Do School Spending Cuts Matter? Evidence from the Great Recession

C. Kirabo Jackson
Cora Wigger, Northwestern University
Heyu Xiong, Northwestern University

Available at: https://works.bepress.com/c_kirabo_jackson/35/
Do School Spending Cuts Matter? Evidence from The Great Recession*

C. Kirabo Jackson  
Northwestern University and NBER  

Cora Wigger  
Northwestern University  

Heyu Xiong  
Northwestern University  

November 15, 2018

Abstract

During the Great Recession, national public-school per-pupil spending fell by roughly seven percent. While increased public-school spending has been linked to improved student outcomes, the impact of large education funding cuts is not well understood. To examine this, first, we document that the drop in spending after the recession coincided with the end of decades-long national test score growth. Next, we show that this stalled educational progress was particularly pronounced in states that experienced larger recessionary budget cuts for plausibly exogenous reasons. To isolate budget cuts that were unrelated to (a) other ill-effects of the recession or (b) endogenous state policies, we use states’ historical reliance on State taxes to fund public schools interacted with the timing of the recession as instruments for reductions in school spending. Cohorts exposed to these spending cuts had lower test scores and lower high-school completion rates. While the test score impacts were similar for poor and non-poor children, the high school completion impacts were largest for Hispanic students.

*Jackson: Northwestern University, 2120 Campus Drive, Evanston IL 60208 (email:kirabo-jackson@northwestern.edu). Wigger: Northwestern University, 2120 Campus Drive, Evanston IL 60208 (email:CoraWigger2021@u.northwestern.edu). Xiong: Northwestern University, 2211 Campus Dr, (email:HeyuXiong2018@u.northwestern.edu). The statements made and views expressed are solely the responsibility of the authors. Wigger and Xiong are grateful for financial support from the US Department of Education, Institute of Education Sciences through its Multidisciplinary Program in Education Sciences (Grant Award # R305B140042).
I Introduction

Public primary and secondary education is one of the largest single components of government spending (OECD, 2016). Accordingly, cuts to education spending are often proposed as a way to reduce government spending and balance government budgets. However, the extent to which such education budget cuts may harm students is not well known. While several papers now use credible research designs to establish a causal link between school spending and student outcomes (Jackson et al. 2016; Candelaria and Shores 2017; Lafortune et al. 2018; Hyman 2017; Cellini et al. 2010; Neilson and Zimmerman 2014), these studies rely on variation that increases school spending. As a result, there is little direct causal evidence on the extent to which school spending cuts impact students. To speak to this issue, we exploit plausibly exogenous reductions in public school spending induced by the Great Recession and examine the effect of school spending cuts on both student test scores and high school graduation rates. To our knowledge, this is the first study to examine the causal impact of large public school budget cuts on student outcomes – speaking directly to the policy question of whether, and to what extent, school spending cuts matter.

While one might expect the estimated impacts on spending increases to be opposite in sign and similar in magnitude (i.e. symmetric) to spending cuts, there are several plausible reasons why this may not be the case. First, because funds may be earmarked for particular uses and districts have contractual obligations, certain kinds of spending may increase when budgets increase but not fall when budgets fall. For example, many states must fund pension obligations (which have no direct impact on students) before they can spend money on core school operations (Zeehandelaar et al. 2013; Dabrowski and Klinger; Calefati 2018). As a result, when budgets increase, districts may increase all kinds of spending. However, when budgets shrink, states must fulfill their pension obligations so that revenue cuts may lead to disproportionately large cuts in other more productive spending categories (such as teacher salaries). In such a scenario, the impacts of a one dollar spending cut could be opposite in sign but larger in magnitude than that of a one dollar spending increase. Alternatively, school districts may be able to “make do with less” (Lazear et al. 2016) in the face of budget cuts. For example, districts may be able to delay less essential spending (such as upgrading the roof or replacing existing textbooks with newer editions) to avoid making cuts to core operational spending. In such a scenario, the impacts of a one dollar spending cut could be much smaller in magnitude than that of a one dollar spending increase, and could even be zero. The extent to which spending cuts matter at all, or may have similar effects as that of spending increases, is thus an open empirical question.

1 There is empirical support for this idea; in the wake of recessionary budget cuts, many districts induced expensive experienced teachers to retire early (Fitzpatrick and Lovenheim 2014), others deferred scheduled maintenance and eliminated non-essential travel (Ellerson 2010), and some districts operated schools for only four days per week (Anderson and Walker 2015). Remarkably, Fitzpatrick and Lovenheim (2014) and Anderson and Walker (2015) find that these cost-saving measures may improve student outcomes.
To provide such empirical evidence, we exploit school budget reductions that occurred in the wake of The Great Recession to examine the extent to which large and unanticipated reductions in public school spending impact student outcomes. The Great Recession began in December of 2007, and was the most severe economic downturn in the United States since the Great Depression. As a result of the decline in economic output and the resulting fall in tax revenues, national public school per-pupil spending fell by roughly seven percent.

As a first pass analysis, we document that, nationally, the onset of the recession is associated with the first time school spending declined in the past 50 years and also the first time that average national test scores declined. The stalled progress in the National Assessment of Educational Progress (NAEP) after 2009 has been documented by education scholars (e.g. West 2018, Loeb 2018) and has been dubbed the "Lost Decade" in educational progress (Petrilli, 2018). A “naive” estimate based on the coincident national time trends, is that a $1,000 reduction in per-pupil spending (about an 8.3 percent increase) was associated with a statistically significant 8.7 percent of a standard deviation reduction in test scores. Any coincident national trends could lead to bias so that one must interpret coincident national trends with caution (Jackson et al., 2015). If one accounts for the fact that both spending and scores may have been increasing over time for potentially unrelated reasons, and only uses the change in the time trend in both spending and test scores after the recession, a $1,000 reduction in per-pupil spending was associated with a statistically significant 5.2 percent of a standard deviation reduction in test scores.\(^2\) Because many other things may have changed nationally after the recession that could drive this association, this relationship may not be causal.\(^3\) To address these concerns and to isolate the impact of school spending cuts from the broader impacts of the recession itself, we propose an instrumental variables approach that uses plausibly exogenous state-level variation in public K12 school spending.

While overall school spending declined after recession onset, revenues from state taxes (i.e. income taxes and sales taxes) fell the most sharply (Figure 2). As a result, states that were more reliant on state sources of revenue to fund public education (due to the particulars of their school funding formulas) tended to experience larger school-spending reductions during the recession. We document that, conditional on powerful ex ante predictors or recession intensity, a state’s reliance on state revenues to fund public education is unrelated to the impact of the recession on the economy of that state. As such, among states with the same recessionary impact on the economy on average, due to differences in their school funding formulas some experienced larger recession-induced

---

\(^2\)For these national estimates we restrict analysis to the years 2002 through 2015 to be consistent with the more comprehensive NAEP analysis conducted in the paper.

\(^3\)In a related study to ours, Shores and Steinberg (2017) find that school districts in locations that were hardest hit by the recession (based on employment losses) had larger test score reductions when these districts hired fewer teachers and spent less on schools. However, they do not isolate the impact of school spending from that of other impacts of the recession.
school spending reductions than others (Evans et al. 2017). Exploiting this fact, we instrument for per-pupil spending with the share of a state’s public school revenues that came from state sources before the recession interacted with the timing of the recession.

Our strategy requires that states with different reliance on state revenues were not differentially affected by the recession for reasons other than through school spending. We show this to be true in a few ways. First we show that our instrument predicts overall school spending through its predictable impact on state revenues (as opposed to local or federal revenues). Next, we show that, in most specifications, school spending (as predicted by our instruments) is unrelated to measures of economic conditions such as employment, unemployment rates, or child poverty rates. Furthermore, our results are robust to the inclusion of strong predictors for economic conditions and housing prices. Finally, we show that our main results persist in models that control for contemporaneous local economic conditions and even house prices directly.

Our main outcome of interest is student test scores from the NAEP. We also examine high school completion rates recorded in Census Data. We link these outcome data to district-level spending data from the Common Core of Data (CCD) and our instruments for public school K12 spending. Our final dataset straddles the recession and includes data between years 2002 and 2015.

For our analysis, we put states into three groups based on reliance on state revenues to fund public K12 schools in 2008. We find a robust monotonic relationship between the reliance on state funds and the annual decline in per-pupil spending after the great recession onset. Relative to each state’s linear time trend, states most reliant on state funds had annual per-pupil declines (post recession) of 3.89 percent per year ($p$-value $< 0.01$), compared to 1.58 percent for the middle group of states ($p$-value $< 0.01$), and a 1.8 percent increase for states that were least reliant on state funds for public schools ($p$-value $> 0.05$). These patterns are mirrored for test scores. Relative to each state’s linear time trend, states that were most reliant on state funds had annual test score declines (post recession) of 1.59 percent of a standard deviation per year ($p$-value $< 0.01$), compared to 1.16 percent for the middle group of states ($p$-value $< 0.01$), and a 1.92 percent increase for states that were least reliant on state funds for public K12 schools ($p$-value $> 0.1$). Using these patterns in an instrumental variables approach, on average, a 10 percent reduction in per-pupil spending led to about 0.062 standard deviations lower test scores ($p$-value $< 0.05$). These effects are robust across specifications and are unchanged as one controls for the prevailing economic conditions, suggesting that (1) our effects on test scores are real and (2) are driven by the changes in public school spending alone. We further test the validity of our results by showing that recessionary spending cuts are only associated with reductions in public school test scores, but not private school scores.

---

4We create a Bartik predictor (Bartik, 1991) for state unemployment (based on the national unemployment rate in various industries) times the industry mix in a particular state. As documented in several studies, this Bartik predictor is strongly predictive of the changes in states’ unemployment rates.
Looking beyond test score impacts, we also examine effects on high school completion. We compare the outcomes of high school graduation class cohorts that were exposed to the recession prior to expected high school graduation with those who were not (i.e. those who graduated before the recession). Using a difference-in-difference approach, we examine the extent to which exposure to the recession had larger impacts in those states that experienced the largest exogenous spending cuts. We find that a 10 percent reduction in per-pupil spending across all four of an individual’s high-school-age years reduced the likelihood of completing high school by about 1.64 percentage points. However, the impacts were particularly pronounced for Hispanic nonwhite-nonblack students and for non-white-non-black-non-Hispanic students for whom the reduction was 3 percentage points. While the overall graduation estimates are somewhat sensitive to specification, the reduced graduation impacts for Hispanic nonwhite and non-black students and nonwhite-non-black-non-Hispanic students are robust. These longer run impacts underscore the importance of examining spending effects on outcomes beyond test scores. We also examine impacts by race and find that the graduation rate effects were most pronounced and robust for Hispanic students.

To explore mechanisms, we examine what kinds of spending were most affected. Districts responded to spending cuts by disproportionately cutting more from construction expenditures and disproportionately less from core K12 spending. While construction spending makes up 6.8 percent of the average school’s budget, it accounted for almost a quarter of the reduced spending. Conversely, while current K12 operating spending accounts for 86 percent of expenditures it accounted for only 62 percent of the cuts. These patterns differ from those documented for spending increases due to school finance reforms (Jackson et al., 2016), suggesting that the marginal propensity to spend on different inputs varies when there are spending increases versus decreases. This is consistent with school districts being more constrained in their ability (or desire) to cut spending on core school operations. These asymmetric spending patterns could have lead to asymmetric spending impacts if the marginal benefit of construction spending differed markedly from that of other kinds of spending. However, as documented in Jackson (2018a) construction spending does, at times, affect student outcomes so that this reallocation of budgets may not have changed the marginal impact of school spending (relative to that for a spending increase). Indeed, our estimates are similar to those based on spending increases (e.g. Lafortune et al. 2018; Jackson et al. 2016; Candelaria and Shores 2017) suggesting that school spending effects may be largely symmetric.

Using school finance reforms, both Jackson et al. (2016) and Lafortune et al. (2018) find that increased school spending disproportionately improves the outcomes of low income children. However, using exogenous variation generated by Michigan’s school finance formulas, Hyman (2017)
finds that school spending effects were concentrated among districts that served lower poverty popu-
lations. Accordingly, whether school spending cuts are particularly deleterious for poor children is unknown. By comparing the NAEP scores of children who are and are not eligible to receive reduced-price lunch within each state, we find that the spending cuts reduced test scores for all children similarly. However, the reduced graduation rate impacts appear to have been most severe for Hispanic students, indicating that effects were not necessarily consistent across all populations.

Our findings contribute to long-standing debates around whether school spending matters and whether schools make do with less by showing that spending cuts harm students. Our analysis contributes to the field of public finance by shedding light on how school districts may react asymmetrically to increases versus decreases in budgets. Finally, our results deepen our understanding of the long-run effects of growing up during a recession. While it is well-documented that growing up during a recession can lead to long lasting ill-effects through channels such as parental job displacement (Oreopoulos et al. 2008; Ananat et al. 2011; Stevens and Schaller 2011) and increased food insecurity (Gundersen et al. 2011; Schanzenbach and Lauren 2017), we provide the first evidence that recessions have lasting ill-effects on young individuals through their effects on the governments’ abilities to provide public education services.

The remainder of the paper is as follows. Section II describes the data. Section IV describes the empirical strategy. Section V presents the results. Section VI concludes.

II Data

We link several data sources for our analysis. School finance data come from the Annual Survey of School System Finances at the U.S. Census Bureau. The surveys contain financial data for all public school districts in the United States (approximately 13,500). The financial surveys are available from 1987 through 2015. They provide education revenue broken down by source (local, state, and federal), and break down expenditures into broad categories. The share of school spending from each source varies substantially by state. Between 2002 and 2015, the average share of state school revenue from each federal, state, and local sources were 9.8%, 48.5%, and 41.8%, respectively. While the share of federal revenue varies only between 4% (Connecticut and New Jersey) and 22% (Mississippi), variation in local and state revenue sources is much broader. The share of state funding that comes from state sources varies between 0% (Washington, D.C.) and 90% (Hawaii) and the share coming from local sources ranges from 2% (Hawaii) to 93% (D.C.). On average, 85% of state public school spending goes to current elementary and secondary spending, which broadly includes expenses for instruction and support services. Ten percent of state expenditures go towards capital, which includes construction, land, and equipment. Salaries and benefits (instructional and non-instructional) make up 68% of state public spending on average.

---

6We provide more detail on our data sources in the Appendix.
Test score data come from the National Assessment of Educational Progress (NAEP). The NAEP is referred to as the Nation’s Report Card as it tests students across the country on the same assessments and has remained relatively stable over time. The NAEP is administered every other year to a population-weighted sample of schools and students. We use restricted-use data files with student-level scores and demographics. We focus on public school students’ 4th and 8th grade Math and Reading assessment scores. To facilitate comparisons over time, we report NAEP scores standardized to a base year of 2003. The NAEP sample has been increasing over time and only stabilized after 2000 (Table C1). We focus on the period between 2002 and 2015. The full dataset includes 4.3 million individual NAEP scores from 11,477 known school districts.

We obtain estimates on the total population, child population, and child population living in poverty for the geographic areas associated with school districts from the United States Census Bureau Small Area Income and Poverty Estimates (SAIPE). We also use area economic indicators of employment and wages from the Bureau of Labor Statistics (BLS). We also include an annual measure of home values in each district from Zillow. To form district-level economic indicators for unemployment, employment, and housing values we create weighted county-level estimates by the overlapping population within the county and corresponding school district. We also include public school district staffing and student enrollment information from the Common Core of Data LEA Universe surveys from the National Center for Education Statistics (NCES).

While achievement data are measured at the individual student-level and demographic and economic indicators are measured at the school district area-level, we rely on state-level variation and therefore collapse our data to create a state-by-year panel for all states and every NAEP-testing year between 2002 and 2015 (summarized in Table 1).  

Our high-school completion data are from the American Community Surveys (ACS) from 2000 to 2016. We pool the individual level ACS data across all years. With the individual level data we compute the high-school completion rate for individuals at each age (between 16 and 18), in each state, in each survey year. The resulting dataset is at the age-state-year level, and summarizes the average high school completion rate in each cell. These data are summarized in Appendix Table D1. Between 1990 and 2016, the high school completion rate among 16 years olds is about 1.5 percent. Among 17 years olds this was about 12 percent and among 18 year olds, this was about 60 percent. Murnane and Hoffman (2013) document that high school graduation rates have been increasing since the 1990s. This is most clearly seen in the high school completion rates of 18 years olds in our data. While 54 percent of 18 year olds in the 1990 Census completed high school, this number was about 64 percent by 2010 and more than 71 percent by 2016. It is against this...
backdrop of secularly rising high school completion that we will examine the impacts of reduced public school spending.

### III Suggestive Evidence From National Trends

The Great Recession began in December of 2007, and was the most severe economic downturn in the United States since the Great Depression. During the following 18 months, over eight million private sector jobs were lost, and the unemployment rate increased by over 5 percentage points (Evans et al., 2017). The reduction in state revenues due to lower sales and income taxes led to cuts in state funding for services such as education (Business Cycle Dating Committee 2010; Chakrabarti et al. 2015; Leachman and Mai 2014). Indeed, on average per-pupil spending fell by about 7 percent between 2008-2009 and 2012-2013 – the most pronounced and sustained decline in school spending levels in the past 30 years.

If there were a causal impact of school spending cuts, one might expect test scores to also have fallen during this time. To establish this, and motivate our state-level analysis below, we first present national trends in per-pupil spending and average NAEP scores. Figure 1 presents the National average math and reading scores on the National Assessment of Educational Progress (NAEP) between 2000 and 2017 against national per-pupil spending (in CPI adjusted 2015 dollars). The NAEP tests state-representative samples of students in math and reading in 4th and 8th grade every two years. To account for the fact that learning is a cumulative process, we use the four-year moving average of per-pupil spending. Figure 1 shows clear co-movement between school spending and NAEP test scores. School spending, math scores and reading scores were all generally increasing until around 2010. At some point around 2010 all three started to decline, and all three hit a low point around 2015. Remarkably, all three variables changed course after the end of the recession and all three increased between 2015 and 2017.

Per-pupil spending had been increasing by about 230 dollars per years annually prior to the recession. Relative to this trend, annual spending declined by 365 dollars per year after the recession. Similarly, average NAEP scores had been increasing annually by about 2.2 percent of a standard deviation before the recession. Relative to this trend, test scores declined by 1.78 percent of a standard deviation per year after the recession. Relating the change in national trends in spending to the change in national trends in test scores implies that reducing per-pupil spending by 1000 dollars is associated with a 4.8 percent of a standard deviation reduction in test scores. In percentage terms, the coincident change in trends after the recession implies that reducing per-pupil spending by ten percent is associated with a 6 percent of a standard deviation reduction in test scores.

The coincident timing of the decline in per pupil spending and the decline in NAEP scores is

---

9Shores and Steinberg (2017) rely on observational variation during the recession. This work is important, but does not estimate the causal impact of spending changes per se.
highly suggestive. However, because many other things may have changed nationally around the timing of the recession that could drive this association, this relationship may not be causal.\textsuperscript{10} To address this concern and to isolate the impact of school spending cuts from the broader impacts of the recession itself, we propose an instrumental variables approach that uses plausibly exogenous state-level variation in public K12 school spending. Remarkably, the resulting estimates are very similar to those presented above.

IV Empirical Strategy

To isolate the impact of school spending cuts from the broader impacts of the recession, we rely on plausibly exogenous within state-level variation in public K12 school spending. We describe our strategy below. While overall school spending declined after recession onset, revenues from state taxes fell most sharply (Figure 2). Because the Great Recession was caused by a housing crisis, one might have expected that local revenues would have been the most responsive, but this was not the case. As shown by Lutz et al., 2010, local tax revenues were relatively resilient to the recession due to “significant lags between market values and assessed values of housing and the tendency of policy makers to offset declines in the tax base with higher tax rates.” As such, it was those States that were more reliant on state sources of revenue to fund public education in 2008 (due to the particulars of their school funding formulas) that tended to experience larger school spending reductions during the recession. To show this, Figure 3 plots the state-level percent change in per-pupil spending between 2007 and 2011 (pre- and post-recession) against the share of K12 spending in the state that came from state sources in 2008. We define the parameter $\Omega_s$ as the share of state K12 revenues in state $s$ that came from state sources in 2008.\textsuperscript{11} It is important to note that we classify states based on the source of the revenue and not based on how the funds are allocated. As such, even though in a state like California the state allocates most property tax revenues, the revenues come from local sources (as opposed to state income and sales taxes) and are coded as such.\textsuperscript{12} As one might expect, Figure 3 shows a clear tendency for states that were more reliant on state sources prior to the recession to have larger reductions in K12 spending during the recession.

This pattern motivates our instrumental variables approach. We use $\Omega_s$ as an exogenous shifter

\textsuperscript{10}Shores and Steinberg (2017) find that school districts in locations that were hardest hit by the recession (based on employment losses) had larger test score reductions when these districts hired fewer teachers and spent less on schools. However, they do not isolate the impact of school spending from that of other impacts of the recession.

\textsuperscript{11}Following Evans et al. (2017), we compute the share of state K12 revenues in state $s$ that came from state sources in 2008 (determined pre-recession) across all districts in the state as follows:

$$\Omega_s = \frac{\sum_{d \in s} StateRevenue_d}{\sum_{d \in s} TotalRevenue_d}$$

$StateRevenue_d$ denotes the school revenue in district $d$ which came from state sources in the 2007-2008 school year; and $TotalRevenue_d$ is the total revenue collected in district $d$ in the same year.

\textsuperscript{12}Figure D1 shows that $\Omega_s$ is evenly distributed across the geographic regions of the nation.
of K12 spending within states during the recession. For our approach to uncover a school spending effect, \( \Omega_s \) should not be correlated with changes in other policies or economic conditions within states. We argue that a state’s reliance on state revenues to fund education prior to the recession is unrelated to the impact of the recession on other dimensions in that state. To assess this, the right panel of Figure 3 plots changes in the state unemployment rate between 2007 and 2011 by \( \Omega_s \). While there was a general increase in unemployment in the average state, \( \Omega_s \) was unrelated to the impact of the recession in that state. We present more formal tests below.

**IV.1 Estimation Equation**

Our empirical approach is to compare the change in outcomes before and after the recession across states that were more or less reliant on state revenues (and therefore experienced larger or smaller reductions in school spending). To rely only on within-state variation, we allow each state to have its own intercept and linear time trend in both spending and in the outcomes.

In our first stage regressions, we show that relative to each state’s own pre-recession trend in school spending states that were more reliant on state revenues to fund public education (in 2008), had a more negative post-recession time trend in school spending. If school spending affects outcomes, then in a reduced-form model the change in the trend in school spending should correspond with a change in the trend in test scores and high school completion. We show that this is the case. Using this variation, our instrumental variables model compares the change in the trend in student achievement before and after the recession across states with a high or low fraction of revenue from state sources. If the only reason for a change in the trend in outcomes across areas with high and low \( \Omega_s \) is the differential effect of the recession on public K12 spending across these states, our instrument is valid. We present many empirical tests revealing that this condition is likely satisfied.

To capture the variation in \( \Omega_s \), we classify states as low, medium, or high reliance on state taxes to fund public k12 schools. Schools that have less than one-third of their revenues from state sources are in the low group \((g = 1)\), those with between one- and two-thirds are in the middle group \((g = 2)\), and those that have more than two-thirds of their revenues from state sources are in the high group \((g = 3)\). The group indicator variable \( I_{gs} \) connotes the group \( g \) of state \( s \). Using the state-by-year level panel, we estimate systems of equations of the following form by 2SLS.

\[
PPE_{st} = \sum_{g=1}^{3} \left[ \pi_{1g} \cdot (I_{gs} \times I_{post} \times T) \right] + \sum_{g=2}^{3} \left[ \pi_{2g} \cdot (I_{gs} \times I_{post}) \right] + \sigma_1 \cdot I_{post} + \delta_1 C_{st} + \alpha_{1s} + (\tau_{1s} \times T) + \epsilon_{1st} \tag{1}
\]

\[
Y_{st} = \beta \cdot (PPE_{st}) + \sigma_2 \cdot I_{post} + \delta_2 C_{st} + \alpha_{2s} + (\tau_{2s} \times T) + \epsilon_{2st} \tag{2}
\]

The endogenous treatment, \( PPE_{st} \), is the log of the average per-pupil school spending in state \( s \).
during the four years between year $t$ and year $t-3$. The outcome $Y_{st}$ is the average test scores for students in state $s$ in year $t$. To account for differences across states we include state fixed effects $\alpha_{1s}$ and $\alpha_{2s}$ in the first and second stage, respectively. The variables $\varepsilon_{2st}$ and $\varepsilon_{2st}$ are random error terms. To compare changes in each state’s outcome to its own time trend, we include the state-specific linear time trends $\tau_{1s}$ and $\tau_{2s}$ in the first and second stages, respectively. Note that this accounts for any pre-recession time-trend differences between high and low $\Omega_s$ states. To account for average differences in outcomes pre- and post recession, we include a post recession indicator $I_{post}$ in both the first and second stage regression. $T$ is a scalar in the calendar year, and $I_{post}$ is a post-recession indicator denoting all years after 2008. The excluded instruments are the interactions between the group indicators $I_{gs}$ and the post recession timing variables $I_{post}$ and $T \times I_{post}$.

Because the recession may have had had ill economic effects through channels other than school spending, even though our instrument does not predict the employment rate, it is important that we control for underlying predictors of recession intensity itself. As such, following (Yagan, 2017) and others, a key conditioning variable in $C_{st}$ is a Bartik predictor of the state unemployment rate. To create this key control, we compute the proportion of all workers in each industry in each state in 2007. We multiply these 2007 industry proportions by the national unemployment rate in that industry for each year. For each state, we sum these products across all industries in each year. In our most restrictive models, we also include individual calendar-year fixed effects en lieu of the simple post-recession indicator. These models yield much weaker first stages and are presented to show the robustness of our central result.

To provide a visual representation of our first stage and reduced form, we present an event-study for the recession’s effect on K12 spending and average NAEP scores by states’ reliance on state revenue sources. To do this, we estimate the model below by OLS on our state-level panel.

$$
Y_{st} = \sum_{t=2002}^{2015} \beta_t \cdot (I_{g>1,s} \times I_{T=t}) + \gamma_s + \gamma_t + \upsilon_{st}
$$

$I_{T=t}$ is an indicator denoting if the observation is for calendar year $t$ and $I_{g=1,s}$ denotes the states that are more reliant on state revenues for public schools (that is, states that relied on state revenues for more than one third of education spending in 2008). Accordingly, the coefficients $\beta_t$ map out the differences in outcomes between states with low and high $\Omega_s$ in each year. We estimate this model whether the main outcome is the log of school spending and also state level average NAEP scores. We plot these coefficients in the black series of Figure 4 where the reference year is 2002.

Prior to the recession, school spending was on a similar trajectory in areas with different levels of $\Omega_s$, but after the recession, districts in states with heavy reliance on state revenues experienced a clear decline in per-pupil spending. As one can see, average NAEP scores followed a very similar pattern. Student test scores in states with greater dependence on state revenues to fund public K12
schools declined following the recession, relative to other areas. Reassuringly, patterns indicate that outcomes in states that relied on revenues raised from primarily state sources were on a similar trajectory as other districts until the onset of the recession. However, in states with greater reliance on state revenues for public school funding (and therefore saw greater declines in per-pupil school spending), student performance dropped following 2008, the start of the recession, and continued to decline thereafter. While Figure 4 is helpful for presenting the variation used, and providing visual evidence that our estimated relationship may be causal, we now turn to the formal first stage and reduced form regression results below.

**IV.2 First Stage and Reduced Form**

Table 2 presents the first-stage relationship between the excluded instruments and per-pupil spending on our state-year panel. Column 1 presents the coefficients on the excluded instruments when the dependent variable is the level of spending in thousands for the most parsimonious model (no additional controls). We focus our attention on the change in the linear trend after the recession across the three state groups. In column 1, the coefficient on LOW × POST × YEAR is positive and non-significant, that for MID × POST × YEAR is negative and significant, and that for HIGH × POST × YEAR is even more negative and significant. The point estimates indicate that the linear trend in school spending was relatively unchanged in states with low reliance on state revenues, school spending declined annually by about $192 per year in states that had moderate reliance on state revenues, and declined by about $435 per year for the states that were most reliant on state revenues to fund public schools. The Cragg-Donald Wald F-statistic for the five excluded instruments presented is 17.9. Column 3 presents the same specification, where the dependent variable is the natural log of the four year moving average of per-pupil spending. The basic patterns in the same. The point estimates indicate that the linear trend in school spending was relatively unchanged in states with low reliance on state revenues, school spending declined annually by about 2.3 percent per year in states that had moderate reliance on state revenues, and declined by about 4.7 percent per year for the states that were most reliant on state revenues to fund public schools. The excluded instruments are better predictors of changes in log spending than in levels such that the Cragg-Donald Wald F-statistic for the five excluded instruments presented is 19.27. Given the somewhat increased precision, we will focus much of our discussion in the log specification.

Echoing the patterns presented in Figure 4, we present the reduced form estimates for average NAEP scores in column 5. The basic pattern of the change in trends in spending are mirrored for NAEP scores. The point estimates indicate that the linear trend in NAEP scores was relatively unchanged in states with low reliance on state revenues, scores declined annually by about 1.16 percent of a standard deviation per year in states that had moderate reliance on state revenues, and declined by about 1.59 percent of a standard deviation per year for the states that were most reliant
on state revenues to fund public schools. The fact that there is a monotonic relationship between
the change in trend and a state’s reliance on state revenues for both school spending and test scores
provides compelling evidence of a systematic association. So long as this association operates only
through school spending, it would be compelling evidence of a causal relationship between school
spending cuts and reduced student achievement.

To show the general robustness of the patterns we also present the first stage and reduced form
for models that include individual year fixed effects (columns 2, 4 and 6). The results reveal that
both the first stage and reduced form patterns are relatively robust across specifications. However,
one notable pattern is that the Kleinbergen-Paap first-stage F and underidentification statistics are
much smaller in models with year fixed effects than in our main model. As such, while we will
show that all of our results are robust to the inclusion of individual year fixed effects, our preferred
model is the more parsimonious one with only a post recession indicator.

**IV.3 Tests of Exogeneity**

Our identification strategy relies on the assumption that the only reason for the systematic as-
sociation between school spending and test scores is a school spending effect driven by changes in
state revenues. It is therefore important to show that our instrument affects school spending through
our proposed mechanism alone. To assess this we estimate our first stage model on revenues col-
lected from different sources. We report the coefficients in Table 3. Columns 1 and 2 present the
reduced form impacts on federal revenues, columns 3 and 4 present the reduced form impacts on
state revenues, and columns 5 and 6 present the reduced form impacts on local revenues. The basic
pattern is similar in models with and without year fixed effects.

In the parsimonious model with a post-recession indicator, the instruments predict that the
difference in federal revenue between the most and middle reliance groups four years after the
recession would be about $150, and that between the most and least reliant groups four years after
the recession would be about $260. These estimates are small and neither of these is significant
at the 5 percent level – compelling evidence that the proposed instruments do not impact school
spending through any impacts on federal revenues. We now turn to local revenues. While the
coefficient on $\text{MID} \times \text{POST} \times \text{YEAR}$ is negative and marginally statistically significant, it is not
significant at the 5 percent level and the the coefficient on $\text{HIGH} \times \text{POST} \times \text{YEAR}$ has the opposite
sign and is not significant. This is compelling evidence that the proposed instruments do not impact
school spending through any impacts on local revenues. In contrast, this model predicts large,
statistically significant, and monotonically increasing differences in state revenues. Specifically,
in the parsimonious model, the difference in state revenue between the most and middle reliance
groups four years after the recession was about $2450, and the difference in state revenue between
the most and least reliant groups four years after the recession was about $3167. Also, both impacts
are significant at the five percent level. In sum, the results reveal that our instruments operate though
their impacts on state revenues. As a robustness check, in the even numbered columns we show
results that include the individual year fixed effects. while all the estimated effects are less precise
in this model, the instruments only predicts changes in state revenues, and do not predict changes
in local or federal revenues.

The great recession had far reaching effects on the overall economy. We argue that our in-
strument isolates the recession’s impact on school spending from its effect on other aspects of the
economy. Having shown that our instrument impacts school spending through the hypothesized
channels, we now test whether instrumented school spending predicts economic conditions. We
have several variables that measure economic conditions, so to avoid problems with multiple hy-
pothesis testing we create predicted NAEP scores based on these economic conditions. We regress
test scores on the state unemployment rate, the 4-year moving average of the state unemployment
rate (to account for cumulative exposure), the total county employment, and the log of county em-
ployment, the number of children living in poverty, the log of county child poverty, public school
enrollment, the fraction of tested students on reduced price lunch, and the fraction of tested students
who are Limited English Proficient. To capture the within state variation in test scores associated
with these economic variable, this model includes both state and year fixed effects. This model
yields a within-entity R-Squared of 0.38 – indicating that these economic variables explain almost
a third of the within state variation over time (after accounting for shared time effects).\textsuperscript{13} To show
the power of this prediction visually, we divided up the data into deciles of predicted scores (after
removing state and year effects), and plot the average predicted NAEP score and the average actual
NAEP scores in each decile (Figure 5). Predicted scores (based on economic variables) and actual
score are very closely related. If our instruments impacts predicted scores similarly to actual scores
it would imply that much our main effects were through economic conditions. However, if we
find no impact on predicted scores and sizable impacts on actual scores, it would be compelling
evidence that our test score impacts are not driven by underlying economic conditions.

We present estimated impacts on predicted scores and actual scores across several specifications
in Table 4. As one can see, school spending (as predicted by our instruments) are unrelated to
predicted outcomes across all specifications. Moreover, the sign of the impact on predicted scores
differs between the log models and level models, suggesting no systematic association between
instrumented school spending and economic outcomes that predict test scores.\textsuperscript{14} In contrast, as we

\textsuperscript{13}The estimation results for the predicted scores are presented in Appendix Table D2.
\textsuperscript{14}We also present impacts on the individuals economic outcomes in Appendix Tables D3 and D4. As one might
expect, there is no consistent association between economic conditions and our instrumented school spending. In many
cases, the impacts on employment and unemployment go in opposite directions, and the signs for many economic
variables flip from one model to the next. Most importantly, across all models, there is no systematic relationship
between instrumented school spending and predicted outcomes based on all of the economic variables.
will discuss below, there is a consistent robust statistically significant relationship between school spending and actual NAEP scores across all these same specifications. This lends credibility to our research design. However, we present further empirical tests of the validity of our strategy in Section V.1.1.

V Results

V.1 Test Scores

The event study plots in Figure 4 shows that the reductions in school spending during the Great Recession (as predicted by states’ reliance on state revenues) coincided with declines in NAEP scores. We now quantify this plausibly causal relationship using our 2SLS models. Models 5 through 8 of Table 4 presents the 2SLS estimated effects of the level of spending (in thousands of dollars) on state average standardized NAEP scores. Models 13 through 16 of Table 4 present the 2SLS estimated effects of the log of spending on state average standardized NAEP scores.

Model 5 of Table 4 presents the effect of per-pupil spending in levels. This is the most parsimonious model with state fixed effects, state-specific linear trends, the Bartik predictor for state unemployment, and a post-recession indicator. Despite no relationship between per-pupil spending and the economic measures (shown in models 1 through 4), the coefficient of 0.0442 indicates that for every $1,000 decrease in per-pupil spending, test scores declined by 4.4 percent of a standard deviation ($p$-value < 0.01). Column 6 adds controls for the log of the population, and the log of the school age population. In this model, the coefficient is virtually unchanged. Column 7 presents the model without the additional controls but with individual year fixed effects. In this model, the coefficient is almost identical at 0.0420, but it is now only statistically significant at the five percent level. In essence, adding the year fixed effect increases the standard errors by about twenty percent while leaving the estimated coefficient unchanged. Finally, model 8 presents results with linear trends for each state, state fixed effects, year fixed effects and population, and economic controls. The point estimate is largely unchanged, and is statistically significant at the 5 percent level. In sum, there is a strong and robust positive relationship between school spending and NAEP scores. Across all models, a $1000 reduction in per-pupil spending (over the previous four years) is associated with between 4.2 and 4.54 percent of a standard deviation reduction in NAEP scores –illustrating the robustness of our result. Importantly, this robust relationship persists even though the instrumented spending cuts are unrelated to differential changes in underlying economic conditions.

Due to diminishing returns to school spending, the log of spending is often a better predictor of outcomes than levels (Jackson et al., 2016). Models 13 through 16 show the effect of log spending (over the previous four years) on NAEP scores. While the log of spending is unrelated to economic
conditions (Models 9 through 12), there is a robust positive impact of log spending on test scores. Overall, across all models, a ten percent reduction in per-pupil spending (over the previous four years) is associated with between 6.2 and 6.45 percent of a standard deviation reduction in NAEP scores. Given that average spending levels were $12,900 in our sample, these impacts in logs are very similar to those we present in levels. The facts that (a) we obtain the same basic result in levels and logs, (b) our instruments do not predict economic variables, and (c) our estimates are robust to the inclusion of rich controls, suggest that our estimated effects can be interpreted causally. We present several additional tests in Section V.1.1 below.

V.1.1 Robustness checks and Falsification

A. One may worry that our results are driven by sorting of students either across districts or across sectors (private/public) within states. To ensure that this is not a problem, we aggregate both the public and private school data to the state year level and analyze both together in Appendix Table D5, columns 1-4. If our results reflected stronger students from public schools switching to private schools after the recession, then there would be no effect on aggregate scores for the state as a whole. In all models, the coefficient for the combined data is positive, statistically significantly different from zero with a $p$-value of 0.1, and similar to the effect on individual public school scores in Table 4.

B. Despite the previous tests of instrument exogeneity, one may still worry that other state-level changes drive our estimates. If true, one would observe similar effects in both public and private schools. However, if our effects operate through reductions in public school spending, we should observe test score effects for public schools but not for private schools. This is what we show in Appendix Table D5, columns 5-8. In all models, the coefficient for log spending on public school scores is around 0.63 and is statistically significantly different from zero at the five percent level. In contrast, the point estimates for private school scores range between 0.06 and 1.02 and are statistically insignificant in all models.

C. To assuage lingering concerns that our estimates are confounded by underlying recession intensity, Appendix Table D6 presents results that control for economic conditions directly. We also add an annual housing value index as an additional “control.” Note that because school spending is capitalized in housing prices (Barrow and Rouse 2004; Cellini et al. 2010), observing a positive association between instrumented school spending and house prices is not indicative of bias. This is why we do not estimate impacts of our instrument on house values. We consider these models to be “over controlling”, but present it to establish the robustness of our result. The fully saturated model (Models 3, 4, 9 and 10) include the state unemployment rate, the state employment level, child poverty level, and average house values. In models with these controls, the estimates are similar to that of our preferred model (and are actually somewhat larger).
Another concern readers may have is that our estimated impacts are driven by comparisons across a small number of highly influential states. For example, both Hawaii and D.C. are likely to be influential states because they have very high and low values of our exogenous instrument. To systematically show that our results are not driven by some small number of states, we conduct permutation tests in which we estimate our main 2SLS models excluding one state at a time, two states at a time and three states at a time (note that this involves running 124,950 regressions). We plot the distribution of point estimates in Figure 6. As one can see, in the preferred models (without year fixed effects), even when dropping any three states, none of the point estimates is negative and in fact none has a \( p \)-value greater than 0.1. This indicates that the main models is quite robust and cannot be attributed to any one, two, or even three states. As further check on this pattern, we also estimated our conservative model (with year fixed effects) dropping any three states. Again, in the conservative model, even when dropping any three states, none of the point estimates is negative. However, not all of these models have \( p \)-values smaller than 0.1. In sum, our main result is robust to excluding any three states. We take the robustness of the positive relationship across the different models and samples to be compelling evidence that our estimated impacts are not driven by any single group of states, but rather reflects a general pattern.

V.2 Effects on Type of Spending

To understand the mechanisms though which spending cuts may impact student outcomes, we use our 2SLS specification to estimate the extent to which different spending and staffing categories were reduced in response to recession-induced expenditure decreases. Table 5 reports the results of 2SLS models. We focus on our preferred model that includes state fixed effects, state linear trends, a post indicator, and the Bartik predictor. In the top panel (models 1 through 10), we regress the level of spending in each sub category (in per-pupil units) on the overall (instrumented) level of spending (in per-pupil units). The resulting coefficient is therefore the marginal propensity to spend in each category. This specification allows for a formal test of whether the marginal and average propensities to spend in any category are equal. If the marginal and average propensities differ, it may suggest that districts respond differently to spending increases than they do to spending reductions.

Models 1 through 3 show the categories that make up total spending. For every dollar in per-pupil spending cuts, districts decrease capital spending by $0.28 and current elementary/secondary spending by $0.619. While capital spending accounted for 9.5% of overall annual spending, it made up 28% of the allocation of reduced spending, suggesting that districts cut capital spending more than other forms of spending on the margin. In contrast, Jackson et al. (2016) find that each dollar increase in total spending was associated with $0.1 increased spending on capital (a marginal propensity similar to the average). An examination of the types of capital spending af-
ected (models 4 and 5) reveals that all of the reduction in capital spending was from construction. The disproportionate cutting of construction projects is consistent with the descriptive patterns documented in Leachman et al. (2016) and reports in the press that budget shortfalls forced schools to defer maintenance and construction. By cutting disproportionately more from construction, states may be able to cut disproportionately less from core operating expenses. Consistent with this, elementary and secondary current spending accounts for 85.5% of overall spending, but only about 62% of spending cuts.

Columns 6 to 10 show effects across additional spending sub-categories of core operations. For every dollar in spending cuts, districts reduced instructional spending by $0.388 on average. Roughly half of this reduction can be accounted for by a reduction in instructional salaries (not shown). The rest of the reduction in instructional spending is accounted for by reductions in benefits and other types of instructional spending. While support services account for about 30 percent of spending on average, each dollar of cuts was associated with about 0.22 fewer dollars spent on support services. In contrast to Jackson et al. (2016) who demonstrate that funding shocks that increased spending resulted in disproportionately higher increases to both instructional spending and support services, we find that spending cuts are disproportionately smaller in support services. The lower panels present the same basic patterns in log spending.

Because reductions in instructional salaries and benefits could have been due to the hiring of fewer staff (which would likely affect outcomes) or the hiring of cheaper staff (which could have little effect on outcomes), we look at staffing directly in Table 6. The bottom panel shows the 2SLS estimates of log per-pupil spending on staffing per pupil. When spending is reduced by 10 percent, there is a reduction of about 0.2 teachers per 100 students (or 2 teachers per thousand students). This is about a 3 percent reduction. We also see reductions in the use of teacher aides. When spending is reduced by 10 percent, there is a reduction of about 0.082 aides per 100 students (or .8 aides per thousand students). This represents a 47 percent reduction, indicating that this is a category of employee that was easily reduced when budgets were strained. While we see no impact on guidance councilors or LEA staff, we do see reductions in library staff. When spending is reduced by 10 percent, there is a reduction of about 0.026 library staff per 100 students. This represents an 11 percent reduction and is proportionate to the overall budget cut. Overall, these findings are consistent with the significant decreases in instructional and non-instructional salaries through reduced hiring of teachers, teacher aides, and library staff.

V.3 Effects by Grade and Subject

To explore heterogeneity, we estimate effects separately by grade and subject. We present models that include bartik and population controls with and without year fixed effects. Table 7 presents the 2SLS/IV coefficients estimated separately by subject and grade. The effect of per-pupil
spending is statistically significant for each sub-sample of the data. However, as is common, the effect of spending is larger for Math than for Reading. The point estimates suggest that decreasing school spending by 10 percent would reduce math test scores by between 7 and 9.5 percent of a standard deviation but reading scores by only between 1.6 and 4.7 percent of a standard deviation. These results are consistent with previous studies which have shown that math scores are more responsive to school interventions. However, the point estimates are sufficiently imprecise that one cannot rule out that the effect is the same across subjects. To put these effects into perspective, these effect sizes are roughly equivalent to that of reducing teacher quality by one standard deviation (i.e. having all teachers in the district go from average quality to the 15th percentile) (Jackson et al., 2014). We also examine effects by grade. The marginal effects are larger for 4th grade than for 8th grade. Specifically, The point estimates suggest that decreasing school spending by 10 percent would reduce 4th grade scores by between 7 and 10.7 percent of a standard deviation but 8th grade scores by only between 1.58 and 5.4 percent of a standard deviation. As with the difference by subject, the point estimates are sufficiently imprecise that one cannot rule out that the effect is the same across grade levels.

V.4 Effects by Reduced Price Lunch Status

Many of the recent studies on the causal impact of increased school spending based on school finance reforms find that low income students are most impacted (Jackson et al. 2016 ; Lafortune et al. 2018). However, Hyman 2017 finds the opposite result in Michigan wherein districts targeted the marginal dollar toward schools serving less-poor populations within the district. As such, the extent to which school spending cuts (which occurred primarily at the state level) may disproportionately harm the poor remains an open question. To examine this, we estimate our main models on the average scores of the students who are free- or reduced-price lunch eligible and those who are not. The 2SLS results are presented in Table 8. The top panel examines the impact of school spending in levels and the lower panel in logs.

Overall, there is limited evidence that the changes in public school spending had a differential impact on low income children. In our preferred model, increasing spending by $1000 increased the average scores of free- or reduced-price lunch students by 4.5 percent of a standard deviation \((p\text{-value}<0.01)\). While the point estimate for non-reduced lunch students is almost identical, it is not statistically significant. In the conservative model with year fixed effects, the impact on the low income and non low-income are 3.56 and 6.75 percent of a standard deviation for a $1000 increase. While both estimates are significant at the ten percent level, one cannot reject that the marginal impacts are the same for the two groups. Indeed, in models 5 and 6, we compute the estimated impact of school spending on the gap between low-income and not low-income students in the state. In both the preferred and conservative models, there is no statistically significant impact, and
the point estimates across the two models differ in sign. The lower panel presents estimates for log spending. The pattern of results mirrors those in levels and indicates little differential spending impact in the test scores on low income and not low income students.

Interestingly, our estimated effects are consistent with other findings in the literature. Even if low-income students are more responsive to spending levels (as found in Jackson et al. 2016), if the spending cuts were larger in those schools within a state that had fewer low-income children (as found in Lafortune et al. 2018) it could explain our results. Because we do not have district level variation in spending we cannot test this explicitly, but we speculate that this may be the case.

V.5 High School Completion

To examine whether our test score reductions translated into worse longer-run outcomes, we examine if cohorts (from the same state) with more years of recessional exposure while in school had lower graduation rates, and if this exposure effect is larger in states that were the most dependent on state revenues to fund public schools. To present visual evidence, we implement an event study of the effect of recessional cuts on high school attainment. Using our birth-cohort by state by year graduation rate data and focusing on individuals between the ages of 16 and 18, we estimate the following by OLS:

\[ Y_{sc} = \sum_{t=2004}^{2014} \beta_t \cdot (I_{g>1, s} \times I_{c=t}) + \gamma_s + \gamma_c + \nu_{sc} \]  

\( Y_{sc} \) is the graduation rate in state \( s \) for birth cohort \( c \). As in equation (3), \( I_{g=1,s} \) denotes the states that are more reliant on state revenues for public schools. \( I_{c=t} \) is an indicator denoting if the observation from cohort \( c \) was expected to graduate high school in calendar year \( t \). The variables \( \gamma_c \) is a birth cohort fixed effect (this is the same as an expected high school graduation year fixed effect). Accordingly, the coefficients \( \beta_t \) map out the differences in outcomes across expected graduation year cohort (with different duration of exposure to the recession) between states with low and high \( \Omega_s \) over time. We estimate this model where the main outcome is the log of school spending in the four years prior to expected high school graduation (i.e. average spending during high school) and also the state level average high school completion rate among 16 to 18 years olds. We plot these coefficients in Figure 7.

If school spending cuts as predicted by reliance on state funds in 2008 matter, cohorts that were expected to graduate high school after the recession would have lower graduation rates than those that graduated before, and this decline would be most pronounced in high-reliance states (which saw the largest relative declines in spending). Consistent with a causal effect, and similar to the test

\[ \text{Jackson (2018b)} \] finds that teacher impacts on test scores are much weaker predictors of their impacts on high-school completion than impacts on grades, attendance, or discipline.
scores results, the difference in graduation rates between high and low \( \Omega_s \) states is relatively stable among cohorts that would have graduated prior to the recession (if anything they were increasing in high reliance states), and exposed cohorts in high \( \Omega_s \) states experienced differentially lower high-school completion rates. The timing of the decline in graduation rates closely tracks the decline in per-pupil spending despite being unrelated to differential changes in economic conditions that predict achievement – compelling evidence of a causal relationship.

Using this variation in a 2SLS framework, we quantify the effect of reduced school spending on graduation rates by estimating the following by 2SLS.

\[
PPE_{sc} = \sum_{g=1}^{3} [\pi_{1g} \cdot (I_{gs} \times I_{post} \times E)] + \sum_{g=2}^{3} [\pi_{2g} \cdot (I_{gs} \times I_{post})] + \sigma_1 \cdot I_{post} + \alpha_1s + (\tau_1s \times E) + \varepsilon_{1st} \tag{5}
\]

\[
Y_{sc} = \beta \cdot (PPE_{sc}) + \sigma_2 \cdot I_{post} + \alpha_2s + (\tau_2s \times E) + \varepsilon_{2st} \tag{6}
\]

All common variables are as described in equations (1) and (2). The endogenous treatment, \( PPE_{sc} \), is the school-spending measure in state \( s \) for birth cohort \( c \). Because we observe multiple ages at the same point in time, our measure of exposure to the recession is no longer the calendar year, but rather the year of expected high school completion relative to 2008. Accordingly, our time variable \( E \) is the birth cohort so that \( \tau_1s \) and \( \tau_2s \) are linear birth-cohort trends for each state. \( I_{post} \) denotes birth cohorts with expected high school completion after 2008 (i.e. graduation year cohorts that should have been exposed to the recession while in school).

Using only variation across birth cohorts within states we include state fixed effects \( \alpha_1s \) and \( \alpha_2s \) in the first and second stage, respectively. Our excluded instruments are the recession exposure variables (\( I_{post} \) and \( E \)) interacted with the state reliance on state fund group indicators (\( I_{gs} \)). Our model is identified off a change in the linear trend after the recession across states with high and low dependence on state funds in 2008. This is a linariization of the patterns depicted in Figure 7.

Table 9 presents the 2SLS results. We examine impacts on all 16 to 18 years olds and also by ethnicity (White, Black, Hispanic, and other). In the preferred models (1 and 11) on the full population, the coefficient on the level of spending is 0.0097 (p-value < 0.01) and that for the log of spending is 0.164 (p-value < 0.01). Focusing on the log specification, reducing school spending by ten percent during high school increased the graduation rate by 1.64 percentage points. Looking by student ethnicity, the results appear to be most pronounced for Hispanic students and non-Hispanics who are not white or black. For whites, reducing school spending by ten percent during high school increased the graduation rate by 1.48 percentage points, while it is 3.7 and 3.0 percentage points for Hispanic students and other students, respectively. In contrast to the significant impacts for most student, there is no impact on the graduation rate of black students.
In our conservative models that also includes year fixed effects, the differences across the ethnic groups becomes more stark. It is important to highlight that we take these conservative estimates with a grain of salt. the Kleibergen-Papp statistics suggest that these model may be unidentified. As such, while it is reassuring that some of the basic patterns persist even in this conservative models, we trust the results in our preferred model (which does not suffer from a possible under-identification problem). In the more conservative models, there is no longer a significant effect overall due largely to there being no impact for white students. However, there remain large and statistically significant impacts on the graduation rates of Hispanic and other students. That is, for Hispanic students, reducing school spending by ten percent during high school increases the graduation rate by 4.5 percentage points, and by 2.7 percentage points for the other student category. Taken together, the results indicate that cohorts that were exposed to greater recession-induced spending cuts while they were in school, particularly during high school, completed high school at lower rates. While the estimated impacts are less robust for white students, they are large, statistically significant, and robust for Hispanic students and those in the Other category.

VI Discussion and Conclusions

The policy and scholarly debates regarding whether public school spending matters have been going on for decades. However, using large permanent increases in public school spending caused by school finance reforms, recent papers have uncovered credible evidence of a causal link between increased school spending and improved student outcomes (Jackson et al. 2016; Candelaria and Shores 2017; Lafortune et al. 2018; Card and Payne 2002). Other studies use quasi-random variation in school spending and find that increased school spending is linked to improved outcomes (Hyman 2017; Miller 2017; Gigliotti and Gigliotti 2017). Despite this growing consensus, there has been no study on how schools respond to large persistent cuts to spending and how such cuts impact student outcomes.

We present new evidence of a causal link between school spending and students outcomes. We find this result using variation that is not derived from school financial reforms, and our results relate to contemporaneous spending levels. Our results speak to questions regarding whether school spending effects are symmetric. Importantly, we show that school districts respond to budget cuts by disproportionately reducing non-core operational spending. However, our results do not support the notion that budget cuts have no effect on outcomes. On the contrary, students that experienced reduced public school spending had both lower test scores and lower high school completion rates. These patterns suggest that school spending cuts do matter, and that the ill-effects of the recession on the affected youth (through reduced public school spending) may be felt for years to come.
References


William N. Evans, Robert M. Schwab, and Kathryn L. Wagner. The Great Recession and Public


Michael Leachman and Chris Mai. Most States Still Funding Schools Less Than Before the Recession. 2014.


Diane Schanzenbach and Bauer Lauren. Food Insecurity among Children in Massachusetts, 2017. URL http://brook.gs/2s6XzKz.


Tables and Figures

Figure 1. School Spending and NAEP Scores Over Time.

Notes: This figure plots ......
**Figure 2.** Source of Revenues for K12 Education after the recession.

Notes: This figure plots the change in national aggregate revenue (summed over all available districts in the CCD data) for public schools relative to 2007 levels. The total revenue numbers are broken down by the source of funding (federal, state, and local); changes in each of which is also shown separately. Nominal dollars in all years are deflated by the CPI and adjusted to real 1999 dollars in billions. As a frame of reference, the values in 2007 are as follows: Total Revenue: 457.95 billions, State Revenue: 217.37 billions, Local Revenue: 204.15 billions, Federal Revenue: 36.42 billions. Note that, owing to the American Recovery and Reinvestment Act of 2009 (ARRA), which sought to temporarily offset for the loss in state funding, education spending from federal sources increased in 2010 and 2011 and then fell back to pre-recession levels thereafter.
Figure 3. Fraction of K12 Revenue from State Sources: Spending Growth and Unemployment.

Notes: The left panel shows the percent change in state average (averaged across all districts in the state) K12 spending between 2007 and 2013 by the percent of state K12 revenues that came from state sources. The right panel shows the percent change in the state unemployment rate between 2007 and 2013 by the percent of state K12 revenues that came from state sources. States with heavy reliance on state sources are depicted by the black dots, those with low reliance are depicted by the black triangles, and those in the middle and depicted with the gray circles.
Figure 4. Difference in Spending and NAEP Scores Between States with High and Low Reliance on State Revenues Over Time

Notes: The dashed connected lines depict the coefficients on the individual calendar year indicators interacted with an indicator for high reliance on state revenue in 2008, lnΩ, > 0.33. The solid lines represent the linear fit during the pre-recession period/cohorts (negative values of exposure) and post-recession periods/cohorts (non-negative values of exposure). The pattern for per-pupil spending is presented in gray, while that for test scores is presented in black.
Figure 5. Predicted Scores Versus Actual Scores.
Figure 6. *Test Score Impacts under Permutation Test.*

Notes: This density plot depicts the distribution of estimated impacts while dropping states. The dashed black line depicts the distribution of estimated coefficient of log spending on NAEP scores if one drops any single state in the main model. The dashed gray line depicts the distribution of estimated coefficient of log spending on NAEP scores if one drops any two states in the main model. The solid black line depicts the distribution of estimated coefficient of log spending on NAEP scores if one drops any three states in the main model. Finally, the solid gray line depicts the distribution of estimated coefficients of log spending on NAEP scores if one drops any three states in the conservative model.
Figure 7. *Difference in High School Completion Between States with High and Low Reliance on State Revenues Over Time*

*Notes:* The dashed connected lines depict the coefficients on the expected high school graduation year interacted with the state reliance on state revenue in 2008, $\Omega_{*0.33}$. The solid lines represent the linear fit during the pre-recession period/cohorts (negative values of exposure) and post recession periods/cohorts (non-negative values of exposure).
Table 1. Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>mean</th>
<th>sd</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Revenue (2015 CPI Adjusted)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Federal Revenue</td>
<td>408</td>
<td>0.0989</td>
<td>0.0314</td>
</tr>
<tr>
<td>% State Revenue</td>
<td>408</td>
<td>0.489</td>
<td>0.143</td>
</tr>
<tr>
<td>% Local Revenue</td>
<td>408</td>
<td>0.412</td>
<td>0.142</td>
</tr>
<tr>
<td>Total Revenue (millions)</td>
<td>408</td>
<td>448.5</td>
<td>821.9</td>
</tr>
<tr>
<td><strong>District Demographics</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Public School Enrollment</td>
<td>408</td>
<td>34,022</td>
<td>53,977</td>
</tr>
<tr>
<td>Total Population (in district)</td>
<td>408</td>
<td>11.65</td>
<td>1.129</td>
</tr>
<tr>
<td>Child Population (in district)</td>
<td>408</td>
<td>9.871</td>
<td>1.162</td>
</tr>
<tr>
<td>Child Poverty Population (in district)</td>
<td>408</td>
<td>7,800</td>
<td>15,417</td>
</tr>
<tr>
<td>Annual Average Employment</td>
<td>408</td>
<td>122,258</td>
<td>195,123</td>
</tr>
<tr>
<td>Bartik Instrument</td>
<td>408</td>
<td>6.331</td>
<td>1.652</td>
</tr>
<tr>
<td>Share of spending from state</td>
<td>408</td>
<td>0.493</td>
<td>0.136</td>
</tr>
<tr>
<td>Unemployment Rate (1 yr)</td>
<td>408</td>
<td>6.169</td>
<td>2.013</td>
</tr>
<tr>
<td>Unemployment Rate (4 yr average)</td>
<td>408</td>
<td>5.916</td>
<td>1.763</td>
</tr>
<tr>
<td>Home Value Index</td>
<td>404</td>
<td>179,579</td>
<td>80,483</td>
</tr>
<tr>
<td><strong>District Staffing (per 100 students)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Teachers</td>
<td>390</td>
<td>6.68</td>
<td>0.00955</td>
</tr>
<tr>
<td>Aides</td>
<td>375</td>
<td>1.7</td>
<td>0.00656</td>
</tr>
<tr>
<td>Guidance Counselors</td>
<td>383</td>
<td>0.249</td>
<td>0.00104</td>
</tr>
<tr>
<td>Library Staff</td>
<td>343</td>
<td>0.234</td>
<td>0.000866</td>
</tr>
<tr>
<td>LEA Staff</td>
<td>350</td>
<td>0.501</td>
<td>0.00249</td>
</tr>
<tr>
<td><strong>Revenues and Expenditures (2015 CPI Adjusted), per student</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Expenditures</td>
<td>408</td>
<td>12,910</td>
<td>3,592</td>
</tr>
<tr>
<td>Capital Outlay Expenditures</td>
<td>408</td>
<td>1,236</td>
<td>849.4</td>
</tr>
<tr>
<td>Construction</td>
<td>408</td>
<td>912.2</td>
<td>804.1</td>
</tr>
<tr>
<td>Non-Construction</td>
<td>408</td>
<td>323.7</td>
<td>177.3</td>
</tr>
<tr>
<td>Current Elementary/Secondary Spending</td>
<td>408</td>
<td>11,009</td>
<td>3,007</td>
</tr>
<tr>
<td>Instructional Spending</td>
<td>408</td>
<td>6,692</td>
<td>1,885</td>
</tr>
<tr>
<td>Services</td>
<td>408</td>
<td>3,847</td>
<td>1,213</td>
</tr>
<tr>
<td>Other</td>
<td>408</td>
<td>469.6</td>
<td>101.5</td>
</tr>
<tr>
<td>Non-Elementary/Secondary Spending</td>
<td>408</td>
<td>655.8</td>
<td>594.8</td>
</tr>
<tr>
<td>Total Salaries</td>
<td>408</td>
<td>6,610</td>
<td>1,588</td>
</tr>
<tr>
<td>Total Benefits</td>
<td>408</td>
<td>2,050</td>
<td>822.2</td>
</tr>
<tr>
<td><strong>Graduation Rates, Ages 16-18</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age</td>
<td>2142</td>
<td>17</td>
<td>0.817</td>
</tr>
<tr>
<td>High School Completion Rates</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>2142</td>
<td>0.258</td>
<td>0.282</td>
</tr>
<tr>
<td>White</td>
<td>2142</td>
<td>0.263</td>
<td>0.292</td>
</tr>
<tr>
<td>Black</td>
<td>1954</td>
<td>0.253</td>
<td>0.294</td>
</tr>
<tr>
<td>Hispanic</td>
<td>2114</td>
<td>0.254</td>
<td>0.296</td>
</tr>
<tr>
<td>Other</td>
<td>2138</td>
<td>0.263</td>
<td>0.291</td>
</tr>
</tbody>
</table>

Notes: See Appendix VI for details on each data source.
Table 2. First Stage and Reduced Form

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Per-Pupil Spending/1000</td>
<td>Log of Per-Pupil Spending</td>
<td>NAEP Score</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post 2008</td>
<td>-803.8+</td>
<td>-36.55*</td>
<td>-38.56</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[412.4]</td>
<td>[15.76]</td>
<td>[24.06]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bartik Instrument</td>
<td>0.365*</td>
<td>0.230</td>
<td>0.0248**</td>
<td>0.00344</td>
<td>-0.000580</td>
<td>-0.0441+</td>
</tr>
<tr>
<td></td>
<td>[0.154]</td>
<td>[0.588]</td>
<td>[0.00728]</td>
<td>[0.0555]</td>
<td>[0.00798]</td>
<td>[0.0251]</td>
</tr>
<tr>
<td>MID * POST</td>
<td>1.108**</td>
<td>1.120**</td>
<td>68.17**</td>
<td>70.17**</td>
<td>61.95*</td>
<td>66.04*</td>
</tr>
<tr>
<td></td>
<td>[340.6]</td>
<td>[335.7]</td>
<td>[15.34]</td>
<td>[16.27]</td>
<td>[26.83]</td>
<td>[30.09]</td>
</tr>
<tr>
<td>HIGH * POST</td>
<td>1.804**</td>
<td>1.813**</td>
<td>114.6**</td>
<td>116.2**</td>
<td>70.51*</td>
<td>73.63*</td>
</tr>
<tr>
<td></td>
<td>[476.4]</td>
<td>[475.0]</td>
<td>[16.61]</td>
<td>[17.21]</td>
<td>[27.15]</td>
<td>[30.60]</td>
</tr>
<tr>
<td>LOW * POST * YEAR</td>
<td>0.399+</td>
<td>0.0181*</td>
<td>0.0192</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.205]</td>
<td>[0.00783]</td>
<td>[0.0120]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>MID * POST * YEAR</td>
<td>-0.152*</td>
<td>-0.558**</td>
<td>-0.0158**</td>
<td>-0.0349**</td>
<td>-0.0116**</td>
<td>-0.0329*</td>
</tr>
<tr>
<td></td>
<td>[0.0702]</td>
<td>[0.167]</td>
<td>[0.00427]</td>
<td>[0.00810]</td>
<td>[0.00427]</td>
<td>[0.0150]</td>
</tr>
<tr>
<td>HIGH * POST * YEAR</td>
<td>-0.499***</td>
<td>-0.903***</td>
<td>-0.0389**</td>
<td>-0.0578**</td>
<td>-0.0159**</td>
<td>-0.0367*</td>
</tr>
<tr>
<td></td>
<td>[0.182]</td>
<td>[0.237]</td>
<td>[0.00530]</td>
<td>[0.00857]</td>
<td>[0.00562]</td>
<td>[0.0152]</td>
</tr>
</tbody>
</table>

Kleibergen-Paap rk Wald F statistic: 7.588 5.683 24.01 17.39  
Cragg-Donald Wald F statistic: 17.90 19.88 19.27 17.35  
Kleibergen-Paap rk LM Underidentification statistic: 14.53 8.383 17.23 7.767  
Observations: 408 408 408 408 408  
State Trends & State Fixed Effects: X X X X X X  
Bartik IV: X X X X X X  
Year Fixed Effects: X X X X  

Notes: Robust standard errors in brackets cluster by state.  
** p<0.01, * p<0.05, + p<0.1  
MID*POST=(Post 2008) × (%State2008 Between .33 and .66) and HIGH*POST=(Post 2008) × (%State2008 > .66)  
LOW*POST*YEAR=(Post 2008) × (%State2008 < .33) × Year, MID*POST*YEAR=(Post 2008) × (%State2008 Between .33 and .66) × Year, and HIGH*POST*YEAR=(Post 2008) × (%State2008 > .66) × Year  
F Statistics are for (Post 2008) × %State2008 and Year×(Post 2008)×%State2008 Trends in all models
Table 3. Revenue Sources

<table>
<thead>
<tr>
<th></th>
<th>Federal Revenue/1000</th>
<th>State Revenue/1000</th>
<th>Local Revenue/1000</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>MID * POST</strong></td>
<td>75.21</td>
<td>359.5*</td>
<td>895.9</td>
</tr>
<tr>
<td></td>
<td>[136.6]</td>
<td>[163.5]</td>
<td>[693.5]</td>
</tr>
<tr>
<td><strong>HIGH * POST</strong></td>
<td>130.7</td>
<td>1,589**</td>
<td>349.3</td>
</tr>
<tr>
<td></td>
<td>[175.1]</td>
<td>[522.0]</td>
<td>[739.9]</td>
</tr>
<tr>
<td><strong>LOW * POST * YEAR</strong></td>
<td>0.0724</td>
<td>-0.0582</td>
<td>0.268</td>
</tr>
<tr>
<td></td>
<td>[0.0819]</td>
<td>[0.0767]</td>
<td>[0.422]</td>
</tr>
<tr>
<td><strong>MID * POST * YEAR</strong></td>
<td>0.0349</td>
<td>-0.237**</td>
<td>-0.178+</td>
</tr>
<tr>
<td></td>
<td>[0.0243]</td>
<td>[0.0477]</td>
<td>[0.104]</td>
</tr>
<tr>
<td><strong>HIGH * POST * YEAR</strong></td>
<td>0.00734</td>
<td>-0.850**</td>
<td>0.0943</td>
</tr>
<tr>
<td></td>
<td>[0.0643]</td>
<td>[0.254]</td>
<td>[0.162]</td>
</tr>
</tbody>
</table>

Observations: 408
State Trends & State Fixed Effects: X X X X X X
Post 2008 Indicator: X X X X X X
Bartik IV: X X X X X X
Year Fixed Effects: X X X

Notes: Robust standard errors in brackets cluster by state.
** p<0.01, * p<0.05, + p<0.1
MID*POST=(Post 2008) × (%State2008 Between .33 and .66) and HIGH*POST=(Post 2008) × (%State2008 > .66)
LOW*POST*YEAR=(Post 2008) × (%State2008 < .33) × Year, MID*POST*YEAR=(Post 2008) × (%State2008 Between .33 and .66) × Year, and HIGH*POST*YEAR=(Post 2008) × (%State2008 > .66) × Year
Table 4. Two-Stage-Least-Squares Regressions: Main Results and Exogeneity

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td>4-Year Spending/1000</td>
<td>-0.00486</td>
<td>0.00425</td>
<td>0.000475</td>
<td>0.0155</td>
<td>0.0442**</td>
<td>0.0454**</td>
<td>0.0420*</td>
<td>0.0429*</td>
</tr>
<tr>
<td></td>
<td>[0.0113]</td>
<td>[0.0119]</td>
<td>[0.01000]</td>
<td>[0.0119]</td>
<td>[0.0154]</td>
<td>[0.0159]</td>
<td>[0.0169]</td>
<td>[0.0190]</td>
</tr>
<tr>
<td>Log of 4 Year Spending</td>
<td>-0.128</td>
<td>-0.0318</td>
<td>-0.00176</td>
<td>0.240</td>
<td>0.632*</td>
<td>0.624*</td>
<td>0.645*</td>
<td>0.620*</td>
</tr>
<tr>
<td></td>
<td>[0.177]</td>
<td>[0.188]</td>
<td>[0.162]</td>
<td>[0.202]</td>
<td>[0.245]</td>
<td>[0.235]</td>
<td>[0.299]</td>
<td>[0.308]</td>
</tr>
<tr>
<td>Observations</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Post 2008 Indicator</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Total and Child Population</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets cluster by state.
** p<0.01, * p<0.05, + p<0.1
Columns 1-4 regress instrumented spending and controls on estimated mean score from economic and demographic district and state data (enrollment, employment, child poverty, and test-taker demographics). See E for model describing predicted score estimation.
Columns 5-8 regress instruments spending on actual district-weighted state-average NAEP scores.
### Table 5. Two-Stage-Least-Squares Regressions: School Spending Categories

<table>
<thead>
<tr>
<th>Expenditure Categories (Per-Pupil)</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9</td>
<td>10</td>
</tr>
<tr>
<td><strong>Total Expenditures</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Capital</td>
<td>280.1**</td>
<td>619.4**</td>
<td>80.35*</td>
<td>243.6**</td>
<td>34.74+</td>
<td>388.9**</td>
<td>215.6**</td>
<td>15.97*</td>
<td>355.3**</td>
<td>185.1*</td>
</tr>
<tr>
<td>[82.25]</td>
<td>[76.82]</td>
<td>[36.87]</td>
<td>[85.50]</td>
<td>[20.41]</td>
<td>[50.80]</td>
<td>[40.18]</td>
<td>[6.608]</td>
<td>[61.98]</td>
<td>[78.47]</td>
<td></td>
</tr>
<tr>
<td><strong>Mean(Independent Var.)</strong></td>
<td>1236</td>
<td>11009</td>
<td>655.8</td>
<td>912.2</td>
<td>323.7</td>
<td>6692</td>
<td>384.7</td>
<td>469.6</td>
<td>6610</td>
<td>2050</td>
</tr>
<tr>
<td><strong>Average Share</strong></td>
<td>0.0951</td>
<td>0.855</td>
<td>0.0494</td>
<td>0.0688</td>
<td>0.0263</td>
<td>0.519</td>
<td>0.297</td>
<td>0.0384</td>
<td>0.518</td>
<td>0.159</td>
</tr>
<tr>
<td><strong>P(Average=Marginal)</strong></td>
<td>0.0289</td>
<td>0.00351</td>
<td>0.405</td>
<td>0.0462</td>
<td>0.682</td>
<td>0.0135</td>
<td>0.0474</td>
<td>0.00133</td>
<td>0.0117</td>
<td>0.739</td>
</tr>
<tr>
<td><strong>Log Capital</strong></td>
<td>2.226**</td>
<td>0.803**</td>
<td>0.829</td>
<td>2.650**</td>
<td>1.364+</td>
<td>0.789**</td>
<td>0.872**</td>
<td>0.630**</td>
<td>0.683**</td>
<td>1.331**</td>
</tr>
<tr>
<td><strong>Log Elem/Sec</strong></td>
<td>[0.524]</td>
<td>[0.0504]</td>
<td>[0.515]</td>
<td>[0.823]</td>
<td>[0.732]</td>
<td>[0.0793]</td>
<td>[0.0647]</td>
<td>[0.177]</td>
<td>[0.0820]</td>
<td>[0.388]</td>
</tr>
<tr>
<td><strong>Log Other</strong></td>
<td>0.0951</td>
<td>0.855</td>
<td>0.0494</td>
<td>0.0688</td>
<td>0.0263</td>
<td>0.519</td>
<td>0.297</td>
<td>0.0384</td>
<td>0.518</td>
<td>0.159</td>
</tr>
<tr>
<td><strong>P(Average=Marginal)</strong></td>
<td>0.0289</td>
<td>0.00351</td>
<td>0.405</td>
<td>0.0462</td>
<td>0.682</td>
<td>0.0135</td>
<td>0.0474</td>
<td>0.00133</td>
<td>0.0117</td>
<td>0.739</td>
</tr>
<tr>
<td><strong>Log of 1-Year Spending</strong></td>
<td>0.0233</td>
<td>0.000276</td>
<td>0.741</td>
<td>0.0503</td>
<td>0.621</td>
<td>0.0104</td>
<td>0.0528</td>
<td>0.00673</td>
<td>0.000316</td>
<td>0.396</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
</tr>
<tr>
<td><strong>State Trends &amp; State Fixed Effects</strong></td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td><strong>Post 2008 Indicator</strong></td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td><strong>Bartik IV</strong></td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td><strong>Total and Child Population</strong></td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

**Notes:**
- Robust standard errors in brackets cluster by state.
- ** p<0.01, * p<0.05, + p<0.1
- a. Other spending includes Non Elem/Sec and Payments.
Table 6. Two-Stage-Least-Squares Regressions: Staffing Categories

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td><strong>Teachers</strong></td>
<td><strong>Aides</strong></td>
<td><strong>Guidance</strong></td>
<td><strong>Library</strong></td>
<td><strong>LEA Staff</strong></td>
</tr>
<tr>
<td>1-Year Spending/1000</td>
<td>0.139**</td>
<td>0.0560*</td>
<td>0.00878</td>
<td>0.0184**</td>
<td>-0.0327</td>
</tr>
<tr>
<td></td>
<td>[0.0407]</td>
<td>[0.0222]</td>
<td>[0.00650]</td>
<td>[0.00435]</td>
<td>[0.0219]</td>
</tr>
<tr>
<td>Log of 1-Year Spending</td>
<td>2.052**</td>
<td>0.818*</td>
<td>0.129</td>
<td>0.264**</td>
<td>-0.273</td>
</tr>
<tr>
<td></td>
<td>[0.463]</td>
<td>[0.314]</td>
<td>[0.0940]</td>
<td>[0.0804]</td>
<td>[0.245]</td>
</tr>
<tr>
<td>Mean (Dependent Var)</td>
<td>6.679</td>
<td>1.704</td>
<td>0.249</td>
<td>0.234</td>
<td>0.501</td>
</tr>
<tr>
<td>Observations</td>
<td>390</td>
<td>375</td>
<td>383</td>
<td>342</td>
<td>349</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Post 2008 Indicator</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Total and Child Population</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets cluster by state.

** p<0.01, * p<0.05, + p<0.1
Table 7. Two-Stage-Least-Squares Regressions: School Spending and NAEP Scores by Grade and Subject

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>5</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean Score: Math</td>
<td>Mean Score: Reading</td>
<td>Mean Score: 4th Grade</td>
<td>Mean Score: 8th Grade</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>0.0606**</td>
<td>0.0477*</td>
<td>0.0166</td>
<td>0.0311**</td>
<td>0.0685**</td>
<td>0.0480*</td>
<td>0.0206</td>
</tr>
<tr>
<td></td>
<td>[0.0201]</td>
<td>[0.0235]</td>
<td>[0.0114]</td>
<td>[0.0103]</td>
<td>[0.0188]</td>
<td>[0.0197]</td>
<td>[0.0189]</td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>0.953**</td>
<td>0.707+</td>
<td>0.156</td>
<td>0.465**</td>
<td>1.074**</td>
<td>0.708*</td>
<td>0.158</td>
</tr>
<tr>
<td></td>
<td>[0.282]</td>
<td>[0.378]</td>
<td>[0.166]</td>
<td>[0.167]</td>
<td>[0.289]</td>
<td>[0.316]</td>
<td>[0.242]</td>
</tr>
</tbody>
</table>

Observations | 357 | 357 | 408 | 408 | 407 | 407 | 408 | 408 |
State Trends & State Fixed Effects | X | X | X | X | X | X | X | X |
Post-2008 Indicator | X | X | X | X | X | X | X | X |
Bartik IV | X | X | X | X | X | X | X | X |
Total and Child Population | X | X | X | X | X | X | X | X |
Year Fixed Effects | X | X | X | X | X | X | X | X |

Notes: Robust standard errors in brackets cluster by state.
** p<0.01, * p<0.05, + p<0.1
Table 8. Two-Stage-Least-Squares Regressions: School Spending and Achievement by Free/Reduced Lunch Status

<table>
<thead>
<tr>
<th></th>
<th>Mean NAEP Score: FRL Recipients</th>
<th>Mean NAEP Score: Non-FRL Recipients</th>
<th>Achievement Gap</th>
</tr>
</thead>
<tbody>
<tr>
<td>4-Year Spending/1000</td>
<td>0.0453** (0.0164)</td>
<td>0.0429 (0.0349)</td>
<td>-0.00238 (0.0290)</td>
</tr>
<tr>
<td></td>
<td>0.0356+ (0.0205)</td>
<td>0.0675+ (0.0366)</td>
<td>0.0319 (0.0308)</td>
</tr>
<tr>
<td></td>
<td>7</td>
<td>8</td>
<td>9</td>
</tr>
<tr>
<td>Log of 4-year Spending</td>
<td>0.640* (0.248)</td>
<td>0.439 (0.462)</td>
<td>-0.201 (0.360)</td>
</tr>
<tr>
<td></td>
<td>0.506 (0.322)</td>
<td>0.979+ (0.582)</td>
<td>0.473 (0.463)</td>
</tr>
<tr>
<td>Observations</td>
<td>408</td>
<td>408</td>
<td>408</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Post 2008 Indicator</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Total and Child Population</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets cluster by state.

** p<0.01, * p<0.05, + p<0.1
Table 9. Two-Stage-Least-Squares Regressions: School Spending and High School Graduation Rates

<table>
<thead>
<tr>
<th>Average Spending during High School</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.00970**</td>
<td>-0.00263</td>
<td>0.00752</td>
<td>-0.0125</td>
<td>-0.000101</td>
<td>-0.0122</td>
<td>0.0274**</td>
<td>0.0275**</td>
<td>0.0207**</td>
<td>0.0148*</td>
</tr>
<tr>
<td></td>
<td>[0.00289]</td>
<td>[0.00535]</td>
<td>[0.00487]</td>
<td>[0.0115]</td>
<td>[0.0118]</td>
<td>[0.0138]</td>
<td>[0.00650]</td>
<td>[0.00664]</td>
<td>[0.00443]</td>
<td>[0.00658]</td>
</tr>
<tr>
<td>Kleibergen-Paap rk Wald F statistic</td>
<td>33.47</td>
<td>24.76</td>
<td>33.47</td>
<td>24.76</td>
<td>26.81</td>
<td>21.25</td>
<td>36.66</td>
<td>23.58</td>
<td>32.76</td>
<td>24.29</td>
</tr>
<tr>
<td>Cragg-Donald Wald F statistic</td>
<td>211.9</td>
<td>151.5</td>
<td>211.9</td>
<td>151.5</td>
<td>173.4</td>
<td>126</td>
<td>200.6</td>
<td>137.5</td>
<td>209.9</td>
<td>148.8</td>
</tr>
<tr>
<td>Kleibergen-Paap Under-identified statistic</td>
<td>30.6</td>
<td>5.976</td>
<td>30.6</td>
<td>5.976</td>
<td>32.07</td>
<td>6.411</td>
<td>30.98</td>
<td>6.269</td>
<td>31</td>
<td>6.063</td>
</tr>
<tr>
<td>log (Average spending during high school)</td>
<td>0.164**</td>
<td>-0.0479</td>
<td>0.148**</td>
<td>-0.228</td>
<td>0.0214</td>
<td>-0.205</td>
<td>0.374**</td>
<td>0.453**</td>
<td>0.300**</td>
<td>0.268*</td>
</tr>
<tr>
<td></td>
<td>[0.0378]</td>
<td>[0.0906]</td>
<td>[0.0517]</td>
<td>[0.203]</td>
<td>[0.162]</td>
<td>[0.221]</td>
<td>[0.113]</td>
<td>[0.129]</td>
<td>[0.0757]</td>
<td>[0.119]</td>
</tr>
<tr>
<td>Kleibergen-Paap rk Wald F statistic</td>
<td>35.44</td>
<td>43.56</td>
<td>35.44</td>
<td>43.56</td>
<td>32.3</td>
<td>41.1</td>
<td>36.7</td>
<td>44.16</td>
<td>35.04</td>
<td>42.7</td>
</tr>
<tr>
<td>Cragg-Donald Wald F statistic</td>
<td>293.7</td>
<td>141.3</td>
<td>293.7</td>
<td>141.3</td>
<td>236.9</td>
<td>114.5</td>
<td>283.3</td>
<td>130.7</td>
<td>292.2</td>
<td>140</td>
</tr>
<tr>
<td>Kleibergen-Paap Under-identified statistic</td>
<td>34.37</td>
<td>6.01</td>
<td>34.37</td>
<td>6.01</td>
<td>35.26</td>
<td>6.488</td>
<td>34.56</td>
<td>6.224</td>
<td>34.6</td>
<td>6.031</td>
</tr>
<tr>
<td>Observations</td>
<td>2.142</td>
<td>2.142</td>
<td>2.142</td>
<td>2.142</td>
<td>1.954</td>
<td>1.954</td>
<td>2.114</td>
<td>2.114</td>
<td>2.138</td>
<td>2.138</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Age Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Exposure Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets cluster by state.

** p<0.01, * p<0.05, + p<0.1

F Statistics are for post-2008*%State2008 and Year*post-2008*%State2008 Trends in all models.
Appendix

A Data on School Spending and Resources

The data on district level school finances is collected from the Census website. The underlying data comes from the Common Core of Data (CCD) School District Finance Survey (F-33). It consists of data submitted annually to the National Center for Education Statistics (NCES) by state education agencies (SEAs) in the 50 states and the District of Columbia. The purpose of the survey is to provide finance data for all local education agencies (LEAs) that provide free public elementary and secondary education in the United States. Both NCES and the Governments Division of the U.S. Census Bureau collect public school system finance data, and they collaborate in their efforts to gather these data. The F-33 data provides information on revenues, expenditures, and the number of students enrolled. Expenditures are reported in a number of categories including instructional spending, capital outlays, and administrative spending. Revenues are reported in several fine categories and aggregated to local, state, and federal sources. We CPI-adjust all spending variables to be in 2015 dollars and divide by district enrollment in the given year to obtain per-pupil spending variables.

The surveys are administered annually from 1992 onward. We link together multiple years of data to create a balanced panel school district data set. In constructing the data set, we found that the financial data contained some extremely large and small values. These values could be valid, but it is more likely that some districts incorrectly reported enrollments or expenditures. We therefore censored the data by deleting extreme values. First, we calculated the (unweighted) 99th and 1st percentile district in total per-pupil current expenditures for each state and year. We then capped values of districts with per-pupil expenditures at greater than 200 per-cent of the 99th percentile of per-pupil revenues or less than 50 percent of the 1st percentile.

For school spending categories (such as capital or instructional salaries), we replace values with missing where the CPI-adjusted per-student categorical spending value is more than twice the 99th percentile. We follow a similar strategy for reported staffing categories, which come from the NCES Common Core of Data LEA Universe surveys (CCD), replacing staffing values with missing if the total staffing variable or the staffing per student (or students per staff) is more than twice the 99th percentile. Note that not all states report staffing data in every year, and so our state-by-year analytic sample for staffing estimates is not balanced.

B Recession Intensity & Employment Data

Important to our identification strategy is controlling for the direct effect of broader recessionary economic conditions. For this purpose, we construct an index of recession severity and exposure. We exploit the fact that the impact of recession varied on basis of local industrial compositions and create a shift-share instrument, along the lines of Bartik (1991), which captures changes in economic conditions attributable to the onset of the recession.

To do so, we follow the steps broadly outlined in Yagan (2017). We retrieve average annual county-level employment data from the Quarterly Census of Employment and Wages (QCEW). Each county’s time-varying shift-share

---

16For instance, data for the school year 2015-2016 is available at https://www.census.gov/data/tables/2015/econ/school-finances/secondary-education-finance.html and data for the other years can be retrieved by modifying the appropriate part of the url.

17The QCEW program publishes an annual count of employment and wages reported by employers covering 98 percent of U.S. jobs, available at the county, MSA, state and national levels by industry. Average annual data were downloaded from the Bureau of Labor Statistics for each county and year from https://www.bls.gov/cew/datatoc.htm
shock is computed as the projected unemployment in each year, based on the interaction between the 2007 (pre-recession) employment composition by two-digit NAICS industry categories and the nationwide unemployment by the same groupings in that year. Formally, in county $c$ during year $t$ the instrument equals:

$$Bartik\ Predictor_{ct} = \sum_j \left( \frac{E_{jc}^{2007}}{\sum_j E_{jc}^{2007}} \times \text{National Unemployment}_{jt} \right)$$

where $j$ denotes a two-digit industry, $E_{jc}^{2007}$ denotes total employment in industry $j$ in county $c$ in 2007, and $\text{National Unemployment}_{jt}$ is the nationwide unemployment rate in industry $j$ in county $c$ in year $t$.

From the same dataset (QCEW) above, we also compile the annual total employment number in each county as an additional measure of economic status. As an additional economic indicator, we obtain county-level estimates of housing values from Zillow and use the January index for each year as an annual indicator of home prices.

To utilize these county-level controls in the analysis, we merged the county level data to district level data from the Common Core and F33 outlined above. It is not straightforward to match the records since the district to county mapping is not one to one in all cases. To overcome this complication, we use a spatial correspondence file from [http://mcdc.missouri.edu/websas/geocorr12.html](http://mcdc.missouri.edu/websas/geocorr12.html). This file identifies the exact percentage of a district’s area contained in each county. Using this information, we link the district level observations to the weighted average of values in the counties that the district overlaps. The resulting value approximates the shift-share predictor in the district, assuming industrial compositions are distributed uniformly within each county.

### C NAEP Data

We use restricted-use individual-level NAEP test score data from the National Center for Education Statistics. The NAEP is administered every other year to a population-weighted sample of schools and students. Schools are selected from 94 geographic areas, 22 of which are always the same major metropolitan areas. Students are selected randomly within the selected schools to complete the assessments. Note that our main results are invariant to the use of sampling weights. We infix the raw files to Stata, including all plausible score values per student, and restricting the sample to the NAEP reporting sample and public school students, except in the case of our public-private robustness test which includes private school students as well. The restriction to the reporting sample and public school students corresponds directly to the sample used to calculate state averages as reported publicly by NCES.

We link student scores to school districts using the reported NCES school identifier. Our dependent variable in all NAEP estimations is the mean of all reported plausible values for each student, year, grade, and subject, standardized to the base year of 2003. We restrict our analyses to the years 2002 and later, as NAEP sampling increased dramatically after 2001 and testing years became more consistent at this time.

### D Panel Construction

We compile the above data to create a student-by-year analytic dataset, where we match individual student test score records to the spending and economic characteristics of their district at the time of testing. Given that we derive variations in spending at the state-level, we collapse the individual-by-year data to the state level, weighting by the original reported NAEP sampling weight, ORIGWT. Our resulting state-by-year data is weighted by district and individual sampling representation from the NAEP. Using this method, we ensure that we are only relating school spending to districts that are also represented in the NAEP sample. We are able to obtain similar results as using the individual-by-year panel with a more manageable dataset, while simultaneously highlighting the source of our variation in school spending to be at the state level.
Table C1: NAEP Availability

<table>
<thead>
<tr>
<th>Year</th>
<th>4th Grade Math</th>
<th>8th Grade Math</th>
<th>4th Grade Reading</th>
<th>8th Grade Reading</th>
<th>Tested Students</th>
</tr>
</thead>
<tbody>
<tr>
<td>2015</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>430438</td>
</tr>
<tr>
<td>2014</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2013</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>575298</td>
</tr>
<tr>
<td>2012</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2011</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>619789</td>
</tr>
<tr>
<td>2010</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2009</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>571308</td>
</tr>
<tr>
<td>2008</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2007</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>620220</td>
</tr>
<tr>
<td>2006</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2005</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>589458</td>
</tr>
<tr>
<td>2004</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2003</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>642244</td>
</tr>
<tr>
<td>2002</td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td>240228</td>
</tr>
<tr>
<td>2001</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2000</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td>22246</td>
</tr>
<tr>
<td>1999</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1998</td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td>15391</td>
</tr>
<tr>
<td>1997</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1996</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td>10805</td>
</tr>
<tr>
<td>1995</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1994</td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td>6030</td>
</tr>
<tr>
<td>1993</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1992</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td>16719</td>
</tr>
</tbody>
</table>

Notes: NAEP availability by subject-grade in sample.
### Additional Tables and Figures

#### Table D1. High School Completion Rates by Age and Survey Year

<table>
<thead>
<tr>
<th>Census Year</th>
<th>Age 16</th>
<th>Age 17</th>
<th>Age 18</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1990</td>
<td>0.01230943</td>
<td>0.13015662</td>
<td>0.53738302</td>
<td>0.22661636</td>
</tr>
<tr>
<td>2000</td>
<td>0.01772539</td>
<td>0.12676409</td>
<td>0.51542109</td>
<td>0.21997019</td>
</tr>
<tr>
<td>2001</td>
<td>0.01429179</td>
<td>0.10515056</td>
<td>0.56417871</td>
<td>0.22787369</td>
</tr>
<tr>
<td>2002</td>
<td>0.01517202</td>
<td>0.1072825</td>
<td>0.54496402</td>
<td>0.22247284</td>
</tr>
<tr>
<td>2003</td>
<td>0.02590672</td>
<td>0.12117072</td>
<td>0.58480036</td>
<td>0.24395927</td>
</tr>
<tr>
<td>2004</td>
<td>0.02103613</td>
<td>0.11377089</td>
<td>0.60187531</td>
<td>0.24556077</td>
</tr>
<tr>
<td>2005</td>
<td>0.0151057</td>
<td>0.11357564</td>
<td>0.59844452</td>
<td>0.24237529</td>
</tr>
<tr>
<td>2006</td>
<td>0.01711242</td>
<td>0.11851842</td>
<td>0.64701247</td>
<td>0.2608811</td>
</tr>
<tr>
<td>2007</td>
<td>0.01438044</td>
<td>0.12582713</td>
<td>0.66160703</td>
<td>0.26727153</td>
</tr>
<tr>
<td>2008</td>
<td>0.01144139</td>
<td>0.11414181</td>
<td>0.64476204</td>
<td>0.25587385</td>
</tr>
<tr>
<td>2009</td>
<td>0.01080886</td>
<td>0.11462998</td>
<td>0.65839404</td>
<td>0.26127763</td>
</tr>
<tr>
<td>2010</td>
<td>0.0085846</td>
<td>0.1006652</td>
<td>0.63535827</td>
<td>0.24820269</td>
</tr>
<tr>
<td>2011</td>
<td>0.01308906</td>
<td>0.10959911</td>
<td>0.68750393</td>
<td>0.27006404</td>
</tr>
<tr>
<td>2012</td>
<td>0.00902476</td>
<td>0.11325162</td>
<td>0.67970443</td>
<td>0.26732694</td>
</tr>
<tr>
<td>2013</td>
<td>0.01162564</td>
<td>0.11823509</td>
<td>0.69098473</td>
<td>0.27361515</td>
</tr>
<tr>
<td>2014</td>
<td>0.01144387</td>
<td>0.11991394</td>
<td>0.69561648</td>
<td>0.2756581</td>
</tr>
<tr>
<td>2015</td>
<td>0.00996074</td>
<td>0.12137599</td>
<td>0.70614487</td>
<td>0.27916053</td>
</tr>
<tr>
<td>2016</td>
<td>0.0116414</td>
<td>0.118203</td>
<td>0.71201313</td>
<td>0.28061917</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>0.01392558</strong></td>
<td><strong>0.11608381</strong></td>
<td><strong>0.6314538</strong></td>
<td><strong>0.25382106</strong></td>
</tr>
</tbody>
</table>
Table D2: Predicted NAEP Score

<table>
<thead>
<tr>
<th>Mean NAEP Score</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Share Grade 8 Test-Takers</td>
<td>-0.539**</td>
</tr>
<tr>
<td>[0.0871]</td>
<td></td>
</tr>
<tr>
<td>Share Reading Test-Takers</td>
<td>-2.677**</td>
</tr>
<tr>
<td>[0.707]</td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate (4 year avg)</td>
<td>0.0117*</td>
</tr>
<tr>
<td>[0.00548]</td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate (1 year)</td>
<td>0.00372</td>
</tr>
<tr>
<td>[0.00442]</td>
<td></td>
</tr>
<tr>
<td>Log of Child Poverty Population</td>
<td>-0.0966**</td>
</tr>
<tr>
<td>[0.0263]</td>
<td></td>
</tr>
<tr>
<td>Log of Annual Average Employment</td>
<td>-0.0147</td>
</tr>
<tr>
<td>[0.0468]</td>
<td></td>
</tr>
<tr>
<td>Child Poverty Population</td>
<td>1.06e-05**</td>
</tr>
<tr>
<td>[1.85e-06]</td>
<td></td>
</tr>
<tr>
<td>Annual Average Employment</td>
<td>9.07e-07**</td>
</tr>
<tr>
<td>[2.10e-07]</td>
<td></td>
</tr>
<tr>
<td>District Enrollment</td>
<td>-5.69e-06**</td>
</tr>
<tr>
<td>[1.03e-06]</td>
<td></td>
</tr>
<tr>
<td>Log of Enrollment</td>
<td>0.0292</td>
</tr>
<tr>
<td>[0.0553]</td>
<td></td>
</tr>
<tr>
<td>Share of Student with Limited English Proficiency</td>
<td>-0.267+</td>
</tr>
<tr>
<td>[0.147]</td>
<td></td>
</tr>
<tr>
<td>Share of Students on Free/Reduced Lunch</td>
<td>-0.529**</td>
</tr>
<tr>
<td>[0.0961]</td>
<td></td>
</tr>
</tbody>
</table>

** p<0.01, * p<0.05, + p<0.1

Notes: Standard errors in brackets. Model includes year and state fixed effects
Table D3: 2SLS Exogeneity Tests - District Employment and Unemployment

<table>
<thead>
<tr>
<th></th>
<th>2SLS Regressions</th>
<th></th>
<th>2SLS Regressions</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1-Year District</td>
<td></td>
<td>4-Year District</td>
</tr>
<tr>
<td></td>
<td>Unemployment Rate</td>
<td></td>
<td>Unemployment Rate</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>-0.164</td>
<td>-0.951*</td>
<td>-0.0518</td>
</tr>
<tr>
<td></td>
<td>[0.242]</td>
<td>[0.461]</td>
<td>[0.184]</td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>-3.055</td>
<td>-18.99**</td>
<td>-1.254</td>
</tr>
<tr>
<td></td>
<td>[3.182]</td>
<td>[5.724]</td>
<td>[2.827]</td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>0.0129</td>
<td>0.0292+</td>
<td>0.0220</td>
</tr>
<tr>
<td></td>
<td>[0.0334]</td>
<td>[0.0159]</td>
<td>[0.0176]</td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>0.364</td>
<td>0.469*</td>
<td>0.342</td>
</tr>
<tr>
<td></td>
<td>[0.600]</td>
<td>[0.195]</td>
<td>[0.274]</td>
</tr>
</tbody>
</table>

Observations: 408
State Trends & State Fixed Effects: X X X X X X X X
Post 2008 Indicator: X X X X X X X X
Bartik IV: X X X X X X X X
Total and Child Population: X X X X X X X X
Year Fixed Effects: X X X

** p<0.01, * p<0.05, + p<0.1
Notes: Robust standard errors in brackets clustered by State.
Table D4: 2SLS Exogeneity Tests - Child Population

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>District Child Poverty</td>
<td>Log of District Child Poverty</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>-1.453</td>
<td>-823.3</td>
<td>-927.7</td>
<td>189.5</td>
<td>-0.107*</td>
<td>-0.0702*</td>
<td>-0.0687</td>
<td>-0.00622</td>
</tr>
<tr>
<td></td>
<td>[872.9]</td>
<td>[853.2]</td>
<td>[634.7]</td>
<td>[708.0]</td>
<td>[0.0531]</td>
<td>[0.0360]</td>
<td>[0.0606]</td>
<td>[0.0218]</td>
</tr>
<tr>
<td></td>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
<td>13</td>
<td>14</td>
<td>15</td>
<td>16</td>
</tr>
<tr>
<td></td>
<td>District Child Poverty</td>
<td>Log of District Child Poverty</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>-24.830+</td>
<td>-17.957</td>
<td>-15.207</td>
<td>3.019</td>
<td>-1.660+</td>
<td>-1.289**</td>
<td>-1.112</td>
<td>-0.121</td>
</tr>
<tr>
<td></td>
<td>[12.907]</td>
<td>[12.350]</td>
<td>[9.402]</td>
<td>[11.376]</td>
<td>[0.832]</td>
<td>[0.445]</td>
<td>[0.956]</td>
<td>[0.322]</td>
</tr>
<tr>
<td></td>
<td>17</td>
<td>18</td>
<td>19</td>
<td>20</td>
<td>21</td>
<td>22</td>
<td>23</td>
<td>24</td>
</tr>
<tr>
<td></td>
<td>Average District Enrollment</td>
<td>State Enrollment</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>-594.2</td>
<td>1.217</td>
<td>-450.4</td>
<td>2.420</td>
<td>0.00873</td>
<td>0.0308*</td>
<td>0.0162</td>
<td>0.0492**</td>
</tr>
<tr>
<td></td>
<td>[1.869]</td>
<td>[1.557]</td>
<td>[1.610]</td>
<td>[1.574]</td>
<td>[0.0330]</td>
<td>[0.0148]</td>
<td>[0.0382]</td>
<td>[0.0165]</td>
</tr>
<tr>
<td></td>
<td>25</td>
<td>26</td>
<td>27</td>
<td>28</td>
<td>29</td>
<td>30</td>
<td>31</td>
<td>32</td>
</tr>
<tr>
<td></td>
<td>Average District Enrollment</td>
<td>State Enrollment</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>14.780</td>
<td>3.141</td>
<td>-8.494</td>
<td>37.439</td>
<td>0.116</td>
<td>0.311</td>
<td>0.242</td>
<td>0.759*</td>
</tr>
<tr>
<td></td>
<td>[29.983]</td>
<td>[25.209]</td>
<td>[25.763]</td>
<td>[26.871]</td>
<td>[0.561]</td>
<td>[0.233]</td>
<td>[0.618]</td>
<td>[0.321]</td>
</tr>
<tr>
<td>Observations</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
<td>408</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Post 2008 Indicator</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Total and Child Population</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

** p<0.01, * p<0.05, + p<0.1

Notes: Robust standard errors in brackets clustered by State.
Table D5. Two-Stage-Least-Squares Regressions: Public School Spending and Public/Private School NAEP Scores

<table>
<thead>
<tr>
<th></th>
<th>Combined</th>
<th>Separate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean NAEP Scores</td>
<td>0.632*</td>
<td>0.624**</td>
</tr>
<tr>
<td></td>
<td>[0.244]</td>
<td>[0.234]</td>
</tr>
<tr>
<td>Log of 4-Year Public Spending</td>
<td>0.0655</td>
<td>0.214</td>
</tr>
<tr>
<td></td>
<td>[0.706]</td>
<td>[0.698]</td>
</tr>
<tr>
<td>Log of 4-Year Public Spending</td>
<td>0.808*</td>
<td>0.754*</td>
</tr>
<tr>
<td></td>
<td>[0.356]</td>
<td>[0.311]</td>
</tr>
<tr>
<td>Observations</td>
<td>388</td>
<td>388</td>
</tr>
<tr>
<td>State Trends &amp; State Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Post 2008 Indicator</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Bartik IV</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Total and Child Population</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>StateXPrivate Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors in brackets cluster by state (combined models) or State*Private (Separate Models).

** p<0.01, * p<0.05, + p<0.1
Table D6. Two-Stage-Least-Squares Regressions: School Spending and NAEP Scores, Full Controls

<table>
<thead>
<tr>
<th>Economic Controls</th>
<th>All Controls</th>
<th>Main Model</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Outcome: Mean NAEP Score</strong></td>
<td><strong>Outcome: Mean NAEP Score</strong></td>
<td><strong>Outcome: Mean NAEP Score</strong></td>
</tr>
<tr>
<td>1</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>4-Year Spending/1000</td>
<td>0.0471*</td>
<td>0.0481*</td>
</tr>
<tr>
<td>[0.0184]</td>
<td>[0.0207]</td>
<td>[0.0172]</td>
</tr>
<tr>
<td>Log of 4-Year Spending</td>
<td>0.676*</td>
<td>0.706*</td>
</tr>
<tr>
<td>[0.296]</td>
<td>[0.347]</td>
<td>[0.323]</td>
</tr>
</tbody>
</table>

Observations | 408 | 408 | 404 | 404 | 408 | 408 |
State Trends & State Fixed Effects | X | X | X | X | X | X |
Post 2008 Indicator | X | X | X | X | X | X |
Bartik IV | X | X | X | X | X | X |
Total and Child Population | X | X | X | X | X | X |
Year Fixed Effects | | X | | X | | |
Economic Controls | X | X | X | X | | |
Housing Value Index | | X | | | X | |

Notes: Robust standard errors in brackets cluster by state.
** p<0.01, * p<0.05, + p<0.1
Economic Controls include Unemployment Rate, log of Child Poverty Population, and log of Annual Average Employment.
Notes: This map shows the extent of variation in $(\%\text{State})$ across the United States.