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Do School Spending Cuts Matter? Evidence from The Great Recession
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**ABSTRACT**

During The Great Recession, national public-school per-pupil spending fell by roughly seven percent, and took several years to recover. The impact of such large and sustained education funding cuts is not well understood. To examine this, first, we document that the recessionary drop in spending coincided with the end of decades-long national growth in both test scores and college-going. 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 (which are more sensitive to the business cycle) 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 college-going rates. The test score impacts were larger for children in poor neighborhoods. Evidence suggests that both test scores and college-going were more adversely affected for Black and White students than Latinx students.

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# I Introduction

During The Great Recession, real pre-tax income fell by almost seven percent (Larrimore et al. 2015), national consumption as a percentage of GDP fell by 6 percentage points (Petev and Pistaferri 2012), and property values fell by about 18 percent.[1] Public schools are largely funded by a combination of property, income, and sales taxes. As such, public-school per-pupil spending fell by roughly seven percent nationally, by over ten percent in 7 states, and more than twenty percent in 2 states (Leachman et al. 2017). While per-pupil spending growth slowed during previous recessions, the Great Recession represents the largest and most sustained decline in national per-pupil spending in over a century (NCES 2017; Jackson et al. 2014).[2] While compelling recent evidence based on localized quasi-experimental variation indicates that increased school spending typically improves child outcomes (Jackson 2018a), the sheer magnitude of this historical episode allows for a unique examination of the extent to which large-scale and persistent education budget cuts may harm students in general, and poor children in particular. In this paper, we exploit plausibly exogenous reductions in public school spending induced by the Great Recession and examine the effect of school spending cuts on student test scores, and college-going rates.

We document that, nationally, the decline in school spending beginning with the onset of the recession is associated with the first time that average national test scores declined (in both math and reading) in the past 50 years (Figure 1). 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). The timing of the recessionary spending cuts also coincides slower growth in the number of first-time college entrants in the united states (Figure 1). A "naive" estimate based on the coincident time trends, is that a \$1,000 reduction in per-pupil spending (about an 8.3 % increase) was associated with an 8.7 percent of a standard deviation reduction in test scores and a 13.7 percent reduction in the number of first-time college entrants. While these patterns are suggestive, one must interpret coincident national trends with caution (Jackson et al., 2015). Because many other things may have changed nationally after the recession that could drive these associations, these relationships may not be causal.[3] To address these concerns and to isolate the impact of recession-induced 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.

[1] This is based on the Case-Shiller Index at the start and end of the recession. Note that housing prices has been on the decline before the onset of the Great Recession, so that this does not reflect the full peak to trough decline.

[2] Indeed, as of 2016, 25 states had not recovered to their inflation adjusted per-pupil spending levels, and by 2019 (more than a decade later) 12 states spent below their 2007 levels (link).

[3] In related work, Shores and Steinberg (2017) find that school districts in locations that were hardest hit by the recession 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.

Our identification strategy relies on the fact that states that were more reliant on state-collected revenues to fund public education (due to the particulars of their funding formulas) tended to experience larger school-spending reductions during the recession. This is for two distinct reasons: First, during the Great Recession, as payments increased for Medicaid and Unemployment Insurance, states allocated a smaller share of their budgets to K12 education – a crowd-out effect. This reduction is illustrated in the left panel of Figure 2. Second, state-collected revenues are based on tax bases that are more responsive to market fluctuations than are local or federal revenues (Sobel and Wagner; Evans et al. 2017) – a revenue effect. For both these reasons, while overall school spending declined after recession onset, revenues from state taxes fell the most sharply (right panel of Figure 2). In Section III, we discuss why this was true even though the Great Recession was associated with declining house prices. We show that a state's reliance on state revenues to fund public education is highly predictive of reductions in per-pupil spending after the recession. Exploiting this pattern, 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 that this is likely to be true in a few ways. First, our instrument predicts overall school spending primarily through its predictable impact on state revenues (as opposed to local or federal revenues). Also, conditional on *ex-ante* predictors of recession intensity, instrumented school spending is unrelated to measures of economic conditions such as unemployment or poverty rates. Finally, our main results persist in models that control for contemporaneous local economic conditions and even house prices directly.

Our first main outcome of interest is student test scores from the National Assessment of Educational Progress (NAEP). Our other main outcome is college-going obtained from the the Integrated Postsecondary Education Data System (IPEDS). We link these outcome data to state-level spending data from the Census F33 School District Finance Survey (CCD) and our instruments for public school K12 spending. Our final dataset straddles the recession and includes data between years 2002 and 2017. We proxy for a state's vulnerability to recessionary budget cuts using its reliance on state revenues to fund public K12 schools in 2008. Relative to each state's own time trend, we document a robust monotonic relationship between greater reliance on state funds in 2008 and the annual decline in per-pupil spending after the onset of the Great Recession. The pattern of greater deteriorating outcomes after the recession for states that are more reliant on state revenues is mirrored for both test scores and college-going rates. These declines in test scores and college-going track the recession-induced decline in per-pupil spending, and do not abate as the economy recovered – evidence that these impacts are driven by the spending changes rather than other effects of the recession *per se*. Using these patterns in an instrumental variables approach, on average, a \$1000 reduction in per-pupil spending led to about 0.045 standard deviations lower test scores (*p*-

value<0.01) and between a 2.6 and 6.2 percentage points lower college-going rate ($p$-value<0.05).

We test for heterogeneous effects in a few ways. First, we use the individual-level NAEP to compute the relationship between the district poverty rate and NAEP scores in each state in each year (following Card and Payne 2002 and Lafortune et al. 2018). We then estimate the impact of recession-induced spending changes on this slope. We find a consistent positive impact of spending on this slope – indicating that test scores in high-poverty areas were more adversely effected by the spending cuts than in low-poverty areas. We also examine impacts by student race. In general, the marginal impacts are similar for Black and White students. However, impacts are larger for Black and White students than for Hispanic students. We also explore heterogeneity in the college-going effect by institution type. We find increases at both 2-year and 4-year colleges. The 4-year college effects are concentrated at non-selective institutions and those with more part-time than full-time students. We also find large increases at minority serving institutions (particularly those serving Black students)– a pattern consistent with our test-score impacts by race.

To explore mechanisms, we examine what kinds of spending categories were most affected. In general, states cut spending across a broad range of categories. States that cut spending (as predicted by our instruments) hired fewer teachers, fewer aides, and fewer library staff. However, states responded to spending cuts by disproportionately cutting more from construction expenditures and less from core K12 spending. While construction spending makes up 6.9 percent of the average school's budget, it accounted for between 31 and 48 percent of the reduced spending. 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 may vary when there are spending increases versus decreases. This may reflect districts attempting to shield core operational spending (to some extent) or districts being more constrained in their ability (or desire) to cut spending on core school operations.[4] To determine whether the impact of these large recessionary spending cuts differ from those found in other studies (based largely on school spending increases and localized quasi-random variation) we summarize the studies outlined in Jackson (2018b).[5] Our estimated impacts are near the median effect across quasi-experimental studies that examine the impacts of spending (not tied to specific uses) on test scores – suggesting that the marginal effects of education spending cuts are largely symmetric to those of spending increases documented in other settings.

Our results shed some light on how the changing structure of school finance may have made student achievement more sensitive to the business cycle. While state taxes account for about

[4]These different marginal propensities to spending categories 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).

[5]We are deeply grateful to Claire Mackevicius for coding the studies and running the formal meta-analysis.

half of all public school spending, this was not always so. Prior to the 1970s, school districts in the United States funded public schools mostly through locally raised property taxes (Howell and Miller 1997; Hoxby). As such, poor districts tended to spend less per pupil than wealthier districts. The school finance reform movement (starting in the 1970s) lead to increased state collected taxes to maintain a more equitable distribution of school spending across districts. Because state funding is more vulnerable to recessionary cuts, one potential "side-effect" of the increased centralization of school funding (i.e. use of more state funding) is an increased vulnerability of education spending and, therefore, student achievement to fluctuations in the business cycle. Our results show this to be the case. Our results also suggest that this increased sensitivity *may be* most pronounced for high-poverty districts that rely more heavily on state aid.

Broadly, 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 the revenue sources used to fund public schools may have implications for student achievement. 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 new compelling 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 III describes the empirical strategy. Section IV presents the results. Section V concludes.

# II Data

We link several data sources for our analysis.[6] 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 2016. 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 2016, the average share of school revenue from federal, state, and local sources were 9.5%, 48.7%, and 41.7%, 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 funding that comes from state sources varies between 0% (Washington, D.C.) and 90% (Hawaii). The share of revenue coming from state sources is central to our empirical strategy. We discuss in

[6]We provide more detail on our data sources in the Appendix.

detail how we use this variable to classify states in Section III. On average, 85% of public school spending goes to current elementary and secondary spending, which broadly includes expenses for instruction and support services. Ten percent of public K12 education expenditures go towards capital, which includes construction, land, and equipment. Salaries and benefits (instructional and non-instructional) make up 68% of public school spending on average.

Test score data come from the 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. For the main analyses, we use publicly-available state-year average scores.[7] 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 A1). We focus on the period between 2002 and 2017.[8] To conduct subsample analyses, we also utilize restricted-use data files with student-level scores and demographics. The individual-level NAEP dataset includes 4.3 million individual NAEP scores from 11,477 school districts between 2002 and 2015.

Our college-going data are from the the IPEDS. These data report surveys submitted at the aggregate-level from postsecondary institutions. These data do not have student-level information. Institutions report on the number of first-time college freshman from each state in each year. By aggregating these data to the state of origin level, we obtain counts for the number of first-time freshmen from each state in each year. Using information on postsecondary institutions from IPEDS and the Carnegie Foundation, we compute enrollments by college type (2-year vs 4-year) and selectivity level. To compute college-going rates for these years, we obtain population counts by age in each state in each year from the American Community Surveys (ACS) from 2000 to 2016. Our college-going measure is the number of first-time college enrollees divided by the number of 17-year-olds in the state two years prior.

As additional variables, 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) and an annual measure of home values in each state from Zillow. 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).[9] Our state-year level dataset is summarized is summarized in Table 1.

[7] Obtained from http://apps.urban.org/features/naep/

[8] Because the 2017 school spending data was not available at the time of our analysis, we use 2016 school spending data for 2017 test scores.

[9] When these data are missing, we impute with the mean. This does not affect the results.

# III Empirical Strategy

The Great Recession led to a historic decline in per-pupil spending. As shown in Figure 1, the decline in school spending during the recession coincides with the first average national test score decline (in both math and reading) in the past 50 years, and with a slowing in the number of first-time college entrants in the United States. While these coincident trends are highly suggestive, they may not reflect causal relationships. As such, we seek to separate the effect of recession-induced school spending declines from that of the recession itself (and other potentially confounding policy or demographic changes). To this aim, we employ an instrumental variables approach. Our instrumental variables strategy relies on the fact that states that were more reliant on states revenues to fund public K12 schools were more likely to experience declines in school spending for reasons unrelated to the intensity of the recession in the state or other policy changes that may have occurred at that time. This basic pattern holds true for two related, but distinct reasons.

The first reason is that as the labor market worsened, demand for state-funded services such as unemployment insurance and Medicaid increased (Moffitt 2013). To cover these additional costs, many states cut their education budgets – resulting in a crowding out effect. This pattern is shown in the left panel of Figure V. Prior to the Great Recession, states spent about 27% of their budgets on K12 schools. However, after the Great Recession, this fell to about 23% and remained at that level through 2015. This pattern is not unique to the Great Recession and can also be observed with the early 2000s recession when the share of state spending going to K12 school fell from about 29% to about 27%. This suggests that, *even even if state revenues were unchanged during the recession*, states that were more reliant on state taxes to fund K12 schools would be more likely to experience education budget cuts. We refer to this as the crowd-out channel.

The second reason that greater reliance on state revenues to fund public schools was associated with deeper education spending cuts has to do with the tax bases. Revenue sources used to collect state taxes (mostly income and sales taxes) are more variable than revenues used to collect local taxes (mostly property taxes). Estimates suggest that income and sales taxes have a short-run elasticity (with respect to the tax base) of over 1 (Holcombe and Sobel 1995). In contrast, property taxes (which comprise the lion's share of local revenues) are more stable. Property tax revenues have a short-run elasticity (with respect to home values) of only between 0 and 0.4 because (a) taxes are collected on assessed values which follow market value with a considerable lag (Lutz 2008), and (b) policy-makers often offset declines in assessed values with higher tax rates (McMillen 2011; Lutz et al. 2010).[10] The greater sensitivity of state taxes (as opposed to federal or local taxes) to the business cycle suggests that, *even if there were no crowd-out channel*, states that were more reliant on state taxes to fund K12 school would experience deeper education budget cuts (as shown

[10] Evans et al. (2017) show that revenues from property taxes actually grew for three years after recession onset.

in Chakrabarti et al. 2015; Leachman and Mai 2014). We refer to this as the revenue channel.

We define the parameter $\Omega_s$ as the share of state K12 revenues in state $s$ that came from state sources in 2008.[11] Through both channels, $\Omega_s$ is meant to capture vulnerability to recessionary school spending cuts.[12] We classify states based on the source of the revenue as reported in Annual Survey of School System Finances at the Census Bureau. For almost all states, this classification captures both channels outlined above. An example of a highly vulnerable state is Hawaii. In 2008, Hawaii received 85% of its education funding from the state (making it vulnerable to the crowd-out effect), and 75% of its state revenues came from income or sales taxes (making a large share of its revenue sensitive to the business cycle).[13] An example of a less vulnerable state is Illinois. In 2008, Illinois received 34% of its K12 spending from the state (making it less vulnerable to the crowd-out effect), and only 30% of its revenues come from income or sales taxes (making a relatively small share of its revenue sensitive to the business cycle). To further elucidate our instrument, we discuss a slightly less straightforward state – California. Due to California's Proposition 98, the state collects some (but not all) locally raised property tax revenue that goes into the State general fund. From the centralization perspective, one may consider the locally raised taxes that are collected by the state to be state revenue. However, because the Proposition 98 funding is typically earmarked specifically for education and cannot be borrowed from, it makes more sense to classify these funds as local (i.e. less sensitive to the centralization channel). Also, from the revenue cyclicality perspective, these property taxes are less variable than income and sales taxes so that one may wish to classify them as local taxes. For both these reasons, we follow the Census Bureau categorization and classify these funds as local funds. Doing so, we classify 58% of California's education revenue as coming from state sources. Note that all of our results are robust to how we classify these potentially ambiguous funds, and to dropping California from the sample.[14]

[11] Following Evans et al. (2017), we compute the share of K12 revenues in state $s$ that came from state sources in 2007-2008 (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.

[12] Figure A3 shows that $\Omega_s$ is evenly distributed across the geographic regions of the nation.

[13] Information on revenue sources come from (Saito, 2008)

[14] There is one "state" for which classification is unclear, which is the District of Columbia (DC). In essence, DC is not a state, so by definition does not have state taxes. Because DC serves as the federal capital, the constitution grants the United States Congress jurisdiction over the District. This is evidenced by the fact that the Federal government covers about one-quarter of DC's budget, and the fact that DC received large amounts of increased Federal funds during the recession. As such, from the centralization perspective, we follows the categorization employed by the Census Bureau and classify municipality funds that support education as "local" and set state revenues equal to 0. This suggests that D.C. experiences little vulnerability to recessionary spending cuts due to our instrument. Based on Figure 2, this categorization appears to hold empirically. Because this is a judgment call, we show that all of our results are robust to dropping data from D.C. entirely.

Through both the crowd-out and revenue channels, while overall school spending declined after recession onset, revenues from state taxes fell most sharply (Figure 2). As such, states that were more reliant on state revenues to fund public education in 2008 (due to the particulars of their school funding formulas) tended to experience larger school spending reductions during the recession. 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. 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. The basic pattern of larger spending cuts in state that were more reliant on state revenues to fund public education motivates our instrumental variables approach. We use $\Omega_s$ as an exogenous shifter 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 asses 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.

## III.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 college-going. We show that this is the case.

In Figure V, we present an event-study for the recession's effect on K12 spending, average NAEP scores, and college-going rates by states' reliance on state revenue sources. We estimate models as below on our state-level panel for various outcomes $Y$ for each state $s$ in each year $t$, $Y_{st}$.

$$Y_{st} = \sum_{t=2002}^{2017} \beta_t \cdot (I_{g>1,s} \times I_{T=t}) + \gamma_s + (\tau_s \times T) + \upsilon_{st} \tag{1}$$

In 1, $I_{T=t}$ 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). To account for differences across states we include state fixed effects $\gamma_s$. To compare changes in each state's outcome to its own time trend, we include the state-specific linear time trends $\tau_s$. The variable $\upsilon_{st}$ is a random error term. The coefficients $\beta_t$ map out the differences in outcomes between states with low and high $\Omega_s$ in each year (relative to each states own pre-recession intercept and linear time trend). We estimate this model by OLS on per-pupil spending (in 2015 dollars), average state-level NEAP scores, and college-going rates. We plot these coefficients along with the 95% confidence interval for each coefficient estimate in Figure 4 where the reference year is 2007.

The event study for test scores are in the top left panel. Prior to the recession, school spending was on a similar trajectory in areas with different levels of $\Omega_s$, but after the recession, states with heavy reliance on state revenues experienced a clear linear decline in per-pupil spending. While there is no visual evidence of a level shift, the decline appears to be roughly linear in time since recession onset. Average NAEP scores (top right) and college-going (bottom left) followed a very similar pattern. Student test scores and college-going rates in states with greater dependence on state revenues to fund public K12 schools declined following the recession, relative to other areas. While the individual point estimates are only significantly different from 2007 levels after 2013, as we show later, the linear post trend in these outcomes is statistically significantly different from the pre-trend. Overall, the patterns indicate that outcomes in states that relied on revenues raised from primarily state sources were on a similar trajectory as other states until the onset of the recession. However, in states with greater reliance on state revenues for public school funding (and which 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.

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. We present three complementary identification strategies that rely on somewhat different sources of variation but all reveal the same overall pattern.

*Linear IV with State Linear Trend Controls*: Our first approach is to model the change in trend to be linear in state reliance on state revenues. To capture this trend change variation in spending parametrically, we model school spending as declining linearly starting with recession onset (as indicated in Figure 1). In our first approach we model the change in this linear trend as varying linearly with $\Omega_s$. Because the slope change may not be linear in $\Omega_s$, we relax this restriction in subsequent models for additional precision. We estimate equations of the following form by 2SLS.

$$PPE_{st} = \pi_1(\Omega_s \times I_{post} \times T) + \rho_{11}(\Omega_s \times I_{post}) + \rho_{12}(I_{post}) + \delta_1 C_{st} + \alpha_{1s} + (\tau_{1s} \times T) + \varepsilon_{1st} \quad (2)$$

$$Y_{st} = \beta \cdot (PPE_{st}) + \rho_{21}(\Omega_s \times I_{post}) + \rho_{22}(I_{post}) + \delta_2 C_{st} + \alpha_{2s} + (\tau_{2s} \times T) + \varepsilon_{2st} \quad (3)$$

The endogenous treatment, $PPE_{st}$, is per-pupil school spending in state $s$ during year $t$. The outcome $Y_{st}$ is either (a) the average standardized NAEP test scores for students in state $s$ in year $t$, or (b) the college-going rate for 17 year olds in year $t-2$ who were expected to graduate from high school 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. $T$ is a scalar in the calendar year, and $I_{post}$ is a post-recession indicator denoting all years after 2008. 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. This accounts for any pre-recession time-trend differences between high and low $\Omega_s$ states. To capture the roughly linear-in-time decline in spending for more reliant states after the recession, our excluded instrument is the interaction between reliance on state funding in 2008 and the post-recession change in linear time trend, $\Omega_s \times T \times I_{post}$. To account for any level shift in outcomes at recession outset, we also include $I_{post}$ and $\Omega_s \times I_{post}$ as additional controls. $\varepsilon_{2st}$ and $\varepsilon_{2st}$ are random error terms.

Because the recession may have had ill economic effects through channels other than school spending, it is important that we control for underlying predictors of recession intensity itself. In principle, the *POST* indicator would control for changes in outcomes that occur after the recession. However, because the recession was not a permanent shift but a transitory spike in unemployment it is important to account for this transitory time pattern to control for the impacts of the recession 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. Our models include the predicted unemployment in year $t$, through $t-3$ to account for dynamic impacts of unemployment. Appendix Figure A4 shows how the Bartik predictors evolve over time to predict transitory changes in outcomes during the recession. As we show in Section IV, these controls remove any systematic correlation between our instrument and economic conditions that may predict our outcomes.

*Group IV with State Linear Trend Controls*: In our second approach, to increase the precision of our estimates, we relax the linearity of the relationship between $\Omega_s$ and the change in the slope after the recession. 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 $s$. We then replace the single scalar variable $\Omega_s$, with three indicators for states in the low, middle, and high groups. Formally, using the state-by year level panel, we estimate systems of equations of the following form by 2SLS.

$$PPE_{st} = \Sigma_{g=1}^{3}[\pi_{1g} \cdot (I_{gs} \times I_{post} \times T)] + \Sigma_{g=1}^{3}[\phi_{1g} \cdot (I_{gs} \times I_{post})] + \delta_1 C_{st} + \alpha_{1s} + (\tau_{1s} \times T) + \varepsilon_{1st} \quad (4)$$

$$Y_{st} = \beta \cdot (PPE_{st}) + \Sigma_{g=1}^{3}[\phi_{2g} \cdot (I_{gs} \times I_{post})] + \delta_2 C_{st} + \alpha_{2s} + (\tau_{2s} \times T) + \varepsilon_{2st} \quad (5)$$

All common variables are as defined in equation (2) and (3). The three excluded instruments are the interactions between the group indicators and the post recession linear time trend, $I_{gs} \times T \times I_{post}$. Models also include a level shift after the recession for each group ($I_{gs} \times I_{post}$) as controls.

*Group IV with State Linear Trend Controls and Year Fixed Effects*: In our third, and most restrictive models, we estimate the group IV model while including individual calendar-year fixed effects (in lieu of the other controls). This is a flexible way to account for any time effects that may affect student outcomes across all states. The resulting 2SLS model is as below.

$$PPE_{st} = \Sigma_{g=2}^{3}[\pi_{1g} \cdot (I_{gs} \times I_{post} \times T)] + \Sigma_{g=2}^{3}[\phi_{1g} \cdot (I_{gs} \times I_{post})] + \theta_{1t} + \alpha_{1s} + (\tau_{1s} \times T) + \varepsilon_{1st} \quad (6)$$

$$Y_{st} = \beta \cdot (PPE_{st}) + \Sigma_{g=2}^{3}[\phi_{2g} \cdot (I_{gs} \times I_{post})] + \theta_{2t} + \alpha_{2s} + (\tau_{2s} \times T) + \varepsilon_{2st} \quad (7)$$

All variables are as defined in equations (4) and (5) and $\theta_{1t}$ and $\theta_{2t}$ are year fixed effects for the first and second stage, respectively.[15] All our main result are robust across these three models.

## III.2 First Stage and Reduced Form

Table 2 presents the first-stage relationship between the excluded instruments and per-pupil spending (in thousands) on our state-year panel. Column 1 presents results from the Linear IV model without any controls, and column 2 presents the first stage with the Bartik controls. As one can see, the point estimates are largely similar. Without controls (column 1) the change in slope is -0.546 (se=0.069) and with the Bartik controls (column 2) the change in slope is slope is -0.344 (se=0.285). while the estimated change in slope is similar across the two models, the standard error is four-times as large with the Bartik controls. Because the Bartik predictors are constructed to predict the recessionary spike in the unemployment rate, it is very highly correlated with the *POST* indicator - which has a $p$-value above 0.8.[16] To remove this likely collinearty, we estimate

[15] In the interest of parsimony, we do not include the Bartik predictors in these models. Note that the results are unchanged from their inclusion.

[16] A regression of the Bartik predictor on the *Post* variable controlling for state-trends yields a t-statistic of 65.74.

the same model without the *POST* indicator. In this model (column 3) the change in slope is -0.402 (se=0.0991) – very similar to the estimates slope with the *POST* indicator. As we will show in Section IV, all the 2SLS results yield similar point estimates both with and without the *POST* indicator, and all our falsification checks look good without the *POST* indicator. Accordingly, this is our preferred Linear IV model. The fact that the *POST* indicator is not significant is not surprising given the visible lack of a level shift at recession onset in Figure V. This point estimate suggests that a state with a state revenue share 10 points higher (in 2008), would have had per-pupil spending fall by $(0.1) \times (0.402) \times (\$1000) = \$40$ per year. Between 2009 and 2015, this would amount to a difference of about \$240 per pupil. In this model, the first stage F-statistic is 16.41 – sufficiently large for reliable statistical inference.

Column 4 presents the coefficients on the excluded instruments from the group IV model when the dependent variable is the level of spending in thousands without the Bartik controls. Consistent with the linear model, there is a monotonic relationship between reliance on state revenues and the relative decline in per-pupil spending after recession onset. That is, the coefficient on LOW$\times$POST$\times$YEAR is positive and significant, that on MID$\times$POST$\times$YEAR is negative and significant, and that for HIGH$\times$POST$\times$YEAR is even more negative and significant. The point estimates indicate that relative to pre-trend, school spending increased by roughly \$280 per year in states that were not very reliant on state revenues, declined by about \$273 per year in states that had moderate reliance on state revenues, and declined by about \$524 per year for the states that were most reliant on state revenues to fund public schools. Column 5 shows the first stage coefficients with the inclusion of the Bartik controls. In this model all of the slope change coefficients are more positive, but the monotonic relationship remains– in fact, the differences in slope across the groups is virtually unchanged with the Bartik predictor. The Kleibergen-Paap rk Wald F statistic for the three excluded instruments presented is 22.74 – indicating some efficiency gains from using the less restrictive model over the linear IV model. Finally, in our most restrictive models (Group IV with year fixed effects) the patterns are largely the same. With year fixed effects, all identification is based on comparisons within each year. As such, there are only two groups for which differential slopes are estimated. In column 6, relative to the low reliance group (the omitted group), after the recession onset the middle reliance group spent about \$553 less per year (p-value$<$0.01), and the high reliance group spend about \$805 less per year (p-value$<$0.01). The F-statistic for the three excluded instruments presented is 26.73 – indicating a strong first stage.

Echoing the patterns presented in Figure 4, we present the reduced form estimates for average NAEP scores in columns 7 through 9. The basic pattern of the change in trends in spending are mirrored for NAEP scores. Conditional on Bartik controls (column 7), the coefficient on the excluded instrument is -0.0167 indicating that a state with a state revenue share 10 points higher, would have had test scores fall by $(0.1) \times (0.0.0167) = 0.167$ of one percent of a standard deviation per year. Between 2009 and 2015, this would amount to a difference of about 1 percent of a standard deviation. This slope change is significant at the 1% level. In the Group IV model, we see a similar pattern. In the models with the Bartik controls (column 8), and those with year fixed effects (column 9), the test score declines are larger for the states that are more reliant on state revenues. Relative to states with low reliance on state revenues, after recession onset NAEP scores fell by roughly $0.0269\sigma$ per year in medium reliance states and fell by by roughly $0.034\sigma$ per year in highly reliant states. In sum, the test score patterns mirror those for spending.

The basic pattern of the change in trends in spending are also mirrored for college-going. We present the reduced form estimates for college-going rates in columns 10 through 12. Conditional on Bartik controls (column 10), the coefficient on the excluded instrument is -0.0348 indicating that a state with a state revenue share 10 points higher, would have had college-going rates fall by $(0.1) \times (0.0.0348) = 0.348$ percentage points per year. Between 2009 and 2015, this would amount to a difference of about 2.1 percentage points. This slope change is significant at the 1% level. In the Group IV model, we see a similar pattern. In the models with the Bartik controls (column 8), and those with year fixed effects (column 9), the college-going declines are larger for the states that are more reliant on state revenues. Relative to states with low reliance on state revenues, after recession onset college-going rates fell by roughly 0.023 per year in medium reliance states and fell by by roughly 0.031 per year in highly reliant states.

# IV Results

***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 1 through 6 of Table A17 present the 2SLS estimated effects of the level of spending (in thousands of dollars) on state average standardized NAEP scores. To provide a basis for comparison we also estimate OLS models that do not instrument for school spending (appendix Table A4). In OLS models with no controls, the estimated coefficient on per-pupil spending is 0.0109 ($p$-value$<$0.01) – indicating that with no controls a $1000 increase in per pupil spending is associated with about a 1 percent of a standard deviation increase in NAEP test scores. Adding the Bartik controls reduces this coefficient to 0.00795, and in models with year fixed effects, the coefficient falls to 0.00465 and is no longer statistically significant at traditional levels.

Model 1 of Table A17 presents the effect of per-pupil spending on NAEP scores. This is the most parsimonious model with state fixed effects and state-specific linear trends (i.e. no controls). The 2SLS coefficient is 0.0471 ($p$-value$<$0.01). Adding the Bartik predictors for state unemployment, the point estimate increases to 0.0698, but is no longer significant owing to a very large standard error (almost 9 times larger than without controls). To help increase precision, we drop the *POST* indicator (which is not significant in the NAEP model). The resulting 2SLS estimate is 0.0415 and is significant at the 5 percent level. Column 4 presents the results of the Group IV estimator without Bartik controls (but including the *POST* indicator), mirroring the specification in model 1 with the group instrument. The point estimate is .0502 (p-value<0.01). Column 5 presents the results of the Group IV estimator with both the Bartik controls and controls for post recession changes in outcomes. The point estimate is 0.0236 (p-value<0.05). Adding year fixed effects to this model in column 6, the coefficient on spending is 0.044 (p-value<0.01). Across all 6 specifications, the point estimates are largely similar and range between 0.0236 and 0.0689. We take models 3, 5, and 6 to be our preferred specifications. Across these models, the point estimate is roughly 0.036. This indicates that a $1000 reduction in per-pupil spending dues to the recession, led to a decline in test scores of about 3.6 percent of a standard deviation.[17]

***College-Going Rates:*** We show the estimated 2SLS results on college-going in Table A17. As with test scores, as a basis for comparison we also estimate OLS models that do not instrument for school spending (appendix Table A4). In OLS models with no controls, the estimates coefficient on per-pupil spending is 0.0112 ($p$-value<0.01) – indicating that with no controls a $1000 increase in per pupil spending is associated with about a 1.1 percentage-points higher college-going. Adding the Bartik controls has little effect on this estimate, and in models with year fixed effects, the coefficient falls to 0.00868 and remains statistically significant at the 5 percent level.

As with the NAEP results, the 2SLS estimates on college-going are larger than the OLS. Model 7 of Table A17 presents the most parsimonious 2SLS model with state fixed effects and state-specific linear trends (i.e. no controls). The 2SLS coefficient is 0.0426 ($p$-value<0.01). Adding the Bartik predictors for state unemployment, the point estimate increases to 0.0574, ($p$-value<0.10). When we drop the *POST* indicator (which is not significant in the college-going model), the point estimate is 0.0624 ($p$-value<0.01). Column 11 presents the results of the Group IV estimator with both the Bartik controls and the post recession indicator. The point estimate is 0.026 ($p$-value<0.01). Adding year fixed effects to in column 12, the coefficient on spending is 0.0279 ($p$-value<0.01). Across the three preferred models (9, 11, and 12), the point estimate is roughly 0.039. This indicates that a $1000 reductions in per-pupil spending due to the recession, led to a decline in the college-going rate of about 3.9 percentage points. In our most conservative model, a $1000 reduction in per-pupil spending led to a 2.6 percentage points decline in the college-going rate.

The national trends in Figure 1, the event-study plots presented in Figure 4, the reduced form patterns in Table 2, and the 2SLS results presented in Table 4 all indicate a strong and robust association between recession induced spending reductions and deteriorating test scores and college-

[17] As is found in other settings, our test score impacts are larger for math than for reading. We also find evidence that impacts are larger on 4th grade scores than 8th grade scores. See Appendix Table A16.

going. However, if our results are to be interpreted causally, it is important that our effects work through the proposed channels, and are driven by any other ill-effects of the recession. We present several empirical tests to support a causal interpretation in Section IV.1 below.

## IV.1 Robustness checks and Falsification Tests

**A.** Our identification strategy relies on the assumption that the reason for the systematic association between school spending and test scores is a school spending effect driven primarily by changes in state revenues. To show that this is the case, we estimate our first stage model on revenues collected from different sources (state, local, federal). We report the coefficients in Table 3. We present our three preferred models (Linear IV excluding the *POST* indicator, the Group IV with all controls, and the Group IV with year fixed effects). Columns 1 through 3 present the reduced form impacts on federal revenues, columns 4 through 6 present the reduced form impacts on state revenues, and columns 7 through 9 present the reduced form impacts on local revenues. To summarize the results, for each specification we test whether the slope is the same for the high and low reliance groups (for the linear IV this is simply the test of significance of the excluded instrument). In none of the models, can one reject that the slope is the same for local revenues. For federal revenues, the slope is statistically significant in the Linear IV model but have $p$-values above 0.3 in both the Group IV models. In contrast, the slope difference is statistically significant for state revenues in all models. In sum, consistent with our proposed mechanisms, the results reveal that our instruments operate though their systematic impacts on state revenues.

**B.** We now test whether instrumented school spending predicts economic conditions. We have several variables that measure economic conditions, so to avoid problems with multiple hypothesis testing we create predicted NAEP scores and college-going rates based on these economic conditions. We regress each outcome on the state unemployment rate, the 4-year moving average of the state unemployment rate (to account for cumulative exposure), the total county employment, the percentage of residents living in poverty, the percentage of children living in poverty, and the logs of the child population, total population, and child population living in poverty. To capture the within-state variation in outcomes associated with these economic variable, this model includes state fixed effects and state-specific linear time trends. This model yields a within-entity $R$-Squared of 0.211 for NAEP scores and 0.08 for college-going rates – indicating that these economic variables explain a meaningful fraction of the within state variation over time (after accounting for shared time effects).[18] If our instruments impact predicted scores similarly to actual scores it would imply that much our 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

[18]The estimation results for the predicted scores are presented in Appendix Table A2. A binned scatter-plot of actual outcomes against the predicted outcomes are in Appendix Figure D.

score impacts are not driven by underlying economic conditions. We present estimated impacts on predicted outcomes for the same specifications as our main results in the lower panel of Table A17. In models with controls, school spending (as predicted by our instruments) is unrelated to predicted NAEP scores across all specifications. Moreover, the magnitudes are small. Looking to predicted college-going, the Linear IV yields small positive impacts on predicted college-going while in the Group IV models, the estimated impacts on predicted outcomes is small and not statistically significant – suggesting no systematic association between instrumented school spending and economic conditions that predict outcomes.[19] This stands on stark contrast to the large and robust impacts on actual outcomes– lending further credibility to our research design.

**C.** Despite the previous tests, one may still worry that other state-level changes drive our estimates. If true, one would observe similar patterns 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. We show this in Appendix Table A9, columns 1 through 3. While the estimated impacts are noisy, there is no evidence of any systematic impacts on private school scores. That is, while the impacts on public school scores are consistent (and significant) across all specifications, the impacts on private scores are never statistically significant and change sign across specifications– precisely what one would observe if there were no effect.

**D.** To assuage lingering concerns that our estimates are confounded by underlying recession intensity, Appendix Table A11 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 models (columns 4, 8, and 12) include total population, child population, state unemployment rate, the state employment level, child poverty rate, total poverty rate, and average house values. In models with these controls, the estimates are largely similar to that of our preferred models.

**E.** 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. Because our Group IV model puts states into groups rather than using the continuous measure, that model is less susceptible to outlier bias. However, to systematically show that our results are

[19]We also present impacts on the individuals economic outcomes in Appendix Table A10. 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.

not driven by some small number of states, we conduct permutation tests in which we estimate our least restricted Linear IV models and our most restricted Group IV with Year effects models, excluding one state at time, two states at a time and three states at a time (note that this involves running about 250 thousand regressions). In sum, in none of the models is the estimated coefficient on spending negative. While not all models yield statistically significant impacts, there is no combination of three excluded states that will yield a negative spending effect (this includes HI, DC, or any other state).[20] To be more representative of the student population and address any concern that small states (in term of population) drive the findings, we also estimate our main models using population-weighted least squares. We use both person weights as well as weights derived from the child population in the state. The results are presented in Appendix Table A14. The results are qualitatively similar. We take the robustness of the positive relationship across the different models for both outcomes 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.

## IV.2 Evidence on Spending Mechanisms

To better understand mechanisms, 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 the 2SLS models. Using our three preferred models, we regress the level of spending in each sub category (in per-pupil units) on the overall (instrumented) level of spending (in thousands per-pupil). The resulting coefficient reveals 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. The vast majority (95%) of overall spending is divided between capital (9.6%) and operating (85.3%) expenditures. For every dollar in per-pupil spending cuts, districts decrease capital spending by \$0.27-\$0.57 and current elementary/secondary spending by \$0.35-\$.78. While capital spending accounted for 9.6% of overall annual spending, it made up 27-57% of overall reductions, 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 affected reveals that all of the reduction in capital spending was from construction (columns 7-9). 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

[20] We present results dropping DC, HI, or CA specifically in Appendix Table A13.

from core operating expenses. Indeed, elementary and secondary current spending accounts for 85.3% of overall spending, but only about 35-78% of spending cuts.

Columns 10 to 12 and Appendix Table A8 show effects across additional spending sub-categories. For every dollar in spending cuts, districts reduced instructional spending by $0.45-$0.62 on average. Roughly half of this reduction can be accounted for by reduced instructional salaries, while reduced instructional benefits make up most of the rest of these cuts (see Appendix Table A8). While support services account for about 30 percent of spending on average, each dollar of cuts was associated with, at most, 0.17 fewer dollars spent on support services (columns 1-3 of Table A8). 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.

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 top panel examines impact on the log of overall staff counts. Looking at the most conservative model, on average a $1000 decline in spending was associated with hiring 4% fewer teachers, 5.8% fewer teachers aides 13% fewer guidance councilors, and 10% fewer library staff. If one controls for student enrollment, these effect are smaller and are only marginally statistically significant. In the conservative model, conditional on total enrollment, a $1000 decline in spending was associated with hiring 2% fewer teachers, 3% fewer teachers aides 11.6% fewer guidance councilors, and 10% fewer library staff. The bottom panel shows the 2SLS estimates of per-pupil spending on student:staff ratios. These impacts are less precise but are consistent with the reduction in staffing conditional on enrollment.[21]

## IV.3 Distributional Impacts

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 follow Lafortune et al. (2018) and measure the relationship between the district poverty rate and test scores within a state. That is, for each year of the NAEP, we use the restricted data to compute the slope between the district poverty rate and individuals NAEP scores. This is a measure of test score regressivity. To

[21] In conservative models, a $1000 decline in spending led to roughly 0.3 more students per teacher and .877 more student per teachers aide (neither effect is statistically significant). However, a $1000 decline in spending led to roughly 81 more students per guidance counselor and 66 more students per library staff (both effects are statistically significant at the 10 percent level).

avoid using any classification that could have been affected by the recession itself, we use each districts poverty rate in 2007 (before recession onset) in all years. A large negative slope would indicate that higher poverty districts perform worse relative to low poverty ones, and a slope of zero would imply that the district poverty rate is unrelated to NAEP scores. The average slope in our data is -3.55. This suggests that on average, in the typical state a district with a poverty rate of zero would have test scores about 1 standard deviations higher than a district with a poverty rate of 30%. The standard deviation of this slope is 1.17.

The bottom right panel of Figure V presents the event study of this slope. While there is little evidence of a pre-existing trend differences in high and low $\Omega$ states before the recession, there is clear evidence of a more negative slope after the recession for states that were more reliant on state taxes to fund public schools. We summarize this patters in our 2SLS models in Table 7. Columns 1 through 3 show the three main specifications where the dependent variable is the slope. The estimated slope is large, statistically significant, and similar across all models. Across all models, a \$1000 decrease in per-pupil spending led to a decrease in the slope of about 0.4. This is one-third of a standard deviation change in the test-score regressivity. For a typical state, this implies that the test score gap between district with a poverty rate of zero and one of 30% would fall by about 0.12 standard deviations (in student achievement units). In sum, the analysis provides compelling evidence that the achievement losses associated with recessionary public school spending cuts were disproportionately experienced by those in high poverty districts.

Using state level averages as reported in the publicly available NAEP, we examine impacts by student race. Results are reported in Table 7, columns 4-12. Because different states may have very different shares of students by race, we weight each state estimate by the population of residents of that race (from the IPUMS). The point estimates do vary across models, but one key pattern does emerge. While the spending effects are positive for both whites and blacks, they are small and of inconsistent sign for Hispanic students. This suggests that the test score reductions are driven largely by Black and White students. Among White and Black students, the point estimates do not allow one to reject that they marginal spending effects were the same.

## IV.4 College Type

Using the IPEDS data, we are able to shed further light on the kinds of colleges that students were less likely to attend due to the recessionary spending cuts. We estimate our main specifications on the percentage of age-eligible students who attend colleges of a particular type. The results are in Table 8. For each institutional category, we calculate the share of all students who attended that type of college by students' home states (we also report marginal impacts as percent changes relative to the average enrollment share for each category). The overall decrease in college-going (associated with a \$1,000 decline in per-pupil spending) was about 2.8 percentage points – a 5.3%

decline relative to the mean (column 3, panel 1). The results in Table 8 suggest that the overall decrease in college going was roughly similar in magnitude across 2-year, 4-year, public, and private institutions. In our most conservative model with the group IV and year fixed effects, enrollments declined by 4.1-6.7% for each of these types of institutions when students experienced a $1,000 decline in per-pupil spending, though not all estimates are statistically significant. However, we do find some differences by school selectivity. Enrollment declines were relatively larger at 4-year institutions where more than 40% of students are attending part-time (32.5% decline per every $1,000 in spending cuts) than at 4-year institutions where less than 40% of students are part-time (3.5% decline per every $1,000 in spending cuts) – only the effect for the less selective schools is significant at the 10 percent level. At selective or highly selective 4-year institutions, effects were also relatively small and insignificant (2.9% decline per every $1,000 in spending cuts), suggesting that enrollment declines were not concentrated in higher selectivity institutions. [22]

Given that there were test score effects for both minority students and white students, one might expect college-going impacts on both Minority Serving Institutions (MSIs) and non-Minority Serving Institutions. While we do find effects for both MSI's and non-MSI's, enrollment declines were much larger at MSI's. Across all specifications, a $1000 reduction in spending led to about a 17% relative decline in attendance at MSI's ($p$-value$<$0.01 or $p$-value$<$0.1). At non-MSI's, the same spending reduction was only associated with between a 2.1 and 9.2% relative decline in enrollments. Given that the test score impacts were larger for Black students and near-zero for Hispanic students, one might expect the college-going impacts to be driven by MSIs that enroll lower shares of Hispanic students. To test this, we compute the effect on enrollments at specifically Hispanic-Serving Institutions (HSI's) and MSI's that were not also HSI's. Using this measure (columns 4-9 of panel 4), we find large, significant effects on enrollments at non-HSI MSI's and smaller, insignificant impacts at HSIs – consistent with the larger test score impacts for Black students. In our most conservative model with group IV and year fixed effects, a $1,000 decline in per-pupil spending led to a 20.2% decline in enrollment at non-HSI MSI's ($p$-value$<$0.05) and a statistically insignificant 10% decline in enrollment at HSI's. [23]

# V Discussion and Conclusions

The policy and scholarly debates regarding whether public school spending matters have been going on for decades. Recent studies using idiosyncratic quasi-random variation in school spending tend to find that increased school spending improves student outcomes (Jackson et al. 2016; Candelaria and Shores 2017; Lafortune et al. 2018; Card and Payne 2002 Hyman 2017; Miller 2017; Gigliotti and Gigliotti 2017). However, despite a growing consensus that money *can* matter, there

[22] Selective/Most Selective institutions have incoming student with test scores between the 40th and 100th percentiles. Additional enrollment breakdowns by selectivity are shown in Appendix Table A15.

[23] We also compute the enrollment rate for Black Serving Institutions (see Table A15). Results are similar.

has been no study on how schools respond to large persistent cuts to spending and large spending cuts impact student outcomes. The Great Recession led to the largest and most sustained decline in national per-pupil spending in decades. The sheer magnitude of this historical episode allows for a unique examination of the extent to which large-scale and persistent education budget cuts may harm students in general, and poor children in particular.

Making use of this episode, we exploit plausibly exogenous reductions in public school spending induced by the Great Recession and examine the effect of school spending cuts on student test scores, and college-going rates. Overall, a \$1000 decline in per-pupil spending reduced test scores by about $0.045\sigma$ and reduced college-going rates by about 3 percentage points. Consistent with these education cuts having disproportionate impact on the poor, states that had deeper recessionary cuts saw a widening of the test score gap between high- and low-poverty districts. We also find that while Latinx student outcomes were largely unaffected, test scores were lower for both Black and White students. Consistent with this, we find that college going fell mainly in non-Minority Serving colleges and universities, and in Black serving colleges and universities.

Given the unique and historic nature of our variation, it is helpful to put our results in context of existing work. Borrowing from data collected in Jackson (2018a), we summarize all papers that rely on causal variation to identify school spending impacts on standardized tests. To focus on general budget increases (as in our setting) we exclude studies that increase spending for a very specific use (such as textbooks or new buildings). Figure V, presents the estimated impact of a \$1000 spending increase (that persists for 4 years) for each study of the 11 such studies.[24] The median effect across all studies of a \$1000 spending change is $0.0595\sigma$. Our estimated population effect for the four-year moving average of spending is about $0.048\sigma$ (See Table A7)– well within the range of estimates across these well-identified studies. Looking at college-going, our college-going estimate of 3 percent per \$1000 is virtually identical to that reported in Hyman (2017). Overall, this suggests that the marginal effect of school spending increases (using school finance reforms and other highly localized sources of variation) are largely similar to those based on large school spending reductions. This speaks 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.

Our results provide further evidence that money matters in education. Moreover they suggest that school spending cuts do matter. Given that the education spending cuts that occurred at recession onset have yet to be fully restored the ill-effects of the recession on the affected youth (through reduced public school spending) may be felt for years to come.

[24] We estimate four year spending impacts because many studies rely on spending changes that take a few years to materialize.

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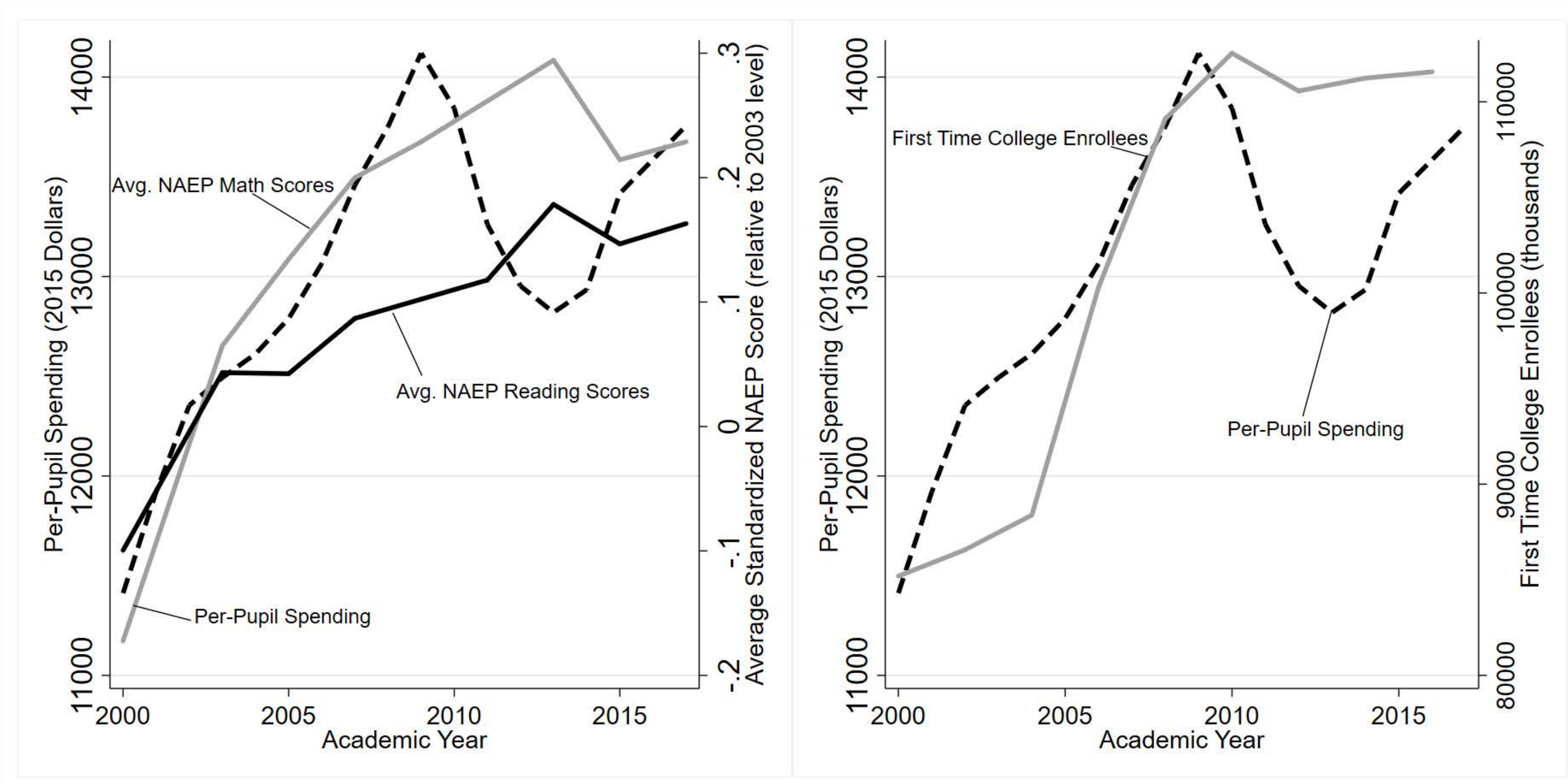
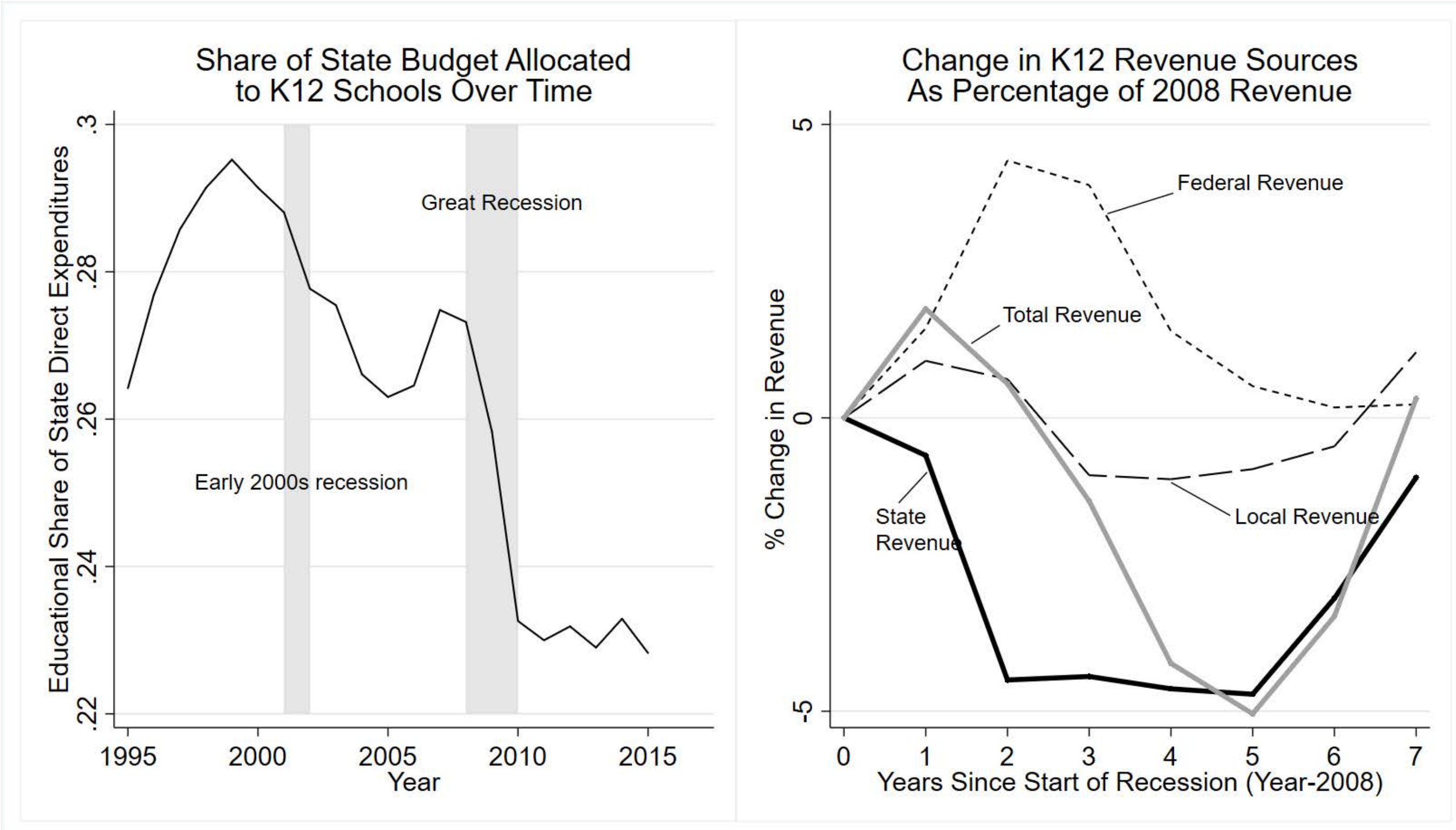
## Tables and Figures

**Figure 1.** *School Spending and NAEP Scores Over Time.*



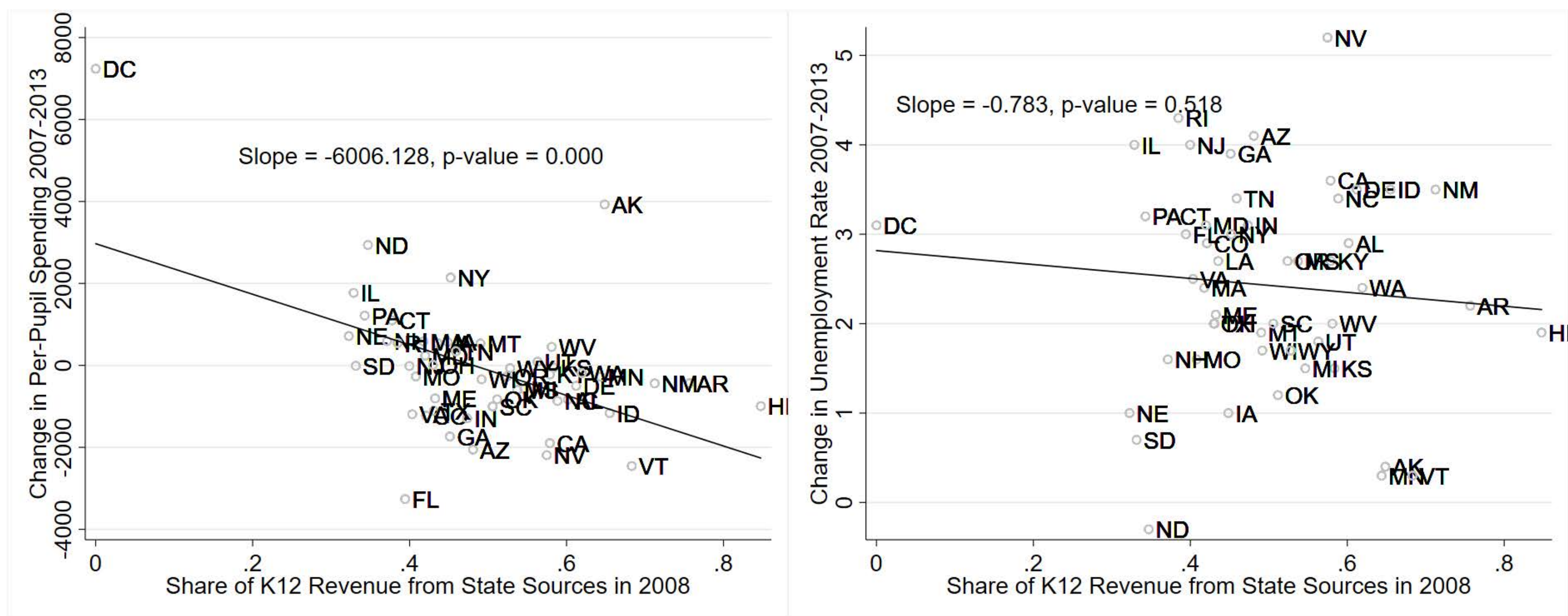

Notes: **Left**: This figure plots 4-year national averages of per-pupil spending in 2015 dollars (dotted line) and NAEP math (gray line) and reading (black line) scores standardized to a base year of 2003, from the 2000 to 2017 school years. **Right**: This figure plots 4-year national averages of per-pupil spending in 2015 dollars (dotted line) and the number of first-time college enrollees in thousands from the 2000 to 2017 school years.

**Figure 2.** *Source of Revenues for K12 Education after the recession.*



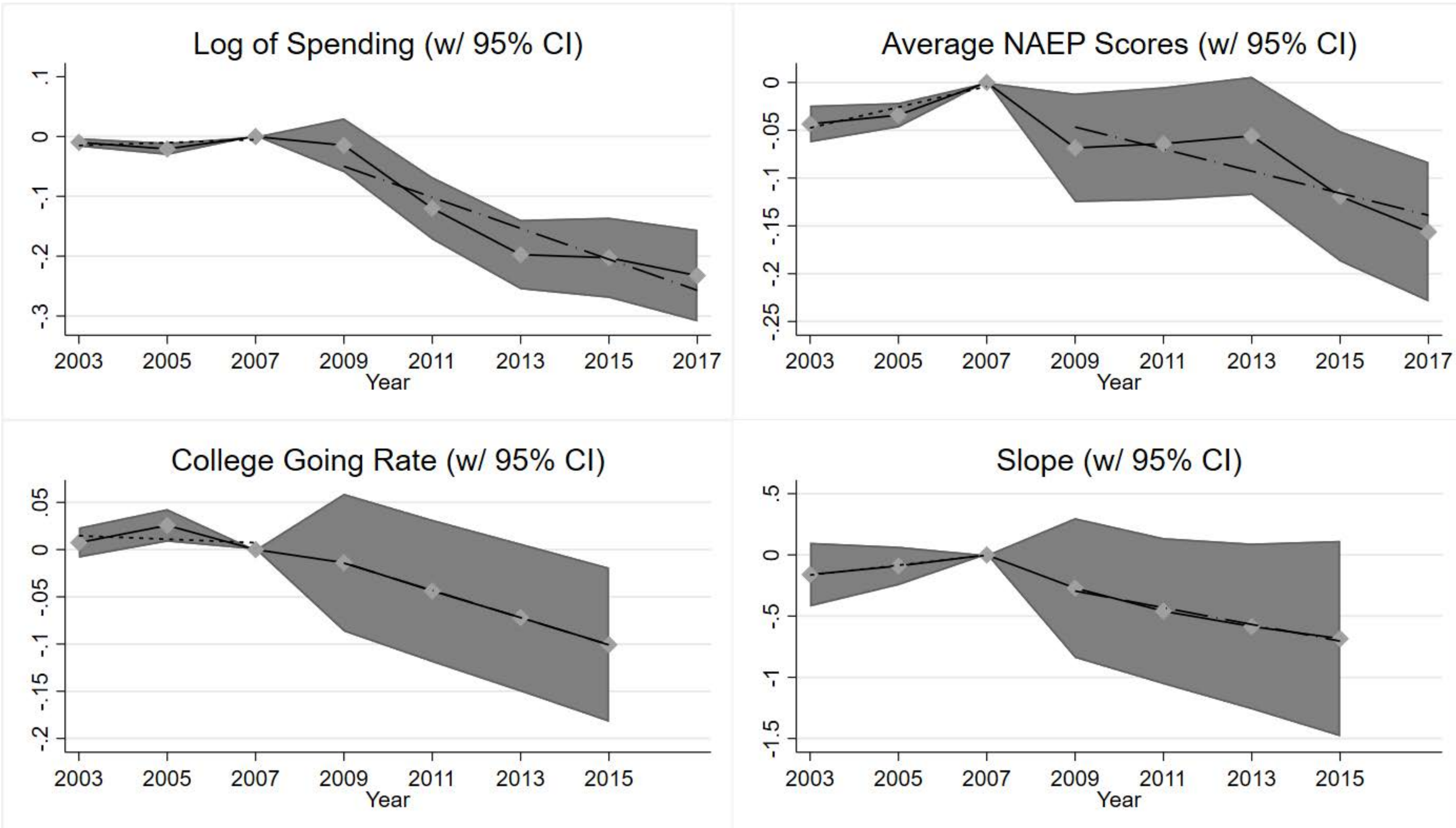

Notes: **Left**: This figure plots the share of all state spending in each year that went to K12 public schools over time. The grey areas indicate recession periods. We obtain state finance data from the U.S. Census Bureau Annual Survey of State and Local Finances (obtained through the Urban Institute-Brookings Institution State Local Government Finance Data Query System) combined with Census F33 data on Local School District Finances. **Right**: This figure plots the change in national aggregate revenue (summed over all available districts in the CCD data) for public schools relative to 2008 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. Due 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.*



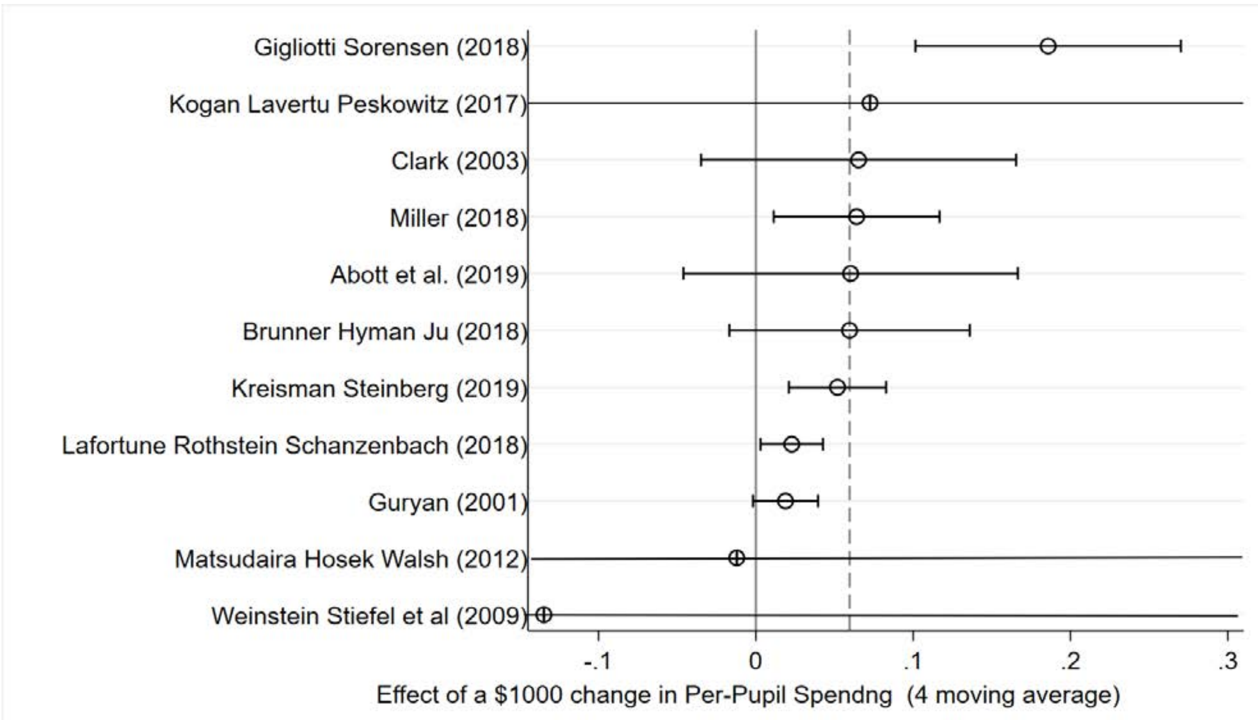

*Notes*: The left panel shows the percent change in state average (averaged across all districts in the state) K12 spending between 2007 and 2013 by the percent of state K12 revenues that came from state sources. The right panel shows the percent change in the state unemployment rate between 2007 and 2013 by the percent of state K12 revenues that came from state sources.

**Figure 4.** *Difference in Spending and Outcomes Between States with High and Low Reliance on State Revenues Over Time*




*Notes*: The dashed connected lines depicts the coefficients on the individual calendar year indicators interacted with an indicator for hight reliance on state revenue in 2008, $\ln\Omega_s > 0.33$. 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 log per-pupil spending is presented in top-left panel; the pattern for test scores is presented in top-right panel; the pattern for college-going is in the bottom-left; and the slope between the poverty rate and NAEP scores is shown in the bottom-right.

**Figure 5.** *Forrest Plot of Existing Studies*




*Notes*: The Forrest plot comes from Jackson and Mackevicius (2020), an ongoing meta-analysis of school spending studies being conducted by Kirabo Jackson and Claire Mackevicius. To facilitate comparison across studies we will convert each study's outcome into standardized units. *Standardize the treatment*: When the effect of spending is reported, we record that estimate of the change in spending directly. However, some studies report the impacts of a particular spending policy on outcomes. To allow for comparison across studies, we define each policy based on the change in per-pupil spending (CPI adjusted to 2018 dollars) that results from that policy. Many studies measure student outcomes some years after the policy change so that the duration of exposure needs to be standardized across studies. To standardize both spending and duration of exposure to said spending, for each study, we compute the average change in per-pupil spending for the four years preceding the observed outcome induced by the policy. *Standardized Effect*: After standardizing both the outcome and the treatment, for each study, we compute the change in the standardized outcome per $1000 policy-induced increase in school spending averaged over four years. We report this effect for the full population in each study along with the 95% confidence interval. For some studies, the policy induced spending change is very small so that the confidence intervals are very wide.

Table 1: Summary Statistics

| | (1) N | (2) Mean | (3) SD |
|---|---|---|---|
| *School Spending* | | | |
| Per-pupil spending (2015 dollars) | 459 | 13,355 | 3,841 |
| Share of revenue from state sources in 2008 (IV) | 51 | 0.493 | 0.137 |
| Share of revenue from federal sources | 459 | 0.0954 | 0.0334 |
| Share of revenue from state sources | 459 | 0.487 | 0.139 |
| Share of revenue from local sources | 459 | 0.417 | 0.144 |
| *Per-Pupil Spending By Use (2015 dollars per pupil)* | | | |
| Elementary/Secondary (Operations) | 459 | 11,364 | 3,196 |
| Instructional | 459 | 6,872 | 2,064 |
| Services | 459 | 4,011 | 1,230 |
| Capital | 458 | 1,280 | 806.3 |
| Construction | 456 | 918.6 | 609.5 |
| Non-Construction | 459 | 331.7 | 175.0 |
| Other | 459 | 481.3 | 106.3 |
| Salaries/Benefits | | | |
| Instructional Salaries | 459 | 4,548 | 1,189 |
| Non-Instructional Salaries | 459 | 2,208 | 560.2 |
| Benefits | 459 | 2,140 | 875.5 |
| *Students Per Staff* | | | |
| Teachers | 446 | 15.42 | 2.526 |
| Aides | 427 | 66.29 | 26.44 |
| Guidance Counselors | 440 | 446.67 | 146.15 |
| Library Staff | 447 | 653.2 | 829.99 |
| *Demographics & Economic Indicators* | | | |
| K12 Enrollment | 459 | 943.6 | 1,113 |
| Total Population (thousands) | 459 | 5,868.8 | 6,613.7 |
| Child-Age Population (thousands) | 459 | 1,025.5 | 1,186.9 |
| % Total Population in Poverty | 459 | 13.51 | 3.282 |
| % Child-Age Population in Poverty | 459 | 17.00 | 5.240 |
| Unemployment Rate (Contemporaneous) | 459 | 5.910 | 2.006 |
| Annual Average Employment (thousands) | 459 | 2,610.7 | 2,830.6 |
| Zillow Home Value Index | 456 | 179,170 | 83,884 |
| *Outcomes* | | | |
| Average NAEP Score | 459 | 0.125 | 0.211 |
| Slope | 390 | -3.552 | 1.171 |
| College-Going Rate | 408 | 0.526 | 0.0926 |
| 4-year schools | 408 | 0.359 | 0.0800 |
| 2-year school | 408 | 0.152 | 0.0638 |

*Notes:* All dollar amounts are CPI-adjusted to 2015. All variables are collected for each state in 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017. 2017 data was not available for school spending so we use 2016 data instead. College enrollment rates were not available for 2016 or 2017. The Zillow Housing Value Index was not available for North Dakota in 2002, 2003, or 2005. NAEP Scores are standardized to 2003 scores using restricted-use individual-level data.

## Table 2: First Stage and Reduced Form

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **First Stage** | | | | | | **Reduced Form** | | | | | |
| *Outcome* | *Per-Pupil Spending (thousands)* | | | | | | *Average NAEP Score* | | | *College-Going Rate* | | |
| Post | 0.828 | -0.459 | | 1.529 | -4.019 | | | 0.372 | | | -0.198 | |
| | [0.494] | [1.847] | | [0.439] | [4.830] | | | [0.117] | | | [0.0833] | |
| Post * IV | -1.802 | -1.551 | -1.741 | | | | -0.0233 | | | 0.0266 | | |
| | [1.145] | [1.118] | [1.222] | | | | [0.0497] | | | [0.0696] | | |
| Post * Year * IV | -0.546 | -0.344 | -0.402 | | | | -0.0167 | | | -0.0348 | | |
| | [0.0690] | [0.285] | [0.0991] | | | | [0.00708] | | | [0.00694] | | |
| Post * (.33<IV<.66) | | | | -1.551 | -2.061 | -1.551 | | -0.0233 | -0.0508 | | -0.0522 | -0.0328 |
| | | | | [0.463] | [1.280] | [0.467] | | [0.0262] | [0.0404] | | [0.0465] | [0.0283] |
| Post * (IV>.66) | | | | -2.909 | -3.296 | -2.909 | | -0.00341 | -0.024 | | 0.0211 | 0.0367 |
| | | | | [1.056] | [1.503] | [1.065] | | [0.0262] | [0.0428] | | [0.0464] | [0.0321] |
| Post * Year * (IV<.33) | | | | 0.28 | 0.801 | | | -0.0144 | | | 0.0267 | |
| | | | | [0.0929] | [0.532] | | | [0.0167] | | | [0.0199] | |
| Post * Year * (.33<IV<.66) | | | | -0.273 | 0.291 | -0.553 | | -0.0435 | -0.0269 | | 0.0064 | -0.0231 |
| | | | | [0.0388] | [0.537] | [0.102] | | [0.0139] | [0.00874] | | [0.0102] | [0.0180] |
| Post * Year* (IV>.66) | | | | -0.524 | 0.0373 | -0.805 | | -0.0506 | -0.0341 | | -0.0025 | -0.031 |
| | | | | [0.0575] | [0.540] | [0.110] | | [0.0145] | [0.00947] | | [0.0109] | [0.0186] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 |
| Kleibergen-Paap Wald F-stat (intercept and slope) | 31.33 | 1.717 | 20.84 | 28.71 | 16.51 | 22.77 | 3.056 | 8.087 | 7.638 | 27.18 | 4.046 | 5.157 |
| Kleibergen-Paap Wald F-stat (slope only) | 62.66 | 1.455 | 16.41 | 47.18 | 22.74 | 26.73 | 5.539 | 7.2 | 6.531 | 25.11 | 1.374 | 2.059 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Bartiks | | X | X | | X | | | X | | | X | |
| Year FX | | | | | | X | | | X | | | X |
| Model | Linear | Linear | Linear | Group | Group | Groups | Linear | Group | Groups | Linear | Group | Groups |

*Notes:* Robust standard errors in brackets cluster by state. The First Stage models regress 2015 per-pupil spending on a set of variables representing our IV, the share of spending from the state in 2008. The Reduced Form results regress our main outcome variables of interest - standardized NAEP scores and College Enrollment Rate - on the same set of IV indicators. *Post* is a dummy variable equal to 1 if the observation is after 2008. All models include data from 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. The First Stage and NAEP models also include 2016/2017. See Table A3 for First Stage results excluding 2017. All models control for state fixed effects and state trends. We add additional controls (a post dummy variable, bartik instruments, and/or year fixed effects) to account for additional trends in timing.

Table 3: Revenue Sources

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| **Per-Pupil Revenue by Source:** | **Federal** | | | **State** | | | **Local** | | |
| Post * IV | 161 | | | -969.7 | | | -2,179 | | |
| | [383.0] | | | [1,424] | | | [1,017] | | |
| Post * Year * IV | -127.4 | | | -173 | | | -34.94 | | |
| | [42.11] | | | [81.72] | | | [109.8] | | |
| Post * (.33<IV<.66) | | -41 | 55.64 | | -452.1 | -409.7 | | -1,173 | -756.7 |
| | | [158.6] | [234.4] | | [200.0] | [168.9] | | [1,378] | [687.5] |
| Post * (IV>.66) | | 0.447 | 74.39 | | -2,439 | -2,408 | | -1,319 | -997.9 |
| | | [210.2] | [256.9] | | [1,179] | [1,200] | | [1,448] | [920.7] |
| Post * Year * (IV<.33) | | 58.5 | | | 165.2 | | | 461 | |
| | | [149.1] | | | [132.4] | | | [482.8] | |
| Post * Year * (.33<IV<.66) | | 24.03 | -41.88 | | 19.41 | -152.9 | | 298.4 | -197.8 |
| | | [119.5] | [58.27] | | [131.9] | [74.97] | | [419.9] | [138.8] |
| Post * Year * (IV>.66) | | -5.634 | -70.56 | | -457.4 | -629.3 | | 561.1 | 67.73 |
| | | [134.2] | [68.81] | | [226.8] | [196.8] | | [436.2] | [187.1] |
| P(Post*Year*IV) | 0.00393 | | | 0.0393 | | | 0.752 | | |
| P(Low=High) | | 0.306 | 0.31 | | 0.00287 | 0.00241 | | 0.527 | 0.719 |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 |
| State Trends | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X |
| Model | Linear | Group | Group | Linear | Group | Group | Linear | Group | Group |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016. All models control for state fixed effects and state trends. Models 1, 2, 4, 5, 7, and 8 also include a state-level bartik instrument and 3 year-lagged bartik instruments to control for additional economic timing trends. Models 3, 6, and 9 include year fixed effects in place of the bartik instruments. *Post* is a dummy variable equal to 1 if the observation is after 2008. For models using the linear IV, P(Post*Year*IV) tests whether states with a higher reliance on state funds for K12 education in 2008 experienced a decline in the trend of each revenue source in the years after the Great Recession. In the Group IV models, P(Low=High) tests whether states with the highest reliance on state-funds experienced greater declines in each revenue source in the years post-Recession than the states with the lowest reliance on state funds.

Table 4: 2SLS Main Results

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Average NAEP Score* | | | | | | *College Enrollment Rate* | | | | | |
| Per-Pupil Spending (thousands) | 0.0471 | 0.0698 | 0.0415 | 0.0502 | 0.0236 | 0.0441 | 0.0426 | 0.0574 | 0.0624 | 0.0384 | 0.026 | 0.0279 |
| | [0.0103] | [0.0925] | [0.0190] | [0.00966] | [0.0106] | [0.0131] | [0.00693] | [0.0331] | [0.0159] | [0.00584] | [0.00856] | [0.0106] |
| Pr(underid) | 3.82E-07 | 0.182 | 0.00103 | 8.10E-07 | 0.0907 | 0.0505 | 9.84E-08 | 0.0373 | 0.00184 | 3.21E-07 | 0.09 | 0.0794 |
| Kleibergen-Paap Wald F-stat | 62.66 | 1.455 | 16.41 | 47.18 | 22.74 | 26.73 | 78.85 | 8.281 | 21.45 | 52.29 | 7.886 | 10.17 |
| Pr(Endog) | 5.63E-05 | 0.28 | 0.0597 | 1.03E-05 | 0.829 | 0.0708 | 2.91E-07 | 0.127 | 0.0027 | 2.45E-07 | 0.115 | 0.101 |
| | *Predicted NAEP Score* | | | | | | *Predicted College Enrollment Rate* | | | | | |
| Per-Pupil Spending (thousands) | 0.0187 | -0.00844 | 0.00752 | 0.016 | 0.00037 | 0.000704 | 0.0139 | -0.000392 | 0.00723 | 0.0112 | 0.00113 | 0.00144 |
| | [0.00405] | [0.0127] | [0.00637] | [0.00346] | [0.00348] | [0.00318] | [0.00307] | [0.00794] | [0.00446] | [0.00287] | [0.00277] | [0.00354] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. *Post* is a dummy variable equal to 1 if the observation is after 2008. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). The top panel presents our main results regressing Average NAEP Scores and College Enrollment Rates on instrumented per-pupil spending. The bottom panel regresses the same outcomes as predicted by economic and demographic variables (see table A2) on instrumented spending to demonstrate that instrumented spending is not endogenous to economic and demographic characteristics that are also correlated with academic outcomes.

Table 5: Spending Categories

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Overall Spending Category:** | **Total Expenditures** | | | | | | **Capital Spending** | | | **Elem/Sec Operating** | | |
| *Subset Spending Category:* | *Capital* | | | *Elem/Sec Operating* | | | *Construction* | | | *Instructional* | | |
| Per-Pupil Spending (thousands) | 267.3 | 571.1 | 482.9 | 778.6 | 353.4 | 475.4 | 306.7 | 478.5 | 441.7 | 618.8 | 446.9 | 472.3 |
| | [130.9] | [234.8] | [223.4] | [179.0] | [189.1] | [193.6] | [137.6] | [201.5] | [190.6] | [158.5] | [51.40] | [55.60] |
| Mean(Dependent Var.) | 1280 | 1280 | 1280 | 11364 | 11364 | 11364 | 918.6 | 918.6 | 918.6 | 6872 | 6872 | 6872 |
| Average Share | 0.0962 | 0.0962 | 0.0962 | 0.853 | 0.853 | 0.853 | 0.0691 | 0.0691 | 0.0691 | 0.515 | 0.515 | 0.515 |
| P(Average=Marginal) | 0.197 | 0.0485 | 0.0896 | 0.679 | 0.011 | 0.0566 | 0.0905 | 0.0475 | 0.0562 | 0.515 | 0.193 | 0.449 |
| Observations | 458 | 458 | 458 | 459 | 459 | 459 | 456 | 456 | 456 | 459 | 459 | 459 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016. *Post* is a dummy variable equal to 1 if the observation is after 2008. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. Coefficients on per-pupil spending can be interpreted as the amount of additional funds cut from each category for every one thousand dollars in exogenous educational spending cuts. More spending categories are reported in table A8.

Table 6: Staffing (2SLS)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Teachers** | | | **Aides** | | | **Guidance Counselors** | | | **Library Staff** | | |
| | *Outcome: Log(Staff)* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0763 | 0.0474 | 0.0407 | 0.092 | 0.0844 | 0.0587 | 0.0688 | 0.123 | 0.132 | 0.0669 | 0.033 | 0.102 |
| | [0.0400] | [0.0121] | [0.00987] | [0.0655] | [0.0211] | [0.0347] | [0.0355] | [0.0363] | [0.0649] | [0.0426] | [0.0241] | [0.0474] |
| | *Outcome: Log(Staff), controlling for Log(Enrollment)* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0388 | 0.0152 | 0.0208 | 0.0551 | 0.0507 | 0.0307 | 0.0462 | 0.0893 | 0.116 | 0.0597 | 0.0137 | 0.1 |
| | [0.0222] | [0.0186] | [0.0121] | [0.0448] | [0.0283] | [0.0250] | [0.0320] | [0.0393] | [0.0530] | [0.0400] | [0.0273] | [0.0443] |
| | *Outcome: Students per Staff* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | -0.5 | -0.211 | -0.292 | -4.292 | -2.443 | -0.877 | 4.088 | -66.41 | -81.93 | -22.16 | -9.067 | -66.23 |
| | [0.302] | [0.280] | [0.206] | [4.571] | [1.469] | [1.637] | [21.55] | [32.64] | [39.65] | [34.14] | [21.76] | [34.96] |
| Observations | 446 | 446 | 446 | 427 | 427 | 427 | 440 | 440 | 440 | 447 | 447 | 447 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey and the NCES Common Core of Data LEA Universe Survey. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016/2017. *Post* is a dummy variable equal to 1 if the observation is after 2008. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. For each staff category (teachers, aides, guidance counselors, and library staff) we estimate three panels. The first and second panels regresses the log of the staff count on instrumented per-pupil spending. The second panel also controls for the log of K12 student enrollment. The third panel regresses the student:staff ratio on instrumented spending. For instance, the coefficient -.5 in column 1 panel 3 indicates that a $2,000 decline in per-pupil spending increases class size by one student per teacher on average. Not all states reported staff counts for each category in every year, but all states and years are represented in the sample.

Table 7: Race and Income (2SLS)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Slope** | | | **Average NAEP Score by Race** | | | | | | | | |
| | | | | *White* | | | *Black* | | | *Hispanic* | | |
| Per-Pupil Spending (thousands) | 0.412 | 0.428 | 0.386 | 0.099 | 0.0446 | 0.0296 | 0.0159 | 0.0353 | 0.0353 | 0.000587 | 0.0223 | -0.00574 |
| | [0.240] | [0.158] | [0.151] | [0.0442] | [0.0201] | [0.00820] | [0.0299] | [0.0318] | [0.0151] | [0.0890] | [0.0252] | [0.0133] |
| Observations | 390 | 390 | 390 | 452 | 452 | 452 | 392 | 392 | 392 | 402 | 402 | 402 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey and the NCES Public and restricted-use NAEP Data. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Average NAEP Score by race results also include 2016/2017. Average NAEP Scores by race are not available in every year for every state, though most states are represented in each sub-sample. Exceptions are Idaho, Montana, and Wyoming (no reported scores for black students) and Maine, Vermont, and West Virginia (no reported scores for hispanic students). The variable "Slope" is computed by regressing individual-level NAEP scores on the district poverty rate (in 2007) for each year in each state. *Post* is a dummy variable equal to 1 if the observation is after 2008. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. All models control for state fixed effects and state trends and include a subset of bartik instruments, year fixed effects, and/or a post dummy indicator to control for additional economic characteristics correlated with years.

# Table 8: 2SLS: College Going by Institution Type

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | *Overall* | | | *2-Year* | | | *4-Year* | |
| Per-Pupil Spending (thousands) | 0.056 | 0.026 | 0.0279 | 0.035 | 0.0068 | 0.0102 | 0.0207 | 0.0171 | 0.0159 |
| | [0.0107] | [0.00856] | [0.0106] | [0.00656] | [0.00493] | [0.00482] | [0.00781] | [0.00997] | [0.0119] |
| Average rate | | 0.526 | | | 0.152 | | | 0.359 | |
| Effective % change | 0.106 | 0.0495 | 0.0531 | 0.231 | 0.0448 | 0.0673 | 0.0578 | 0.0476 | 0.0443 |
| | | *Public (2 or 4 year)* | | | *Private (2 or 4 year)* | | | *More than 40% Part-Time (4 year)* | |
| Per-Pupil Spending (thousands) | 0.0423 | 0.0191 | 0.0211 | 0.0134 | 0.00475 | 0.00499 | 0.00539 | 0.00326 | 0.00359 |
| | [0.00834] | [0.00379] | [0.00355] | [0.00333] | [0.00567] | [0.00653] | [0.00199] | [0.00181] | [0.00199] |
| Average rate | | 0.388 | | | 0.122 | | | 0.011 | |
| Effective % change | 0.109 | 0.0493 | 0.0544 | 0.11 | 0.0388 | 0.0408 | 0.488 | 0.295 | 0.325 |
| | | *Less than 40% Part-Time (4 year)* | | | *Selective/Most Selective (4 year)* | | | *Minority Serving Institution (2 or 4 year)* | |
| Per-Pupil Spending (thousands) | 0.0131 | 0.0134 | 0.0117 | 0.00881 | 0.00993 | 0.00846 | 0.0163 | 0.0169 | 0.0171 |
| | [0.00663] | [0.00806] | [0.00972] | [0.00555] | [0.00616] | [0.00733] | [0.00696] | [0.00672] | [0.00787] |
| Average rate | | 0.333 | | | 0.294 | | | 0.0948 | |
| Effective % change | 0.0394 | 0.0403 | 0.0351 | 0.0299 | 0.0337 | 0.0287 | 0.172 | 0.178 | 0.18 |
| | | *Non-MSI (2 or 4 year)* | | | *Hispanic-Serving Institutions (2 or 4 year)* | | | *Non-HSI MSI (2 or 4 year)* | |
| Per-Pupil Spending (thousands) | 0.0397 | 0.00915 | 0.0109 | 0.0045 | 0.0016 | 0.00206 | 0.0118 | 0.0153 | 0.015 |
| | [0.0104] | [0.00399] | [0.00568] | [0.00323] | [0.00270] | [0.00293] | [0.00638] | [0.00590] | [0.00702] |
| Average rate | | 0.431 | | | 0.0205 | | | 0.0743 | |
| Effective % change | 0.092 | 0.0212 | 0.0252 | 0.219 | 0.0781 | 0.1 | 0.159 | 0.206 | 0.202 |
| Observations | 408 | 408 | 408 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IPEDS, IPUMS, and the Carnegie Foundation. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Enrollment rates are calculated from the number of first time enrollees at institutions meeting the stated criteria (e.g. public or 4-year) that graduated from high school in the past year, by which state the students lived at the time of application (IPEDS), divided by the number of 17 year-olds 2 years prior to the enrollment year (IPUMS). We report the average enrollment rate at each type of institution over all states and years to scale the effects to be respective to each institution type's representation in overall enrollment rates. *Post* is a dummy variable equal to 1 if the observation is after 2008. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. All models control for state fixed effects and state trends and include a subset of bartik instruments, year fixed effects, and/or a post dummy indicator to control for additional economic characteristics correlated with years. Minority Serving Institutions and Hispanic-Serving Institutions are defined according to IES definitions (page V) using 2005 undergraduate enrollment data from IPEDS. Selective/Most Selective schools are those 4-year institutions that enroll fewer than 40% part-time students and admissions test scores are in the 40th to 100th percentile of selectivity. We use the Carnegie Classifications from 2005 for all institutions. Additional results by college type are reported in table A15.

# Appendix

# A Data on School Spending and Resources

The data on district level school finances is collected from the Census website.[25] 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. The last year for which data is available is the 2015-2016 academic year. We link together multiple years of data to create a balanced state-year panel (summarizing first across school districts). 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 winsorizing 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).[26] Each state's time-varying shift-share

[25] For 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.

[26] The 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.`

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 state $s$ during year $t$ the instrument equals:

$$Bartik\ Predictor_{st} = \sum_j \left( \frac{E_{js2007}}{\sum_{j'} E_{j's2007}} \times \text{National Unemployment}_{jst} \right) \tag{8}$$

where j denotes a two-digit industry, $E_{js2007}$ denotes total employment in industry j in state s in 2007, and $National\ Unemployment_{jt}$ is the nationwide unemployment rate in industry $j$ in state $s$ 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 state-level estimates of housing values from Zillow and use the January index for each year as an annual indicator of home prices.

# C NAEP Data

For our main analyses, we use publicly available state-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 use restricted-use individual-level for three purposes: 1) to compute average private school scores, 2) to compute the relationship between district poverty rates and NAEP scores, and 3) to compute the mean and standard deviation of all scores in 2003 for standardization. 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 private school students (for 2 and 3) or private school students (for 1). 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.

Our dependent variable in all public-school NAEP estimations is the average of all publicly available scores per state, year, grade, and subject, standardized to the base year of 2003 (determined from the restricted-use individual-level scores). We also use publicly available scores by race in the same way. 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 College Enrollment Data

Our college-going data are obtained from the the Integrated Postsecondary Education Data System (IPEDS). These data report surveys submitted at the aggregate-level from postsecondary institutions. These data do not have student-level information. Institutions report on the number of first-time college freshman from each state in each year who graduated high school in the past 12 months. By aggregating these data to the state of origin level, we obtain counts for the number of first-time freshmen from each state in each year. We drop years where the survey was optional, and match each year of college enrollment to spending data from the year prior (when enrolling students would have graduated high school). To compute college-going rates for these years, we obtain population counts by age in each state in each year from the American Community Surveys (ACS) from 2000 to 2016. Our college-going measure is the number of first-time college enrollees divided by the number of 17-year-olds in the state two years prior to the year of enrollment. We use this lagged cohort measure instead of 18-year olds in the year prior to minimize measurement error from counting individuals who had already enrolled in college or moved away from home.

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Using information on postsecondary institutions from IPEDS and the Carnegie Foundation, we are able to compute enrollments by college type (2-year vs 4-year), selectivity level, and other characteristics about populations served. We use information on institution characteristics from Carnegie in 2005. Given that some institutional classifications are based on the populations served, which may be endogenously affected by the recession and K12 spending cuts, we classify institutions based on their 2005 categories for every year in our data. The Carnegie data provides information from IPEDS on the level served (2 or 4 year) and control (public or private), as well as its own selectivity and part-time enrollment categories. Per their definitions, Inclusive 4-year institutions are those for whom test score data was not available or otherwise indicated that admissions were offered inclusively. Selective 4-year institutions were those with test scores that placed them between the 40th and 80th percentiles in selectivity, and More/Most Selective 4-year institutions were those with thest scores that placed them between the 80th and 100th percentiles in selectivity.

The Carnegie data also includes IPEDS categories to note Historically Black Colleges and Universities (HBCU's) and Tribal Colleges. However, we use 2005 IPEDS fall enrollment data by race to define categories for Minority Serving Institutions, Hispanic Serving Institutions, and Black Serving Institutions using IES definitions (page V of this document). We define these categories ourselves because exact definitions for 2005 were not available and the data reported by Carnegie did not seem to match any known definitions precisely (for example, not all HBCU's were designated as MSI's).

Table A1: NAEP Availability

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

*Notes:* This table reports the availability of NAEP scores by year, grade, and subject using restricted-use individual NAEP data.

Table A2: Predicted Outcomes

| | (1) | (2) |
|---|---|---|
| *Outcome* | *Average NAEP Score* | *College Enrollment Rate* |
| Unemployment Rate (4yr) | 0.0163 | -0.00753 |
| | [0.00451] | [0.00405] |
| Unemployment Rate (1yr) | 0.0064 | -0.000171 |
| | [0.00195] | [0.00272] |
| % Total Population in Poverty | 0.00572 | 0.013 |
| | [0.00980] | [0.00751] |
| % Child Population in Poverty | -0.0166 | -0.00601 |
| | [0.00919] | [0.00928] |
| Log (Child Population) | -0.525 | -0.512 |
| | [0.222] | [0.261] |
| Log (Total Population) | 0.364 | -0.0499 |
| | [0.411] | [0.376] |
| Log (Child Population in Poverty) | 0.176 | 0.109 |
| | [0.102] | [0.133] |
| Annual Average Employment | 5.08E-08 | 4.68E-08 |
| | [1.82e-08] | [2.63e-08] |
| Constant | -0.461 | 6.7 |
| | [5.301] | [4.758] |
| Observations | 459 | 408 |
| R-squared | 0.963 | 0.839 |
| Within R-Squared | 0.211 | 0.0853 |
| State FX | X | X |
| State Trends | X | X |

*Notes:* Robust standard errors in brackets cluster by state. We regress outcomes on economic and demographic covariates, state fixed effects, and state trends and create linear predictions of the outcomes based on the model. The samples here include state by year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Column 1 also includes data from 2017. Predicted outcomes are used as placebo outcome measures in our main results reported in table A17.

Table A3: First Stage, College-Going Sample (No 2017)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Per-Pupil Spending (thousands)* | | | | | |
| Post | 0.309 | 0.136 | | 1.103 | -5.62 | |
| | [0.603] | [1.483] | | [0.263] | [4.554] | |
| Post * IV | -0.56 | -0.417 | -0.373 | | | |
| | [1.297] | [1.279] | [1.081] | | | |
| Post * Year * IV | -0.677 | -0.576 | -0.558 | | | |
| | [0.0763] | [0.200] | [0.120] | | | |
| Post * (.33<IV<.66) | | | | -0.992 | -1.67 | -0.992 |
| | | | | [0.297] | [0.969] | [0.300] |
| Post * (IV>.66) | | | | -2.312 | -2.811 | -2.312 |
| | | | | [1.009] | [1.291] | [1.018] |
| Post * Year * (IV<.33) | | | | 0.493 | 1.143 | |
| | | | | [0.316] | [0.614] | |
| Post * Year * (.33<IV<.66) | | | | -0.339 | 0.395 | -0.833 |
| | | | | [0.0427] | [0.481] | [0.322] |
| Post * Year* (IV>.66) | | | | -0.61 | 0.107 | -1.103 |
| | | | | [0.0638] | [0.481] | [0.326] |
| Observations | 408 | 408 | 408 | 408 | 408 | 408 |
| Kleibergen-Paap Wald F-stat (all) | 40.58 | 4.163 | 17.52 | 32.26 | 6.914 | 9.891 |
| Kleibergen-Paap Wald F-stat (slope only) | 78.85 | 8.281 | 21.45 | 52.29 | 7.886 | 10.17 |
| State Trends | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X |
| Bartiks | | X | X | X | X | |
| Year FX | | | | | | X |
| Instrument | Linear | Linear | Linear | Groups | Groups | Groups |

*Notes:* Robust standard errors in brackets cluster by state. This table replicates the first stage panel of table 2 dropping 2017 in order to correspond to the college-going sample. These results can be interpreted as the first stage for the 2SLS models when we examine the outcome College Enrollment Rate.

Table A4: OLS Models

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Average NAEP Score | | | College Enrollment Rate | | |
| Per-Pupil Spending (thousands) | 0.0109 | 0.00795 | 0.00465 | 0.0112 | 0.0124 | 0.00868 |
| | [0.00364] | [0.00359] | [0.00314] | [0.00268] | [0.00294] | [0.00439] |
| Observations | 459 | 459 | 459 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X |
| Bartiks | | X | | | X | |
| Year FX | | | X | | | X |

*Notes:* Robust standard errors in brackets cluster by state. State-by-year observations include all states in years 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Models predicting NAEP scores also include 2016/2017. For each column, we regress the outcome (Average NAEP Score or College Enrollment Rate) on per-pupil spending, state fixed effects, and state trends. Models 2 and 5 also control for a series of bartik instruments, and models 3 and 6 include year fixed effects.

Table A5: 2SLS Models with Year Fixed Effects and Linear IV

| | (1) | (2) |
|---|---|---|
| | *Average NAEP Score* | *College Enrollment Rate* |
| Per-Pupil Spending (thousands) | 0.00146 | 0.0374 |
| | [0.0357] | [0.0102] |
| Observations | 459 | 408 |
| Pr(underid) | 0.0759 | 0.129 |
| Kleibergen-Paap Wald F-stat | 8.495 | 5.332 |
| Pr(Endog) | 0.92 | 0.135 |
| State Trends | X | X |
| State FX | X | X |
| Post*IV | X | X |
| Year FX | X | X |
| Model | Linear IV | Linear IV |

*Notes:* Robust standard errors in brackets cluster by state. State-by-year observations include all states in years 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Models predicting NAEP scores also include 2016/2017. Each column reports 2SLS results for the effect of instrumented per-pupil spending on outcomes, using our linear instrument for spending (share of state spending in 2008). The excluded instrument in these models is Post*Year*IV. Models here include state fixed effects, state trends, and year fixed effects. The Kleibergen-Paap Wald F-stat reports the first stage F-statistic for each model.

## Table A6: 2SLS Main Effects, Log Specification

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Average NAEP Score* | | | | | | *College Enrollment Rate* | | | | | |
| Log (Per-Pupil Spending) | 0.567 | 0.691 | 0.436 | 0.621 | 0.505 | 0.618 | 0.513 | 0.727 | 0.758 | 0.494 | 0.437 | 0.444 |
| | [0.128] | [0.697] | [0.191] | [0.126] | [0.187] | [0.215] | [0.0804] | [0.422] | [0.181] | [0.0757] | [0.204] | [0.221] |
| Pr(underid) | 2.99E-07 | 0.0583 | 0.000329 | 4.45E-07 | 0.133 | 0.0674 | 6.49E-08 | 0.0329 | 0.000598 | 1.64E-07 | 0.137 | 0.0843 |
| Kleibergen-Paap Wald F-stat | 67.61 | 3.749 | 28.3 | 31.79 | 17.46 | 28.56 | 90.41 | 8.548 | 30.82 | 35.93 | 11.73 | 10.58 |
| Pr(Endog) | 5.33E-05 | 0.276 | 0.0755 | 5.68E-06 | 0.393 | 0.0634 | 8.31E-07 | 0.117 | 0.00213 | 1.94E-07 | 0.0705 | 0.0767 |
| | *Predicted NAEP Score* | | | | | | *Predicted College Enrollment Rate* | | | | | |
| Log (Per-Pupil Spending) | 0.225 | -0.0836 | 0.0791 | 0.205 | 0.00095 | 0.00682 | 0.167 | -0.00497 | 0.0879 | 0.152 | 0.0166 | 0.0217 |
| | [0.0480] | [0.107] | [0.0665] | [0.0431] | [0.0496] | [0.0463] | [0.0361] | [0.101] | [0.0529] | [0.0350] | [0.0475] | [0.0568] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* This table replicates table A17 using the Log of Per-Pupil Spending (2015 dollars) instead of Per-Pupil Spending (2015 thousands of dollars). Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for the log of per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). The top panel presents our main results regressing Average NAEP Scores and College Enrollment Rates on instrumented log spending. The bottom panel regresses the same outcomes as predicted by economic and demographic variables (see table A2) on instrumented log spending to demonstrate that instrumented spending is not endogenous to economic and demographic characteristics that are also correlated with academic outcomes.

## Table A7: 2SLS Main Effects, 4-Year Spending

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Average NAEP Score* | | | | | | *College Enrollment Rate* | | | | | |
| 4yr Per-Pupil Spending (thousands) | 0.043 | 0.0541 | 0.0296 | 0.0465 | 0.0538 | 0.0489 | 0.045 | 0.0549 | 0.0523 | 0.0421 | 0.0347 | 0.0353 |
| | [0.00835] | [0.0486] | [0.0108] | [0.00811] | [0.0181] | [0.0166] | [0.00722] | [0.0321] | [0.0120] | [0.00682] | [0.0173] | [0.0174] |
| Pr(underid) | 2.46E-08 | 0.0299 | 3.77E-05 | 3.67E-08 | 0.0875 | 0.0706 | 5.06E-08 | 0.0197 | 0.000183 | 5.72E-08 | 0.066 | 0.0991 |
| Kleibergen-Paap Wald F-stat | 98.55 | 4.763 | 57.43 | 46.12 | 10.07 | 13.76 | 84.86 | 9.32 | 46.66 | 39.17 | 7.269 | 6.782 |
| Pr(Endog) | 0.0281 | 0.458 | 0.464 | 0.0153 | 0.505 | 0.308 | 0.000422 | 0.153 | 0.0061 | 0.000372 | 0.228 | 0.199 |
| | *Predicted NAEP Score* | | | | | | *Predicted College Enrollment Rate* | | | | | |
| 4yr Per-Pupil Spending (thousands) | 0.0171 | -0.00654 | 0.00536 | 0.0156 | -0.00114 | -0.000532 | 0.0147 | -0.000375 | 0.00606 | 0.0128 | 0.000917 | 0.00149 |
| | [0.00309] | [0.00893] | [0.00415] | [0.00281] | [0.00424] | [0.00423] | [0.00323] | [0.00761] | [0.00360] | [0.00296] | [0.00369] | [0.00425] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* This table replicates table A17 using a 4-year moving average of Per-Pupil Spending (2015 dollars) instead of contemporaneous Per-Pupil Spending (2015 thousands of dollars). Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for 4-year per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). The top panel presents our main results regressing Average NAEP Scores and College Enrollment Rates on instrumented 4-year spending. The bottom panel regresses the same outcomes as predicted by economic and demographic variables (see table A2) on instrumented 4-year spending to demonstrate that instrumented spending is not endogenous to economic and demographic characteristics that are also correlated with academic outcomes.

## Table A8: 2SLS: Additional Spending Categories

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Overall Spending Category:** | **Elem/Sec Operating** | | | **Salaries & Benefits** | | | | | | | | | | | |
| *Subset Spending Category:* | *Support Services* | | | *Instructional Salaries* | | | *Non-Instructional Salaries* | | | *Instructional Benefits* | | | *Non-Instructional Benefits* | | |
| Per-Pupil Spending (thousands) | 166.6 | -93.45 | -3.721 | 228.7 | 243.4 | 282.9 | 113.5 | -32.08 | -0.191 | 312.5 | 196 | 228.2 | 119.4 | -33.98 | -43.37 |
| | [110.9] | [161.2] | [162.2] | [78.59] | [55.23] | [85.64] | [49.88] | [61.48] | [58.74] | [101.2] | [65.84] | [96.78] | [104.4] | [82.68] | [93.84] |
| Mean(Dependent Var.) | 4011 | 4011 | 4011 | 4548 | 4548 | 4548 | 2208 | 2208 | 2208 | 1408 | 1408 | 1408 | 732.3 | 732.3 | 732.3 |
| Average Share | 0.3 | 0.3 | 0.3 | 0.344 | 0.344 | 0.344 | 0.168 | 0.168 | 0.168 | 0.105 | 0.105 | 0.105 | 0.055 | 0.055 | 0.055 |
| P(Average=Marginal) | 0.233 | 0.0181 | 0.0666 | 0.149 | 0.0745 | 0.479 | 0.281 | 0.00205 | 0.00615 | 0.0457 | 0.173 | 0.209 | 0.54 | 0.287 | 0.3 |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 | 459 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | | X | | | X | | | X | | | X | | | X | |
| Bartiks | X | X | | X | X | | X | X | | X | X | | X | X | |
| Year FX | | | X | | | X | | | X | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. Coefficients on per-pupil spending can be interpreted as the amount of additional funds cut from each category for every one thousand dollars in exogenous educational spending cuts. More spending categories are reported in table 5.

Table A9: Private School NAEP Scores (2SLS)

| | (1) | (2) | (3) |
|---|---|---|---|
| | *Private School NAEP Score* | | |
| Per-Pupil Spending (thousands) | -0.134 | -0.0111 | 0.0429 |
| | [0.116] | [0.0394] | [0.0343] |
| Observations | 388 | 388 | 388 |
| State Trends | X | X | X |
| State FX | X | X | X |
| Post*IV | X | X | X |
| Post | | X | |
| Bartiks | X | X | |
| Year FX | | | X |
| Model | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Spending data come from the F33 School District Finance survey. We calculate private school NAEP scores using the restricted-use NAEP data. We calculate NAEP scores for private school students, standardizing to 2003 private school NAEP scores. We take the mean for each state and year, weighting by the ORIGWT sampling weight. We are not able to calculate a mean private school NAEP score for every state and year, but all states and years are represented in the sample. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. We instrument for per-pupil spending here using Post*Year*IV in the Linear model (column 1) and Post*Year*[IV Group] in Group models (columns 2 and 3).

Table A10: Exogeneity Test of Instrument (2SLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Unemployment Rate (1yr)* | | | *Unemployment Rate (4yr)* | | |
| Per-Pupil Spending (thousands) | 0.179 | -0.0111 | 0.0607 | -0.0729 | -0.0267 | -0.0476 |
| | [0.496] | [0.166] | [0.187] | [0.267] | [0.146] | [0.155] |
| | *Child Poverty Population* | | | *Log(Child Poverty Population)* | | |
| Per-Pupil Spending (thousands) | -9,471 | -1,789 | -4,645 | 0.0112 | -0.00872 | -0.0162 |
| | [7,689] | [4,197] | [5,139] | [0.0311] | [0.0291] | [0.0373] |
| | *Annual Average Employment* | | | *Employment Ratio** | | |
| Per-Pupil Spending (thousands) | 50,695 | 12,664 | 15,640 | 0.0244 | -0.00441 | 0.00429 |
| | [56,942] | [15,134] | [13,721] | [0.00976] | [0.00781] | [0.00883] |
| | *K12 Enrollment* | | | *Log(K12 Enrollment)* | | |
| Per-Pupil Spending (thousands) | 15,732 | -54.94 | -3,537 | 0.042 | 0.0328 | 0.0221 |
| | [8,236] | [8,158] | [9,379] | [0.0353] | [0.0178] | [0.0215] |
| | *White Population* | | | *Black Population* | | |
| Per-Pupil Spending (thousands) | 7,988 | 595.3 | 3,557 | 316,019 | -53,132 | 5,954 |
| | [6,144] | [5,211] | [5,668] | [214,896] | [87,864] | [93,591] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 |
| State Trends | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X |
| Post | | X | | | X | |
| Bartiks | X | X | | X | X | |
| Year FX | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. Spending data are collected from the F33 School District Finance survey. Other data sources include BLS, SAIPE, NCES CCD, Zillow, and ACS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1 and 4) and Post*Year*[IV Group] in Group models (columns 2-3 and 5-6). We regress each economic of demographic variable on instrumented per-pupil spending. All models include state fixed effects and state trends. Columns 1, 2, 4, and 5 also control for a series of lagged bartik instruments. Columns 3 and 6 instead control for year fixed effects.

*Employment ratio = Log(Annual Average Employment) - Log(Total Population)

Table A11: Saturated Models (2SLS)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Model 1** | | | | **Model 2** | | | | **Model 3** | | | |
| | *Average NAEP Score* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0415 | 0.0463 | 0.0691 | 0.106 | 0.0236 | 0.0331 | 0.00688 | 0.0036 | 0.0441 | 0.0568 | 0.0648 | 0.0607 |
| | [0.0190] | [0.0200] | [0.0356] | [0.0649] | [0.0106] | [0.0156] | [0.0160] | [0.0157] | [0.0131] | [0.0310] | [0.0296] | [0.0249] |
| Observations | 459 | 459 | 459 | 456 | 459 | 459 | 459 | 456 | 459 | 459 | 459 | 456 |
| | *College Enrollment Rate* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0624 | 0.0659 | 0.0867 | 0.0617 | 0.026 | 0.0273 | 0.0268 | 0.0273 | 0.0279 | 0.0296 | 0.0297 | 0.0279 |
| | [0.0159] | [0.0172] | [0.0310] | [0.0329] | [0.00856] | [0.0139] | [0.0125] | [0.0127] | [0.0106] | [0.0173] | [0.0160] | [0.0154] |
| Observations | 408 | 408 | 408 | 405 | 408 | 408 | 408 | 405 | 408 | 408 | 408 | 405 |
| State Trends & State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | | | | | X | X | X | X | | | | |
| Bartiks | X | X | X | X | X | X | X | X | | | | |
| Year FX | | | | | | | | | X | X | X | X |
| Population Controls | | X | X | X | | X | X | X | | X | X | X |
| Economic Controls | | | X | X | | | X | X | | | X | X |
| Housing Values | | | | X | | | | X | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Group IV | Group IV | Group IV | Group IV | Group IV |

*Notes:* Robust standard errors in brackets cluster by state. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). The top and bottom panels present results regressing Average NAEP Scores or College Enrollment rates on instrumented per-pupil spending. Each set of models builds on a preferred specification from our main results in table A17. Column (1) directly replicates the results presented in columns (3) and (9) from table A17. Column (2) adds population controls (total population and child population), column (3) adds economic controls (1 and 4 year unemployment rates, child poverty rate, total poverty rate, and log(employment)), and column (4) adds a control for the housing value index (not available for North Dakota in 2002, 2003, or 2005). COlumn (5) replicates the model from columns (5) and (11) from table A17. Column (9) here replicates models from columns (6) and (12) from A17. We add additional population and economic controls to each of these preferred specfications following the same progression as in columns (1) through (4).

Table A12: 2SLS Main Results: Alternative IV for California

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Average NAEP Score** | | | | | | **College Enrollment Rate** | | | | | |
| Per-Pupil Spending (thousands) | 0.047 | 0.0706 | 0.0413 | 0.0503 | 0.0221 | 0.0441 | 0.0427 | 0.0598 | 0.0638 | 0.0387 | 0.0265 | 0.0285 |
| | [0.0103] | [0.103] | [0.0194] | [0.00987] | [0.0116] | [0.0139] | [0.00689] | [0.0343] | [0.0160] | [0.00587] | [0.00849] | [0.0106] |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| Pr(underid) | 3.54E-07 | 0.234 | 0.00135 | 8.27E-07 | 0.126 | 0.0713 | 9.32E-08 | 0.0396 | 0.00166 | 3.30E-07 | 0.0854 | 0.0779 |
| Kleibergen-Paap Wald F-stat | 60.63 | 1.133 | 13.35 | 29.43 | 16.69 | 19.17 | 78.72 | 7.098 | 18.63 | 42.09 | 6.726 | 7.4 |
| Pr(Endog) | 5.47E-05 | 0.319 | 0.0689 | 1.07E-05 | 0.558 | 0.129 | 2.73E-07 | 0.113 | 0.0017 | 2.14E-07 | 0.0836 | 0.0706 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* This table replicates the top panel of table A17 using an alternative specification of our instrument for California. The majority of school district property tax revenue in California is collected and redistributed by the State. In our main models, we classify this revenue as ”local.” In results presented here, we re-classify property tax revenue reported by independent school districts as ”state” revenue instead. This causes our instrument for California (share of 2008 education revenues from state sources) to increase from .58 to .78.

Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). We regress Average NAEP Scores and College Enrollment Rates on instrumented spending.

## Table A13: 2SLS Main Results: Dropping States

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Average NAEP Score** | | | | | | **College Enrollment Rate** | | | | | |
| | *Dropping DC* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0471 | 0.0181 | 0.0374 | 0.05 | 0.055 | 0.0368 | 0.0426 | 0.0339 | 0.0688 | 0.0399 | 0.0164 | 0.015 |
| | [0.0103] | [0.0319] | [0.0178] | [0.00991] | [0.0202] | [0.0166] | [0.00693] | [0.0264] | [0.0199] | [0.00619] | [0.0102] | [0.00903] |
| Observations | 450 | 450 | 450 | 450 | 450 | 450 | 400 | 400 | 400 | 400 | 400 | 400 |
| Pr(underid) | 3.82E-07 | 0.04 | 0.000827 | 1.25E-06 | 0.0874 | 0.0758 | 9.84E-08 | 0.0646 | 0.00148 | 4.20E-07 | 0.132 | 0.0787 |
| Kleibergen-Paap Wald F-stat | 62.63 | 6.352 | 23.08 | 45.59 | 14.55 | 20.84 | 78.81 | 5.407 | 23.36 | 51.92 | 10.94 | 15.62 |
| Pr(Endog) | 5.66E-05 | 0.691 | 0.101 | 1.89E-05 | 0.00303 | 0.111 | 2.96E-07 | 0.21 | 0.00583 | 3.39E-07 | 0.311 | 0.316 |
| | *Dropping CA* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0474 | 0.0696 | 0.0411 | 0.0505 | 0.0232 | 0.0442 | 0.0422 | 0.0551 | 0.0613 | 0.0381 | 0.0263 | 0.0279 |
| | [0.0105] | [0.0902] | [0.0189] | [0.00979] | [0.0106] | [0.0131] | [0.00705] | [0.0325] | [0.0159] | [0.00590] | [0.00854] | [0.0106] |
| Observations | 450 | 450 | 450 | 450 | 450 | 450 | 400 | 400 | 400 | 400 | 400 | 400 |
| Pr(underid) | 5.18E-07 | 0.168 | 0.000809 | 1.08E-06 | 0.0922 | 0.0509 | 1.55E-07 | 0.0356 | 0.00169 | 4.94E-07 | 0.0892 | 0.0796 |
| Kleibergen-Paap Wald F-stat | 61.5 | 1.543 | 17.08 | 46.74 | 22.76 | 26.69 | 75.21 | 8.517 | 21.67 | 51.25 | 7.886 | 10.12 |
| Pr(Endog) | 6.80E-05 | 0.272 | 0.0591 | 1.30E-05 | 0.828 | 0.0713 | 4.96E-07 | 0.142 | 0.00314 | 3.95E-07 | 0.107 | 0.0992 |
| | *Dropping HI* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0489 | 0.103 | 0.0413 | 0.0522 | 0.0189 | 0.0479 | 0.0423 | 0.0517 | 0.0572 | 0.0379 | 0.0243 | 0.0268 |
| | [0.0110] | [0.176] | [0.0239] | [0.0103] | [0.0116] | [0.0138] | [0.00742] | [0.0448] | [0.0182] | [0.00622] | [0.00908] | [0.0116] |
| Observations | 450 | 450 | 450 | 450 | 450 | 450 | 400 | 400 | 400 | 400 | 400 | 400 |
| Pr(underid) | 4.53E-07 | 0.353 | 0.00156 | 1.31E-06 | 0.136 | 0.0792 | 8.80E-08 | 0.0576 | 0.00292 | 5.06E-07 | 0.111 | 0.12 |
| Kleibergen-Paap Wald F-stat | 55.24 | 0.628 | 10.27 | 35.78 | 20.13 | 22.22 | 70.11 | 4.453 | 14.88 | 52.14 | 11.05 | 8.57 |
| Pr(Endog) | 8.90E-05 | 0.3 | 0.106 | 1.47E-05 | 0.453 | 0.0492 | 4.26E-07 | 0.251 | 0.00397 | 4.03E-07 | 0.229 | 0.224 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* This table replicates the top panel of table A17 dropping individual states from the analysis. Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). We regress Average NAEP Scores and College Enrollment Rates on instrumented spending, dropping either DC, California, or Hawaii from the sample to demonstrate the robustness of the main findings.

## Table A14: Main Results (Weighted)

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Average NAEP Score | | | | | | College Going Rate | | | | | |
| | *Weight = K12 Enrollment* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0603 | -0.00329 | 0.0537 | 0.0565 | 0.0469 | 0.0255 | 0.0412 | 0.0218 | 0.054 | 0.0349 | 0.00332 | 0.00537 |
| | [0.0163] | [0.0481] | [0.0187] | [0.0153] | [0.0199] | [0.00884] | [0.00746] | [0.0436] | [0.0284] | [0.00680] | [0.00768] | [0.00511] |
| Pr(underid) | 7.26E-05 | 0.314 | 0.0278 | 0.000368 | 0.129 | 0.191 | 0.000105 | 0.228 | 0.00252 | 0.000315 | 0.183 | 0.183 |
| Kleibergen-Paap Wald F-stat | 27.27 | 0.99 | 3.349 | 47.46 | 48.16 | 68.54 | 56.97 | 1.529 | 6.459 | 60.48 | 48.01 | 73.3 |
| Pr(Endog) | 0.00325 | 0.714 | 0.0386 | 0.0285 | 0.00414 | 0.15 | 0.000143 | 0.463 | 0.00601 | 2.40E-05 | 0.193 | 0.12 |
| | *Weight = Total Population* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.06 | 0.00469 | 0.0566 | 0.0563 | 0.0479 | 0.0259 | 0.0405 | 0.0195 | 0.0504 | 0.0344 | 0.00356 | 0.00667 |
| | [0.0151] | [0.0469] | [0.0189] | [0.0142] | [0.0206] | [0.00887] | [0.00757] | [0.0416] | [0.0262] | [0.00688] | [0.00747] | [0.00648] |
| Pr(underid) | 9.30E-05 | 0.349 | 0.028 | 0.000503 | 0.141 | 0.183 | 9.13E-05 | 0.231 | 0.0021 | 0.000304 | 0.188 | 0.171 |
| Kleibergen-Paap Wald F-stat | 25.27 | 0.852 | 3.49 | 48.3 | 49.84 | 70.79 | 51.03 | 1.524 | 7.184 | 56.91 | 44.37 | 67.13 |
| Pr(Endog) | 0.00144 | 0.867 | 0.0278 | 0.0125 | 0.00386 | 0.138 | 0.000116 | 0.497 | 0.00639 | 2.33E-05 | 0.164 | 0.0845 |
| | *Weight = Child-Aged Population* | | | | | | | | | | | |
| Per-Pupil Spending (thousands) | 0.0598 | -0.00168 | 0.0526 | 0.0561 | 0.0467 | 0.0255 | 0.0411 | 0.0205 | 0.0529 | 0.0348 | 0.00324 | 0.00575 |
| | [0.0158] | [0.0478] | [0.0181] | [0.0149] | [0.0199] | [0.00872] | [0.00739] | [0.0422] | [0.0273] | [0.00673] | [0.00753] | [0.00566] |
| Pr(underid) | 6.90E-05 | 0.322 | 0.0261 | 0.000339 | 0.138 | 0.187 | 9.04E-05 | 0.223 | 0.00199 | 0.000265 | 0.188 | 0.177 |
| Kleibergen-Paap Wald F-stat | 27.55 | 0.962 | 3.466 | 49.16 | 50.37 | 71.6 | 57.02 | 1.582 | 6.879 | 60.27 | 47.47 | 72.15 |
| Pr(Endog) | 0.00253 | 0.748 | 0.0401 | 0.0231 | 0.00409 | 0.153 | 0.000124 | 0.478 | 0.00585 | 2.31E-05 | 0.184 | 0.105 |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

*Notes:* This table replicates the top panel of table A17, weighting the regressions by population measures. Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). We regress Average NAEP Scores and College Enrollment Rates on instrumented spending, weighting by either K12 enrollment, total population, or child population in the state-year.

Table A15: 2SLS: College Going by Institution Type, Continued

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Black-Serving Institution* | | | *Inclusive (4-year)* | | |
| Per-Pupil Spending (thousands) | 0.00617 | 0.00845 | 0.00792 | 0.00431 | 0.00349 | 0.00321 |
| | [0.00209] | [0.00431] | [0.00497] | [0.00182] | [0.00204] | [0.00247] |
| Average rate | | 0.047 | | | 0.0385 | |
| Effective % change | 0.131 | 0.18 | 0.168 | 0.112 | 0.0906 | 0.0834 |
| | *Selective (4-year)* | | | *Most Selective (4-year)* | | |
| Per-Pupil Spending (thousands) | 0.0109 | 0.00428 | 0.00435 | -0.00212 | 0.00564 | 0.00411 |
| | [0.00452] | [0.00413] | [0.00472] | [0.00232] | [0.00229] | [0.00281] |
| Average rate | | 0.184 | | | 0.11 | |
| Effective % change | 0.0594 | 0.0233 | 0.0236 | -0.0192 | 0.0512 | 0.0373 |
| Observations | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X |
| Post | | X | | | X | |
| Bartiks | X | X | | X | X | |
| Year FX | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

*Notes:* This table is a continuation of table 8. Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IPEDS, IPUMS, and the Carnegie Foundation. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. Enrollment rates are calculated from the number of first time enrollees at institutions meeting the stated criteria (e.g. inclusive) that graduated from high school in the past year, by which state the students lived at the time of application (IPEDS), divided by the number of 17 year-olds 2 years prior to the enrollment year (IPUMS). We report the average enrollment rate at each type of institution over all states and years to scale the effects to be respective to each institution type's representation in overall enrollment rates. We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. All models control for state fixed effects and state trends and include a subset of bartik instruments, year fixed effects, and/or a post indicator to control for additional economic characteristics correlated with years. Black-Serving Institutions are defined according to IES definitions (page V) using 2005 undergraduate enrollment data from IPEDS. BSI's include Historically Black Colleges and Universities or institutions where undergraduate enrollment is at least 25% black, and no other minority group makes up more than 25% of the undergraduate student body. Inclusive, Selective, and Most Selective schools are defined by Carnegie Classifications from 2005 for all institutions. Inclusive institutions are those for whom test scores are not available or suggests that admission is offered broadly. Selective institutions are those whose incoming test scores classify the institutions between the 40th and 80th percentile of selectivity. Most Selective institutions fall between the 80th and 100th percentile in selectivity.

Table A16: NAEP by Grade and Subject (2SLS)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Average NAEP Score** | | | | | |
| | *Math* | | | *Reading* | | |
| Per-Pupil Spending (thousands) | 0.201 | 0.0017 | 0.063 | -0.0258 | 0.0067 | 0.0169 |
| | [0.0452] | [0.0228] | [0.0280] | [0.0181] | [0.00537] | [0.00421] |
| Observations | 408 | 408 | 408 | 454 | 454 | 454 |
| | *4th Grade* | | | *8th Grade* | | |
| Per-Pupil Spending (thousands) | 0.0818 | 0.025 | 0.0511 | 0.0194 | 0.0209 | 0.0368 |
| | [0.0224] | [0.0152] | [0.0187] | [0.0175] | [0.0110] | [0.0108] |
| Observations | 454 | 454 | 454 | 453 | 453 | 453 |
| State Trends | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X |
| Post*IV | X | X | X | X | X | X |
| Post | | X | | | X | |
| Bartiks | X | X | | X | X | |
| Year FX | | | X | | | X |
| Model | Linear IV | Group IV | Group IV | Linear IV | Group IV | Group IV |

Robust standard errors in brackets

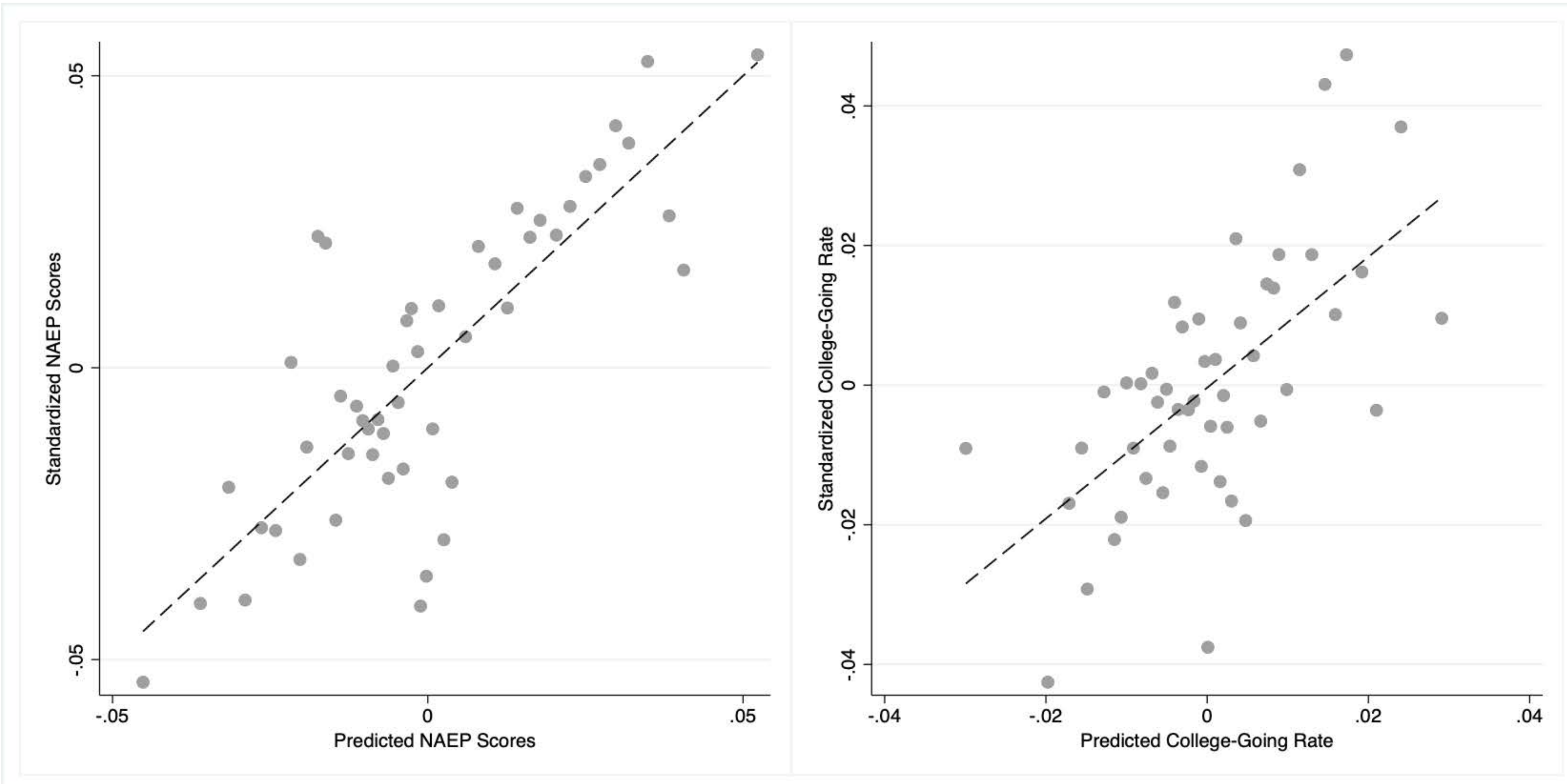
*Notes:* Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey and the NCES Public NAEP Data. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2016/2017. There was no Math NAEP test in 2002 (see table A1. Not all NAEP by grade/subject scores were available for every state in 2002 (AK, CO, NH, NJ, and SD do not have any grade/subject specific scores for 2002, Iowa does not report 8th grade specific scores for 2002). We instrument for per-pupil spending using Post*Year*IV in Linear models and Post*Year*[IV Group] in Group models. All models control for state fixed effects and state trends and include a subset of bartik instruments, year fixed effects, and/or a post dummy indicator to control for additional economic characteristics correlated with years.

Table A17: 2SLS Main Results, Controlling for Predicted Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Average NAEP Score* | | | | | | *College Enrollment Rate* | | | | | |
| Per-Pupil Spending (thousands) | 0.0283 | 0.0733 | 0.0382 | 0.0336 | 0.0234 | 0.0437 | 0.0372 | 0.0575 | 0.0608 | 0.0341 | 0.0259 | 0.028 |
| | [0.00901] | [0.0909] | [0.0192] | [0.00924] | [0.0109] | [0.0136] | [0.00756] | [0.0328] | [0.0152] | [0.00633] | [0.00859] | [0.0107] |
| Pr(underid) | 6.20E-06 | 0.162 | 0.00212 | 4.78E-07 | 0.0993 | 0.0594 | 4.73E-06 | 0.0376 | 0.000603 | 7.56E-07 | 0.0894 | 0.0787 |
| Kleibergen-Paap Wald F-stat | 42.86 | 1.649 | 11.98 | 42.32 | 23 | 26.58 | 47.85 | 8.207 | 29.57 | 45.53 | 7.95 | 9.958 |
| Pr(Endog) | 0.0118 | 0.264 | 0.081 | 0.00239 | 0.749 | 0.0601 | 3.39E-05 | 0.126 | 0.00279 | 3.19E-05 | 0.125 | 0.102 |
| Observations | 459 | 459 | 459 | 459 | 459 | 459 | 408 | 408 | 408 | 408 | 408 | 408 |
| State Trends | X | X | X | X | X | X | X | X | X | X | X | X |
| State FX | X | X | X | X | X | X | X | X | X | X | X | X |
| Predicted Outcome | X | X | X | X | X | X | X | X | X | X | X | X |
| Post-2008*IV | X | X | X | X | X | X | X | X | X | X | X | X |
| Post-2008 | X | X | | X | X | | X | X | | X | X | |
| Bartiks | | X | X | | X | | | X | X | | X | |
| Year FX | | | | | | X | | | | | | X |
| Model | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV | Linear IV | Linear IV | Linear IV | Group IV | Group IV | Group IV |

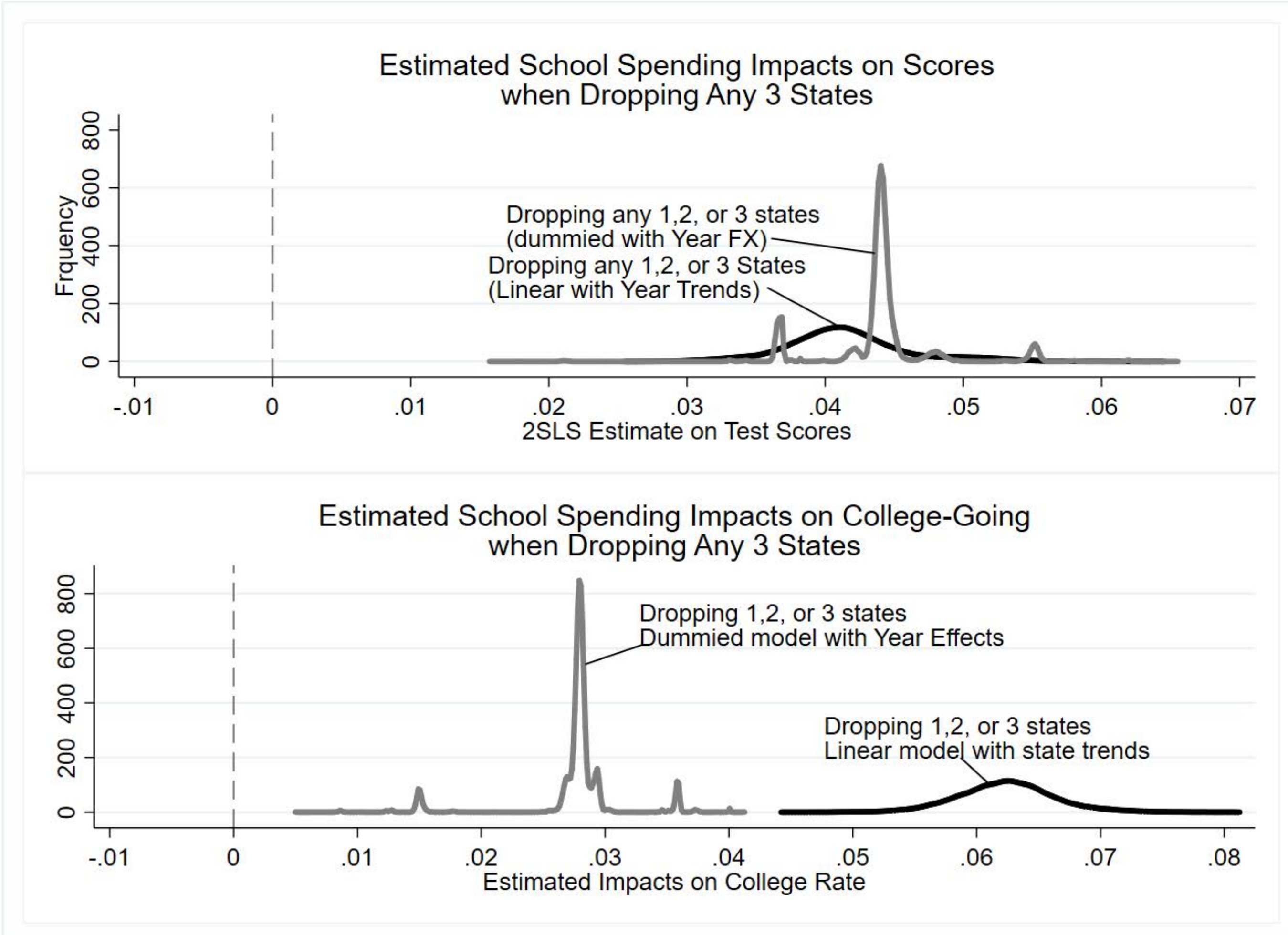
*Notes:* This table replicates the top panel of table A17, including additional controls for the predicted outcomes (predicted NAEP scores or predicted college enrollment). Robust standard errors in brackets cluster by state. Data are collected from the F33 School District Finance survey, IES NAEP results, and IPEDS. All models include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, and 2015. NAEP models also include 2016/2017. We instrument for per-pupil spending here using Post*Year*IV in Linear models (columns 1-3 and 7-9) and Post*Year*[IV Group] in Group models (columns 4-6 and 10-12). We regress Average NAEP Scores and College Enrollment Rates on instrumented spending.

**Figure A1** *Predicted Scores Versus Actual Scores.*



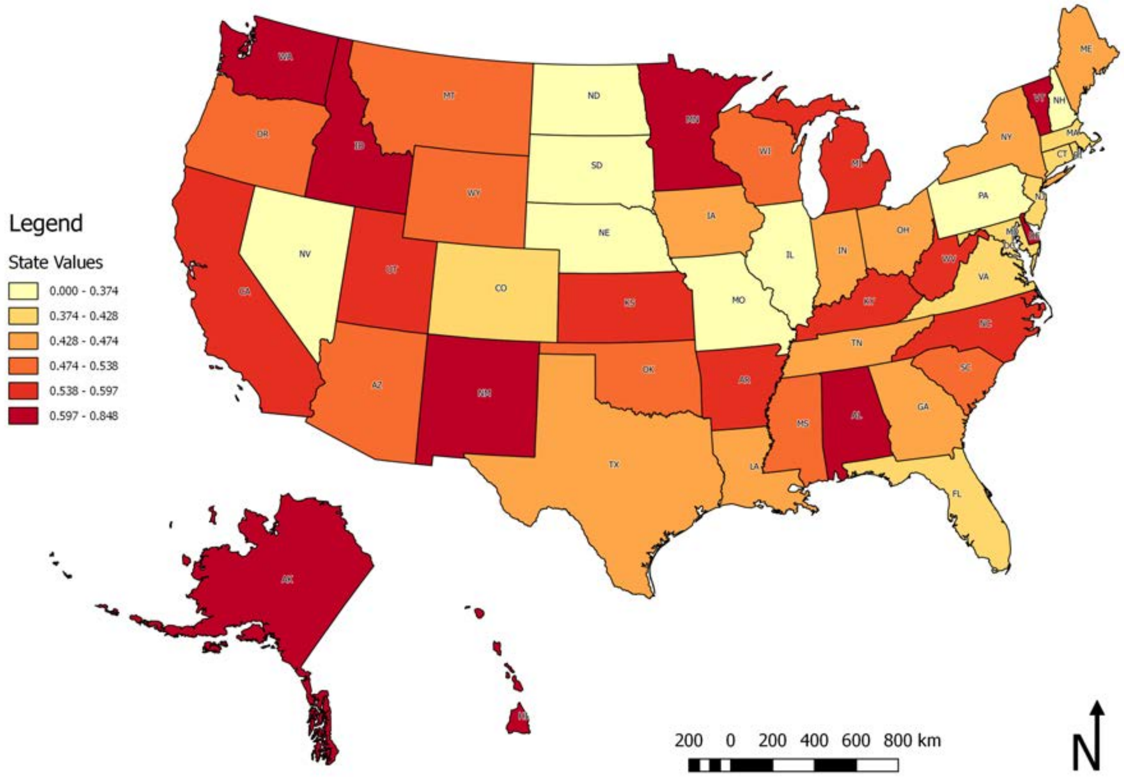

*Notes*: The two figures are binned scatter plots of the actual annual standardized NAEP test scores and actual college enrollment rates on the predicted annual NAEP scores and college enrollment rates respectively. To construct these binned scatter plots, we first predict the x-axis variable using measures of state-level population, poverty, and (un)employment. We next residualize all variables with respect to state trends. We then divide the predicted outcomes into fifty equal-sized groups and plot the means of the y-variable residuals within each bin against the mean value of the predicted outcome within each bin.

**Figure A2** *Outcome Impacts under Permutation Test.*



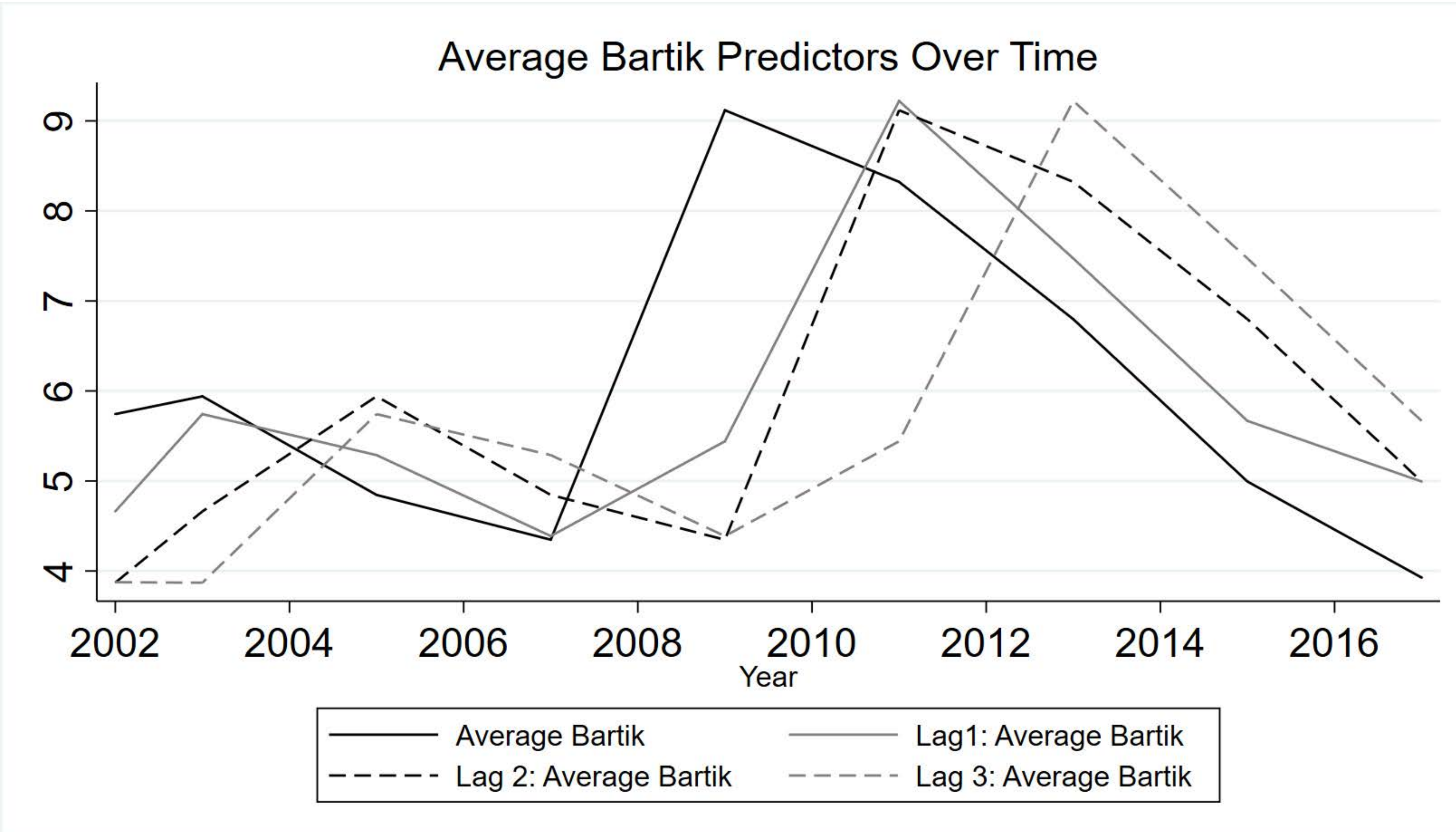

*Notes*: This density plot depict the distribution of estimated impacts while dropping states. The dashed black line depicts the distribution of estimated coefficient of log spending on NAEP scores if one drops any single state in the main model. The dashed gray line depicts the distribution of estimated 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 A3. Fraction of Education Revenue from State Sources




*Notes*: This map shows the extent of variation in $(\%)State_s$ across the United States.

Figure A4. Bartik Predictors Over Time




*Notes*: To create this Bartik predictor, 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. See the data appendix for further detail. Using this approach we predicted unemployment in year $t$, through $t-3$ to account for dynamic impacts of unemployment.