differ significantly by birth weight in both fixed effects and DID models when predicting achievement among low-income students. Coefficients predicting low-income achievement also differ by poverty and spending in fixed effects models. Spending coefficients do not differ significantly at the 95% level when predicting high-income achievement. Contrary to *Hypothesis 1b*, spending benefits are smaller in high-investment counties and are not limited to high-income students. Rather, results are generally consistent with *Hypothesis 2d*: spending benefits emerge mainly among low-income students and differences by previous investments are smaller among high-income students.

Alternative Explanations

We find evidence of diminishing returns with higher previous investments, but other theories suggest we could overestimate the degree of variation in spending returns. Higher spending benefits among disadvantaged students could be due to *changes in the distribution of funds*: as spending increases it could become more progressive, with higher spending in low-income districts and schools. Once spending demands are met among high-income families, states and districts may be able to devote additional funds primarily to districts or schools with disadvantaged students. In this scenario, spending progressivity by income should increase with spending increases.

We use state-level measures of funding progressivity 2009-2018 from the School Finance Indicators Database: the ratio of predicted spending per pupil in districts with 30% poverty to that in districts with 0% poverty (Baker et al. 2021). To measure district-level spending progressivity, we use Uniform Chart of Accounts data on school-level spending 2010-2018 from the Rhode Island Department of Education (RIDE 2020). Rhode Island is unique in providing school-level spending data for several years before the Every Student Succeeds Act required all states to begin reporting this information by 2020. Longitudinal school-level spending data for all states would be ideal, but they do not exist for the years of this study and we therefore estimate the relationship between spending and district-level progressivity in one state. District-level spending progressivity is the (logged) ratio of spending on the average low-income student (eligible for free/reduced-price lunch) to the average non-low-income student in each district (Ejdemyr and Shores 2017). For both state- and district-level measures, higher values indicate more progressive spending. Spending ratios are unequal due to unequal spending and unequal distribution of students across schools within districts and across districts within states.

We estimate the extent to which spending progressivity changes in a district as per pupil spending changes. We predict spending progressivity with per pupil spending, district and year fixed effects, and the same controls in the main analyses, measured at the district-level. We use the same approach to predict changes in state spending progressivity, with state and year fixed effects and state-level controls. The coefficient for spending estimates the extent to which spending progressivity changes as spending increases in the district or state.

Results are not consistent with the progressivity explanation. Table 4 shows estimates predicting district and state progressivity of the distribution of spending. In Rhode Island districts, spending increases are not related to more progressive spending across schools by student income. Using national state-level data, estimates in Table 4 also find no relationship between spending and spending progressivity. Results are also null in DID models.

A second theory suggesting overestimated variation in returns is *ceiling effects*. Advantaged students may have less room to improve test scores if they already have high achievement. Achievement data from an assessment that uses Item Response Theory (IRT) reduces the chance of ceiling effects (Fries et al. 2011) because test responses are analyzed based on the student's performance on the whole test and the likelihood of answering specific questions correctly. In addition, evidence suggests limited ceiling effects on estimates of achievement growth and on National Assessment of Educational Progress scores, which are used in SEDA data (Coedel and Betts 2010; Linton and Kester 2003).

Nevertheless, we take steps to assess potential ceiling effects and present results in a Supplemental Online Appendix. If ceiling effects explain the variation, we expect to find no variation in the relationship between spending and student characteristics when limited to counties with consistently high achievement. Supplemental Appendix Table S1 tests the ceiling effects explanation by limiting analyses to counties in the top achievement tercile. Spending coefficients do not differ significantly by birth weight in fixed effects models, but do differ in DID models. Coefficients are also larger in high poverty counties in fixed effects models and in low-spending counties in both fixed effects and DID approaches. Thus, results suggest that achievement benefits of spending still vary by previous investments among counties in the top achievement tercile. We also predict an alternative outcome variable with no ceiling: home values. Results (Appendix Table S2) reveal that spending coefficients also differ by previous investment when predicting county home values. Overall, results are not consistent with a ceiling effects explanation for our results.

A third theory suggests we could overestimate variation if the same dollar increase in disadvantaged areas is a larger ***relative increase in spending***. Child development research finds that family income improves child performance on a variety of academic outcomes and the benefit is larger among children in low-income families (Dearing et al. 2001; Dahl and Lochner 2012). This finding could reflect diminishing returns to income or a larger relative increase in income. Consider two hypothetical schools: one with low-income students spends $10,000 per pupil and a second with higher-income students spends $15,000 per pupil. If both schools increase spending by $1,000, low-income student achievement may increase more not from a higher return to each dollar spent, but because spending increased by 3 percentage points more than in the higher-income school.

Our analyses use log-transformed spending to incorporate relative changes. Given the same relative increase in spending, our estimates suggest higher benefits in areas with lower previous investments. In addition to our main analyses, we also test whether the relationship between achievement and spending varies by human capital and income when holding constant previous spending. Appendix Table S3 repeats the estimates in Tables 1 and 2 when controlling for spending in the previous year. Results indicate that the benefits of spending still differ by initial human capital and poverty when controlling for previous-year spending. Thus, higher returns in counties with lower previous investments do not appear to be due to a larger relative increase in spending.

Sensitivity Analyses

Results of sensitivity analyses are provided in the Supplemental Online Appendix. First, results (Table S4) are consistent with the main analyses (Tables 1 and 2) when predicting county-grade-year observations and including county-cohort and grade rather than county fixed effects. Results (Table S5) are also consistent when including county-grade and cohort rather than county fixed effects. While the main analyses examine within-county variation over time, these estimates examine variation over time within the same county and grade or cohort. Second, results are consistent when excluding observations with outlying values of per pupil spending (Table S6). Third, we repeat analyses using three alternative specifications of academic cohort and when assigning birth weight tercile based on the rate of low birth weight for each county-cohort observation rather than the county average across all cohorts. Results (Table S7) are similar to those in the main analyses.

Fourth, we use two alternative measures of initial human capital: county preterm birth rate and the sum of county rates of preterm and low birth weight. Results (Table S8) are consistent with the main analyses and add further evidence of higher spending returns in counties with low initial human capital (*Hypothesis 2a*).

Fifth, results (in Table S9) are consistent when allowing the effect of enrollment on achievement to vary over time. Sixth, Table S10 shows estimates for lag and lead spending measures in fixed effects models. There is no evidence that spending in a future year influences current-year achievement. Current-year spending remains marginally related to higher achievement in low-birth weight counties, even when controlling for previous- and future-year spending. Table S11 shows estimates of a pro-funding decision when limiting the sample to observations in years before and after the court decision. Two years before the decision, achievement is higher in low-birth weight counties with a pro-funding decision, which raises some concern about the exogeneity of court decisions. This could suggest that courts in states with high achievement may be more likely to rule in favor of school funding. However, achievement is consistently unrelated to a pro-funding decision in high-birth weight counties and is unrelated to a pro-funding decision in the year before and the year of the court decision in both low- and high-birth weight counties. Achievement is significantly higher in years 2 and 3 after a pro-funding decision in low-birth weight counties and does not differ significantly by court decision in high-birth weight counties. Furthermore, results are consistent using the doubly robust DID estimator to address heterogeneity in timing (csdid; Callaway and Sant'Anna 2020; Sant'Anna and Zhao 2020) and suggest no effects before the court decision and significantly larger benefits of a pro-funding decision in low-birth weight counties (Table S12). Overall, results provide little evidence that changes in achievement precede the treatment.

Conclusion

The equity-efficiency tradeoff argument and cumulative return theories predict larger returns to school spending in areas with higher previous investment, because redistributing funds is considered inefficient and those with more initial human capital are expected to benefit more from later investments (Okun 1975; Heckman 2000; Cunha and Heckman 2007; Johnson and

Jackson 2019). If school spending yields larger benefits in areas that already invest more in children, then an equal increase in school spending across all communities would increase inequality of achievement. We link ten years of county-level panel data (2009-2018) from the Stanford Education Data Archive and F-33 finance data with administrative birth data from National Vital Statistics to assess how achievement returns to school spending vary between counties with low and high levels of previous investment.

Achievement returns to school spending are consistently higher among counties with low initial human capital. Achievement increases by an average of 0.01 standard deviations with a ten percent increase in per pupil spending in low-birth weight counties. Compared to other education interventions, this achievement return to spending in low-birth weight counties represents a small-moderate cost effectiveness ratio at mean spending (Kraft 2020). Even controlling for previous-year spending, the achievement return in low-birth weight counties is over 20 times larger than that in high-birth weight counties (0.062/0.003 = 20.7). The variation in returns by birth weight holds when using school finance court case decisions to address potential endogeneity in spending and in a series of sensitivity checks, including alternative measures of human capital. Spending benefits are also larger in counties with high child poverty and low previous school spending and the variation in returns by previous investment is larger among low-income students than among high-income students.

We examine several potential explanations for variation in returns to spending, including diminishing returns, changes in the distribution of spending, ceiling effects, and relative spending changes. Results are most consistent with diminishing returns to school spending at high levels of previous investments. That is, school spending returns are higher in areas with low previous investment and diminish in areas with higher investment. Diminishing returns have been found for time investment in child academic achievement (Walberg and Tsai 1984). While some evidence suggests returns to spending do not diminish by baseline school spending (Jackson and Mackevicius 2021), we find evidence of diminishing returns to school spending by initial human capital. This study compares returns to school spending using the same measures and methods, which makes estimates more directly comparable than in recent work (Jackson and Mackevicius 2021). Examining variation by multiple measures of previous investments, evidence from this study suggests that returns to school spending for achievement may diminish over previous – particularly early – investments.

Our finding that spending benefits vary by county birth weight has implications for racial equality. The proportion of Black students is much higher in low birth weight counties than in high birth weight counties (30% and 2%, respectively; Appendix Table A2 provides descriptive information). Higher returns to school spending in low birth weight counties therefore suggest higher returns in counties with a high proportion of Black students. Directing more education funds to low birth weight counties has potential to reduce Black-white achievement gaps, given higher returns to spending and higher concentrations of Black students in those counties compared to others. School spending may also increase equality of achievement within counties: Figures S3 and S4 compare spending estimates by student income and by race. Estimates tend to be larger among low-income students and in some cases among Black students, compared to higher-income and white students. Potential variation in spending returns by race warrants future study.

Higher spending returns in counties with a high concentration of Black students suggest community reparations invested in local organizations, including schools, can help increase racial equality (Kaiman 2016; Darity and Mullen 2020). Our results also add to concerns about accountability policies that base school funding on achievement (Jennings and Bearak 2014; Jennings and Sohn 2014). At the same time, the relatively small effect sizes for K-12 school spending echo evidence that earlier or other types of investments may increase equality more effectively (Downey and Condron 2016; Heckman 2000; Cunha and Heckman 2007). Achievement gaps are formed before children begin school (Downey et al. 2004; Hart and Risley 1995) and closing them therefore requires investing in children and families before and outside of school (Downey 2020). However, evidence comparing benefits of public policies find large returns to investments throughout childhood, including K-12 school funding (Hendren and Sprung-Keyser 2020). We examine short-term effects on achievement and school spending could yield larger benefits later in childhood and in adulthood. Results of this study show the value of investing in low-resource communities, but cannot identify the most effective target of that investment. Future research could examine variation in effects of spending earlier in childhood and on longer-term outcomes, such as high school graduation and unemployment, to identify the most effective investments.

This study has several other limitations. First, we examine the relationship between spending and achievement at the county level. Rigorous individual-level studies also find that investments – including school spending – have larger benefits among disadvantaged children (Jackson et al. 2016). However, investments may work differently at the individual and aggregate levels due to spillover effects or institutional changes. For example, teachers may be more likely to alter their level of teaching based on aggregate but not individual human capital. Second, the data provide limited ability to identify mechanisms. Potential mechanisms relate to how teachers and schools distribute resources and time. For example, higher school spending could allow teachers to give students with lower skill levels more attention. Rich micro-level data would be required to investigate these relationships. Third, we take steps to address potential endogeneity, but our estimates could be biased by unobserved heterogeneity. Finally, we focus on achievement rather than longer-term outcomes, which evidence suggests are more dependent on school spending (Jackson et al. 2016). Future research on longer-term outcomes would be valuable.

Despite these limitations, this study contributes to knowledge about how schools reduce inequality. Research in sociology of education and stratification often identifies how schools reproduce inequality; comparatively less is known about how schools reduce inequality (Downey 2020; DiPrete and Fox-Williams 2021). Despite substantial inequalities, we know that schools increase equality of achievement overall by providing more equal contexts than students would experience outside of school (Downey et al. 2004; Raudenbush and Eschmann 2015). Beyond increasing equality of context, our results consistently suggest that school spending also improves equity by increasing equality of outcomes: Achievement benefits of school spending are larger in areas with fewer advantages, including low human capital and high poverty. Coupled with existing evidence that school spending has stronger effects for low-income and non-white students (Biddle and Berliner 2002; Lafortune et al. 2018; Rauscher 2020a, 2020b), our results suggest that schools not only increase equality of context (Downey et al. 2004; Raudenbush and Eschmann 2015), but also equality of outcomes (equity).

Progressive school funding is typically justified for equity reasons: low-income students and communities have higher needs and it costs more to achieve a given level of achievement (Baker 2017). Examining variation in the relationship between changes in spending and achievement, we find higher returns to spending in areas with low initial human capital, high child poverty, and low previous spending. Higher returns to school spending in areas with lower previous investments in children suggests that progressive school spending is more efficient. Results contribute to evidence that equality and efficiency are not necessarily a tradeoff (Brighouse et al. 2018; Okun 1975) and that progressive investments in children can be both equalizing and efficient.

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Tables and Figures

Table 1: Predicted Achievement by Previous Investment: Fixed Effects

	Achievement (std deviation units)					
	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
Spending/Pupil 2018$ (log)	-0.01	0.07**	-0.02	0.05**	0.06**	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Enrollment (log)	-0.02	0.12*	0.04	0.02	0.02	0.03
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)
Number of Schools	-0.00	0.00	-0.00	0.00	0.00*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
% Urban	0.04+	0.03	0.04+	0.01	0.03	0.02
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
% Suburban	0.03	0.02	0.04**	0.00	0.03*	0.02
	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)
% Town	-0.01	-0.00	-0.02	-0.01	0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
% American Indian in grade	-0.43	-0.29	-0.98+	-1.14**	-0.90**	-0.81
	(0.33)	(0.46)	(0.53)	(0.38)	(0.33)	(0.50)
% Asian in grade	0.69+	-0.89	0.08	-0.90	-0.66	0.10
	(0.40)	(0.68)	(0.29)	(0.92)	(0.53)	(0.35)
% Hispanic in grade	-0.38**	-0.77**	-0.29*	-0.49**	-0.88**	-0.16
	(0.15)	(0.15)	(0.13)	(0.15)	(0.17)	(0.12)
% Black in grade	-0.51	-0.45**	-0.66*	-0.46**	-0.17	-0.70**
	(0.33)	(0.15)	(0.26)	(0.17)	(0.22)	(0.21)
% English Learner	-0.18	-0.24	-0.50**	-0.15	-0.17	-0.46**
	(0.12)	(0.25)	(0.17)	(0.20)	(0.25)	(0.16)
% Indiv Educ Plan	0.01*	0.13	0.01**	0.08	0.28+	0.01*
	(0.00)	(0.11)	(0.00)	(0.08)	(0.16)	(0.00)
Income Segregation	-0.20*	-0.07*	-0.19**	-0.06	-0.13**	-0.02
	(0.09)	(0.04)	(0.07)	(0.04)	(0.05)	(0.05)
Black-White Segregation	0.00	-0.03	-0.00	-0.04	0.02	-0.04
	(0.04)	(0.07)	(0.05)	(0.06)	(0.05)	(0.05)
Median Income (log)	-0.19**	-0.05	-0.22**	-0.02	-0.15*	-0.11+
	(0.07)	(0.05)	(0.07)	(0.05)	(0.06)	(0.06)
% BA+ Education	0.08	0.42*	0.27	0.27	0.22	0.31
	(0.19)	(0.18)	(0.17)	(0.21)	(0.18)	(0.21)
% Poverty	0.10	-0.02	-0.20	0.01	-0.02	-0.23
	(0.20)	(0.15)	(0.20)	(0.14)	(0.16)	(0.20)
% Unemployed	-0.25	-0.12	-0.17	-0.09	0.11	-0.14
	(0.19)	(0.16)	(0.21)	(0.16)	(0.17)	(0.21)
% SNAP Receipt	-0.36+	-0.11	-0.59**	0.01	0.22	-0.28+
	(0.19)	(0.13)	(0.18)	(0.14)	(0.16)	(0.17)
% Single Mother Hh	0.01	0.17	0.02	-0.04	0.02	-0.02

	(0.17)	(0.14)	(0.16)	(0.14)	(0.14)	(0.17)
% Free/Red Lunch in grade	-0.07	-0.03	-0.06	0.02	-0.06	-0.04
	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)	(0.05)
Constant	2.47**	-0.71	2.33*	-0.19	1.17	1.19
	(0.90)	(0.69)	(0.95)	(0.64)	(0.77)	(0.88)
Difference in Spending Coeffs: |*z*-score|	3.28**		2.86**		2.54*	
County-Grade, Cohort, & Year Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	6,550	8,253	7,886	7,470	7,782	7,300
R-squared	0.91	0.87	0.87	0.85	0.86	0.92

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with nonmissing data on achievement, spending, birth weight, and control variables. Spending in thousands per pupil is adjusted for inflation to 2018 dollars.

High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. High and low poverty indicates counties in the top and bottom terciles of 2008 % child poverty. High and low spending indicates counties in the top and bottom terciles of 2008 spending per pupil.

Robust standard errors adjusted for county-level clustering in parentheses, + p<0.10 * p<0.05 ** p<0.01

Table 2: Predicted Achievement by Previous Investment: Difference-in-Differences

	Achievement year *t*+1 (std deviation units)					
	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
Post*Treat	-0.01	0.06**	0.01	-0.00	0.01	-0.00
	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
Post	-0.01	-0.03**	-0.02**	0.01	-0.02	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treat (redundant w/ county fixed effects)	-	-	-	-	-	-
Enrollment (log)	-0.07	-0.12	-0.14	-0.20	-0.14	-0.10
	(0.11)	(0.13)	(0.13)	(0.15)	(0.16)	(0.11)
Number of Schools	-0.00	-0.00	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
% Urban	-0.03	0.02	-0.01	0.03	0.01	0.03
	(0.08)	(0.02)	(0.02)	(0.04)	(0.02)	(0.04)
% Suburban	-0.06+	0.04+	0.03	0.06*	0.02	0.07*
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
% Town	-0.00	-0.00	-0.02	-0.00	-0.00	-0.03
	(0.03)	(0.01)	(0.04)	(0.02)	(0.01)	(0.03)
% American Indian in grade	1.04	-2.04	-0.45	1.37	-0.15	0.68
	(0.83)	(1.93)	(1.31)	(0.95)	(0.98)	(0.92)
% Asian in grade	-0.74	-0.46	0.31	-1.79	0.31	-0.51
	(0.92)	(1.78)	(0.50)	(1.70)	(0.62)	(1.17)
% Hispanic in grade	0.16	-0.13	-0.68	0.28	-1.09+	0.13
	(0.23)	(0.47)	(0.47)	(0.39)	(0.56)	(0.27)
% Black in grade	0.60	-0.14	0.84	0.09	-0.34	0.04
	(0.86)	(0.32)	(0.61)	(0.34)	(0.43)	(0.57)
% English Learner	0.08	0.66	-0.28	0.32	0.10	0.21
	(0.21)	(0.56)	(0.40)	(0.30)	(0.41)	(0.28)
% Indiv Educ Plan	0.00	-0.48	-0.41	-0.67+	-0.86+	-0.08
	(0.23)	(0.42)	(0.51)	(0.40)	(0.51)	(0.23)
Income Segregation	0.02	0.02	0.24	-0.10	0.09	0.20
	(0.16)	(0.11)	(0.15)	(0.13)	(0.13)	(0.18)
Black-White Segregation	-0.06	-0.29**	-0.00	-0.14	-0.09	-0.14
	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)	(0.12)
Median Income (log)	-0.04	0.05	0.06	-0.01	-0.13	0.08
	(0.14)	(0.10)	(0.16)	(0.10)	(0.12)	(0.14)
% BA+ Education	0.40	0.41	0.76+	0.03	0.55	-0.37
	(0.37)	(0.34)	(0.46)	(0.34)	(0.37)	(0.45)
% Poverty	-0.63+	0.16	0.37	0.20	0.39	-0.02
	(0.34)	(0.32)	(0.42)	(0.31)	(0.34)	(0.35)
% Unemployed	0.23	0.81*	-0.11	0.51	0.87*	-0.10
	(0.45)	(0.35)	(0.58)	(0.32)	(0.40)	(0.43)
% SNAP Receipt	-0.36	-0.21	-0.26	-0.42	-0.25	-0.40

	(0.41)	(0.32)	(0.63)	(0.29)	(0.35)	(0.34)
% Single Mother Hh	-0.10	0.60+	-0.02	0.65*	-0.20	1.06**
	(0.40)	(0.33)	(0.48)	(0.29)	(0.32)	(0.37)
% Free/Red Lunch in grade	0.01	-0.02	0.19	-0.00	-0.12	0.01
	(0.16)	(0.07)	(0.18)	(0.08)	(0.10)	(0.15)
Constant	1.09	0.25	0.62	1.39	2.67	-0.15
	(1.93)	(1.57)	(2.13)	(1.57)	(1.77)	(1.87)
Difference in Post*Treat Coeffs: |z-score|	3.03**		0.62		0.72	
County & Year Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	746	1,253	856	1,194	1,134	884
R-squared	0.98	0.96	0.96	0.95	0.95	0.97

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with nonmissing data on achievement, spending, birth weight, and control variables in states with a school finance court case decision since 2009 (see Figure 1). Post indicates observations after the court decision. Treat indicates court decision favored additional education funding.

High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. High and low poverty indicates counties in the top and bottom terciles of 2008 % child poverty. High and low spending indicates counties in the top and bottom terciles of 2008 spending per pupil.

Robust standard errors adjusted for county-level clustering in parentheses, + p<0.10 * p<0.05 ** p<0.01

Table 3: Coefficients Predicting Achievement by Family Income and Previous Investment

Panel A: Fixed Effects Models

	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
	Low-Income Achievement (std dev units)					
Spending/Pupil 2018$ (log)	-0.01	0.06**	-0.04*	0.05*	0.04+	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Difference in Coeffs: |z-score|	2.56*		3.22**		1.90+	
	High-Income Achievement (std dev units)					
Spending/Pupil 2018$ (log)	0.01	0.04+	-0.01	0.00	0.04*	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Difference in Coeffs: |z-score|	1.26		0.40		1.27	

Panel B: Difference-in-Differences Models

	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
	Low-Income Achievement year *t*+1 (std dev units)					
Post*Treat	-0.00	0.07**	0.02	0.03+	0.03	0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Difference in Coeffs: |z-score|	3.31**		0.43		0.75	
	High-Income Achievement year *t*+1 (std dev units)					
Post*Treat	-0.00	0.03	0.01	0.01	0.01	-0.03+
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Difference in Coeffs: |z-score|	1.27		0.31		1.79+	

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with nonmissing data on achievement, birth weight, control variables, and spending (Panel A) or in states with a school finance court case decision since 2009 (Panel B). Spending in thousands per pupil is adjusted for inflation to 2018 dollars. Post indicates observations after the court decision. Treat indicates court decision favored additional education funding. Low-income students are defined by the state as economically disadvantaged. High-income students are not economically disadvantaged.

High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. High and low poverty indicates counties in the top and bottom terciles of 2008 % child poverty. High and low spending indicates counties in the top and bottom terciles of 2008 spending per pupil.

All models include county and year fixed effects and all time-varying controls included in Tables 1 & 2.

Robust standard errors adjusted for county-level clustering in parentheses, + $p<0.10$ * $p<0.05$ ** $p<0.01$

Table 4: Coefficients Predicting District and State Progressivity of School Spending

Panel A: Fixed Effects Models

	Progressivity of Spending/Pupil (year *t*)			
	(1)	(2)	(3)	(4)
	Districts		States	
Variables	Baseline	Controls	Baseline	Controls
Spending/Pupil 2018$ (log)	0.13	0.05	0.04	0.08
	(0.54)	(0.29)	(0.10)	(0.13)
Observations	327	230	1,220	504
R-squared	0.25	0.75	0.86	0.91
Number of Districts | States	37	35	51	51

Panel B: Difference-in-Differences Models

	Progressivity of Spending/Pupil			
	year *t*		year *t*+1	
	(1)	(2)	(3)	(4)
	States		States	
Variables	Baseline	Controls	Baseline	Controls
Post*Treat	-0.04	-0.03	-0.09	-0.05
	(0.05)	(0.08)	(0.05)	(0.04)
Observations	494	189	475	170
R-squared	0.78	0.89	0.79	0.89
Number of States	19	19	19	19

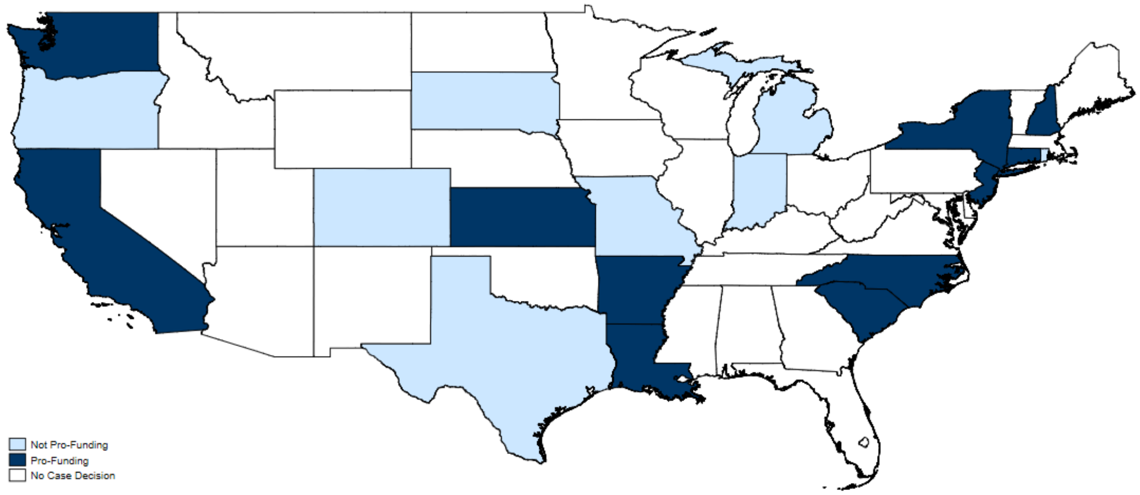
Source: RIDE 2010-2018; State Indicators Database 2009-2018 (Baker et al. 2021); F-33; SEDA 2009-2018. Sample is limited to observations with nonmissing data on spending progressivity, control variables, and spending (Panel A) or in states with a school finance court case decision since 2009 (Panel B). Spending in thousands per pupil is adjusted for inflation to 2018 dollars. Post indicates observations after the court decision. Treat indicates court decision favored additional education funding.

Within-district spending progressivity is the (log) ratio of spending on the average low-income student (eligible for free/reduced-price lunch) to the average non-low-income student in each district in Rhode Island. Models predicting within-district progressivity include district and year fixed effects and the same time-varying controls included in Tables 1 & 2, measured at the district-level.

Between-district spending progressivity is the state-level ratio of predicted spending per pupil in districts with 30% poverty to that in districts with 0% poverty (Baker et al. 2021). Models predicting between-district progressivity include state and year fixed effects and the same time-varying controls included in Tables 1 & 2, measured at the state-level.

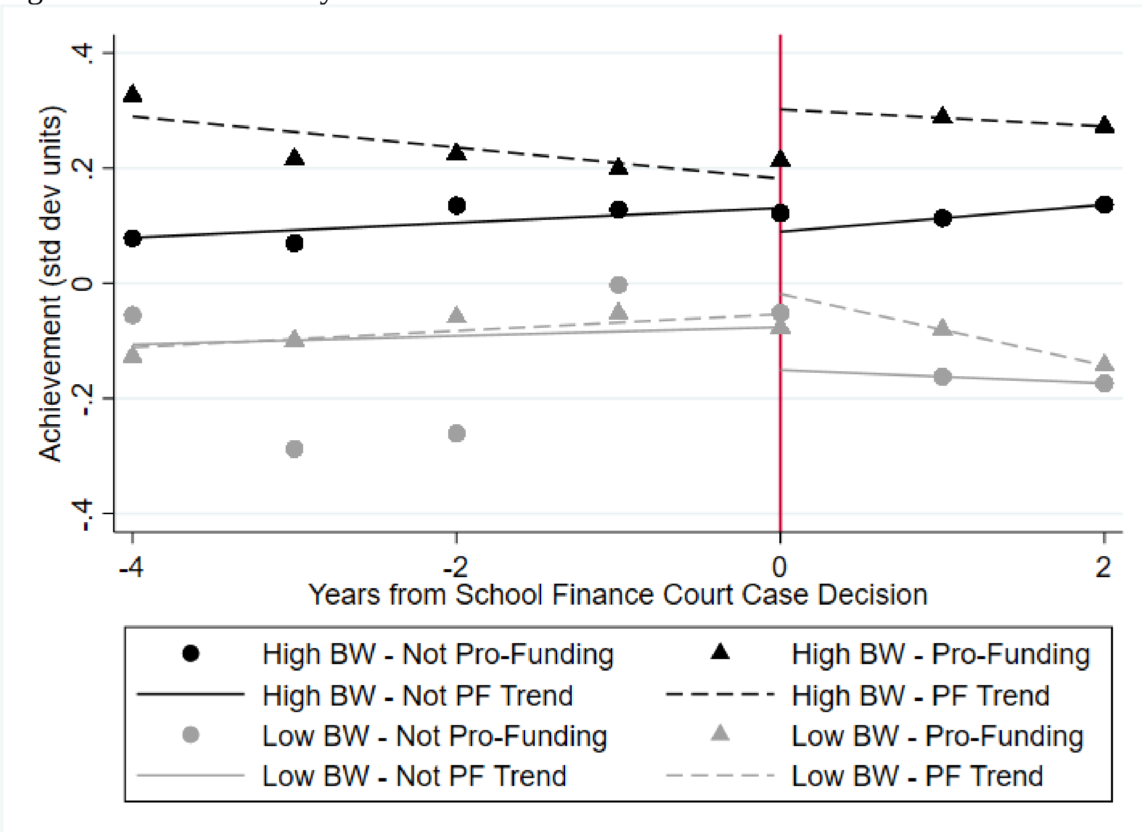
Robust standard errors adjusted for geographic clustering in parentheses, + $p<0.10$ * $p<0.05$ ** $p<0.01$

Figure 1: State Supreme Court School Finance Case Decisions 2009-2018



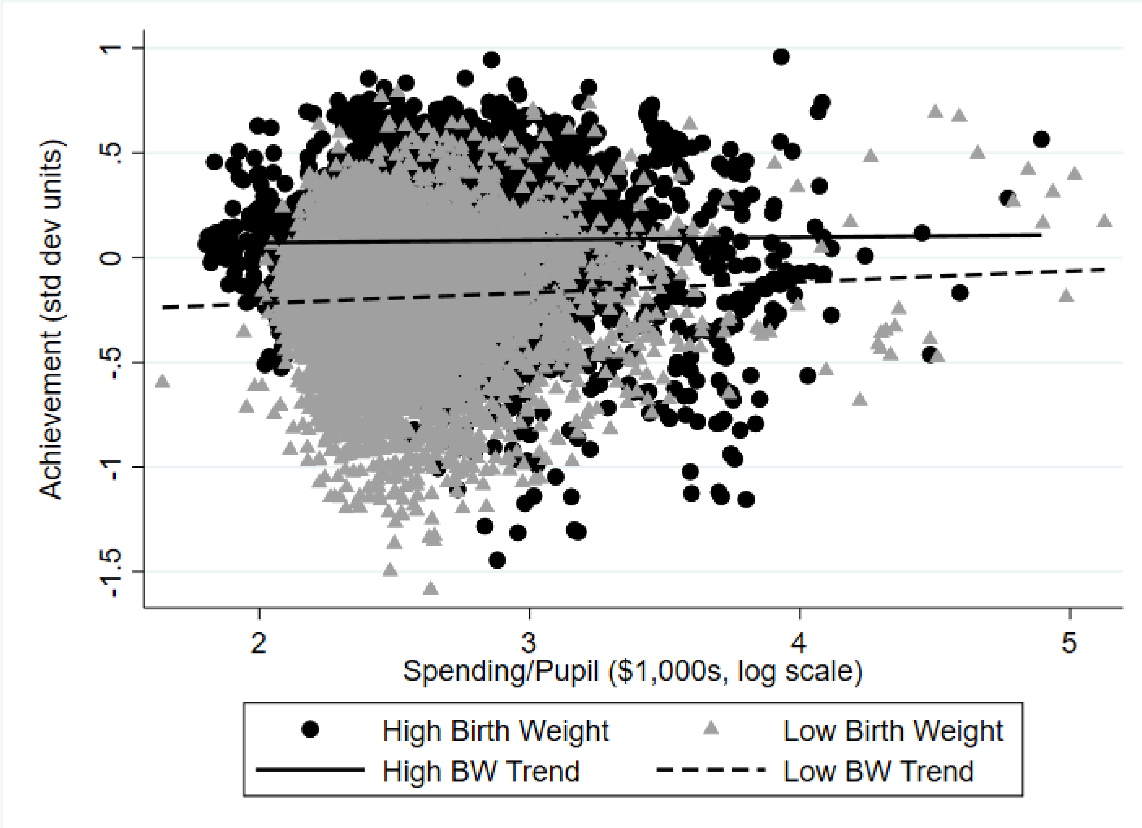
Court cases and decisions gathered from Rebell 2017 supplement to *Courts and Kids*, Baker et al. 2021, Education Law Center (https://edlawcenter.org/states/). The first court case in each state is included, with adequacy or equity cases taking precedence over compliance cases. Pro-funding states are those in which the court decision favored additional education funding. Not pro-funding states are those in which the court decision was not in favor of additional education funding.

Figure 2: Achievement by Time from School Finance Court Case Decision



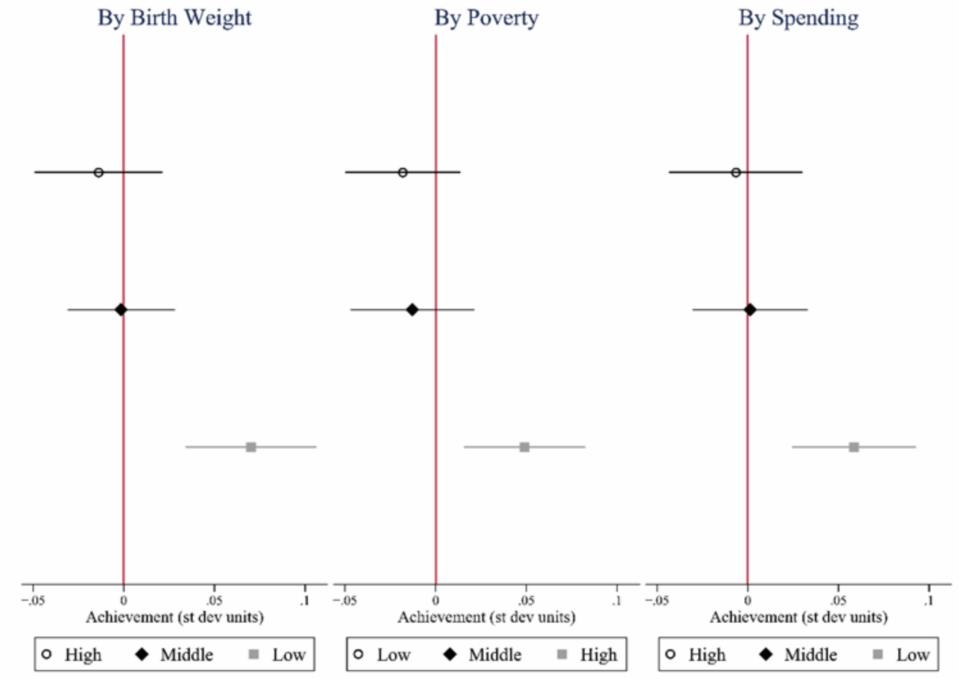
County-year grade 7 observations 2009-2018 in states with a school finance court case decision since 2009 with achievement and birth weight information from SEDA and NVSS. High birth weight (BW) indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. Pro-funding states are those in which the court decision favored additional education funding. Not pro-funding states are those in which the court decision was not in favor of additional education funding.

Figure 3: Achievement by Per Pupil Spending and County Birth Weight Tercile



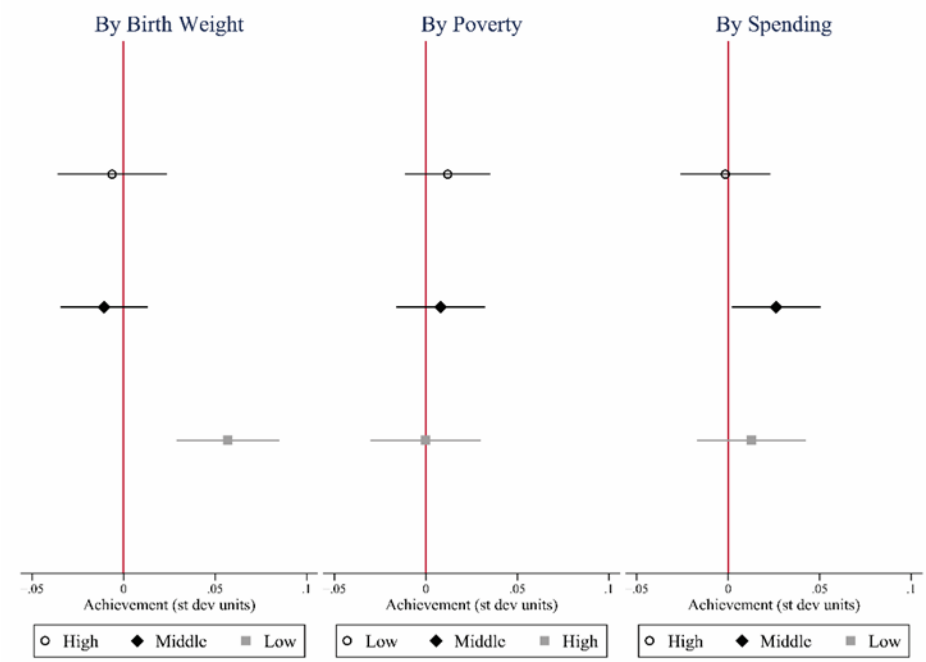
County-year observations 2009-2018 with achievement, spending, and birth weight information from SEDA, F-33, and NVSS. High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. Spending in thousands per pupil is adjusted for inflation to 2018 dollars.

Figure 4: Coefficients Predicting Achievement by Previous Investment
A: Fixed Effects Models



Estimates in Table 1.

B: Difference-in-Differences Models



Estimates in Table 2.

Appendix

Table A1: State Supreme Court School Finance Case Decisions 2009-2018

School Finance Court Case	Pro-Funding Decision	Compliance Case
Bonner v. Daniels (Ind.) 2009		
Comm for Educ Equality v. State of MO 2009		
Pendleton School District v. State of Oregon 2009		
Conn Coalition for Justice in Educ Funding, inc. v. Rell 2010	x	
Davis v. State of SD 2011		
Abbott v. Burke (NJ) 2011	x	x
California Sch Bds Assn v. State 2011	x	
McCleary v. State (Wash.) 2012	x	
Hoke Cnty Bd. of Ed v. State of NC 2012	x	x
Deer/Mt. Judea School District v. Kimbrell (Ark.) 2013	x	x
Lobato v. State of Colorado 2013		
Lousiana Fed of Teachers v. the State of Louisiana 2013	x	
Abbeville Cnty. Sch. Dist. v. the State of South Carolina 2014	x	
Gannon v. State of Kansas 2014	x	
Woonsocket Sch. Comm. V. Chafee (RI) 2014		
S.S. v. the State of Michigan 2014		
City of Dover v. State of New Hampshire 2016	x	
Morath v. The Texas Taxpayer and Student Fairness Coalition et al. 2016		
Maisto v. State of New York 2016		x

Court cases and decisions gathered from Rebell 2017 supplement to *Courts and Kids*, Baker et al. 2021, Education Law Center (https://edlawcenter.org/states/). The first court case in each state is listed, with adequacy or equity cases taking precedence over compliance cases. Pro-funding states are those in which the court decision favored additional education funding. Not pro-funding states are those in which the court decision was not in favor of additional education funding.

Table A2: Descriptive Statistics

	All		Birth Weight		Child Poverty		Spending/Pupil	
Variable	Mean	Std Dev	Low	High	Low	High	Low	High
Math Achievement	-0.04	0.27	-0.19	0.10	0.16	-0.23	-0.08	0.01
Per Pupil Spending (2018$)	13.22	4.16	12.50	14.51	14.31	12.20	10.83	16.37
% Low Birth Weight	7.94	1.79	9.84	6.07	6.81	9.27	8.25	7.63
Enrollment	17999.33	55172.99	16480.81	15954.18	24647.43	8352.51	9640.42	27469.15
Number of Schools	34.82	80.68	31.22	33.21	44.61	20.09	20.84	51.51
% Urban	9.37	24.10	11.02	7.56	9.77	6.26	6.28	10.56
% Suburb	12.28	27.97	10.30	12.36	22.80	2.95	6.66	19.24
% Town	31.00	36.66	27.23	36.08	29.18	33.16	35.07	25.77
% American Indian	1.88	6.56	0.74	2.66	1.13	2.63	2.32	1.93
% Asian	1.46	2.76	1.00	1.87	2.44	0.67	0.83	2.24
% Hispanic	12.04	16.34	10.16	11.86	10.84	12.63	9.71	13.94
% Black	13.78	20.07	29.71	2.43	6.38	24.90	13.10	13.44
% English Learner	3.95	5.78	3.01	4.79	4.00	3.65	3.10	4.58
% Individual Education Plan	14.01	13.73	13.15	15.02	14.51	13.44	13.63	14.85
Income Segregation	0.06	0.08	0.07	0.05	0.07	0.05	0.05	0.08
Black-White Segregation	0.14	0.12	0.16	0.12	0.13	0.14	0.12	0.16
Median Income	46268.04	11836.74	40368.71	51859.33	57364.31	36434.32	42470.55	50536.55
% BA+ Education	20.06	8.90	17.20	23.35	26.06	14.61	17.24	23.35
% Poverty	16.24	5.69	19.75	12.94	11.05	21.71	17.55	14.81
% Unemployed	7.99	2.84	9.33	6.66	6.33	9.66	8.36	7.63
% SNAP Receipt	13.28	5.67	16.78	9.98	8.51	18.39	14.67	11.90
% Single Mother Household	17.27	5.36	21.43	13.46	13.99	20.86	17.47	17.02
% Free/Reduced Lunch in Grade	55.51	16.99	66.44	44.69	39.78	71.02	59.01	51.52
% Child Migration	4.32	1.99	4.53	3.95	4.24	4.12	5.26	3.81
N County-Years	22,864		8,253	6,550	7,886	7,470	7,782	7,300

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with non-missing data on achievement, spending, birth weight, poverty, and control variables. Spending in thousands per pupil is adjusted for inflation to 2018 dollars. Child migration from ACS 2009-2018 is the proportion who moved from another county in the last year (N=4,164).

Table A3: DID Coefficients Predicting Spending
Panel A: Including Outlying Values of Spending/Pupil

	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
	Spending/Pupil year *t*+1 (log)					
Post*Treat	-0.00	0.05**	0.03	0.04*	0.01	0.02
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Difference in Coeffs: |*z*-score|	1.48		0.32		0.18	
	Total Current Spending/Pupil year *t*+1 (log)					
Post*Treat	0.01	0.02*	0.02**	0.01	0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	0.24		1.17		1.18	
	Instructional Spending/Pupil year *t*+1 (log)					
Post*Treat	0.02+	0.03**	0.04**	0.02*	0.02**	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	0.60		1.23		0.88	
	Revenue/Pupil year *t*+1 (log)					
Post*Treat	-0.00	0.05**	0.05**	0.04**	0.02	0.04**
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	2.13*		0.49		0.78	

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with nonmissing data on achievement, birth weight, control variables, spending, and in states with a school finance court case decision since 2009. Spending in thousands per pupil is adjusted for inflation to 2018 dollars. Post indicates observations after the court decision. Treat indicates court decision favored additional education funding.

High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. High and low poverty indicates counties in the top and bottom terciles of 2008 % child poverty. High and low spending indicates counties in the top and bottom terciles of 2008 spending per pupil.

All models include county and year fixed effects and all time-varying controls included in Tables 1 & 2.
Robust standard errors adjusted for county-level clustering in parentheses, + p<0.10 * p<0.05 ** p<0.01

Panel B: Excluding Outlying Values of Spending/Pupil

	(1)	(2)	(3)	(4)	(5)	(6)
	By Birth Weight		By Poverty		By Spending	
Variables	High	Low	Low	High	Low	High
	Spending/Pupil year *t*+1 (log)					
Post*Treat	0.01	0.05**	0.03	0.03*	0.01	0.02
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Difference in Coeffs: |*z*-score|	1.20		0.09		0.24	
	Total Current Spending/Pupil year *t*+1 (log)					
Post*Treat	0.01	0.02*	0.02**	0.01	0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	0.27		1.18		1.11	
	Instructional Spending/Pupil year *t*+1 (log)					
Post*Treat	0.02+	0.03**	0.04**	0.02*	0.02**	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	0.58		1.27		1.02	
	Revenue/Pupil year *t*+1 (log)					
Post*Treat	-0.00	0.05**	0.05**	0.04**	0.02	0.03**
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in Coeffs: |*z*-score|	1.90+		0.69		0.74	

Source: 2009-2018 SEDA; F-33; NVSS; SAIPE. Sample is limited to county-year observations with nonmissing data on achievement, birth weight, control variables, spending, and in states with a school finance court case decision since 2009. Sample in Panel B further excludes outlying observations of spending/pupil – more than 3 times the interquartile range above the 75th percentile. Spending in thousands per pupil is adjusted for inflation to 2018 dollars. Post indicates observations after the court decision. Treat indicates court decision favored additional education funding.

High birth weight indicates counties in the bottom tercile of % low birth weight. Low birth weight indicates counties in the top tercile of % low birth weight. High and low poverty indicates counties in the top and bottom terciles of 2008 % child poverty. High and low spending indicates counties in the top and bottom terciles of 2008 spending per pupil.

All models include county and year fixed effects and all time-varying controls included in Tables 1 & 2.

Robust standard errors adjusted for county-level clustering in parentheses, + p<0.10 * p<0.05 ** p<0.01