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Do School Spending Cuts Matter? Evidence from the Great Recession[†]

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During the Great Recession, national public school per-pupil spending fell by roughly 7 percent, and persisted beyond the recovery. 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 (i) other ill-effects of the recession or (ii) endogenous state policies, we use states' historical reliance on state-appropriated funds (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 spending cuts led to larger test score gaps by income and race. (*JEL* E32, H52, H75, I21, I28, J15)

During the Great Recession, real pre-tax income fell by almost 7 percent (Larrimore, Burkhauser, and Armour 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 7 percent nationally, by over 10 percent in 7 states, and more than 20 percent in 2 states (Leachman, Masterson, and Figueroa 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

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

over a century (NCES 2019c; Jackson, Johnson, and Persico 2014).[2] While compelling recent evidence based on localized quasi-experimental variation indicates that increased school spending typically improves child outcomes (Jackson 2020), 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.

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 with 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 percent 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, Johnson, and Persico 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.

Our identification strategy relies on the fact that states that were more reliant on state-appropriated funds for 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 (UI), 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-appropriated budgets are based on tax bases that are more responsive to market fluctuations than local or federal revenues (Sobel and Wagner 2003; Evans, Schwab, and Wagner 2019)—a revenue effect. For both these reasons, while overall school spending declined after recession onset, revenues from state-appropriated funds (hereafter "state taxes") fell the most sharply (right panel of Figure 2). In Section II, we discuss why this was true even though the Great Recession was associated with declining house prices. We show that a state's reliance

[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 (Picchi 2019).

[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.

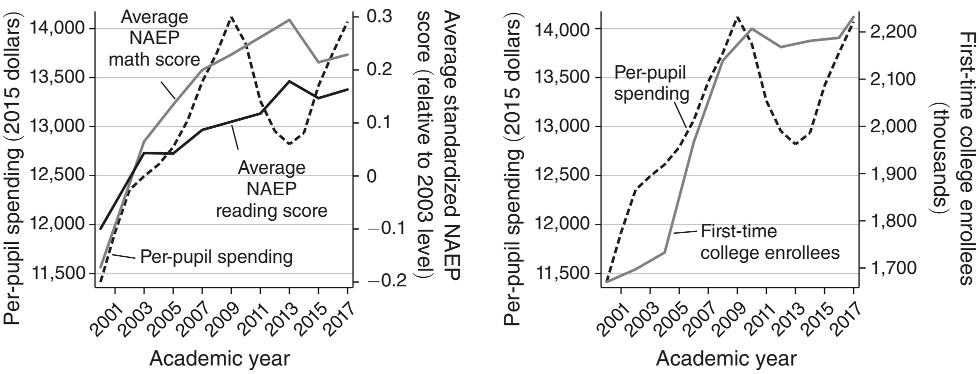

Figure 1. School Spending, NAEP Scores, and College Enrollment over Time

Notes: Left: This figure plots the 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 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.

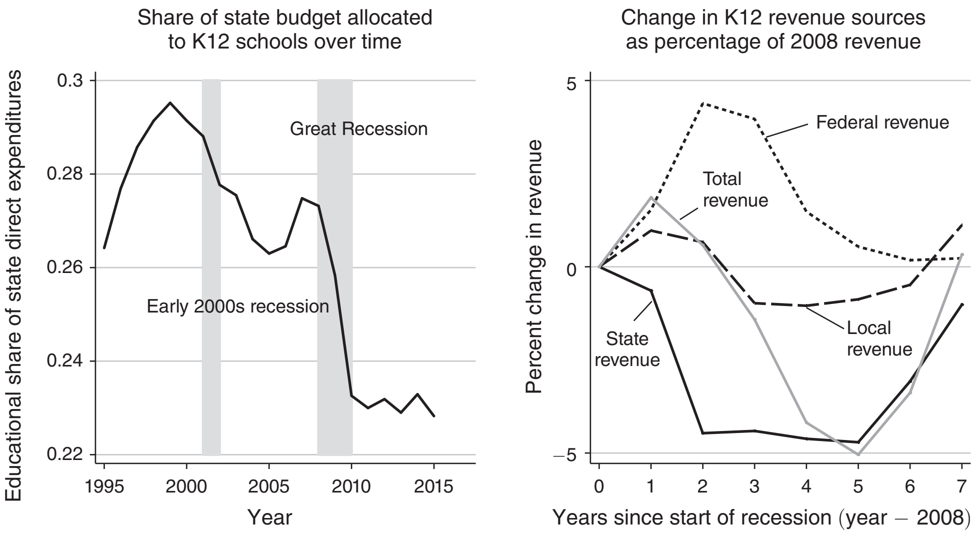

Figure 2. Source of Revenues for K12 Education after the Recession

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 US Census Bureau Annual Survey of State and Local Finances (obtained through the Urban Institute-Brookings Institution State and 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 prerecession levels thereafter.

on state-appropriated funds to finance 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.

Because we control for national time shocks directly, our instrumental variables model compares the differences in the change in student attainment after the recession across states with a high versus low reliance on state-appropriated revenues. Our strategy requires that states with different reliance on state-appropriated funds 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, median incomes, poverty rates, or demographic changes such as population size and racial composition. 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 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. We show that spending levels remained low even after the recession ended, likely because many states reduced support for public K12 schools in the wake of the recession (see Figure 2). The pattern of greater deteriorating outcomes after the recession for states that are more reliant on state appropriated revenues (*that continues through the recovery*) is mirrored for both test scores and college-going rates—compelling evidence that these impacts are driven by the continued spending declines rather than other effects of the recession per se. Using these patterns in an instrumental variables framework, our preferred models show that, on average, a \$1,000 reduction in per-pupil spending reduced test scores by 0.0385 standard deviations and lowered the college-going rate by 1.24 percentage points.

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, Rothstein, and Schanzenbach 2018). We then estimate the impact of recession-induced spending changes on this slope. We find a positive impact of spending on this slope, indicating that test scores in high-poverty areas were more adversely affected by the spending cuts than in low-poverty areas. We also examine impacts by student race. In general, the marginal impacts are larger for Black and white students and not distinguishable from zero for Hispanic students. Looking at test score gaps, a $1,000 reduction in per-pupil spending increased states' Black-white test score gaps by roughly 6 percent. We also explore heterogeneity in the college-going effect by institution type. We primarily find decreases at 2-year and 4-year public colleges. Importantly, our instrumented school spending reductions are not systematically correlated with changes in in-state tuition, ruling out a direct tuition effect.

To explore mechanisms, we examine what kinds of spending categories were most affected. States that cut spending (as predicted by our instruments) hired fewer teachers and other personnel. However, states responded to spending cuts by disproportionately cutting more from capital expenditures and less from core K12 spending. These patterns differ from those documented for spending increases due to school finance reforms (Jackson, Johnson, and Persico 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 compare our estimates to existing work (Jackson 2020, Jackson and Mackevicius 2021). 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-collected taxes account for about 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 1996). As such, poor districts tended to spend less per pupil than wealthier districts. The school finance reform movement (starting in the 1970s) led 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

[4] These different marginal propensities to spend across categories could have lead to asymmetric spending impacts *if* the marginal benefit of capital spending differed markedly from that of other kinds of spending. However, as documented in Jackson (2020) capital 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).

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, Page, and Stevens 2008; Ananat et al. 2011; Stevens and Schaller 2011) and increased food insecurity (Gundersen, Kreider, and Pepper 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 I describes the data. Section II describes the empirical strategy. Section III presents the results. Section IV concludes.

I. Data

We link several data sources for our analysis.[5] School finance data come from the Annual Survey of School System Finances at the US Census Bureau (US Census Bureau 2017). 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 2017. They provide education revenue broken down by source (local, state, and federal) and break down expenditures into broad categories. Using the CPI (BLS 2000–2017) all dollar amounts are adjusted to 2015. The share of school spending from each source varies substantially by state. Between 2002 and 2017, the average share of school revenue from federal, state, and local sources (i.e., appropriations) were 9.54 percent, 48.7 percent, and 41.7 percent, respectively. While the share of federal revenue varies only between 4 percent (Connecticut and New Jersey) and 16 percent (Mississippi), variation in local and state revenue sources is much broader. The share of funding that comes from state sources varies between 0 percent (Washington, DC) and 87 percent (Hawaii). The share of revenue coming from state appropriations is central to our empirical strategy. We discuss how we use this variable to classify states in Section II. On average, roughly 85 percent of public school spending goes to current elementary and secondary spending, which broadly includes expenses for instruction and support services. About 10 percent of public K12 education expenditures go towards capital, which includes construction, land, and equipment. Salaries and benefits (instructional and noninstructional) make up 67.1 percent of public school spending on average.

Test score data come from the NAEP—referred to as the Nation's Report Card as it tests students across the country on similar assessments 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 (NAEP 2017).[6] We focus on public school students' fourth and eighth grade math and reading assessment scores. To facilitate comparisons over time, we report NAEP

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

[6] For five states (Alaska, Colorado, New Hampshire, New Jersey, and South Dakota), the public-release NAEP data do not report scores in 2002. To obtain a balanced panel, we compute state-level averages using the restricted-use NAEP (NCES 2018) for these five states. The results are virtually identical with or without these observations.

scores standardized to a base year of 2003. The NAEP sample has been increasing over time and only stabilized after 2000 (online Appendix Table A1). We focus on the period between 2002 and 2017. To conduct subsample analyses, we also utilize restricted-use NAEP data files with student-level scores and demographics (NCES 2018). 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 IPEDS (NCES 2019). These data report surveys submitted by postsecondary institutions. These data do not have student-level information. Institutions report on the number of first-time college freshmen 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 (Carnegie Classification of Institutions of Higher Education 2005), we compute enrollments by college type (2-year versus 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 census from 2000 to 2017 (US Census Bureau 2019a, 2020b). Our college-going measure is the number of first-time college enrollees divided by the average of the number of 17-year-olds and 18-year-olds in the state the year prior to enrollment.[7]

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 (US Census Bureau 2019b). We also use area economic indicators of employment and wages from the Bureau of Labor Statistics (US BLS 2018, 2019a, 2019b) and an annual measure of home values in each state from Zillow (Zillow 2018). 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 2019). Our state-year level dataset is summarized in Table 1.

II. 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 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 state-collected and state-appropriated revenues to fund public K12 schools were more likely to experience declines in school spending for reasons unrelated to the

[7] Our results are robust to using other definitions of the base cohort and to using the log of college enrollment.

Table 1—Summary Statistics

	Observations	Mean	SD
Panel A. School Spending			
Per-pupil spending (2015 dollars)	510	13,208	3,807
Share of revenue from state sources in 2008 (Ω_s)	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	0.138
Share of revenue from local sources	459	0.417	0.143
Panel B. Per-pupil spending by use (2015 dollars per pupil)			
Elementary/secondary (operations)	459	11,383	3,218
Instructional	459	6,883	2,075
Services	459	4,019	1,239
Capital	458	1,285	818.7
Construction	455	906.9	541.8
Nonconstruction	459	332.5	175.1
Other	459	481.1	106.5
Salaries/benefits			
Instructional salaries	459	4,549	1,187
Noninstructional salaries	459	2,212	568.4
Benefits	459	2,145	879.9
Panel C. Students per staff			
Teachers	447	15.41	2.517
Aides	428	66.04	26.37
Guidance counselors	441	445.5	145.7
Library staff	448	659.9	840.4
Panel D. Demographic and economic indicators			
K12 enrollment (thousands)	510	940.77	1,106.76
Total population (thousands)	510	5,839.43	6,579.29
Child-age population (thousands)	510	1,024.73	1,185.45
Percent of total population in poverty	510	13.25	3.316
Percent of child-age population in poverty	510	16.62	5.227
Unemployment rate	510	5.777	1.965
Annual average employment (thousands)	510	2,603.81	2,820.57
Zillow home value index	506	174,078	82,015
Panel E. Outcomes			
Average NAEP score	459	0.125	0.211
Slope	390	−3.552	1.171
College enrollment rate	459	0.475	0.0778
4-year schools	459	0.324	0.0685
2-year schools	459	0.137	0.0578
Public	459	0.352	0.0678
Private	459	0.100	0.0564

Notes: All dollar amounts are CPI-adjusted to 2015. Variables are collected for each state in 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017. College-going variables are available for 2001 instead of 2002 based on required survey years. We report demographic and economic indicators, as well as per-pupil spending, for both 2001 and 2002. 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.

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 noneducation 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 2. Prior to the Great Recession, states spent about 27 percent of their budgets on K12 schools. However, after the Great Recession, this fell to about 23 percent 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 percent to about 27 percent. This suggests that 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. Note that because the crowd-out effect continued well beyond the recession, the resulting decline in K12 spending may persist even after the economy recovered—indeed we show that this is the case.

The second reason that greater reliance on state appropriations 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 (i) taxes are collected on assessed values, which follow market value with a considerable lag (Lutz 2008), and (ii) policymakers often offset declines in assessed values with higher tax rates (McMillen 2011; Lutz, Molloy, and Shan 2011).[8] 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 appropriations to fund K12 school would experience deeper education budget cuts (Chakrabarti, Livingston, and Setren 2015; Leachman and Mai 2014). We refer to this as the revenue channel.

We define the parameter Ω_s as the share of state K12 revenues in state s that came from state appropriations in 2008 (the 2007–2008 academic year).[9] Through both channels, Ω_s is meant to capture vulnerability to recessionary school spending cuts.[10] We classify states based on the source of the revenue as reported in ASSSF (2017) at the US Census Bureau. For almost all states, this classification captures both channels outlined above. An example of a highly vulnerable state is Hawaii.

[8] Evans, Schwab, and Wagner (2019) show that revenues from property taxes actually grew for three years after recession onset.

[9] Following Evans, Schwab, and Wagner (2019), we compute the share of K12 revenues in state s that came from state sources in 2007–2008 (determined prerecession) across all districts in the state as follows:

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

The term $StateRevenue_d$ denotes the K12 education 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.

[10] Because some states (i.e., Hawaii and DC) have a single school district, the "state" and "district" are the same set of people. However, state-appropriated and locally-appropriated budgets are governed by entirely different bodies (with different responsibilities) and rely on different revenue streams that are differentially procyclical online Appendix Figure A1 shows that Ω_s is evenly distributed across the geographic regions of the nation. We report the values of Ω_s for each state in online Appendix Table A2.

In 2008, Hawaii received 85 percent of its education funding from the state (making it vulnerable to the crowd-out effect), and 75 percent of its state revenues came from income or sales taxes (making a large share of its revenue sensitive to the business cycle).[11] An example of a less vulnerable state is Illinois. In 2008, Illinois received 33 percent of its K12 spending from the state (making it less vulnerable to the crowd-out effect), and only about 30 percent of K12 revenues comes from income or sales taxes (making a relatively small share of its revenue sensitive to the business cycle).[12] To further elucidate our instrument, we discuss a less straightforward state—California. Under California's Proposition 98, there is a guaranteed K12 funding level determined by the state. The state collects locally raised property tax revenue and supplements these funds with the state General Fund to meet the guarantee. Overall 58 percent of K12 spending comes from the State General Fund, of which income, corporate, and sales tax comprise 98 percent.[13] From the centralization perspective, one may consider the locally raised taxes that are collected by the state to be state revenue. However, because the local taxes are typically earmarked for schools in those districts that collected it, from a centralization standpoint it makes sense to classify these funds as local (i.e., less sensitive to the centralization channel). From the revenue cyclicality perspective, the property taxes collected under proposition 98 are less variable than income and sales taxes so that one would classify them as local taxes. For both these reasons, we follow the US Census Bureau and classify these locally raised funds as local funds and the general fund as state funds. Doing so, we classify 58 percent of California's education revenue as coming from state sources. All of our results are robust to how we classify these potentially ambiguous funds and to dropping California (see online Appendix Table A14).[14]

Through both the crowd-out and revenue channels, while overall school spending declined after recession onset, revenues from state-collected 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. To show that this holds empirically, Figure 3 plots the linear trend in per-pupil spending before the recession (2003–2007) on the left, and after recession onset in the middle (2007–2017). The left panel shows that most states experienced per-pupil spending growth in the lead up to the recession (as shown nationally in Figure 1). It also shows that states that were more reliant on state revenues had similar spending increases

[11] Information on revenue sources come from Saito (2018).

[12] See the Illinois Department of Revenue Annual Report of Collections and Distributions (IDOR 2009).

[13] See the Public Policy Institute of California Report (Murphy and Paluch 2018) and the CA Legislative Analyst's Office Report (Petek 2019).

[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 US Congress jurisdiction over the District. This is evidenced by the fact that the Federal government covers about one-quarter of DC's budget, and that DC received considerable additional Federal funds during the recession. From the centralization perspective, we follow the census categorization and classify municipality funds that support education as "local" and set state revenues equal to zero. This suggests that DC experiences little vulnerability to recessionary spending cuts due to our instrument. Based on Figure 2, this categorization holds empirically. Importantly, because this is a judgment call, we show that all of our results are robust to dropping data from DC (see online Appendix Table A13).

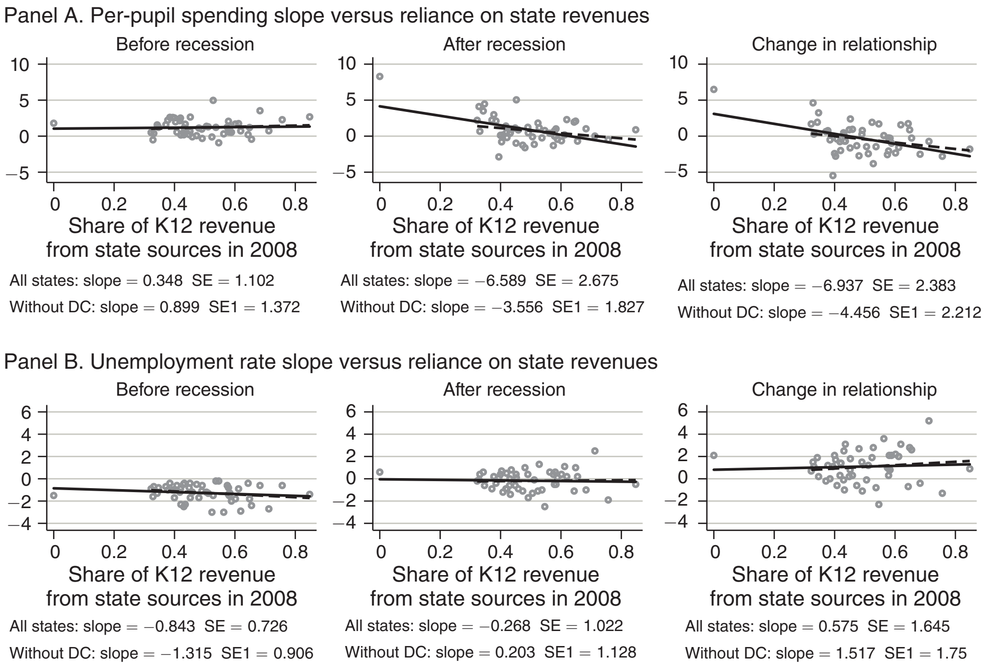

FIGURE 3. FRACTION OF K12 REVENUE FROM STATE SOURCES: SPENDING GROWTH AND UNEMPLOYMENT

Notes: This figure shows the relationship between the share of K12 revenues that came from state sources in 2008 (i.e., Ω_s) and the change in per-pupil spending (panel A) and the unemployment rate (panel B) over time. The left panels show this relationship before the recession, the middle panels show this relationship after recession onset, and the rightmost panels show the change in this relationship before versus after recession onset. The solid lines are the lines of best fit for all states, and the dashed lines are the lines of best fit excluding DC.

as other states. In contrast, the middle panel 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 right panel shows the *change* in the linear trend for each state after versus before recession onset against Ω_s. Relative to each state's own prerecession trend in spending, states that were more reliant on state-appropriated revenues to fund public K12 schools experienced larger annual spending reductions after recession onset. The negative relationship between Ω_s and the change in slope is statistically significant at the 5 percent level (and is similar both with and without the potentially influential data-point—DC).

The pattern of larger spending cuts in states that were more reliant on state-appropriated revenues to fund public education motivates our instrumental variables approach. We use functions of Ω_s interacted with the timing of the recession as exogenous shifters in K12 spending within states. For our approach to uncover a school spending effect, Ω_s should not be correlated with changes in other policies or economic conditions within states. To assess this, panel B of Figure 3 plots changes in the trend in state unemployment rates before versus after the recession by Ω_s. While there was a general increase in unemployment in the average state, Ω_s was unrelated to the impact of the recession in that state. We present more formal tests below.

A. *Identification Strategy*

Our identification strategy is to compare the change in outcomes after the recession, across states that were more or less reliant on state-appropriated 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. We will show that relative to each state's own prerecession trend in school spending, states that were more reliant on state-appropriated revenues to fund public education (in 2008) had a more negative postrecession time trend in school spending. If school spending affects outcomes, the change in the trend in school spending should correspond with a change in the trend in test scores and college-going. We show this.

To motivate our regression models, in Figure 4, we present an event study for the recession's effect on K12 spending, 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}:

$$Y_{st} = \sum_{t=2003}^{2017} \beta_t \cdot \left(I_{\Omega_s > q(50),s} \times I_{T=t}\right) + \gamma_s + \left(\tau_s \times T\right) + \upsilon_{st}. \tag{1}$$

In (1), $I_{T=t}$ is an indicator denoting if the observation is for calendar year t and $I_{\Omega_s > q(50),s}$ denotes the states that are more reliant on state revenues for public schools (that is, states that have above-median reliance on state revenues in 2008). To account for differences across states we include state fixed effects γ_s. To compare changes in each state's outcome to its own prerecession time trend, we include the state-specific linear time trends τ_s.[15] The variable υ_{st} is a random error term. The coefficients β_t map out the differences in outcomes between states with low and high Ω_s in each year (relative to each state's own prerecession intercept and linear time trend). We estimate this model by OLS on per-pupil spending in thousands (in 2015 dollars), average state-level NAEP scores, and college-going rates. We plot these coefficients along with the 95 percent confidence interval for each coefficient estimate in Figure 4 where the reference year is 2007.

The event study for test scores is in panel A of Figure 4. As suggested by Figure 3, after recession onset, states with heavy reliance on state revenues experienced a gradual decline in per-pupil spending. While one might have expected a sudden decline in spending in 2009, the infusion of federal money (see Figure 2) due to the The American Recovery and Reinvestment Act of 2009 (ARRA) softened the blow of the recession, making the spending declines gradual rather than sudden. The decline is roughly linear in time since recession onset. Average NAEP scores (panel B) and college-going (panel C) followed a similar pattern. Student test scores

[15] Note that because estimation of a linear time pre-trend requires the exclusion of two prerecession years, we exclude indicators for 2007 (the last prerecession year and the reference year) and the first year the outcome is observed (2002 for test scores or 2001 for college-going).

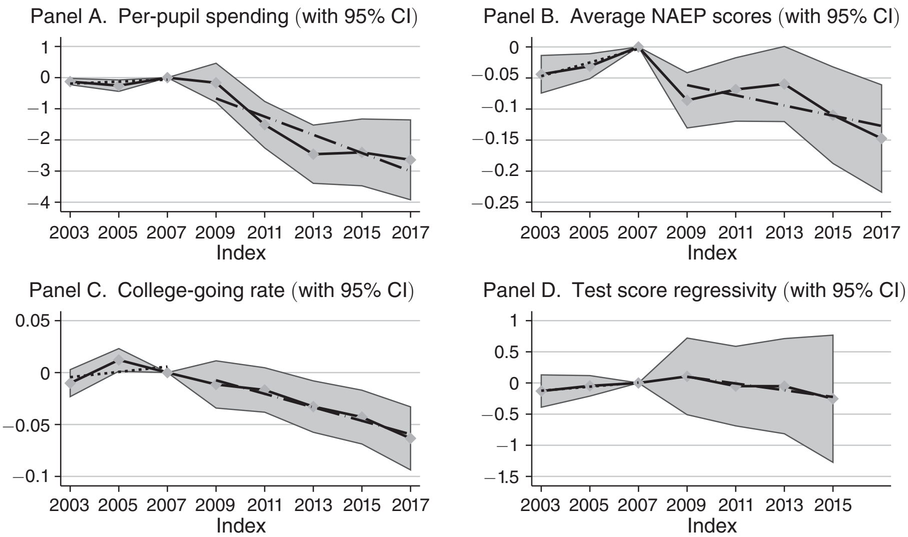

Figure 4. Difference in Spending and Outcomes between States with High and Low Reliance on State Revenues over Time

Notes: The dashed connected lines depict the coefficients on the individual calendar year indicators interacted with an indicator for above-median reliance on state revenue in 2008, $\Omega_s > 0.48$. The dashed lines represent the linear fit during the prerecession period/cohorts (negative values of exposure) and postrecession periods/cohorts (nonnegative values of exposure). The pattern for per-pupil spending is presented in panel A; the pattern for test scores is presented in panel B; the pattern for college-going is in panel C; and the slope between the poverty rate and NAEP scores is shown in panel D.

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, the differential change in the linear trend in these outcomes by Ω_s is statistically significant. 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 for providing visual evidence that our estimated relationship may be causal, we now turn to the formal first-stage and reduced-form regression results below.

The Instrumental Variables Regression Model.—Using the variation outlined above, our instrumental variables model compares the differences in the change in the trend in student attainment after the recession across states with a high or low fraction of revenue from state sources, while accounting for the possible direct recessionary effects on outcomes. To classify states by Ω_s, we look to the empirical patterns. Figure 3 shows that there are states with low reliance on state-appropriated funds with a positive change in slope, states with moderate reliance with little change in slope, and states with high reliance with a negative change in slope. As such, we model variation in Ω_s by classifying states as having low, medium, or high reliance on state taxes to fund public K12 schools.[16] Schools that have one-third or less 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 $(g = 3)$.[17] The group indicator variable I_{gs} connotes the group g of state s. To capture the trend change variation in spending parametrically, we model school spending as declining linearly starting with recession onset (as indicated in Figure 1). Formally, using the state-by-year level panel, we estimate systems of equations of the following form by 2SLS:

$$(2) \quad PPE_{st} = \sum_{g=2}^{3} \left[\pi_{1g} \cdot \left(I_{gs} \times I_{post} \times (T - 2008)\right)\right] + \sum_{g=2}^{3} \left[\phi_{1g} \cdot \left(I_{gs} \times I_{post}\right)\right] + \delta_1 C_{st} + \theta_{1t} + \alpha_{1s} + \left(\tau_{1s} \times T\right) + \varepsilon_{1st};$$

$$(3) \quad Y_{st} = \beta \cdot \left(PPE_{st}\right) + \sum_{g=2}^{3} \left[\phi_{2g} \cdot \left(I_{gs} \times I_{post}\right)\right] + \delta_2 C_{st} + \theta_{2t} + \alpha_{2s} + \left(\tau_{2s} \times T\right) + \varepsilon_{2st}.$$

The endogenous treatment, PPE_{st}, is per-pupil school spending in state s during year t. The outcome Y_{st} is either (i) the average standardized NAEP test scores for students in state s in year t, or (ii) the college-going rate for 17- and 18-year-olds 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 α_{1s} and α_{2s} in the first and second stage, respectively. The variable T is a scalar in the calendar year, and I_{post} is a postrecession indicator denoting all years after 2008. The variable $(T - 2008)$ represents time relative to the 2008–2009 school year (the first post-recession-onset year in our data). To compare changes in each state's outcome to its own time trend, we include the state-specific linear time trends τ_{1s} and τ_{2s} in the first and second stages, respectively. This accounts for prerecession time-trend differences between high and low Ω_s states. To capture the roughly linear-in-time decline in spending for more reliant states after the recession, the two excluded instruments are the interactions between the group indicators and the postrecession linear time trend, $I_{gs} \times I_{post} \times (T - 2008)$. To account for any level shift in outcomes at recession outset, models also include a level shift after the recession for each group $(I_{gs} \times I_{post})$ as controls. The variables ε_{1st} and ε_{2st} are random error terms.

[16] In the online Appendix, we show that defining the instrument based on groups of states yields a much stronger first stage than a linear specification. This is because the grouping of states better captures the relationship between Ω_s and the change in slope than a straight line. In the online Appendix, we show that our 2SLS results are similar in models that specify the change in slope to be linear in Ω_s.

[17] See online Appendix Table A2 for the values of reliance on state sources to fund public schools in 2008 by state. The low group includes DC, Nebraska, and Illinois. The high group includes Hawaii, Arkansas, New Mexico, and Vermont. All other states are in the middle group. To assuage concerns that these groupings are small, in Section IIIA, we show that our effects are robust to dropping any three states.

Because the recession may have had ill economic effects through channels other than school spending, we must control for underlying predictors of recession intensity itself. To this aim, θ_{1t} and θ_{2t} are individual year fixed effects for the first and second stage, respectively. The year fixed effects account for overall changes in spending and outcomes that may have occurred due to the recession. Accordingly, the identifying variation comes from comparing the change in trend at recession onset for states with higher levels of reliance on state-appropriated revenues (relative to the omitted low-reliance group).[18] While year fixed effects account for national economic conditions, we follow convention in the urban and regional economics (see Baum-Snow and Ferreira 2015) and also account for *state-specific recessionary shocks* by including Bartik predictors of each state's unemployment rate *and* average income level in the state in C_{st}.[19] If the only reason for a *differential* postrecession change in the trend in outcomes across areas with high and low Ω_s is the differential effect of the recession on public K12 spending across these states, then our instrument is valid. We will present many empirical tests showing that this condition is likely satisfied.

B. *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. Identification in our model comes from comparing the difference in the change in the trend in outcomes between states with differential reliance on state-appropriated revenues to fund public schools. As such, before presenting the differences in the change in trends across states, it is helpful to present the underlying trend changes in spending for states with high and low reliance on state-appropriated revenues. To this aim, column 1 presents a model with state fixed effects and state-specific trends but without year fixed effects. We present the coefficients on the change in trend for all three groups of states. In column 1, the coefficient on $I_{post} \times \left[T - 2008\right] \times \left(\Omega_s \leq 0.33\right)$ is 0.448 (p-value < 0.01). That is, despite the national downturn, relative to their own linear pre-trends, states that were least reliant on state funds had public school spending increase by \$448 per year. In contrast, and consistent with a monotonic relationship between Ω_s and school spending declines, the coefficient for $I_{post} \times \left[T - 2008\right] \times \left(0.33 < \Omega_s < 0.66\right)$ is negative and significant, and that for $I_{post} \times \left[T - 2008\right] \times \left(\Omega_s > 0.66\right)$ is even more negative and significant. Specifically, the middle group had declines of \$256 per year, while the most reliant states had declines of \$511 per year—relative to these states' pre-trends. These differential trend changes illustrate that the national aggregate decline in per-pupil spending was not uniform across states. Indeed, the standard deviation of average state-level K12 spending increased after recession onset. The estimates indicate

[18] We show an event study figure corresponding to this Group IV specification with year fixed effects in online Appendix Figure A4.

[19] Following Yagan (2017), to create these key controls, 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 (and the income) in that industry for each year. For each state, we sum these products across all industries in each year. We provide further detail in the online Appendix.

Table 2—First Stage and Reduced Form

Outcome	First stage: Per-pupil spending (thousands)			Reduced form: Average NAEP		Reduced form: College enrollment rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$I_{post} \times (0.33 < \Omega_s < 0.66)$	−1.248	−1.248	−1.897	−0.0508	−0.0197	0.00322	−0.00767
	[0.304]	[0.306]	[1.155]	[0.0404]	[0.0266]	[0.0168]	[0.0111]
$I_{post} \times (\Omega_s > 0.66)$	−2.598	−2.598	−3.144	−0.024	0.00616	0.0182	0.00947
	[1.030]	[1.039]	[1.409]	[0.0428]	[0.0267]	[0.0166]	[0.00907]
$I_{post} \times (T - 2008) \times (\Omega_s < 0.33)$	0.448						
	[0.194]						
$I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$	−0.256	−0.705	−0.669	−0.0269	−0.029	−0.00634	−0.0054
	[0.0405]	[0.200]	[0.154]	[0.00874]	[0.0103]	[0.00358]	[0.00308]
$I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$	−0.511	−0.96	−0.941	−0.0341	−0.034	−0.0111	−0.0103
	[0.0631]	[0.206]	[0.150]	[0.00947]	[0.0112]	[0.00393]	[0.00368]
Kleibergen-Paap Wald F-statistic (slope only)	37.02	13.38	21.71	6.531	4.717	4.402	4.107
Observations	459	459	459	459	459	459	459
State trends	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
I_{post}	X	X^s	X^s	X^s	X^s	X^s	X^s
Bartiks			X		X		X
Year fixed effects		X	X	X	X	X	X

Notes: Robust standard errors in brackets are clustered by state. The first-stage models regress per-pupil spending (in 2015 dollars) on our exogenous instruments and controls. The reduced-form results regress our main outcome variables of interest—standardized NAEP scores and college enrollment rate—on the same exogenous instruments and controls. Ω_s is the share of spending from the state in 2008. I_{post} is a dummy variable equal to 1 if the observation is after 2008, and $(T - 2008)$ represents years relative to the 2008–09 school year. All models include data from 2003, 2005, 2007, 2009, 2011, 2013, and 2015. The first-stage and NAEP models also include 2002 while the college enrollment models include 2001. See the online Appendix for first-stage results that include 2001 instead of 2002 to align with the college-going models. All models control for state fixed effects and state trends. We add additional controls (employment and income Bartik predictors and year fixed effects) to account for additional trends in timing. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well. First-stage and reduced-form results for additional, preliminary models are reported in online Appendix Table A3.

that, for the least-reliant states, spending between 2009 and 2015 increased by roughly $6 \times 448 = \$2{,}688$ above trend (an increase of about 18 percent relative to average spending levels in 2009), while for the most-reliant states spending between 2009 and 2015 decreased by roughly $6 \times 511 = \$3{,}066$ below trend (a decrease of about 21 percent). These estimates indicate that our instruments may identify large differences in the changes in the trend in spending across states. We now focus on the differential changes in trends across states, which is the variation we use for identification.

Column 2 presents the coefficients on the excluded instruments when the dependent variable is the level of spending in thousands with year fixed effects but without the Bartik controls. Because this model includes year fixed effects, the excluded instruments are indicators for the medium- and high-reliance states (relative to the least-reliant states). As one can see, 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 $I_{post} \times [T - 2008] \times (0.33 < \Omega_s < 0.66)$ is negative and significant, and that for $I_{post} \times [T - 2008] \times (\Omega_s > 0.66)$ is even more negative and significant. We add the Bartik controls to account for state-specific economic shocks in column 3. The results in columns 2 and 3 are largely unchanged so we focus on the point estimates from the model in column 3 with all controls. In column 3, relative to the low-reliance group (the omitted group), after recession onset the middle-reliance group spent about \$669 less per year (p-value < 0.01), and the high-reliance group spent about \$941 less per year (p-value < 0.01). The first-stage F-statistic for our two excluded instruments $I_{post} \times [T - 2008] \times (0.33 < \Omega_s < 0.66)$ and $I_{post} \times [T - 2008] \times (\Omega_s > 0.66)$ is 21.71. This is our preferred model.[20]

Echoing the patterns presented in Figure 4, we present the reduced-form estimates for average NAEP scores in columns 4 and 5. The basic pattern of the change in trends in spending are mirrored for NAEP scores. The test score declines are larger for the states that were more reliant on state revenues. Relative to states with low reliance on state revenues, after recession onset NAEP scores fell by roughly 0.029σ per year in medium-reliance states and fell by by roughly 0.034σ per year in high-reliance states. That is, relative to states' own pre-trends, between 2009 and 2015 test scores decreased by roughly $6 \times 0.034 = 0.204\sigma$ more in the least-reliant sates relative to the most-reliant states. The standard deviation of mean scores across states before the recession was 0.22σ so that this is an economically meaningful effect. The pattern of the change in trends in spending are also mirrored for college-going. In the preferred model (column 7), 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 0.0054 per year (i.e., 0.54 percentage points) in medium-reliance states, and fell by roughly 0.0103 per year (i.e., 1.03 percentage points) in high-reliance states. To put these reported effects into perspective, it is helpful to look to the instrumental variables regression that relate the changes in outcomes (due to the instruments) to the induced changes in spending. We present such analyses below.

III. Results

The event study plots in Figure 4 and the reduced-form patterns in Table 2 show that the reductions in school spending during the Great Recession (as predicted by states' reliance on state-appropriated revenues) coincided with declines in NAEP scores and college-going rates. We now quantify this plausibly causal relationship using our 2SLS models.

Test Scores.—Models 1 through 4 of Table 3 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 estimate OLS models that do not instrument for school spending (online Appendix Table A5). In OLS models with no controls, the estimated coefficient on per-pupil spending is 0.00551 and is marginally significant at the 10 percent level. With Bartik controls, the point estimate

[20] Given that our college-going sample includes 2001 instead of 2002 (like the NAEP sample) based on differences in data availability, we also present first-stage results for the college-going sample in online Appendix Table A4.

Table 3—2SLS Main Results

	Average NAEP score				College enrollment rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Per-pupil spending (thousands)	0.0498	0.0394	0.0365	0.0385	0.0255	0.0272	0.0127	0.0124
	[0.00991]	[0.0102]	[0.0103]	[0.0110]	[0.00403]	[0.00563]	[0.00303]	[0.00387]
Kleibergen-Paap Wald F-statistic	37.02	31.19	13.38	21.71	109.6	46.27	21.93	36.97
	Predicted NAEP score				Predicted college enrollment rate			
Per-pupil spending (thousands)	0.00323	0.00428	−0.000114	−4.75E-05	0.00519	0.00488	0.00427	0.00357
	[0.00205]	[0.00229]	[0.00281]	[0.00338]	[0.00131]	[0.00221]	[0.00322]	[0.00398]
Observations	459	459	459	459	459	459	459	459
State trends	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
I_{post}	X	X	X^s	X^s	X	X	X^s	X^s
$I_{post} \times I_{gs}$	X	X	X	X	X	X	X	X
Bartiks		X		X		X		X
Year fixed effects			X	X			X	X

Notes: Robust standard errors in brackets are clustered 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 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017. NAEP models (columns 1–4) also include 2002 while college models (columns 5–8) include 2001. In models with year fixed effects (columns 3–4 and 7–8), we instrument for spending using $I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$, and $I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$, where I_{post} is a dummy variable equal to 1 if the observation is after 2008, Ω_s represents the share of the state's education funding from state sources in 2008, and $(T - 2008)$ represents years relative to the 2008–2009 school year. Models without year fixed effects (columns 1–2 and 5–6) also include $I_{post} \times (T - 2008) \times (\Omega_s < 0.33)$. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well. I_{gs} is an indicator variable for whether the state's value of Ω_s is in the low, middle, or high group. 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 online Appendix Table A8) on instrumented spending to demonstrate that instrumented spending is not endogenous to economic and demographic characteristics that are also correlated with academic outcomes.

falls to 0.00318 and is no longer marginally significant. In models with Bartik controls and year fixed effects, the coefficient increases to 0.00643 and is marginally statistically significant at the 10 percent level. Taken at face value, the OLS estimate indicates that a \$1,000 decrease in per pupil spending would decrease test scores by roughly 0.6 of 1 percent of a standard deviation—this is much smaller than the 2SLS estimates we present below.

To illustrate the importance of the various controls, we first present some intermediate (nonpreferred) specifications. Model 1 of Table 3 presents the 2SLS effect of per-pupil spending on NAEP scores excluding the year fixed effects or Bartik controls. This model does not rely *solely* on the differential change in time trends but does include a postrecession indicator dummy to account for any level shift in outcome at recession onset. The first stage for this intermediate model is column 1 of Table 2. The 2SLS coefficient is 0.0498 (p-value < 0.01). Adding the Bartik predictors for state unemployment and mean income, the point estimate falls somewhat to 0.0394 (p-value < 0.01). In models with year fixed effects (which rely only on the differential change in trends) the estimates are essentially the same. In model with state intercepts, state trends, and year fixed effects, the 2SLS coefficient is 0.0365 (p-value < 0.01) and in the full model with all controls (column 4) it is 0.0385 (p-value < 0.01). Across all four specifications, the point estimates are largely similar and range between 0.0385 and 0.0489.[21] The fact that the estimates are very similar in models that include flexible year fixed effects to account for transitory recessionary effects that may affect all states and also models that include the Bartik predictors for state-specific recession induced economic shocks suggests that our instruments are largely unrelated to economic conditions (as shown panel B of Figure 3). Even so, out of an abundance of caution, the model with full controls (model 4) is our preferred specification. In this preferred model, a \$1,000 reduction in per-pupil spending due to the recession led to a decline in test scores of about 3.85 percent of a standard deviation.[22]

College-Going Rates.—Models 5 through 8 in Table 3 present similar 2SLS models for the state college-going rate as for test scores. As with test scores, as a basis for comparison we also estimate OLS models that do not instrument for school spending (online Appendix Table A5). In OLS models with year fixed effects and state-specific trends but no additional controls, the estimate coefficient on per-pupil spending is 0.00635, indicating that with no controls a \$1,000 increase in per-pupil spending is associated with about 0.63 percentage points higher college-going rates. Adding the Bartik controls and year fixed effects, the coefficient falls to 0.00139 and is statistically insignificant. As with test scores, this stands in stark contrast to our 2SLS estimates.

Model 5 of Table 3 presents the parsimonious IV model with state fixed effects, state-specific linear trends, a postrecession indicator, and a postrecession indicator interacted with the different groups of Ω_s. This is the intermediate model that exploits both the overall trend change in outcomes and the differential trend change. The 2SLS coefficient is 0.0255 (p-value < 0.01). Adding the Bartik predictors in column 6, the point estimate increases slightly to 0.0272 (p-value < 0.01).

In the preferred models with year fixed effects (which rely only on the differential change in trends) the estimates are somewhat smaller but similar. In models with state intercepts, state trends, and year fixed effects (column 7), the point estimate is 0.0127 (p-value < 0.01); and in the full model, which also includes the Bartik controls, the point estimate is 0.0124 (p-value < 0.01). In this preferred model, a \$1,000 reduction in per-pupil spending led to a 1.24 percentage points decline in the college-going rate—about a 2.5 percent change. The 95 percent confidence interval indicates that the real effect likely lies between roughly 0.5 and 2 percentage points.

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 3 all indicate a strong and robust association between recession-induced spending reductions and deteriorating test scores and college-going rates. However, if our results are to be interpreted causally, it is important that our effects work through the proposed channels and are not driven by any other ill effects of the recession. We present several empirical tests to support a causal interpretation in Section IIIA below.

[21] We also present these same specifications using the log of spending in and find largely similar results as in levels.

[22] As is found in other settings, our test score impacts are larger for math than for reading. We also find suggestive evidence that impacts are larger on fourth grade scores than eighth grade scores. See online Appendix Table A7.

Table 4—Revenue Sources

Per-pupil revenue by source:	Federal (1)	State (2)	Local (3)
$I_{post} \times (0.33 < \Omega_s < 0.66)$	−83.15	−416.8	−1,071
	[166.0]	[250.9]	[1,271]
$I_{post} \times (\Omega_s > 0.66)$	−35.7	−2,308	−1,240
	[219.0]	[1,181]	[1,341]
$I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$	−37.41	−177.3	−274.4
	[54.26]	[108.7]	[191.8]
$I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$	−70.09	−707.7	2.473
	[68.09]	[220.8]	[225.2]
Pr(low = high)	0.308	0.00235	0.991
Kleibergen-Paap Wald F-statistic (slope only)	0.553	5.152	2.587
Observations	459	459	459
State trends	X	X	X
State fixed effects	X	X	X
I_{post}	X^s	X^s	X^s
Bartiks	X	X	X
Year fixed effects	X	X	X

Notes: Robust standard errors in brackets are clustered 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 2017. All models control for state fixed effects, state trends, income and employment Bartik instruments, and year fixed effects. I_{post} is a dummy variable equal to 1 if the observation is after 2008, $(T - 2008)$ represents years relative to the 2008–2009 school year, and Ω_s is the share of state education funding from state sources in 2008. Pr(low = high) tests whether states with the highest reliance on state-funds experienced greater declines in each revenue source in the years postrecession than the states with the lowest reliance on state funds, and the reported F-statistic reports the Wald F-statistics for the slope variables $I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$ and $I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well.

A. *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 4. We present our preferred models with all controls. We present effect on federal, state, and local revenues in column 1, 2, and 3, respectively. To summarize the results, for each specification we test whether the slope is the same for the high and low reliance groups. While one cannot reject that the change in slope for the least and most reliant states is the same at the 10 percent level for federal and local revenues, one rejects that the slope is the same at the one percent level for state revenues. 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 or demographic changes that are associated with the outcomes. We have 11 variables that measure economic conditions and demographics. These include: the unemployment rate, the child poverty population and its log, the average annual employment and its log, the ratios of employment to the population, median household income, K12 enrollment and its log, and the number of Black and white residents in the state. We estimate our 2SLS on each of these 11 variables and find no significant effect on any of these economic and demographic variables at the 5 percent level for each of them (see online Appendix Table A8). In fact, the point estimates go in opposite directions for related variables (such as unemployment and annual average employment, or K12 enrollment and the log of K12 enrollment), indicating that there is no systematic relationship between our instruments and these economic and demographic predictors of student outcomes. To summarize these tests and to avoid problems with multiple hypothesis testing, we create predicted NAEP scores and college-going rates based on these same 11 variables. We regress each outcome on these 11 variables. To capture the within-state variation in outcomes associated with these economic variable, this model includes state fixed effects, state-specific linear time trends, and year fixed effects. This model yields a within-entity R^2 of 0.0558 for NAEP scores and 0.0789 for college-going rates, indicating that these variables explain meaningful within-state variation in our outcomes over time (even after accounting for time effects).[23] We present estimated impacts on predicted outcomes for the same specifications as our main results in the lower panel of Table 3. Consistent with no effect on the 11 economic and demographic variables, in models with controls, instrumented school spending is unrelated to predicted outcomes, which lends 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 online Appendix Table A10. Across the same specification as in Table 3, none of the point estimates is statistically significant and the sign changes across models—precisely what one would observe if there were no effect.

D. To assuage lingering concerns that our estimates are confounded by underlying recession intensity, online Appendix Table A11 presents results that control for predicted outcomes. The results are largely the same as our main results. To be even more flexible, online Appendix Table A12 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, Ferreira, and Rothstein 2010), observing a positive association between instrumented school spending and housing prices is not indicative of bias. We consider these models to be "over controlling," but present them to establish the robustness of our result. The fully saturated models

[23] The estimation results for the predicted scores are presented in online Appendix Table A9. A binned scatterplot of actual outcomes against the predicted outcomes are in online Appendix Figure A2.

(columns 4 and 8) 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 similar to (and slightly larger than) that of our preferred models.

E. Another concern readers may have is that our estimated impacts are driven by one or two influential states. For example, both Hawaii and DC may be influential states because they have high and low values of Ω_s. To assuage this concern, we present results dropping DC or Hawaii in online Appendix Table A13, dropping California in online Appendix Table A14, and then dropping all three (DC, Hawaii, and California) in the lower panel of online Appendix Table A13. In all cases, there is a strong first stage, and in all cases the results are similar to that using the full sample. We even go a step further. To systematically show that our results are not driven by some small number of states, we conduct permutation tests in which we estimate our preferred linear and group IV models, excluding any combination of one, two, or three states at a time (this involves running over 127,000 regressions). In none of the models is the estimated coefficient on spending negative for either outcome. The estimated effect is significant at the 10 percent level in over 99 percent of regressions, and there is no combination of any three excluded states that will yield a negative spending effect (online Appendix Figure A3).

F. Our estimates are based on a comparison of three groups of states. While this does not lead to bias, readers may wonder if we would obtain similar estimates using a continuous measure of Ω_s. In the online Appendix, we present 2SLS estimates from models that use Ω_s interacted with the recession timing as the instrument directly. Because our grouping of states better capture the relationship between Ω_s and the change in slope than a simple linear specification, in a full model with year fixed effects, the first stage using a linear instrument is considerably weaker (a first-stage F-statistic of 7.37). However, the point estimates from the linear IV approach are generally similar to (but much less precise than) those from our preferred specification. To test this formally, we estimate models with both the linear instrument and the group-based instruments simultaneously (online Appendix Table A21). In this model, a test of overidentifying restrictions indicates that both sets of instruments identify the same parameter. We also show that the main estimates are similar (albeit much less precise) using the linear instrument alone. Moreover, in the online Appendix, we present an alternative 2SLS model using the linear instrument that (i) has a strong first stage, (ii) performs well in the specification checks, and (iii) yields very similar results to our preferred specification.[24]

G. Finally, because large states like New York have the same weight as small states like Vermont in our main model, readers may wonder if our effects are driven

[24] In Table 3, we show that so long as one includes the Bartik controls and a postrecession indicator, our main results are the same with and without the individual year fixed effects. As such, in the online Appendix, we also present an alternate 2SLS model using a linear IV but that relaxes the set of year fixed effects to obtain a strong first stage. This alternative model includes controls for groups of years rather than individuals years. This alternative model yields a strong first stage and yields very similar results to our preferred approach.

by small states that may not be representative of the United States as a whole. To be more representative of the student population and address this concern, we also estimate our preferred models using population-weighted least squares. We use student enrollment weights, child population weights, and total population weights. The results using the various weights are presented in online Appendix and are similar to our main results. We take the robustness of the positive relationship across different models for both outcomes and numerous samples to be compelling evidence that our estimated impacts are not driven by any single group of states but rather reflect a general pattern.

B. *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 preferred 2SLS model. We regress the level of spending in each subcategory (in per-pupil units) on the overall (instrumented) level of spending (in thousands per-pupil). The resulting coefficients reveal the marginal propensity to spend in each category—i.e., how much is spent in each category for each thousand-dollar increase in overall spending. 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 percent) of overall spending is divided between capital (9.6 percent) and operating (85.3 percent) expenditures. For every dollar in per-pupil spending cuts, districts decrease capital spending by \$0.47 (column 1) and current spending by \$0.51 (column 2). While capital accounted for 9.6 percent of overall annual spending, it made up 47 percent of overall reductions, suggesting that districts cut capital spending more than other forms of spending on the margin. In contrast, Jackson, Johnson, and Persico (2016) find that each dollar increase in total spending was associated with \$0.15 increased spending on capital (a marginal propensity similar to the average). Much of the reduction in capital spending was from construction (column 3). The disproportionate cutting of capital projects is consistent with the descriptive patterns documented in Leachman et al. (2016) and reports in the press (e.g., Bloomberg 2017) that budget shortfalls forced schools to defer maintenance and construction. By cutting disproportionately more from capital, states may have been able to cut disproportionately less from core operating expenses. Indeed, elementary and secondary current spending accounts for 85 percent of overall spending, but only 51 percent of spending cuts.

Much of the reduced operational spending came from instruction. For every dollar in spending cuts, districts reduced instructional spending by \$0.45 on average (column 4). Online Appendix Table A16 shows effects across additional spending subcategories. Roughly half of the reduction in instructional spending can be accounted for by reduced instructional salaries, while reduced instructional benefits make up most of the rest of these cuts (see online Appendix Table A16). While support services account for about 30 percent of spending on average, each dollar of

Table 5—Spending and Staffing Categories

Overall spending category:	Total expenditures		Capital spending	Elem/Sec operating
Sub category:	Capital (1)	Elem/Sec operating (2)	Construction (3)	Instructional (4)
Per-pupil spending (thousands)	469.5	508.5	151.6	449.8
	[183.7]	[159.2]	[77.57]	[65.96]
Mean (dependent variable)	1,285	11,383	906.9	6,883
Average share	0.0964	0.853	0.0688	0.514
Pr(average = marginal)	0.0476	0.0353	0.291	0.332
Observations	458	459	455	459
Staff category:	Teachers	Aides	Counselors	Library staff
Outcome: log(staff)				
Per-pupil spending (thousands)	0.0369	0.053	0.123	0.033
	[0.00997]	[0.0385]	[0.0619]	[0.0165]
Outcome: students per staff				
Per-pupil spending (thousands)	−0.318	−0.178	−80	46.06
	[0.165]	[2.660]	[34.30]	[53.91]
Mean (dependent variable)	15.41	66.04	445.5	659.9
Observations	447	428	441	448
State trends	X	X	X	X
State fixed effects	X	X	X	X
I_{post}	X^s	X^s	X^s	X^s
$I_{post} \times I_{gs}$	X	X	X	X
Bartiks	X	X	X	X
Year fixed effects	X	X	X	X

Notes: Robust standard errors in brackets are clustered 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 2017. We instrument for spending using $I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$, and $I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$, where I_{post} is a dummy variable equal to 1 if the observation is after 2008, Ω_s represents the share of the state's education funding from state sources in 2008, and $(T - 2008)$ represents years relative to the 2008-2009 school year. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well. I_{gs} is an indicator variable for whether the state's value of Ω_s is in the low, middle, or high group. Coefficients on per-pupil spending in the top panel (spending categories) 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 online Appendix Table A16. For each staff category (teachers, aides, guidance counselors, and library staff) we estimate two panels. The first panel regresses the log of the staff count on instrumented per-pupil spending. The second panel regresses the student:staff ratio on instrumented spending. For instance, the coefficient −0.318 in column 1 of the bottom panel indicates that a $1,000 decline in per-pupil spending increases class size by approximately 0.3 students 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.

cuts was associated with just 0.047 fewer dollars spent on support services (column 1 of online Appendix Table A16). In contrast to Jackson, Johnson, and Persico (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 spending could have been due to the hiring of fewer staff (which could affect outcomes) or the hiring of cheaper staff (which could have little effect on outcomes), we look at staffing in the lower two panels of Table 5. The middle panel examines impact on the log of overall staff counts. On average, a \$1,000 decline in spending was associated with hiring 3.69 percent fewer teachers, 5.3 percent fewer teachers aides, 12 percent fewer guidance counselors, and 3.3 percent 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 overall. Focusing on those ratios that are significantly affected, a \$1,000 decline in spending led to roughly 0.3 more students per teacher (column 1) and 80 more students per guidance counselor (column 3). Overall, these findings are consistent with the significant decreases in instructional and noninstructional salaries through reduced hiring of teachers, teacher aides, and library staff.

C. *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, Johnson, and Persico 2016; Lafortune, Rothstein, and Schanzenbach 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. This discrepancy underscores that the extent to which state-level changes in per-pupil spending had a disproportionate impact on low-income children depends on (i) the extent to which low-income students are more (or less) responsive to the spending changes, and (ii) the extent to which school spending declines were larger (or smaller) at the schools attended by low-income students. Our instruments predict spending changes at the state level, so we cannot distinguish which of these mechanisms is at play. However, we show that when states reduced public school spending levels, the test score declines were most pronounced for low-income students and Black students.

To examine impacts by income, we follow Lafortune, Rothstein, and Schanzenbach (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 avoid using any classification that could have been affected by the recession itself, we use each district's 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, such that in the typical state, a district with a poverty rate of zero would have test scores about 1σ higher than a district with a poverty rate of 30 percent. The standard deviation of this slope is 1.17.

Panel D of Figure 4 presents the event study of this slope. While there is little evidence of preexisting trend differences in high and low Ω_s states before the recession, there is evidence of a more negative slope after the recession for states that were more reliant on state appropriated moneys to fund public schools. We summarize this pattern in our 2SLS models in Table 6. Column 1 shows the preferred specification where the dependent variable is the slope. The estimated slope is

Table 6—Race and Income (2SLS)

	Slope (1)	Black-White gap (2)	Average NAEP Score by Race: White (3)	Black (4)	Hispanic (5)
Per-pupil spending (thousands)	0.396	−0.0617	0.0267	0.0337	−0.00795
	[0.161]	[0.0346]	[0.00566]	[0.0119]	[0.0128]
Observations	390	392	452	392	402
State trends	X	X	X	X	X
State fixed effects	X	X	X	X	X
I_{post}	X^s	X^s	X^s	X^s	X^s
$I_{post} \times I_{gs}$	X	X	X	X	X
Bartiks	X	X	X	X	X
Year fixed effects	X	X	X	X	X
Weight	Test Count		White population (2000)	Black population (2000)	Hispanic population (2000)

Notes: Robust standard errors in brackets are clustered by state. Data are collected from the F33 School District Finance survey and the NCES Public and restricted-use NAEP data. We include state-by-year observations for 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017. Average NAEP scores by race are not available in every year for every state, though most states are represented in each subsample. 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, through 2015 (2017 individual-level data was not available). We instrument for spending using $I_{post} \times (T - 2008) \times (0.33 < \Omega_s < 0.66)$, and $I_{post} \times (T - 2008) \times (\Omega_s > 0.66)$, where I_{post} is a dummy variable equal to 1 if the observation is after 2008, Ω_s represents the share of the state's education funding from state sources in 2008, and $(T - 2008)$ represents years relative to the 2008–2009 school year. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well. I_{gs} is an indicator variable for whether the state's value of Ω_s is in the low, middle, or high group. All models control for state fixed effects and state trends and include income and employment Bartik instruments, as well as year fixed effects, to control for additional economic characteristics correlated with years.

0.396 (p-value < 0.05) such that a $1,000 decrease in per-pupil spending led to a decrease in the slope of 0.4. This is about one-third of a standard deviation change in the test-score regressivity. For a typical state, a decline of a third implies that the test-score gap between a district with a poverty rate of zero and one of 30 percent would fall by about 0.12 standard deviations (in student achievement units). In sum, the achievement losses associated with recessionary public school spending cuts were disproportionately experienced by those in high-poverty districts (either due to greater exposure or greater responsiveness to spending cuts).

Using state-level averages as reported in the publicly available NAEP, we also examine impacts by student race (Table 6, columns 3 through 5). Because different states have different shares of students by race, we weight each state estimate by the population of residents of that race (from the IPUMS). While the spending effects are positive for both whites and Blacks, they are small, negative and not distinguishable from zero for Hispanic students. Among white and Black students, effects are larger for Black students, suggesting that the spending cuts may have increased Black-white test score gaps. To test this directly, we restrict analysis to states with both Black and white respondents and compute the Black-white test score gap in each state in each year. We then estimate effects on this gap (column 2). The point

Table 7—College-Going by Institution Type (2SLS)

	Overall (1)	4-Year (2)	2-Year (3)	Public (2- or 4-year) (4)	Private (2- or 4-year) (5)	>40 percent part-time (4-year) (6)
Per-pupil spending (thousands)	0.0124	0.00403	0.00817	0.0141	−0.00191	0.00115
	[0.00387]	[0.00510]	[0.00452]	[0.00323]	[0.00245]	[0.00128]
Average rate	0.475	0.324	0.137	0.352	0.108	0.00953
Effective percent change	0.0262	0.0124	0.0597	0.04	−0.0176	0.121
	<40 percent part-time (4-year)	Selective/ most selective (4-year)	Minority-serving institution	Non-MSI	Hispanic-serving institution	Non-HSI MSI
Per-pupil spending (thousands)	0.0028	0.00187	0.00894	0.0035	0.00244	0.00651
	[0.00417]	[0.00354]	[0.00596]	[0.00613]	[0.00370]	[0.00407]
Average rate	0.301	0.267	0.0836	0.391	0.0173	0.0663
Effective percent change	0.0093	0.007	0.107	0.00894	0.141	0.0981
Observations	459	459	459	459	459	459
State trends	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
I_{post}	X^s	X^s	X^s	X^s	X^s	X^s
$I_{post} \times I_{gs}$	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X
Year fixed effects	X	X	X	X	X	X

Notes: Robust standard errors in brackets are clustered by state. Data are collected from the F33 School District Finance survey, IPEDS, the US census, and the Carnegie Foundation. All models include state-by-year observations for 2001, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017. 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 average number of 17- and 18-year-olds in the state in the year prior to the enrollment year (census counts). 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 spending using $I_{post} \times (T-2008) \times (0.33 < \Omega_s < 0.66)$, and $I_{post} \times (T-2008) \times (\Omega_s > 0.66)$, where I_{post} is a dummy variable equal to 1 if the observation is after 2008, Ω_s represents the share of the state's education funding from state sources in 2008, and $(T-2008)$ represents years relative to the 2008–2009 school year. X indicates that the corresponding variable was controlled for, while X^s indicates that the corresponding variable was subsumed by other variables, effectively controlling for it as well. I_{gs} is an indicator variable for whether the state's value of Ω_s is in the low, middle, or high group. All models control for state fixed effects and state trends and include income and employment Bartik instruments as well as year fixed effects to control for additional economic characteristics correlated with years. "Minority-serving institutions" and "Hispanic-serving institutions" are defined according to IES definitions (see https://nces.ed.gov/pubs2008/2008156.pdf) using 2005 undergraduate enrollment data from IPEDS. "Selective/most selective" schools are those 4-year institutions that enroll fewer than 40 percent part-time students and admissions test scores are in the fortieth to one hundredth percentile of selectivity. We use the Carnegie Classifications from 2005 for all institutions (see http://carnegieclassifications.iu.edu/classification_descriptions/undergraduate_profile.php). Additional results by college type are reported in online Appendix Table A17.

estimates suggest that a \$1,000 spending cut would increase the gap by 0.061σ (or by about 6 percent). However, this effect is only significant at the 10 percent level. While we do find evidence of some differential impacts, we cannot distinguish between some groups being more exposed to spending cuts or being more responsive to spending cuts on the margin.

D. *College Type*

Using the IPEDS data, we explore 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 7. 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). Overall, a $1,000 decline in per-pupil spending reduced college-going by 1.24 percentage-points overall (about 2.62 percent). This effect is relatively larger for 2-year colleges (a 5.9 percent decrease) than for 4-year colleges (a 1.24 percent increase). The increased college-going rates were largely driven by public colleges where there is a 4 percent increase compared to a small (not significant) 1.7 percent decrease for private school attendance. While we found no strong relationships by school selectivity, the patterns are consistent with larger enrollment declines at less-selective institutions.[25] We also explored effects for Minority Serving Institutions (MSIs) and non-MSIs. The relative enrollment declines were larger at MSIs than non-MSIs but this difference is not statistically significant. A $1,000 reduction in spending led to about a 10 percent relative decline in attendance at MSIs.[26] These suggestive patterns are broadly consistent with the larger test score declines in low-income areas and for Black students.

While some of the enrollment patterns across college types are only suggestive, a few consistent patterns emerge. Specifically, the recessionary spending cuts resulted in lower college enrollment rates, and this was driven largely by 2-year schools and public colleges. Given that the college enrollment effects are driven by public institutions, one may wonder if our results reflect a tuition effect. Specifically, if those states that had the largest recessionary budget cut (as predicted by our instrument) were also likely to raise in-state tuition, it could partially explain our college-going results. To assess this possibility, we compiled data from Census State Finance Surveys, IPEDS, and the US Department of Education on states' higher education finances, college tuition charges, and financial aid and Pell grant receipts (US Census Bureau 2020a). In online Appendix Table A18, per-pupil spending is unrelated to state's higher education charges, tuition charges, in-state tuition, Pell grant awards, or private school tuition.[27] We find no evidence that our college-going effects are driven by a tuition effect.

[25] Selective/most selective institutions have incoming students with test scores between the fortieth and one hundredth percentiles. While neither effect was significant, enrollment declines were relatively larger at 4-year institutions where more than 40 percent of students are attending part-time (12.1 percent decline per every $1,000 in spending cuts) than at 4-year institutions where less than 40 percent of students are part-time (0.9 percent decline per every $1,000 in spending cuts). At selective or highly selective 4-year institutions, effects were also relatively small and insignificant (0.7 percent decline per every $1,000 in spending cuts), suggesting that enrollment declines were not concentrated in higher selectivity institutions. Additional enrollment breakdowns by selectivity are shown in online Appendix Table A17.

[26] 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 (see online Appendix Table A17). Due to a lack of precision, we find little evidence that the enrollment declines were relatively larger at Black-serving institutions than Hispanic-serving institutions.

[27] We do find an increase in out-of-state tuition, but this would not explain our in-state college-going effects. This effect goes away once we drop DC from the model. Moreover, the magnitude of the out-of-state tuition increases are too small to explain our college enrollment effects.

V. Discussion and Conclusion

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, Johnson, and Persico 2016; Candelaria and Shores 2019; Lafortune, Rothstein, and Schanzenbach 2018; Card and Payne 2002; Hyman 2017; Miller 2018; Gigliotti and Sorensen 2018). However, despite a growing consensus that money *can* matter, there has been no study on how schools respond to large persistent cuts to spending and how 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. A \$1,000 decline in per-pupil spending reduced test scores by about 0.0385σ and reduced college-going rates by roughly 1.24 percentage points. Consistent with these education cuts having disproportionate impact on the poor, states with deeper recessionary cuts saw a widening of the test score gap between high- and low-poverty districts. While Latinx student outcomes were largely unaffected, test scores were lower for Black and white students, and the spending cuts are associated with suggestive increases in Black-white test score gaps.

Given the unique and historic nature of our variation, it is helpful to put our results in context of existing work. Jackson and Mackevicius (2021) summarize existing papers that rely on causal variation to identify school spending impacts on standardized tests. The median effect across these studies of a \$1,000 general spending change (that persists for four years) is 0.515σ. Our estimated population effect for the four-year moving average of spending is 0.0529σ (see online Appendix Table A19)—well within the range of estimates across these well-identified studies. Looking at college-going rates, our college-going estimate of 1.24 percentage points per \$1,000 is similar to (in fact somewhat smaller than) 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 noncore operational spending.

Our results provide further evidence that money matters in education. Moreover, they suggest that school spending cuts do matter. Given that some of 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.

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