Public School Funding and Private Schools in the United States: Evidence from the Great Recession

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Abstract

This paper asks whether funding for public schools affects private school enrollment. To examine the causality, I utilize the fact that states with greater historical reliance on state appropriations and states with no income tax experienced larger cuts for public K-12 education funding after the Great Recession. I find that students exposed to a $1,000 (9.2 percent) decrease in per-pupil funding are more likely to enroll in private schools by 0.46 to 0.62 percentage points. I show further that the effect is strongest among high socioeconomic status students living in disadvantaged areas, which suggests a change in student composition.

Keywords: Private school, K-12 funding, Great Recession

JEL Classification: H61, H75, I21, I22, I28

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I Introduction

Private schools are the largest alternative to traditional public schools, serving 10.3 percent or 5.7 million schoolchildren in the US primary and secondary education (Snyder, de Brey and Dillow, 2019). Private schools are often criticized for increasing inequality and reducing intergenerational mobility by attracting high socioeconomic status (SES) students (Davies, Zhang and Zeng, 2005; Glomm and Ravikumar, 1992; Iyigun, 1999); however, they provide variety in school choice and induce competition (Dee, 1998; Hoxby, 1994), improving the overall quality of education. Competition between public and private schools suggests public school characteristics affect private school enrollment (and vice versa), in addition to a preference for religion, discipline, and alternative pedagogical methods (Goldring and Phillips, 2008).

In this paper, I investigate how funding for public education affects private school participation in US K-12 education. Greater public school funding is relevant to high education quality, represented by test scores and class size,[1] which is inversely related with private school attendance (Blundell, Dearden and Sibieta, 2010; Brasington and Hite, 2012). In addition, if public and private investment in education are substitutes (Houtenville and Conway, 2008), when there is a decline in public school funding, parents may respond by switching to private schools. It is important to understand this relationship to make efficient policy decisions for public education and school choice; however, this topic has received limited attention in the literature.

To estimate the causal impact of public school funding on private school attendance, I exploit an exogenous variation in funding cuts for public education induced by the Great Recession. In the wake of the Great Recession, funding for K-12 education fell precipitously in many states, on average of 5.3 percent per pupil from 2007 to 2012, and stayed low for several

[1] See Jackson and Mackevicius (2021) for the survey of recent papers in this literature.

years. I show that two idiosyncratic state characteristics—education funding scheme and tax structure—help me isolate the exogenous variation in the funding cuts, allowing me to use the Recession as a natural experiment despite its far-reaching impact on the economy and society. First, states that historically relied more on state appropriations to fund K-12 education rather than on local appropriations experienced a deeper cut during the Great Recession because of the larger income and crowding-out effect of state tax revenue than local tax (Evans, Schwab and Wagner, 2019; Jackson, Wigger and Xiong, 2021; Moffitt, 2013). Second, education funding stayed low after the Recession in seven states without income taxes because the lack of diversification in their tax portfolio makes the tax revenue more volatile during recessions (Cornia and Nelson, 2010; Jordan, Yan and Hooshmand, 2017; Yan and Carr, 2019). The two factors were determined years and decades before the Recession, changed little over time, and are unrelated to several state characteristics relevant to the impacts of the Great Recession, including the intensity of the economic shock (unemployment rate), property value, and the household income of each state before and after the Recession. Thus, these features provide conditions for an instrument by separating the effects of funding cuts for K-12 from the Great Recession itself. I combine the two sources of variation with the timing of the Great Recession in an event study framework as an instrument to predict the local K-12 education appropriations per pupil. Using the two-stage least squares (2SLS) model, I compare private school enrollment in regions with larger and smaller funding cuts.

The 2SLS results suggest that a $1,000—approximately nine percent—decrease in K-12 revenue per pupil increases the private school enrollment rate of schoolchildren by 0.46-0.62 percentage points or 4.4-6.0 percent. The estimated elasticity is -0.65 in the preferred specification, meaning a one percent decrease in public education funding raises private school enrollment by 0.65 percent. This implies that, in response to a 5.3 percent funding cut (the average cut from 2007 to 2012), 174,548 students switched to private schools. I further show that funding cuts resulted in a decline in instructional spending and less generous teacher salary and benefits, which are relevant measures of the quality of education (Card and Krueger, 1992). I cannot directly show these changes caused the increase in private school enrollment, because my instruments do not allow me to separate the impact on education quality from private school enrollment; however, Jackson, Wigger and Xiong (2021)'s findings on test scores in the same period support that a decline in education quality is the most likely mechanism. Finally, in the heterogeneity analysis, I show important findings related to student diversity and composition. I find high SES (high income or white) students are more likely to leave for private schools, a potential source of deepened inequality, and this pattern is reinforced when the high SES students live in more diverse (on average low SES) areas. This implies a potential decline in student diversity (negative change in student composition) and an increase in inequality as high SES students can avoid the negative impact of funding cuts by leaving public schools.

This paper makes three contributions to the literature. First, this is one of the few papers estimating the relation between public school funding and private school attendance. Because my work utilizes an extreme case of funding cuts, generalizability is questionable. Reassuringly, the elasticity in this paper is not much different from those using cross-sectional instruments (Goldhaber, 1999; Mavisakalyan, 2011) and school finance reforms (SFR) as the identification strategy (Dinerstein and Smith, 2021; Husted and Kenny, 2002), weakly suggesting some generalizability. Second, I provide evidence that education funding is relevant to inequality and student composition and diversity in public schools. This finding suggests that we should take caution when interpreting the impact of funding on students' outcomes in recent papers such as Baron (2019); Hyman (2017); Jackson, Wigger and Xiong (2021); Lafortune, Rothstein and Schanzenbach (2018) because of compositional change itself and the impact of diversity on test scores (Dills, 2018). Third, I contribute to the identification of education funding cuts by complementing Jackson, Wigger and Xiong (2021)'s strategy to explore the impact of funding on student outcomes. In their work, they use historical reliance on state-appropriated funds as the source of the variation and use the change in slope of education funding, rather than the level, associated with the variation to improve statistical power in the first stage. Instead, I add another source of variation, whether a state collects an income tax, and use a conventional event study strategy. To my knowledge, this is the first paper showing that slower tax revenue recovery in no-income-tax states affected education funding stability while using the income tax status to identify variation in public school funding.

The remainder of this paper is organized as follows. Section II provides the background of the K-12 education funding and the sources of identifying variation in funding cut followed by the Great Recession. In Section III, I describe the data sources, and in Section IV, the econometric model. In Section V, I present the results and the potential mechanism. Section VI presents heterogeneity analysis. Section VII concludes.

II Background: K-12 Budget and the Great Recession

The business cycle makes funding for education not stable over time because tax revenue declines during recessions (income effect), and at the same time, the government needs to spend more money on other social safety net programs like unemployment benefits and cash assistance, crowding-out expenditure for K-12 (crowding-out effect) (Jackson, Wigger and Xiong, 2021; Moffitt, 2013). In consequence, the growth rate of K-12 funding per pupil declines during and after recessions.[2] In most recessions, this fall is small; however, the funding cut followed by the Great Recession was unprecedented. Nationally, education funding decreased by $673 per pupil or 5.3 percent from 2007 to 2012, which was the first decline in

[2]K-12 appropriation is interchangeable with K-12 budget or funding. This is not the realized spending, but the amount of money allocated to K-12.

funding since the 1980s recession, and lasted for years (Figure 1). The magnitudes of the funding cuts differ substantially across states and were magnificent in some states (Figure 2). For example, Florida, the state with the largest cut, curtailed its funding by 28 percent during five years, much greater than the national average.

While the Great Recession made parents pull out their children from private schools due to the income shock (Ewert, 2013)), the decrease in funding cuts may have increased the demand for private schools. Figure 3 suggests these two dynamics. While overall private school enrollment drops in the wake of the Great Recession (income effect), the decline is smaller in states that experienced larger funding cuts, implying a relative increase (substitution effect). In the remainder of this section, I show that two state-level characteristics unrelated to the Great Recession allow me to isolate this substitution effect and estimate the causal impact of public school funding.

First, states that relied more on the state appropriations to fund K-12 before the Great Recession experienced deeper cuts, an identifying variation first used by Jackson, Wigger and Xiong (2021) (JWX henceforward) to examine the impact of education spending on student achievement. Their strategy relies on the variation in changes in education funding from three different sources: state, local and federal governments. To be specific, I visualize the trend of K-12 funding per pupil by the source in Figure 4. According to the figure, there was an immediate and steep drop in state funding, which was compensated by the federal government, making total education funding stable for the first two years from the beginning of the recession. On the other hand, local funding remained stable over time.

Why were the trends so different? First, state appropriations experienced both large income and crowding-out effects explained above. State tax revenue mostly consists of income and sales taxes (66 percent (US Census Bureau, 2020)), which fluctuate along with the business cycle. At the same time, state governments are responsible for other welfare programs together with the federal government, crowding-out budget for K-12. In contrast, income and crowding-out effects were smaller for local education appropriations. Local tax revenues rely heavily on property tax (72 percent (US Census Bureau, 2020)), which is stable during recessions.[3] [4] Also, public K-12 education is the largest expenditure for local governments, so the crowd-out effect for local governments is smaller than the state. The federal government substantially increased the funding through the American Reinvestment and Recovery Act (ARRA) to compensate the loss from the state governments, and when the fund ran out, a deep funding cut followed (Evans, Schwab and Wagner, 2019).[5]

Because of the different trends by sources, the composition of K-12 funding in each state played an essential role in the magnitude of the funding cut. There was a considerable variation in the share of funding coming from state revenue ($S_s = \frac{State\ App_s}{Total\ App_s}$, state share henceforth) before the Great Recession, which becomes the identifying variation.[6] On average, 47 percent of the total K-12 revenue came from the state government in School Year 2007-2008, varying from 86 percent in Vermont to 31 percent in Nevada (See Panel A of Appendix Figure A.3 and A.6). The variation in the share is associated with the variation in the magnitude of the funding cuts: because state revenue was more sensitive to the recession, funding cuts were larger in states with greater state share, as displayed in Figure 5.

The state share is determined by the particulars of the state's funding formula, which is a combination of multiple factors including state and local law, tax rate and base, govern-

[3]The reliance on each tax source is calculated using tax revenues in the fiscal year 2007 (US Census Bureau, 2020). See Appendix Table A.1 for variation across states.

[4]Local governments smooth property tax revenues by raising the tax rate or slowly adjusting the assessed value on which the tax is based. This is true for the Great Recession as well, although it started from the collapse in the housing market and was followed by substantial foreclosures (Lutz, Molloy and Shan, 2011).

[5]Unlike federal funds that are mostly earmarked to specific federal programs such as the National School Lunch Program and Title I, a large portion of ARRA was provided as discretion appropriations for quicker recovery.

[6]State appropriations here is the K-12 appropriations "distributed" by the state government. For example, Texas's recapture process (Robin Hood Plan) redistributes property tax revenues from wealthy to poor districts. While this fund is locally raised property tax, it is considered state appropriations in receiving districts because it is distributed by the state government. To address a potential problem arising from this, I exclude Texas from the sample in the robustness check, and the result does not change much. (See Appendix Table A.8.)

ment programs, and overall fiscal centralization (Alm, Buschman and Sjoquist, 2011). Thus, education funding structure is a combination of multiple factors that were determined years or even decades before the Great Recession and changed little over time, implying little relevance to the Great Recession itself.[7] Critically, a greater share in a given state does not mean the state cares more about public education; it appears that there is no correlation between the share and total K-12 appropriations per pupil before the recession (Panel B of Appendix Figure A.6).[8]

Along with the education funding structure, the tax structure is an important factor that predicts the trend of tax revenue and funding for K-12 in each state after the Great Recession. I find that funding cuts for K-12 were greater in states that do not collect income tax. Whether a given state collects an income tax or not was determined decades ago, providing exogenous variation to the education budget cuts.[9] There are seven states with no individual income tax—Alaska, Florida, Nevada, South Dakota, Texas, Washington, and Wyoming.[10]

Three factors led to a larger decline in education revenue in these seven states. First, because they do lack one tax source with a very wide base, it is difficult for these states to diversify their tax revenue (Cornia and Nelson, 2010), which hurts their tax portfolio volatility (Jordan, Yan and Hooshmand, 2017; Yan and Carr, 2019). Second, while these states tend to heavily rely on sales tax (Cornia and Nelson, 2010),[11] and states with higher reliance on sales tax had suffered longer to recover their tax revenues after the Great Recession Alm and Sjo-

[7]See Section IV.2 for formal tests.

[8]State share had been very stable during 2000-2007 (Panel A of Appendix Figure A.6). The correlation coefficient between state share in 2000 and 2007 is over 0.9. The correlation is weaker for the share in 1990 (0.6); however, the correlation between rankings is 0.75. In the robustness check, I use the share in 1990, 2000, and the five-year average of 2002-2006 instead of the share in 2006 and obtain very similar results (See Table 5).

[9]The state income tax status was mostly determined during the early 20th century. In 1901, Hawaii was the first state that adopted a state income tax. Since then, 44 states had implemented state income tax up until 1976. In 1979, Alaska repealed its income tax, and since then, seven states do not have a state income tax (US Advisory Commission on Intergovernmental Relations, 1995).

[10]New Hampshire and Tennessee collect tax on dividend and interest income, but not on labor income. In the robustness check, I include these two states as no income tax states as well. The results are very similar (See Table 5).

[11]This is not true for Alaska, which collects most of its tax revenue through natural resource taxes.

quist (2014). Finally, states also took efforts to recover their tax revenues quickly. One way is to make income tax more progressive, which is not a viable option for no-income-tax states. In addition to the progressive income tax, these states were not very successful in revising their tax portfolio (Seegert, 2015). Consequently, states without income taxes faced a longer-lasting reduction in tax revenues after the Great Recession (See Appendix Figure A.7).[12]

Figure 6 summarizes the identifying variation. Panel A shows the difference in education appropriations per pupil in states with high and low state shares (above and below 0.54) over time relative to the 2007 level. From 2009, there had been a decline in funding in both groups of states; however, the magnitude was smaller and recovered more quickly in states with a smaller share. Panel B conducts the same exercise with states with and without an income tax. While education funding in other states recovered to the pre-recession level by 2014-15, it was still lagging in no-income-tax states. To my knowledge, I am the first to show that having an income tax can affect education funding stability after the Great Recession.

III Data

My analysis draws data from two sources. First, I use the 2000 Census and the 2005-2016 American Community Survey (ACS) IPUMS data (Ruggles et al., 2020) to obtain information on private school enrollment. The Census and ACS ask every respondent whether she is enrolled in a private school when she is in school. I restrict my sample to children between the ages of 6 and 17 years (equivalent to grade 1 to 12) to make sure they are school-aged.[13] I omit 2001-2004 ACS because I cannot identify geographical units smaller than the state in these

[12]This is not true for five states with no general sales tax (Alaska, Delaware, Montana, Oregon, and New Hampshire). Reliance on income tax in these five states is no different from other states with sales tax, from 5 to 40 percent, except for Oregon (see Appendix Table A.1). These four states diversify their tax sources from other sources such as excise taxes and license fees.

[13]I remove five years old because some states don't have public funding for pre-Kindergarten at all or provide only a half-day Kindergarten program. I exclude eighteen years old because some of them are not school-aged anymore.

years.[14] Washington D.C. is also removed from the main sample because the state share is zero in D.C. by definition and thus it becomes an outlier. My final sample consists of 8,095,073 children.

I collect the financial data of all school districts in the U.S. during the 2000-2016 fiscal year from the Common Core of Data (CCD) from the National Center for Education Statistics (NCES). CCD provides rich data on school financing such as funding sources (state, local, and federal government) and expenditure in broad categories as well as school enrollment and staffing. Because the district-level data is noisy, I exclude school districts with too small enrollment or suspicious growth patterns emulating Lafortune, Rothstein and Schanzenbach (2018).[15] To merge two datasets, I aggregate the school finance data into the Consistent Public Use Microdata Areas (CPUMA) in the Census and ACS.[16] I also take a weighted average of two fiscal years to construct school finance data at the calendar year level because the Census and ACS do not provide information on survey month. [17]

In Table 1, I show the summary statistics in the pre- and post-recession period. About 10 percent of the total children are enrolled in private school (as opposed to in public school or not enrolled at all). This number decreases by 0.45 percentage points after the Great Recession. The average inflation-adjusted total revenue per pupil is about $10,852 before the recession. The funding is larger after the recession despite the cut because of an increasing trend from 2000 to 2007. In the next columns, I divide the states into two groups: states with high and low state shares using the median. While there is a decrease in private school enrollment

[14]I include these years and use the state-level education revenue for robustness check. The results are presented in Appendix Table A.7 and Figure A.8.

[15]See the Appendix for further information.

[16]PUMA is the smallest geographical unit available in the Census and ACS Public Use Microdata Sample. PUMA is a population-based geographical unit, so its boundaries change every ten years, and the Consistent PUMA is an aggregate of some of the contingent PUMAs to make the boundary consistent over time. While there are slightly more than 2,000 PUMAs, they are aggregated to 1,075 CPUMAs. When aggregating to CPUMA level, I use the number of students in each school district as the weight.

[17]See Appendix Section A for further details of the crosswalk.

in both groups, the decline is larger in low share states, 0.26 versus 0.56 percentage points. When comparing states with and without income taxes, the change is similar; a larger decline in private school enrollment in states with income taxes. Consistent with these patterns, states with low state share and no income tax had recovered their education appropriations more quickly. Panel B compares the major covariates before and after the Recession, showing little difference. Comparing the groups of states, we can notice that low state share states and income tax states are more resourceful in terms of education funding and private school enrollment. While this is a potential problem, I show the validity of the empirical strategy in Section IV.2.

IV Econometric Model and Validity

IV.1 Estimation Equations

My empirical strategy leverages the Great Recession as a natural experiment that generated an exogenous variation in education funding as I explained in Section II to address endogeneity between local public education funding and private school enrollment. I estimate the following system of equations using two-stage least squares (2SLS):

$$Private_{ipst} = \delta \widehat{App}_{pst} + X_{ipst}\pi + P_p \times t + \mu_p + \theta_t + \varepsilon_{ipst}, \tag{1}$$

$$App_{pst} = \sum_{k \neq 2007} \left[\beta_k S_s \times \mathbb{1}(k=t) + \gamma_k NT_s \times \mathbb{1}(k=t) \right] + X_{ipst}\lambda + P_p \times t + \rho_p + \tau_t + \nu_{ipst}, \tag{2}$$

where $Private_{ipst}$ is an indicator whether individual i in CPUMA p of state s in (calendar) year t is in a private school and App_{pst} is the total K-12 appropriations per pupil in thousands (in 2010 dollars).[18] [19] I include a vector of student and household level controls (X_{ipst}) and baseline CPUMA characteristics (in 2000) interacted with the time trend ($P_p \times t$), respectively. CPUMA fixed effects (μ, ρ) absorb the time-invariant differences across CPUMA, and year fixed effects (θ, τ) controls for any common national shocks specific to given years. The standard errors are clustered at the state level, and the regressions are weighted using sample weights of the Census and ACS.

I instrument for App_{pst} by combining S_s, the share of total K-12 funding coming from state-appropriated funds in the school year 2007-2008 ($\frac{State\ App_{s,2006}}{Total\ App_{s,2006}}$), and NT_s, the indicator for having no state income tax, with the year dummies in an event study setting. Although the instrumental variables are at the state level, I use CPUMA level funding as my main independent variable to improve the precision of the estimates.[20] I take 2007 as the base year, so all coefficients can be interpreted as changes relative to 2007.[21] This framework helps me extract the exogenous variation in education funding cuts induced by the Great Recession. Because funding did not decline until 2010 and slowly recover until 2016 (Figure 1), I prefer an event study model because it has more flexibility than the traditional difference-in-differences model (DiD). In the Appendix Table A.9, I show that my results are robust to using alternative instruments such as traditional DiD and using only one source of variation.

My identification strategy using the variation in the reliance on the state government owes JWX's work. The most important difference is that instead of using the change in the slope of education expenditure induced by the Great Recession ($S_s \times Post_t \times t$), I utilize the change in the level, which is a more conventional DiD strategy, and add NT_s as another source of

[18] "Not in private school" means children in public schools and not in school at all, including homeschoolers and dropouts. I include all of school-aged sample to avoid the impact of "leaving the education system". Excluding these students does not make much difference in the robustness check.

[19] I use levels instead of logs to avoid the assumption that a one dollar increase of revenue has stronger impact on low-spending CPUMAs than high-spending CPUMAs. The results using logs are available in the Appendix Table A.6.

[20] State level analysis is available in Appendix Table A.7.

[21] The Bureau of Economic Analysis states the Great Recession officially started in December of 2007.

variation. By doing so, I can improve the first stage F-statistic without assuming a specific functional form as JWX do. I argue that my specification complements their work by using traditional DiD and making it easier to interpret the compliers. For the discussions of compliers, see Appendix Section C.)

IV.2 Placebo Tests

The crucial identifying assumption of my empirical strategy is that the instruments should not affect private school attendance through channels other than the change in public K-12 funding. This assumption is fundamentally unprovable because I cannot show my instruments are unrelated to any potential confounding factors (or it is impossible to show my instruments are independent of the error term ε_{ipst}). Nevertheless, in this subsection, I provide evidence that my instrumental variables are uncorrelated with important individual and regional characteristics. In particular, I demonstrate that my instruments are independent of the characteristics closely relevant to the income effects of the Great Recession, the most concerning confounding factor, showing they can separate the substitution effect caused by cuts for education funding from the overall impact of the Great Recession. In addition, I also show whether the change in education funding is associated with changes in the regional demographic characteristics and other spending categories that may affect private school attendance.

In Table 2, I show the 2SLS results of 18 possible confounders. In Panel A, I use the main sample to test individual-level characteristics. Four variables, whether the household head is employed, total household income (in $1,000), whether the household is under 150% of the poverty line, and indicators for minority are not associated with the total education funding per pupil. I find that ownership of dwelling is positively associated with education funding.

However, the point estimate of 0.65 percentage points is very small compared to the average of 69.5 percent before the Great Recession. The foreign-born indicator is also significant but also small. Other important characteristics such as employment of household head and total household income have little relation to K-12 funding per pupil. Next, I test CPUMA level covariates in Panel B, including the median house value and the number of children. Median house value is an important characteristic as the Recession started from the collapse of the housing market, and reassuringly, it is not associated with education funding. The number of children in all categories has little to do with the funding. Lastly in Panel C, I test whether other state-level spending categories were associated with state K-12 funding and (indirectly) affected private school enrollment.[22] Because the identification strategy relies on the fact that the total tax revenue was lower in no-income-tax states, the total expenditure increases when the education funding increases. Nevertheless, the point estimate is small because the total expenditure includes transfers from the federal government, compensating the loss from the state government. Spending on other categories has little association with the total K-12 funding per pupil. Interestingly, higher education spending per capita is also not correlated, meaning that K-12 and higher education funding are determined under different mechanisms. Overall, the placebo tests support that my instruments can remove the income effects of the Great Recession and focus on the variation in education funding.

IV.3 First Stage and Reduced-Form Results

In this subsection, I present the first stage and reduced-form results to confirm the relevance of the instrumental variables using Equation 2. When estimating the first stage, I scale the per-pupil public education appropriations to thousands of 2010 dollars. The result is presented

[22] I collect the data from the Annual Survey of State and Local Government Finances (US Census Bureau (2020), through Urban Institutes) and Medicaid expenditure reports from MBES/CBES (Centers for Medicare and Medicaid Services). The state expenditure in 2002 and 2004 is not available, thus I exclude the years in the Medicaid sample as well.

in Panel A of Figure 7. This figure displays the set of coefficients of state share, and the no-income-tax indicator interacted with the year dummies, along with 95% confidence intervals. All estimates are estimated relative to the base year of 2007. The regression includes the sets of individual and household controls and CPUMA characteristics in 2000 interacted with time trend (preferred specification).[23] The first stage F-statistic is 28.4, which passes the weak IV test threshold.

The figure shows that my identifying variation strongly predicts the extent of funding cuts. The coefficients are generally larger for state share because their scales are different. The state share is a continuous variable ranging from zero to one, while the no-income-tax indicator is binary. The coefficient means that in 2013, a ten percentage points (0.1) increase in the state share decreases the education budget by \$510 per pupil, and states with no income tax have \$1,000 less education budget per pupil than states with income taxes. Considering the average revenue per pupil before the recession, about \$10,852, this is a very large impact. The funding cuts induced by the Great Recession were long-lasting even after 2012 when the economy bounced back to the pre-recession period. The funding cuts driven by the state share seem to gradually fade out, and on the other hand, the coefficients for the no-income-tax indicator stay stable as in Figure 6.

Panel B of Figure 7 demonstrates how exactly my instrumental variables are associated with private school attendance. Interestingly, the patterns of point estimates match them in the first stage. The increase in private school enrollment induced by the no-income-tax indicator stay large and stable even after 2012. The point estimates for state share attenuate to zero in 2016, which is consistent with an increasing trend from 2014 in Panel A.

Both panels in Figure 7 question the existence of the pre-trends.[24] In fact, I can reject the

[23]Regressions with different specifications are available in the Appendix Figure A.9; however, there are no noticeable differences.

[24]Excluding 2001-2004 makes it difficult to examine the pre-trends. However, even in Appendix Figure A.8, the no pre-trends hypotheses are rejected.

hypotheses of no pre-existing trends for both first stage and reduced-form. To address the pre-trends, I follow an estimation suggested by Freyaldenhoven, Hansen and Shapiro (2019) (FHS henceforward) with the state-level sample using a covariate as a proxy of a confounder that induces the pre-trends. In Appendix Section B, I show the first stage and reduced form results using four covariates. While none of the covariates can completely remove the pre-trends in all four regressions, the point estimates in the post-period do not vanish; it rather gets larger. Thus, I argue that the pre-existing trends are not the substantial confounders of my main results.

V Results

V.1 Main Results

I begin by estimating the main model presented in Equations 1 and 2 in Table 3, where the outcome variable is the indicator for private school attendance. I multiply the coefficients and standard errors by 100 to represent changes in private school enrollment in percentage points. All specifications include the year and state fixed effects. In columns 1 to 4, the coefficients are consistent and robust to the inclusion of controls, falling within the small range of -0.461 to -0.624 percentage points. When I control for individual demographic characteristics, the point estimate increases in magnitude by 0.04 percentage points (column 2). This jump is consistent with the correlation between individual characteristics and private school enrollment. Controlling for household and parental characteristics, the coefficient slightly increases by 0.04 percentage points. I add the CPUMA characteristics in 2000 interacted with time trends in column 4, and the point estimate increases by 0.08 percentage points (preferred specification). The magnitude of the impact is much larger (more negative) in 2SLS regressions than OLS results (*-0.120 to -0.151*, see the Appendix Table A.6.), which means the OLS estimate is biased toward zero. In Appendix Table A.7, I estimate the treatment effect of state-level K-12 appropriations per pupil. The point estimates are smaller than Table 3 and less precisely estimated; nevertheless, show similar patterns.

In column 4, my preferred specification, the coefficient indicates that a \$1,000 reduction in public education appropriations per pupil in the CPUMA increases private school enrollment by 0.62 percentage points. This represents a 9.21 percent decrease in the public education budget and a 5.99 percent increase in private school enrollment, given that the mean of budget and private school attendance was \$10,852 and 10.34 percent before the Great Recession, respectively. This implies the elasticity of the demand for private schools with respect to public school funding is -0.65.[25] Using this elasticity, I calculate that roughly 162 students in the median CPUMA or 174,548 students in the US leave for private schools in response to a 5.3 percent funding shock, the average funding cut from 2007 to 2012.[26]

V.2 Comparison to Existing Literature

In this subsection, I compare the elasticity estimated in this paper (-0.65) to existing estimates. Using a structural model, Goldhaber (1999) suggests an elasticity of -0.5.[27] A more recent study by Mavisakalyan (2011), a cross-country study with 80 countries, shows the elasticity of -0.34, about half of mine.[28] While they investigate different periods and regions, the cross-

[25] $-0.65 = \frac{5.99\%}{-9.2\%}$. It means that a one percent decline in public education revenue increases private school enrollment by 0.65 percent.

[26] The 5.3 percent decline in K-12 appropriations implies a 3.3 percent increase (-5.3×-0.65) in private school enrollment. In the average CPUMA, there were 45,533 students before the Recession, and 10.34 percent of them were in private schools. The back of the envelope calculation suggests that 155 students ($= 45{,}533 \times 10.34\% \times 3.3\%$) transfer to private schools in this state. We can do a similar calculation for the total school-aged children in the US, 48,948,264.

[27] His estimate shows that a \$1,000 (in 1983 dollars) increase in public school expenditure per pupil decreases private school enrollment by 1.5 percentage points in the school district. Average private school enrollment is 4.64 percent in his sample, New York State in 1981, and the instructional revenue per pupil is \$1,565.

[28] The point estimate suggests that a one percentage point increase in public education spending relative to the country's GDP decreases private school enrollment by -8.5 percent. Education spending accounts for 4.0 percent of US GDP in 2016 (Snyder, de Brey and Dillow, 2019), so a one percentage point increase corresponds

sectional instrumental variables used in these papers may not completely rule out reverse causality and omitted variables, generating a smaller elasticity. Thus, I would expect estimates to be biased toward zero like my OLS estimation.

A couple of papers examine the relationship between funding and private school attendance using the School Finance reform (SFR) as an identification strategy. Husted and Kenny (2002) utilize SFR started in the 1970s, finding elasticity of -0.5.[29] Dinerstein and Smith (2021)'s recent work using the SFR in New York City has the estimated elasticity of -0.84. In fact, this paper estimates how an increase in public school funding through financial reform reduces private school attendance.

My elasticity lies in the range of -0.34 to -0.84, estimates using different models and samples. Unlike most of these studies, especially ones using SFR, this paper examines the Great Recession-induced funding cuts for public education, which is rather an extreme case of funding changes. While it provides a chance to test the impact of massive funding cuts, using it as identification raises the question of the extent to which my results are generalizable to more typical funding changes. As stated above, it seems that my results are generalizable at least direction-wise. It may be difficult to conclude my results are generalizable just by comparing with previous literature; nevertheless, it still provides an interesting and compelling case study. Furthermore, although not as typical as SFR, it is very important to look into the negative shock followed by major economic downturns because it may have a persistent effect on the education market.

to a 25 percent increase in education spending. The estimated elasticity is -0.34 (-8.5/25).

[29]They find 100 percent increase in state-level public education funding decreases private school enrollment by 5 percentage points. The private school enrollment in their sample is 10 percent; the estimated elasticity is -0.5.

V.3 Possible Mechanism: Impact on Expenditures and Staffing

A subsequent critical question is whether students switch to private schools because of a decline in the quality of public schools. A literature review by Jackson (2018) and Jackson and Mackevicius (2021) point out that the causal effect of school funding is overwhelmingly supported by recent quasi-experimental works. JWX also finds a negative impact of school funding cuts on students' academic performance through cuts in actual spending. To test whether my specification results in the same conclusion, I estimate the impact on outcomes that are related to the real quality of education, similar to JWX's analysis. To match the main specifications, I aggregate relevant variables into the CPUMA level and estimate the impact of revenue per pupil with the 2SLS model.

In Panel A of Table 4, I first examine in which spending category was actually affected by the funding cut. I regress the level of spending in each category: expenditure on the total operation, instruction, capital, and student support. There are statistically significant increases in all spending categories, except for capital spending. The impacts on total operational, instruction and student support are all somewhat proportionate to the change in revenue ($1,000 or 9.2 percent), from seven to nine percent. There is a small and insignificant impact on capital expenditure in column 3. This does not mean there was no decline in capital investment. Instead, school districts that experienced relatively small funding cuts also cut capital spending to secure instructional expenditure, especially when they expect a long funding freeze. The result for capital expenditure is not consistent with JWX who find a large effect on capital spending reduction in the same period because my IVs estimate a different LATE.[30] In particular, Baron (2019) finds that an increase in capital spending does not improve student achievement compared to instructional expenditure, which implies that

[30] In fact, I get the same result with JWX when I follow their specification. Thus, I believe this discrepancy is coming from that we are relying on different LATE.

instructional spending is more critical to education quality.

Next, I examine compensations for teachers, an important characteristic correlated with the quality of education (Card and Krueger, 1992). Although teacher salary and employee benefits may not be directly visible to students and parents, higher monetary compensations can attract productive teachers from other school districts or outside of the education market and prevent competent teachers from leaving. In column 1 of Panel B, a $1,000 reduction in K-12 revenue per pupil results in a statistically significant decrease in real average teacher salary (total instructional salary expenditure divided by the number of teachers) by $1,441 or 2.2 percent. There is a strong impact on employee benefits for teachers in column 2, about $3,384, or 18.3 percent. The result seems reasonable; it may be difficult to cut teachers' salaries too much, and thus the school districts curtail employee benefits, which are less salient, to save expenses.

In Panel C, I examine whether the number of staff per 100 students declined during the budget cut. Overall, the impact on the number of staff was not very large, except for instructional aides, a reduction in the ratio by 0.200 or 15 percent. The interpretation is analogous to Panel B. It is difficult to reduce the number of teachers because of teacher unions (Young, 2011) or regulations to maintain a certain level of class size; thus, schools may let go of supplementary employees to save expenses. Guidance counselors and librarians are also supplementary compared to teachers; however, they often cover the entire school alone and may be considered more essential than aides, leaving little room to reduce them. Thus, the coefficient is close to zero and not statistically significant in columns 3 and 4.

Table 4 shows an overall decline in education quality in terms of spending. However, it is not clear whether this change was visible to local residents. The decline in the number of staff may not be large enough to be perceived, and parents may not exactly know the education expenditure in the local school districts. While I cannot directly test how much parents were aware of this quality decline, I provide some anecdotal evidence that the budget cut happened conspicuously. For example, a report by American Federation of Teachers (2018) states that many school districts suffered from overcrowded classrooms, outdated textbooks, broken-down buses and leaky roofs, and a loss of critical staff such as nurses and guidance counselors. Schools also have eliminated electives and shortened the school year, and more extremely, some school districts in Oklahoma had to cut their school week down to four days. The funding cut was also heavily covered by the local and national media, raising public awareness of the issue. While these pieces of evidence are not perfect, I believe that parents may have felt the impending cuts.

V.4 Robustness Checks

In this subsection, I perform several robustness checks to examine the sensitivity of my results. In column 1 of Table 5, I aggregate the data to the state-year level, the level of variation, and estimate whether my result is robust. The point estimate is smaller, but similar to column 1 of Appendix Table A.7. Column 2 controls for CPUMA specific linear time trend ($\eta_p \times t$). Adding this term explicitly controls for any effects through differential trends across CPUMAs and addresses potential pre-trend issues in education funding. The point estimate in column 2 is similar to the preferred specification, although I lose precision, implying that differential trends cannot explain the main finding. In column 3, I add state Bartik controls introduced in Jackson, Wigger and Xiong (2021) that predict unemployment rate and average wages using pre-Recession industrial composition. I use state-level appropriations and realized expenditure in columns 4 and 5, respectively, showing little difference.[31] In columns 6 to 8, I test whether the point estimate is robust to using alternative definitions of the state share: five-

[31] If there is a large discrepancy between K-12 budget and spending, for example, if the districts could take on debt, then the negative impact of funding on private school enrollment is underestimated (biased upward). In general, this is not the case for K-12 education because a balanced budget is highly recommended to school districts and often required by law in some states and cities.

year average share, the share defined in 2000, and 1990, respectively. The share stayed very stable from the school year 2000 to 2006, with a correlation coefficient over 0.9. Because some states implemented SFR in the 1990s, the correlation between 1990 and 2006 is weaker, but still over 0.6.[32] Because of this high correlation, the point estimates stay almost the same in columns 5 to 7. In column 9, I include New Hampshire and Tennessee in the no-income-tax states because these states collect income tax only on interest and dividend income, but not on labor income. The point estimate is almost identical to the preferred specification.

In Online Appendix, I perform several other robustness checks and address other confounding factors: using alternative samples (Table A.8), alternative instrumental variables (Table A.9), and lagged appropriations (Table A.10). I consider statewide private school choice programs like voucher (Table A.11), number of public schools (Table A.12), and selective migration (Table A.14 and A.13) as alternative mechanisms. All of them suggest my results are robust.

VI Heterogeneity in Effect

Preference for private schools is correlated with household SES (Brunner, Imazeki and Ross, 2010; Long and Toma, 1988). In addition, extensive research studies the relationship between regional characteristics and private schools attendance, indicating that private school enrollment depends on characteristics such as the poverty rate (Winkler and Rounds, 1996), the share of minorities (Fairlie, 2002; Fairlie and Resch, 2002; Li, 2009) and immigrants (Betts and Fairlie, 2003; Murray, 2016). Because parents' preferences for private schools affect how sensitive they are to public education funding (Sonstelie, 1979), these characteristics may affect the magnitude of the treatment effect. In the following subsections, I empirically evaluate

[32] See Appendix Figure A.6.

how responses vary by household and regional characteristics .[33]

VI.1 Heterogeneity by Age, Race, and Household Income

I start by examining heterogeneity by children's age. Preference for private schools may vary across age for various reasons: accessibility to private schools, parents' belief in critical stages, and experience in previous (public) schools (Goldring and Phillips, 2008). In columns 1 and 2 in Panel A of Table 6, I separately estimate the impact of K-12 revenue on private school enrollment for elementary/middle and high school age. The estimate is larger for lower grade age students by 0.195 percentage points. However, the higher point estimate for younger age students does not necessarily mean the effect is stronger for younger students as the two coefficients are not statistically different from each other.

Next, I consider race. Racial variation in private school enrollment is well-documented; however, whether a particular racial group is more responsive to public school spending is not evident. I examine heterogeneity by three race categories and present them in columns 3 to 5 of Panel A of Table 6. I find significant effects for whites and Hispanics. To be specific, a \$1,000 decline in public education revenue per pupil increases private school enrollment by 0.729 and 0.564 percentage points for white and Hispanic students, respectively. The effect on black students in column 5 is not statistically significant and different from columns 3 or 4. While the point estimates are similar between whites and Hispanics, a smaller baseline mean of private school enrollment for Hispanics suggests the elasticity is larger for Hispanics. The point estimates correspond to the elasticity of -0.60 and -1.07 for white and Hispanic students, respectively. Back of the envelope calculation suggests 122,400 and 29,055 white and Hispanic students were leaving for private schools in the country in response to -5.3 percent funding

[33] Preference for private schools depends on other (unobservable) factors as well. Parents' religious beliefs and desire for disciplined education are examples. The listed characteristics here are the ones I examine in this paper.

shock from 2007 to 2012, respectively.[34]

Similar point estimates for whites and Hispanics are interesting, implying it is not just a white effect. While this is surprising, previous literature suggests that Hispanics have as strong preferences for private schools as their white peers. Fairlie (2002) shows that increase in the black population in the neighborhood induces Hispanic students to transfer to private schools, and this "Latino Flight" is no weaker than "White flight". Hispanic students also have a high preference for Catholic schools and benefit more from them than white students (Evans and Schwab, 1995; Neal, 1997). Thus, Hispanic students may have a strong preference for private schools, making them leave for private schools when facing funding cuts.[35]

In Panel B, I divide the sample by household income and separately estimate the impact of the K-12 budget. Household income percentile thresholds are defined by the national income percentile each year. The results show evident heterogeneity in response to budget cuts across income groups. The point estimate is small and insignificant for column 5, the poorest households because a large portion of the households in this group is never-takers of private schools. The point estimate in this group is different from all other four groups. Although not statistically different, it is interesting that the point estimate is larger in the middle-income group (90-75th percentiles) than the richest. Private school enrollment was already high in the richest group, some of which are never-takers of public schools, making the effect smaller than middle-income households.[36]

Results in Table 6 imply that relatively high SES students can avoid the adverse effects of the funding freeze by switching to private schools. Public school advocates criticize the gov-

[34]The total number of white, Hispanic, and black school-aged children before the Great Recession is 29,192,371 and 8,934,661, respectively.

[35]In Appendix Section F, I use the Private School Universe Survey (PSS) to examine which types of schools are most responsive by religious affiliation. The results reveal that Catholic schools are receiving more students than other religious and nonsectarian schools. Hispanics also tend to switch to Catholic private schools too.

[36]When I divide the sample into two groups using the median, rich households are more responsive than poor households (-0.88 vs. -0.41). The point estimates are statistically significantly different in 5 percent level.

ernment for reducing education funding because public schools reduce inequality (Johnson and Jackson, 2019), and given my results, cuts for public school spending can have a broader impact on inequality and intergenerational mobility than expected. In other words, while the adverse effects of funding cuts on remaining students could be partially alleviated by high SES students leaving for private schools (Akyol, 2016), inequality in student outcomes may increase by directly affecting remaining students in public schools and by inducing some students to opt-out from public schools.

VI.2 Heterogeneity by Regional Characteristics

Consistent with the literature, the flight to private schools is stronger for high SES households (Table 6) and households living in high SES areas (Panel A of Appendix Table A.15). In this subsection, I further examine whether regional characteristics reinforce this flight. While findings in Table 6 imply that the overall student composition in the public sector may have changed, it does not necessarily mean that this leads to poor peer quality in each school. If high SES students living in high SES areas (areas whose median SES is high) are mostly leaving, then the compositional effect in each school is limited. The school-level composition gets especially poorer when high SES students living in areas with high diversity (or low median SES) leave the public system. I investigate the potential change in student composition in public schools by exploring heterogeneity in effect by household income and regional characteristics together. Three CPUMA level characteristics are used: poverty rate and share of minority and foreign-born. In Table 7, I first divide the sample into high and low CPUMA using the national means in 2000.[37] To further assess who is leaving for private schools in disadvantaged areas, I divide the sample one more time by household income. Thus, four groups for each regional

[37] I divide the CPUMAs by their characteristics in 2000 to avoid any endogenous change happening together with the change in the education budget.

characteristic are separately estimated, and the results are presented in Table 7.

Columns 1 and 2 divide the sample by the poverty rate (high and low CPUMAs in columns 1 and 2, respectively). I then show the results of high and low-income families in each area in Panels A and B, respectively. For example, Panel A in column 1 is the impact on high-income families living in high poverty areas. The table also shows the *p*-value of the difference in point estimates. When comparing the same income group in high and low poverty CPUMAs, we can refer to the end of each panel. When comparing the income groups within the same region, the corresponding *p*-value is presented at the bottom of each column. I do the same analysis for minority and foreign populations in columns 3-6.

The point estimates are always larger for high-income families (Panel A) than low (Panel B) in all columns, consistent with the results in Table 6. On the other hand, the impacts are larger in disadvantaged regions (columns 1, 3, and 5) for both income groups, implying that households in low SES areas are more responsive than people in high SES areas. Interestingly, the point estimate is the largest for high-income families in high (disadvantaged) areas for all three regional characteristics. In other words, the results show that high-income families in low SES areas are responding to education funding cuts the strongest. The differences of point estimates are significant for poverty and minority population, but not for foreign populations. I also observe a similar pattern when I conduct the same analysis by CPUMA characteristics and race, finding larger impacts for whites in low SES areas than other races (See Appendix Table A.15). Together with Table 7, high-income and white families in low SES areas tend to leave public schools when exposed to funding cuts for public education.

Schools in high SES areas are somewhat immune to this competition between public and private schools. It may be that public schools in high SES areas are already resourceful relative to their local private schools, or the teachers and the school administration in these schools can more efficiently manage the financial hardships. Or, it may be that households with a very high preference for public schools have already been sorted in these areas. This study cannot answer why these school districts could be exempt from this competition, and it could be an important topic for future research.

VI.3 Discussion on Heterogeneity Effects

Despite the significant point estimates in Table 6, the change in student composition in the entire public sector is limited. For example, the back of the envelope calculation shows 122,400 white students left for private schools; however, it is only 0.36 percent of total white students. My estimates suggest that funding cuts from 2007 to 2012 reduced the proportion of white students in public school by 0.06 percent and increases Hispanic and black students by 0.01 and 0.05 percent, respectively, which is almost negligible. While we need not to concern about the negative change in student composition with this result, the negative impact in neighborhoods and communities may be somewhat large when considering the results in Table 7. According to the estimates, the proportion of high-income students in high poverty areas decreases by 0.21 percentage points, while the change is trivial in low poverty areas. The average decrease in education funding is larger in high poverty areas ($770 vs. $354), and they are even suffering from losing wealthier students, a possible decline in peer quality.[38] Similarly, the proportion of rich students in high minority and immigrant areas decreases by 0.17 and 0.1 percent, respectively. These figures are non-negligible; the fraction of Black students in public schools increased by 0.9 percent from 1970 to 1980 in California after the famous

[38]Here is the number of students in each category in columns 1 and 2 in Table 7. Numbers in the parentheses are percent in private schools.

	High Poverty PUMA	Low Poverty PUMA
High Income HH	10,173,153 (14.93%)	14,043,554 (14.09%)
Low Income HH	15,516,839 (5.798%)	9,214,719 (7.187%)

I estimate the number of students left for private schools using the point estimates in Table 7 and calculate the change in share in public schools using the numbers in the table above.

school finance reform following *Serrano I* and *Serrano II*.

My findings suggest that funding cuts may have reduced diversity among public school students. While the impact of diversity on students' academic outcomes may be ambiguous (Dills, 2018; Mar, 2018), it has positive effects on behavioral and social outcomes (Boisjoly et al., 2006). In addition, the composition change can be one channel amplifying the effects of school resources because of peer effects. If high SES students who flee to private schools tend to be high achievers as well, the performance of low-scoring children remaining in public schools would be especially undermined (Akyol, 2016; Dills, 2005). Therefore, we should take this into account when we interpret the impacts of public education spending on students in public schools; otherwise, the effect of funding may be overstated.

VII Conclusion

Private schools serve a significant portion of students in K-12 and play an essential role in improving education quality by providing an alternative and inducing competition. Parents often choose private schools because they believe private schools are better resourced than public schools. Considering this, a shock to the public school budget may influence parents' choice to enroll their children in private schools. Understanding how sensitive students are to public school funding is important for policymakers to make an informed decision on K-12 spending, one of the largest government expenditures.

By leveraging the education funding cuts caused by the Great Recession, I find robust evidence that private K-12 enrollment is responsive to public education resources. I separate the impact of the funding cuts from that of the Great Recession by exploiting two plausibly exogenous sources of variation, the share of state-appropriated funds for K-12 and an indicator for no-state-income-tax in a given state. I combine these two sources with the timing of the

Great Recession in an event study framework and use the event study interaction terms as the instruments for the local K-12 revenue per pupil.

I find that a $1,000 decrease in public education budget per pupil increases private school enrollment by 0.624 percentage points, implying the elasticity is -0.65. A decline in public schools' perceived quality represented by the instructional spending and spending per teacher seems to be a likely mechanism. Moreover, the impact of funding cuts is concentrated within white and Hispanic students and high- to middle-income households. I also show that high SES children are responsive especially when they live in disadvantaged areas. My heterogeneity results shed some light on how public school funding increases inequality through school choice and change in student composition.

Finally, the Great Recession has an important lesson in handling the current economic crisis caused by COVID-19. We are already experiencing another financial shock for K-12 education due to the pandemic. It has been only a few years since the schools have fully recovered from the Great Recession, and another cut may result in devastating impacts. Anecdotal evidence shows that wealthy families already have left for private schools that were under fewer regulations and have greater resources to deal with remote classes than local public schools. This is especially critical during the COVID crisis where public schools physically shut down, and if some private schools can avoid this, it could lead to a striking learning inequality.

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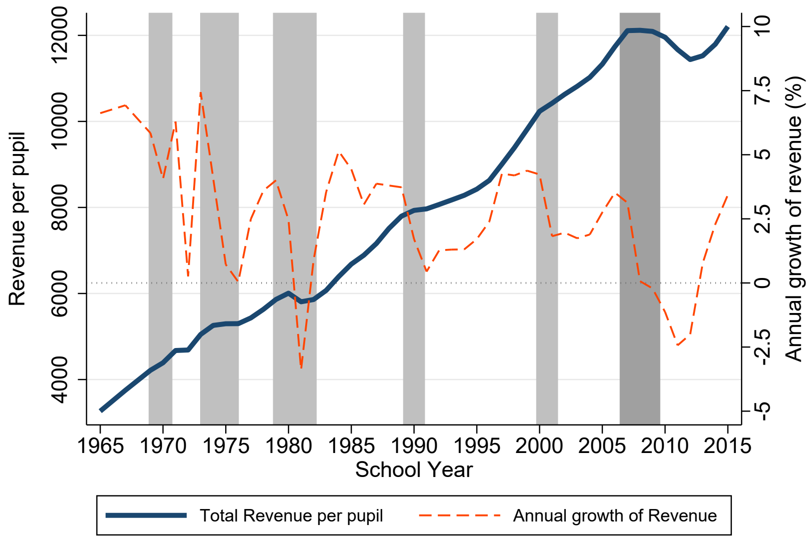
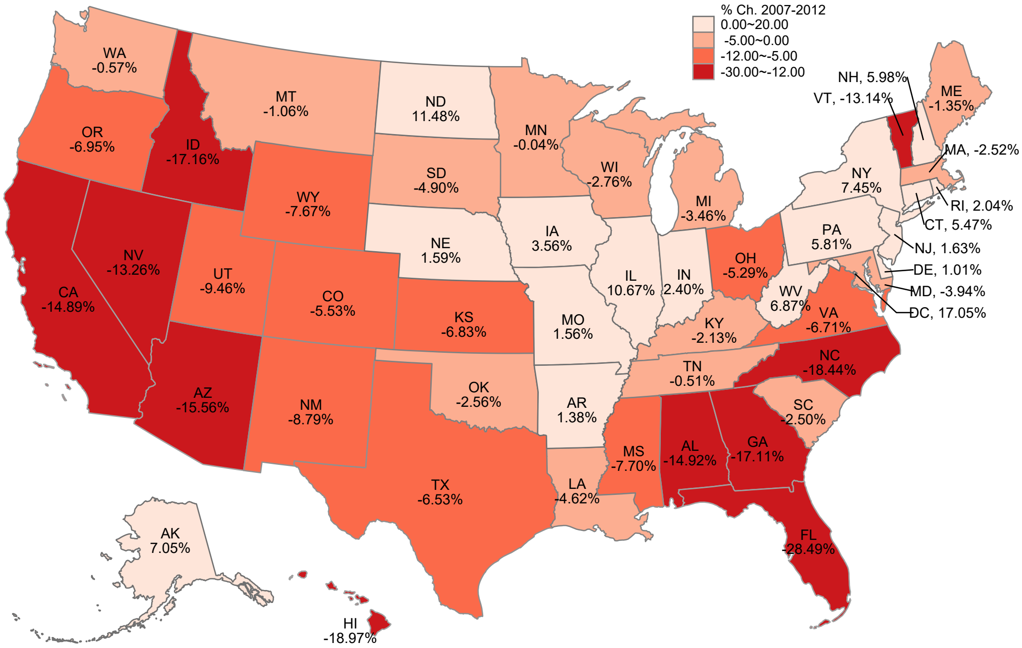
Figures

Figure 1: Real Total K-12 Appropriations Per Pupil and Growth Rate



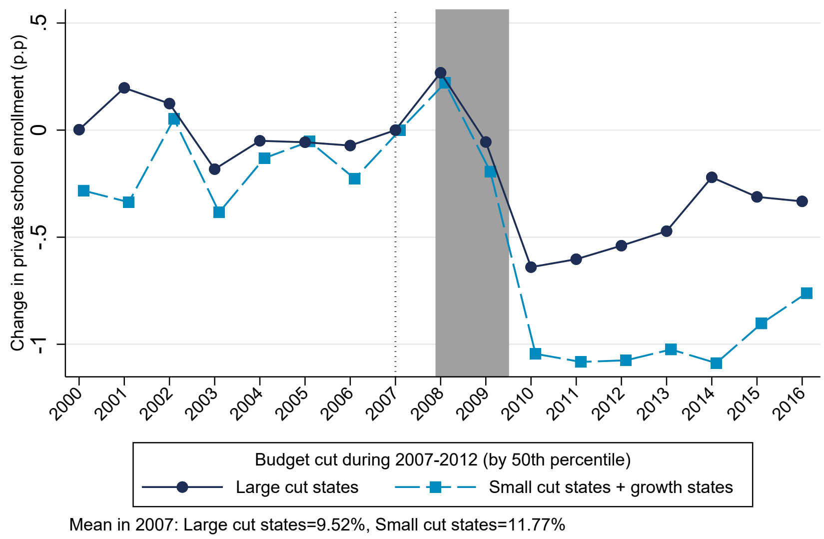
Notes: Data from the Common Core of Data (CCD) of the National Center for Education Statistics (NCES). Data aggregates the K-12 appropriations in 50 states and divides it by the full-time equivalent enrollment. The appropriations per pupil are adjusted for inflation (in 2010 dollars). The orange dash line depicts the annual growth rate of the appropriations per pupil in percent. Shaded areas represent recessions retrieved from the Bureau of Economic Analysis. The Great Recession is marked with a darker shade. The figure presents that the growth rate of education appropriations per pupil decreases during or after recessions, and the Great Recession is followed by an unprecedented cut that lasted for almost a decade.

Figure 2: Change of Appropriations Per Pupil from 2007 to 2012



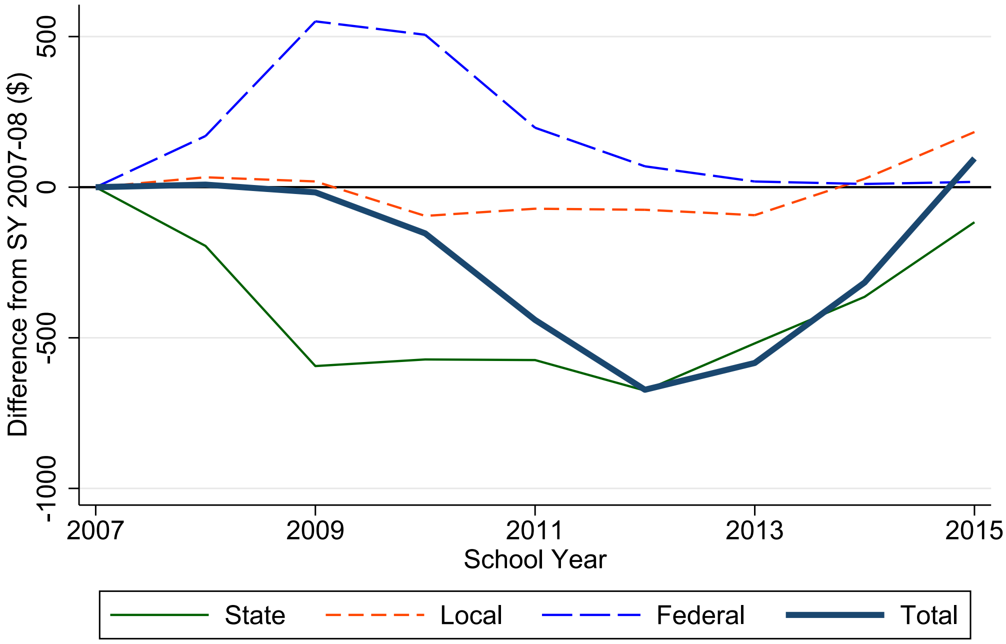
Notes: Panel A shows the variation in funding cut across states induced by the Great Recession from 2007 to 2012. The percent change is calculated using the real value of revenue per pupil in 2010 dollars. Darker shade means larger cuts and the 16 states with the brightest shade are states with growths in K-12 funding.

Figure 3: Trend of Private School Enrollment Relative to 2007 by the Magnitude of Funding Change



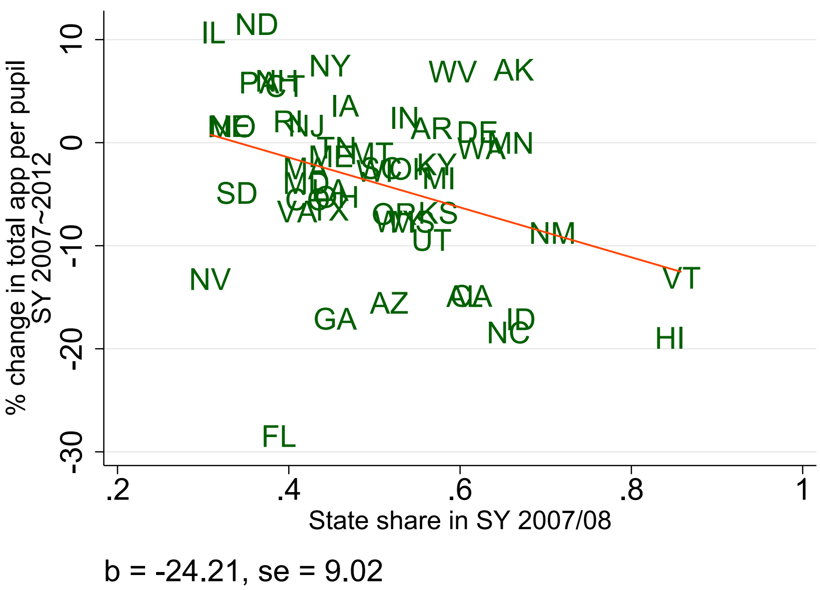
Notes: The figure shows the trend of private school enrollment relative to 2007 separately by large and small budget cut states using the Census and ACS. Large cut states are 25 states with a growth rate below the median (-5 percent). Small budget cut states include 16 states with positive growth. The mean private school enrollment has no difference relative to the 2007 level before the Great Recession. After the recession, while both groups of states had experienced a decline in private school enrollment, there is a smaller decline or a relative increase in large cut states.

Figure 4: Trend of Appropriations Compared to 2007, by Sources



Notes: This figure shows the trend of the change in public education appropriations by source. I calculate the dollar difference from the school year 2007-2008 level by sources to show how the budget had changed over time since the start of the Great Recession. All monetary values are in 2010 dollars.

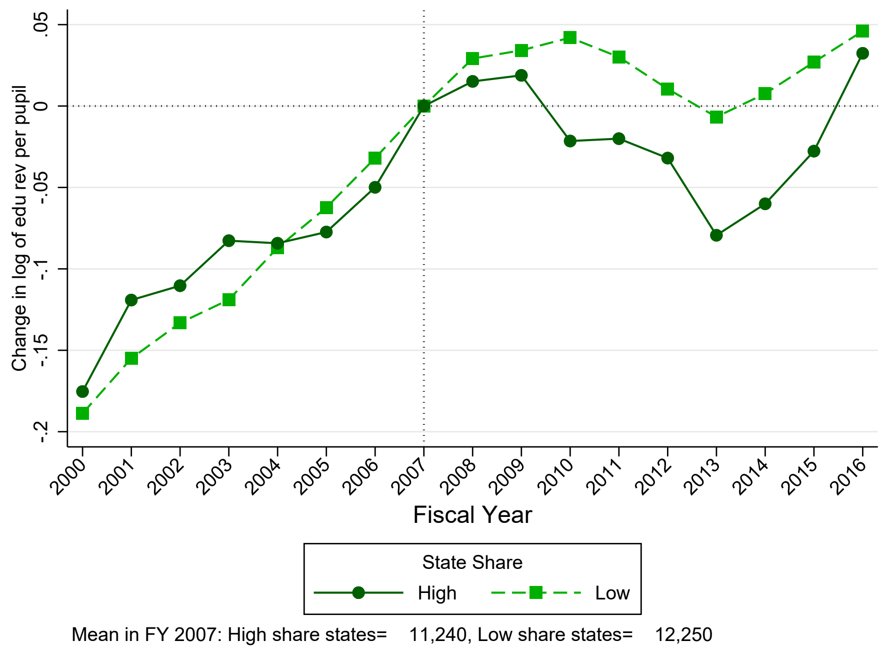
Figure 5: Relation Between State Share and Change in Funding



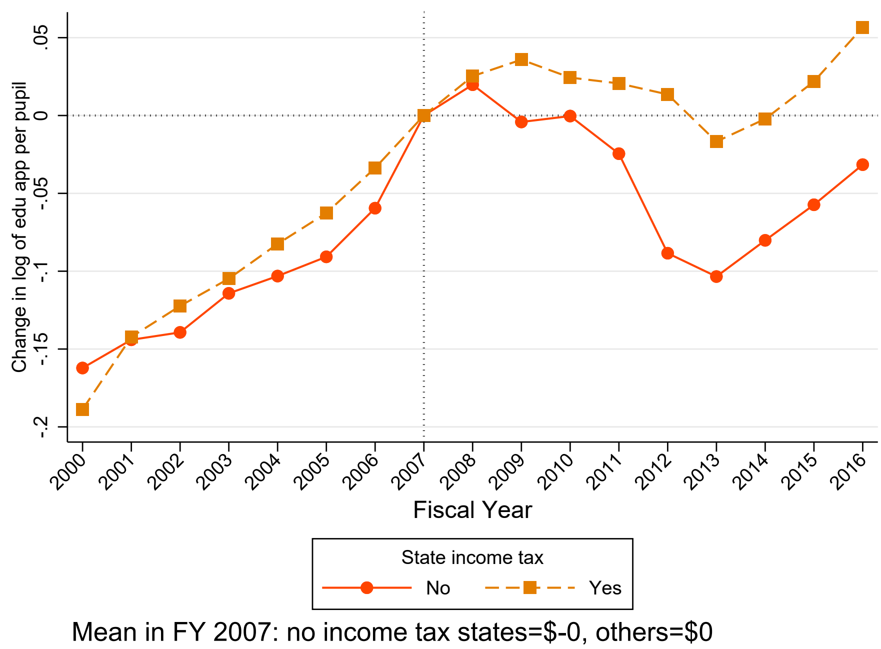
Notes: This figure shows the relation between state share in SY 2007 and % change in education funding from 2007 to 2012. The orange straight represents the linear prediction of two variables. The coefficient is -24 and the standard error is 9, significant in 1% level.

Figure 6: Trend of K-12 Funding Compared to 2007, by State Share and Income Tax Status

(a) By State Share



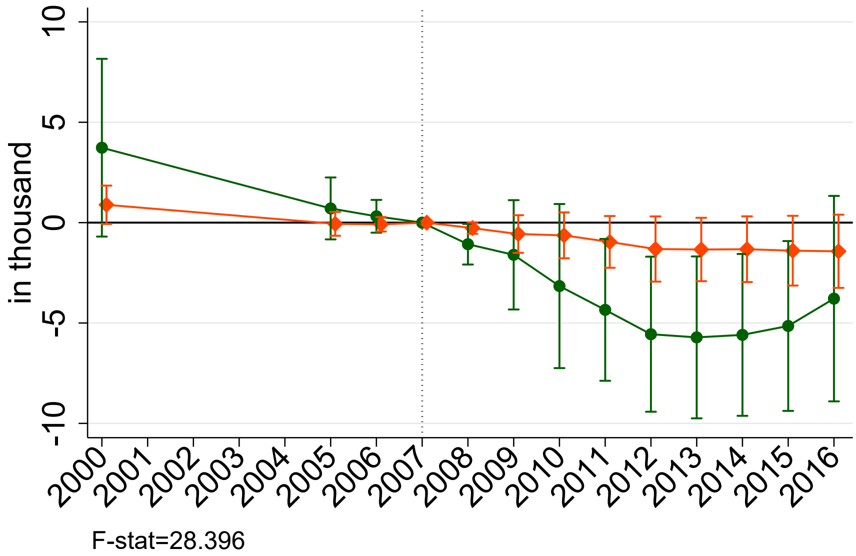
(b) By Income Tax Status



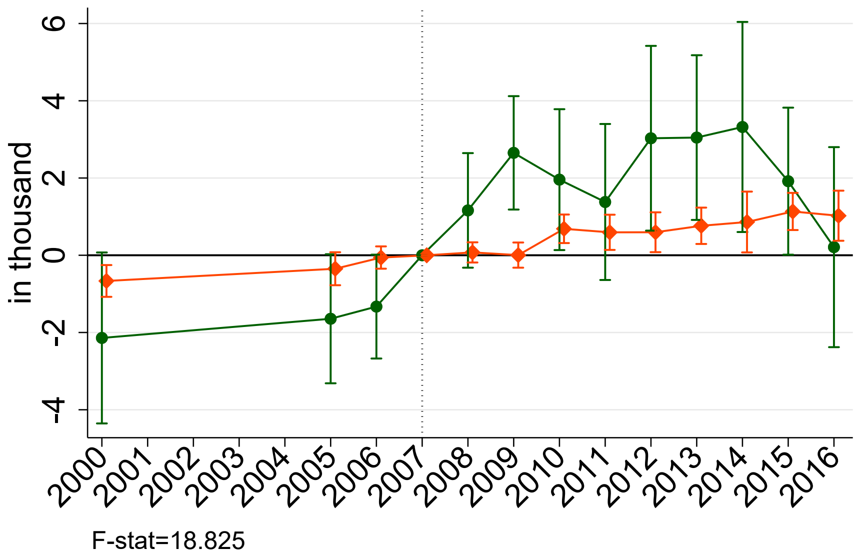
Notes: Panel A shows the trend of the mean tax revenue per capita relative to FY 2007 in two groups of states (states with and without an individual income tax). The mean of each group is the weighted mean with state population in 2000. Panel B shows the trend of mean K-12 funding per pupil relative to FY 2007, also weighted with the school-aged population in each state in 2000. All monetary values are in 2010 dollars.

Figure 7: First Stage and Reduced Form Results

(a) First Stage



(b) Reduced Form



Notes: N=8,095,073. The first stage result in the most preferred specification (including the full sets of controls) is presented in this figure. I display the coefficients of interaction terms of year dummies and state share, and income tax status (β_k's and γ_k's) along with 95% confidence intervals. F-statistics of the instrument variables are 28.396 and 18.825 for Panels A and B, respectively. The state share is a continuous variable from 0 to 1 representing the contribution of state-distributed revenue to the total education revenue, and the no income tax indicator is a binary indicator. 2001-2004 ACS are excluded from the sample because CPUMA is not identified in these years. See the notes of Table 3 for further information on the controls. Standard errors clustered at the state level. See Appendix Figure A.8 for the results, including 2001-2004. See Appendix Figure A.9 for other specifications.

Tables

Table 1: Summary Statistics in the Pre- and Post-Recession Periods

		Pre-Recession					Post-Recession				
			State Share		Income Tax			State Share		Income Tax	
		All states (1)	High (2)	Low (3)	No (4)	Yes (5)	All states (6)	High (7)	Low (8)	No (9)	Yes (10)
Panel A. Private School Enrollment and Appropriate per pupil											
Private School Enrollment (Percent)											
All		10.34	9.13	10.97	8.18	10.79	9.89	8.87	10.41	8.02	10.32
By age	6 - 13	11.05	9.83	11.68	8.84	11.52	10.23	9.23	10.74	8.39	10.66
	14 - 17	8.92	7.73	9.54	6.85	9.35	9.21	8.15	9.75	7.27	9.65
By race	White	13.01	11.71	13.61	11.59	13.26	13.13	12.09	13.61	12.1	13.3
	Hispanic	5.74	4.56	6.18	4.04	6.06	5.72	4.26	6.24	4.54	5.97
	Black	5.30	4.67	5.75	4.21	5.74	4.93	4.36	5.3	4.36	5.18
By income percentile	>90	23.78	23.12	24.09	24.15	23.71	22.55	22.28	22.67	22.12	22.62
	90-75	14.51	13.37	15.07	12.50	14.88	13.41	12.73	13.74	11.2	13.86
	75-50	10.64	9.41	11.26	8.34	11.10	10.11	9.15	10.59	8.41	10.48
	50-25	7.79	6.64	8.40	5.80	8.24	7.53	6.59	8.03	5.92	7.93
	<25	4.89	3.95	5.40	3.42	5.24	5.13	4.04	5.71	4.02	5.42
Appropriations per pupil	Total	10,852	9,865	11,361	9,671	11,100	11,978	10,457	12,751	10,201	12,385
	State Share	0.47	0.60	0.42	0.43	0.48	0.46	0.58	0.40	0.42	0.46
Panel B. Covariates											
Female		0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Number of Siblings		1.45	1.49	1.43	1.43	1.46	1.50	1.53	1.48	1.50	1.50
Age		11.54	11.53	11.54	11.51	11.54	11.53	11.52	11.53	11.48	11.54
Foreign Born		0.06	0.07	0.06	0.09	0.06	0.06	0.06	0.06	0.08	0.05
White		0.60	0.56	0.63	0.51	0.62	0.54	0.51	0.56	0.42	0.57
Black		0.15	0.12	0.16	0.13	0.15	0.14	0.11	0.15	0.13	0.14
Hispanic		0.18	0.22	0.16	0.29	0.15	0.23	0.26	0.21	0.36	0.20
Household Income		79,729	77,407	80,926	74,589	80,806	81,565	79,082	82,828	75,713	82,904
Two Parents		0.69	0.70	0.69	0.68	0.70	0.69	0.70	0.68	0.68	0.69
One Parent		0.26	0.25	0.26	0.26	0.25	0.27	0.26	0.28	0.28	0.27

Notes: This table presents the averages of variables before and after the Great Recession. I divide the states with high and low shares (cut off 0.54) and states without and with income tax. Panel A shows the averages of private school enrollment by subsample and education appropriations per pupil. Panel B presents the averages of important covariates.

Table 2: Placebo Test in 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Individual Level Covariates						
	HH Head Employed	HH Income	<150% Poverty Line	Ownership of Dwelling	Minority	Foreign Born
App per pupil	0.00157	0.614	0.00192	0.00650*	-0.00497	0.00529*
(in thousand)	(0.00299)	(0.619)	(0.00411)	(0.00325)	(0.00389)	(0.00299)
	0.797	*80.87*	*0.280*	*0.695*	*0.404*	*0.0643*
Panel B. State Level Covariates in thousands						
	Median House Value	Num of Children	Num of Whites	Num of Hispanics	Num of Blacks	Num of Poverty Children
App per pupil	2.476	-6.06	-0.404	-4.87	-0.911	-2.87
(in thousand)	(7.165)	(4.54)	(1.62)	(4.05)	(0.682)	(2.35)
	230.6	*131*	*75.2*	*27.1*	*19.2*	*37.4*
Panel C. State Spending per Capita (in $1,000)						
	Total Expenditure	Higher Education	Health	Medicaid	Welfare	Unemployment Benefits
App per pupil	244.8***	-12.41	-12.08	0.426	18.61	9.908
(in thousand)	(89.00)	(17.24)	(14.65)	(44.24)	(43.02)	(7.725)
	8656.6	*664.0*	*245.3*	*970.6*	*1248.1*	*123.4*

Notes: Panel A: N=8,095,073, first stage F-stat=29.01. Panel B: N=13,975, first stage F-stat=27.64. Panel C: N=750, first stage F-stat=14.01. The dependent variables of the regressions are indicated in the column title. Panels A, B, and C are at the individual, CPUMA, and state levels, respectively. Each entry is a coefficient from a separate 2SLS regression of the dependent variable on real K-12 appropriations per pupil in the CPUMA (in thousands of 2010 dollars). The instruments are the sets of interaction terms of state share and no state income tax status interacted with year dummies. Regressions are weighted using sample weights, schoolchildren population of CPUMA in 2000, and population in 2000, respectively, in Panels A, B, and C. All monetary values are in real term (in 2010 dollars) and in $1,000. Panels A and B are from the ACS and Census. Panel C is from the Annual Survey of State and Local Government Finance (US Census Bureau (2020), through Urban Institutes). Medicaid expenditure data is from the Centers for Medicare and Medicaid Services. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 3: Main Effects on Private School Enrollment

Dependent variable: private school enrollment(in percentage point)

	(1)	(2)	(3)	(4)
App per pupil	-0.461***	-0.505***	-0.545***	-0.624***
(in thousand)	(0.154)	(0.153)	(0.161)	(0.160)
First stage F-Stat	29.01	29.08	28.95	28.40
Individual Controls		Yes	Yes	Yes
Household Controls			Yes	Yes
Baseline CPUMAControls×Time Trend				Yes

Notes: N=8,095,073. This table reports the estimates of the impact of K-12 appropriations per pupil on private school enrollment using Equation 1. Each entry is a coefficient from a separate regression. The coefficients are rescaled to represent private school enrollment in percentage points. All regressions are estimated with the 2SLS model using Equation 2 as the first stage. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. See the main text for further information. The K-12 appropriations per pupil are adjusted for inflation in 2010 dollars and scaled in $1,000. All specifications include students' age in the full set of dummy variables with state and year fixed effects and controls described in the table. Individual controls include race, sex, number of siblings, and an indicator for limited English proficiency and foreign-born. Household controls include the log of total household income, parental characteristics such as education, race, foreign-born indicator, employment status, and the presence of parents 1 and 2. CPUMA controls include the share of minority, foreign-born, under 150% of the poverty line at the state level, and the number of school-aged children. I interact these characteristics in 2000 with the time trend. Regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 4: Impact on Staff and Expenditure Categories

	(1)	(2)	(3)	(4)
Panel A. Expenditure per pupil				
	Total Operational	Instruction	Capital	Student Support
App per pupil	667.2***	437.0***	17.82	40.41**
(in thousand)	(75.08)	(61.21)	(52.82)	(17.69)
	9,363	*5,791*	*1,266*	*462*
Panel B. Expenditure per teacher				
	Salary	Employee Benefits		
App per pupil	1440.6*	3383.5***		
(in thousand)	(853.2)	(804.4)		
	64,208	*18,488*		
Panel C. Staff per 100 students				
	Teacher	Aides	Guidance Counselor	Library Staff
App per pupil	0.0249	0.200***	0.007	0.0056
(in thousand)	(0.112)	(0.0663)	(0.0059)	(0.0117)
	6.153	*1.318*	*0.204*	*0.155*

Notes: N=18,273. First stage F-stat = 20.24. The unit of observation is CPUMA. Dependent variables are indicated above the point estimates. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. All regressions include year and CPUMA fixed effects and CPUMA controls in 2000 interacted with year. Regressions are weighted using the schoolchildren population of the CPUMA in 2000. Robust standard errors are in parentheses clustered by state. The means of the dependent variables before the Regression are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 5: Alternative Specifications

Dependent variable: private school enrollment(in percentage point)

				Alternative Appropriations		Alternative definition of state share and NT			
	State Level Mean	State linear time trend	Bartik Controls	State App	CPUMA Expenditure	5-yr avg state share	2000 state share	1990 state share	Add NH,TN in NT states
	(1)	(2)		(4)	(5)	(6)	(7)	(8)	(9)
App per pupil	-0.395**	-0.651**	-0.635***	-0.588***	-0.644***	-0.577***	-0.527***	-0.589***	-0.624***
(in thousand)	(0.187)	(0.319)	(0.155)	(0.207)	(0.176)	(0.158)	(0.140)	(0.187)	(0.160)
First stage F-Stat	12.95	.	16.01	10.48	16.81	21.26	18.38	23.07	28.40
Observations	850	8,095,073	8,095,073	8,095,073	8,095,073	8,095,073	8,095,073	8,095,073	8,095,073

Notes: Each entry is a coefficient from separate 2SLS regressions of the private school enrollment on real K-12 appropriations per pupil in CPUMA (in thousands of 2010 dollars). The coefficients are rescaled to represent private school enrollment in percentage points. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. All regressions include year and CPUMA fixed effects and sets of controls in column 4 of Table 3. Regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. In column 1, I collapse the sample to the state level and estimate the impact of state-level appropriations per pupil on the mean of private school enrollment. No other control variable is added. Column 2 includes a linear time trend of states ($\eta_{ps} \times t$). F-statistic of the first stage is not estimated due to high collinearity. In column 3, I add state Bartik controls predicting unemployment rates and average wages using industrial composition before the Recession. In column 4, I use state appropriations instead of CPUMA. In column 5, I use realized CPUMA expenditure. Columns 6-9 use alternative definitions of the state share and no-income-tax. In column 5, I use the average state share from 2002 to 2007. Columns 7 and 8 use state shares in 2000 and 1990, respectively. In column 9, I add New Hampshire and Tennessee to no income tax states. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 6: Heterogeneity in Effect by Age, Race, and Household Income

Dependent variable: private school enrollment (in percentage point)

	(1)	(2)	(3)	(4)	(5)
Panel A. By age and Race					
	Age		Race		
	6-13	14-17	White	Hispanic	Black
App per pupil	-0.692***	-0.497***	-0.729***	-0.564***	-0.141
(in thousand)	(0.190)	(0.140)	(0.224)	(0.133)	(0.170)
	10.98%	*8.75%*	*12.40%*	*5.57%*	*5.52%*
First stage F-Stat	28.59	25.54	20.96	45.41	15.94
Observation	5,335,982	2,759,091	4,989,117	1,454,688	955,404
Panel B. By household income					
	Richest >90	90-75	75-50	50-25	Poorest <25
App per pupil	-0.591**	-0.926***	-1.068***	-0.661***	-0.182
(in thousand)	(0.253)	(0.288)	(0.257)	(0.161)	(0.126)
	22.98%	13.70%	*10.08%*	*7.44%*	*5.20%*
First stage F-Stat	16.36	34.90	45.47	7.044	15.68
Observation	838,280	995,395	1,482,107	1,258,325	1,926,443

Notes: Each entry is a coefficient from separate 2SLS regressions of the private school enrollment on real K-12 appropriations per pupil in CPUMA (in thousands of 2010 dollars). The coefficients are rescaled to represent private school enrollment in percentage points. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. All regressions include year and CPUMA fixed effects and controls as in column 4 of Table 3. Regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. In Panel A, the sample is divided by age and race, respectively in columns 1-2 and 3-5. Panel B divides the sample by the household income. I use the national percentile in each year to define the subsamples. Means of the private school enrollment of each group in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 7: Heterogeneity by CPUMA Characteristics and Household Income

Dependent variable: private school enrollment (in percentage point)

	Poverty		Minority Population		Foreign Population	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. High income households						
App per pupil	-1.387***	-0.556***	-1.299***	-0.424***	-1.011**	-0.497***
(in thousand)	(0.427)	(0.165)	(0.378)	(0.153)	(0.379)	(0.157)
	14.93%	*14.09%*	*16.72%*	*12.99%*	*15.38%*	*13.86%*
p-value of difference	0.0218		0.0166		0.173	
First stage F-Stat	15.80	40.18	61.66	20.49	875.9	23.16
Observations	1,737,279	2,400,000	1,537,126	2,600,153	1,520,182	2,617,097
Panel B. Low income households						
App per pupil	-0.446***	-0.389**	-0.597***	-0.0465	-0.616**	-0.0939
(in thousand)	(0.151)	(0.157)	(0.213)	(0.140)	(0.253)	(0.149)
	5.80%	*7.19%*	*5.84%*	*6.77%*	*5.71%*	*6.68%*
p-value of difference	0.729		0.0184		0.0719	
First stage F-Stat	18.74	13.63	28.66	15.70	63.29	20.46
Observations	2,489,307	1,468,487	1,819,671	2,138,123	1,406,057	2,551,737
p-value of difference of panel A and B	0.025	0.232	0.0496	<0.01	0.133	0.048

Notes: Each entry is a coefficient from separate 2SLS regressions of the private school enrollment on real K-12 revenue per pupil in CPUMA (in thousands of 2010 dollars). The coefficients are rescaled to represent private school enrollment in percentage points. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. All regressions include year and CPUMA fixed effects and the full sets of controls, as in column 4 of Table 3. Regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. The sample is first divided into two groups by CPUMA characteristics presented in the title of each column. High and low is defined by whether the mean in CPUMA in 2000 was higher or lower than the national average in 2000. Then, I divide each group by the household income percentile and display them in Panels A and B. Thus, each regional characteristic has four subgroups. The *p*-values of the difference in coefficients of the same income group in high and low CPUMAs are presented at the bottom of each panel (column difference). *p*-values of the difference between different income groups in the same area are also presented at the bottom of the column. Means of the private school enrollment of each group in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

Online Appendix

A School District and CPUMA Crosswalk

District-level CCD data contains districts that are not credible and suitable for the research. I first eliminate vocational, special education, and education service agency districts. This drops 1 percent of the sample and 0.27 percent of the total enrollment. Then, I follow Lafortune, Rothstein and Schanzenbach (2018) to eliminate tiny district-year observations and observations with suspicious enrollment and funding growth. I remove district-year observations with less than 100 enrollment, which drops 8 percent of observations or 0.12 percent of the enrollment. I then drop observations with negative funding or less than 20 percent or greater than 500 percent of the state average funding (0.21 percent of observations, 0.04 percent of enrollment) and singleton districts (0.16 percent of observations, 0.03 percent of enrollment). I also remove district-year observation with enrollment (1) more than twice of the district average in the sample, (2) more than 15 percent above or below the prior year, and (3) more than 50 percent above or below the predicted trend of enrollment. Eight percent of observations and 0.11 percent of enrollment are dropped. Finally, I remove charter school districts, which account for 9 percent of observations and 1.3 percent of the enrollment. My final sample excludes 26 percent of district-year observations and 1.87 percent of total enrollment.

The smallest geographical unit identifiable is the PUMA (Public Use Microdata Area) in the publicly available Census and ACS. Throughout this paper, I use Consistent PUMA (CPUMA), an aggregate of contingent PUMAs to make the boundaries consistent over time. PUMA is based on the population; each PUMA should have at least 100,000 population. Because PUMAs are based on population, they are sometimes very small areas in populated cities. There are slightly more than 2,000 PUMAs, while 15,000 to 16,000 school districts in the US.

Matching school districts to PUMAs or CPUMAs is difficult because 1) school district boundaries slightly change every year, and 2) school districts and PUMAs are based on different geographical units. While PUMAs are based on population, school districts are usually defined within a county, a city boundary, or a commuting zone. Therefore, a single school district may contain several CPUMAs in a large metropolitan area. For example, Austin, Texas, comprises more than ten PUMAs, while most public schools are under Austin Independent School District except for charter schools that are a separate school district. In most parts of the country, however, CPUMAs are larger than the school districts and consist of several (Appendix Figure A.5). To match the school district to CPUMAs, I use the geocoordinates of school district offices. To be specific, I do the following:

1. Match geocoordinate of the school district office to PUMA.
2. Aggregate all matched school districts into PUMA level.
3. For PUMAs with no matched school district, use the average in the MPUMA level.[39]
4. Using the matched PUMA level finance data, take a weighted average of PUMAs to construct CPUMA level data.

The CCD provides the geocoordinates of school districts from 2005 to 2014. For the rest of the years, addresses are only available. Using Google and Bing map, I retrieved the latitude and longitude of school district offices in the remaining years. I identify the PUMA on which each school district office lies using QGIS and then construct PUMA level total funding and expenditure and public school enrollment. In most cases, several school districts fit in a single PUMA; in other words, most PUMAs have at least one matched SD. This is largely true for less populated rural areas or small cities. However, this is not the case for some metropolitan areas. The east coast of Florida, for example, is served by large pockets of school districts, while this area is divided into a couple of small PUMAs. For these PUMAs without any matched school district, I use MPUMA level financial and enrollment data instead.[40] Then, I estimate the average of constitutive PUMAs weighted by population and construct CPUMA level finance information.[41]

There are a few adjustments that I made. First, some school districts are aggregated into one district in the CCD finance file. Hawaii and New York City school districts are divided into several districts and zones in practice; however, the state and the city report their financial data as a united school district to CCD. I assign all PUMAs in Hawaii and New York City to the same school district to adjust this. Second, three PUMAs in Louisiana are combined into one PUMA from 2006 to 2011 because the population went below 100,000 in each PUMA after Hurricane Katrina. Therefore, I combine these three PUMAs to one in 2000 and 2005 and define a new PUMA to make it consistent over time.

[39] Migration PUMA (MPUMA) resembles commuting zones and is used to identify the place in which the respondent lived in the previous year (if migrated) and working area.

[40] There is at least one matched school district in all MPUMAs as they represent commuting zones.

[41] I take a weighted average of PUMAs instead of directly matching SDs to CPUMAs because it is difficult to determine the MPUMAs for some CPUMAs.

B Potential Pre-Trends

The first stage and reduced form results in Figure 7 raise pre-existing trend problem, although excluding 2001-2004 makes it difficult to interpret. In Appendix Figure A.8, the pre-trends seem to be quite evidence. The p-value of the joint hypothesis test of pre-event coefficients for both first stage and reduced form is less than 0.01, suggesting the existence of pre-trends. While I cannot remove the trend, I follow Freyaldenhoven, Hansen and Shapiro (2019) (FHS)'s suggestion to address the pre-trends. In their paper, they show that unobservable confounds can induce pre-trends in the event study model, and including the covariates correlated with the confounds may eliminate the pre-trends. The true treatment (policy) effect can be estimated with a 2SLS estimator using the leads of the policy variable as the excluded instrument for the covariate. FHS's setting requires panel event study design, so I use the state-level aggregated sample. Furthermore, because the FHS model does not allow multiple treatment variables, I separately estimate using state share and no-income-tax indicator for the treatment.[42]

I choose four covariates: log of labor population, the share of intergovernmental revenue of total state revenue, and Bartik predictions of the unemployment rate and average wage. I have four event study estimates to test, first stage and reduced form with state share and no-income-tax indicator treatment variables. Labor population and Bartik covariates are relevant to the economic conditions of the state. Intergovernmental revenue represents how much states are self-sufficient, which may affect education funding. In the first panel of Appendix Figure A.10, I present the event study results of the covariates using the treatment variables. Ln(Labor Population) does not show pre-trends for the state share, but the rest of the results seem to have pre-trends (and post-trends). The rest of the four figures in each panel present the pre-trend adjusted result. Unfortunately, I could not find a covariate with pre-trends and removing pre-trends from all four estimates. Pre-trends for state share are mostly addressed. On the contrary, I find two covariates that remove pre-trends of no-income-tax indicator in the first stage: ln(labor population) and Bartik predictions for the unemployment rate. The exact point estimates are available in Appendix Table A.3.

In Appendix Table A.4, I summarize the FHS estimators together with the four separate main results. In columns 3 and 6, I take the ratio of the first stage and reduced form that

[42] FHS provide a stata package *xtevent*, however, I do not use it because it does not allow regression weights. I instead emulate their 2SLS estimators in Freyaldenhoven, Hansen and Shapiro (2019) using the first lead of the treatment variable.

suggests the size of the 2SLS estimator. The ratio for state share treatment (column 3) is -0.661, and the p-value of the joint hypothesis test for pre-event estimators does not reject no pre-existing trends for both first stage and reduced form. The ratio of -0.661 also lies in

$$-2.348, -0.488$$

. There are pre-trends for the no-income-tax indicator in columns 4 and 5. While I could not find a covariate that removes pre-trends in both the first stage and reduced form, Panel B and D adjusts the pre-trends in the first stage. The ratio of -0.352 of the main result seems to be overestimated by the pre-trends. However, the average point estimators for both the first stage and reduced form rather get larger, as in Appendix Figure A.10, and the smaller ratio is the consequence of the disproportional adjustments. Thus, I show that the post-period estimates do not vanish even after eliminating pre-trends using FHS estimator, arguing that pre-existing trends are not the major confounders of my main results.

C Complier Characteristics

My instrument variable strategy identifies the local average treatment effect (LATE) for states which reduced their total K-12 funding per pupil because they had been relying heavily on the state budget and do not collect income taxes, but would not have reduced education funding if their state reliance was small and had imposed income taxes. In other words, the LATE shows the impact on "complier" areas. The compliers may be a set of areas with particular characteristics, thus not random, making the LATE context-specific. While compliers cannot be individually identified, it is easy to describe the distribution of compliers' characteristics (Angrist and Pischke, 2008).

In Table A.5, I compare the characteristics of complier CPUMAs to the entire sample. Following Angrist and Fernández-Val (2013); Angrist and Pischke (2008)'s methodology, I define two binary instrument variables Z_{1i} and Z_{2i}, high state share (above median of the share in 2007) and no income tax indicators interacted with post-Recession indicator. Angrist and Pischke (2008) shows that the IV estimate with multiple exclusive instruments is a linear combination of the LATE using each instrument. The two instruments are not mutually exclusive; so I redefinethree mutually exclusive instrument variables $(Z_1(1-Z_2), (1-Z_1)Z_2, Z_1Z_2)$ and use them. Binary treatment status D_i equals one if the total K-12 funding per pupil in the CPUMA exceeds the national average funding in the same year, while 0 and 1 denote potential outcomes. Thus, $D_{1i} > D_{0i}$ indicates the compliers. When estimating $E[X_{1i}|D_{1i} > D_{0i}]$, I use Abadie (2003)'s kappa-weighting scheme.[43] I choose ten covariates in Table A.5. Except for the number of school-aged children, it seems that compliers are very similar to the entire population. This is true for both of the instruments. Reassuringly, the result in Table 2 shows that the number of total children is not associated with education funding per pupil.

D Additional Robustness Checks

D.1 Alternative Sample

In Table A.8, I estimate the impact of education appropriations per pupil in different samples. First, I include Washington DC in the sample in column 1. My main sample excludes DC because DC's state share is zero by definition. Although DC constitutes about 0.13 percent of total observation, including DC may change the result because it is such an outlier. The point estimate is about 0.025 percentage points smaller than the main model. In column 2, I restrict the sample to 2000-2012 to examine whether the effect is driven by the later years that are less affected by the Great Recession. The point estimate is smaller only by 0.04, supporting that the change right after the Great Recession is the difference that I am identifying. In column 3, I remove the dropouts, and because this group is less likely to enroll in private schools, the point estimate slightly increases. Columns 4 and 5 compare native-born and foreign-born students and find that the impacts are larger for native students, although not statistically different. Next, in columns 6 to 9, I remove some states that may respond differently to the funding shock. First, I exclude Florida and Nevada because they are two states without income tax known to have a very large decline in property values during the Great Recession. Next, I remove the two largest states among no income tax states, Florida and Texas. In column 8, I remove California and Texas from the sample because of their unique funding schemes. California's Proposition 98 guarantees a minimum amount of education funding from the state's General Funds and local property taxes, making education funding less volatile. Texas state education agency redistributes the locally raised tax from wealthy to poor school districts un-

[43] Abadie (2003) shows a generalized method to estimate the distribution of covariates for compliers. To be specific,

$$E[X_i|D_{1i} > D_{0i}] = \frac{E[\kappa_i X_i]}{E[\kappa_i]},$$

where

$$\kappa_i = 1 - \frac{D_i(1-Z_i)}{1-P(Z_i=1|X_i)} - \frac{(1-D_i)Z_i}{P(Z_i=1|X_i)}.$$

der Chapter 49 (well known as Robin Hood Plan). Thus, the state appropriated funding may be more stable because part of it is coming from local tax revenue. I also exclude Alaska in column 9 because Alaska does not collect either income or sales tax.[44] Estimates in columns 6-9 are all smaller than the main result; however, none of them are statistically different. Finally, I remove the top 10 percent CPUMAs in private school enrollment in 2000 in column 10 to test whether the impact is concentrated in certain areas with high access to private schools. The point estimate in column 10 is smaller than the main estimate because I remove the most responsive areas; however, it is not statistically different from the main result, implying the impact is still found in less responsive areas.

D.2 Alternative IVs

I try alternative instrumental variables to examine the robustness of the IV used in the paper. In the main analysis, the instrumental variables are the state share and the no-income-tax indicator interacted with year indicators, taking 2007 as the base year. I consider the event study framework, which is more flexible, and thus more appropriate than the traditional difference-in-differences because the treatment effect changes over time (Figure 7).

In Table A.9, I test whether my results stay consistent with the specification of instrumental variables. In column 1, I use traditional difference-in-differences variables, $S_s \times Post_t$ and $NT_s \times Post_t$, as the IVs. The point estimate is larger than the main analysis, by 0.07 percentage points. This could be partly interpreted as households "predicting" funding cuts and responding accordingly. The first stage F-statistics become much smaller because (1) the funding cut started in 2010, and (2) it fades out after 2013. When I use the event study variables of state share only in column 2, as in Jackson, Wigger and Xiong (2021), the point estimate gets smaller with a much smaller F-statistics. In column 3, the coefficient is larger when I use the no-income-tax indicator as the sole identifying variation, with losing precision. Two columns show that the impact of education funding driven by the no-income-tax indicator is stronger than the state share, and the main specification captures some weighted average of the two. In column 4, I add the interaction term of state share and no income tax indicator interacted with the year dummies. A state with a high state share and no income tax may have been through an even deeper education funding cut if two sources of variation strengthen each other. The point estimates get smaller by 0.2 percentage points with a very large F-statistics.

[44] Some local governments collect local sales tax in Alaska. However, most of Alaska's tax revenue comes from natural resources.

None of the point estimates in the table is statistically different from the main estimate.

D.3 Lagged Treatment Variables

I use the lagged value of K-12 appropriations per pupil in Table A.10 as the independent variable. This helps to examine the cumulative impact of the funding cut. Parents may not perceive the funding cut immediately and make a decision based on cumulative experience. If the lagged K-12 revenue has a much smaller impact than the concurrent revenue, then it would raise a question of the true impact of K-12 revenue. In columns 1 to 3, I use 1, 2, 3 year lagged education revenue per pupil ($App_{t-1}, App_{t-2}, App_{t-3}$), respectively. The first stage F-statistics is reasonably smaller than the main result and decreases over the column as I use more lagged values. The point estimates in columns 1 to 3 are also smaller than the main result, however still large and statistically significant. When using the three-year moving average (average of $t-1$, t, and t), the point estimate is slightly larger than the main specification. Overall, results in the table suggest the "cumulative exposure" to funding cut is as important as the current level of funding.

D.4 School Choice Programs and Number of Schools

Several states have various school choice programs. The programs include (but are not limited to) private school programs like vouchers and tax-credit scholarships and alternative public schools like charter schools and magnet schools. The most well-known private school program is a voucher, and extensive literature proves that vouchers increase private school enrollment for certain students (Epple, Romano and Urquiola, 2017). To meet the demand for increasing interest in school choice, states have implemented a variety of school choice programs since the Great Recession. While only 12 states and DC had any school choice program in 2007, it has increased to 28 states in 2016 (EdChoice, 2020).[45] If states with lower funding for public schools instead provide generous private school vouchers or tax credit, parents may choose a private school because of the private school program, not of the public education funding. In addition, the number of schools may affect private school choice. The number of public schools may decrease following the funding cut, and children may be forced to choose private schools because of accessibility. On the other hand, an increase in the number of charter and magnet schools substituting traditional public schools (TPS) may affect private school

[45] Several cities and local governments have their own programs. Thus, the population living in an area with school choice policies is much larger in 2007.

attendance. It may be that areas with low funding also lack alternative public schools, forcing parents to choose a private school. Thus, the existence of these school choice programs could partially affect the result, regardless of the true effect of public school funding.

In Appendix Table A.11, I first show whether my results are affected by private school choice policies. In columns 1 to 3, I add an indicator for private school policies (any policy, voucher, and tax credit) and re-estimate the treatment effect. The point estimates are almost identical to the main estimate. In columns 4 to 6, I estimate whether education appropriation per pupil is associated with an indicator for policies with the state sample. Point estimates are all small and statistically insignificant.

Appendix Table A.12 examines whether the number of schools can be a confounder. Similar to Appendix Table A.11, in Panel A, I add the number of each type of school as a covariate and estimate the treatment effect on private school enrollment. Like Appendix Table A.11, the point estimates are very similar to the main effect. In Panel B, I estimate the impact of appropriations per pupil on the number of schools. Surprisingly, the signs of point estimates are negative for all types of public schools. The sum of point estimates of columns 1 to 3 matches column 4. The coefficient in column 4 is weakly statistically significant. However, The point estimates mean that a decline in funding improves the access to public schools, indicating that a decrease in access to public schools cannot be an alternative mechanism. In particular, the results in column 2 are consistent with Chakrabarti and Roy (2016)'s findings that charter schools have little impact on private school enrollment. In column 5, I also include the number of private schools from the Private School Universe Survey (PSUS).[46] Number of private schools seems to decline with the public school funding cut, however, the point estimate is not statistically significant.

D.5 Selective Migration

People strategically migrate due to their preference for education (Barrow, 2002). When observing or expecting budget cuts for education, families with a high preference for quality public schools may relocate to greater spending school districts. Assuming pre-existing students in these districts tend to stay in public schools, this migration pattern would increase the public school enrollment rate (and reduce the private school enrollment rate) in high spending areas and overestimate my results. If selective migration is prevalent, this means

[46] This survey is a biennial survey. Thus, the years included in Panel A of Column 5 are the odd years between 2005 and 2015.

funding cuts for public education rather stimulate competition within the public education system than between public and private systems, which is a serious challenge to my results. Thus in this subsection, I examine whether issues of selective migration confound my results.

Critically, my specification is robust to migration because my specification utilizes the state-level variation and between-state migration is rare. After the Recession, only 1.6 percent of households relocated between states. Nevertheless, I further examine whether my results are robust to migration using the migration history within a year available in the ACS. The ACS asks where each respondent lived one year earlier and identifies the Migration PUMA (MPUMA) she lived in if she did not live in the same residence. This information allows me to determine each respondent's migration status and where she moved from if migrated, and the amount of in- and out-migration in a given MPUMA.[47] The 2000 Census is excluded in this analysis for consistency because it identifies migration status from five years earlier. Reassuringly, it seems that there is little relation between education appropriations and the total number of school-aged children or in- and out-migration at the state level. (Appendix Table A.13).

This is not sufficient to rule out the possibility of selective migration because families often move across school districts within a state. If migration for education is a prevalent reason for relocation, then migrants' response to the funding cuts for public schools would be different than non-migrants. In Appendix Table A.14, I test whether the impact of public school funding varies by migration status and show it does not. I first split the households by migration status in columns 1 to 3.[48] From columns 1 to 3, I divide the sample into those who have migrated across MPUMAs (column 1), stayed in the same MPUMA (column 2), lived in the same house (column 3, a subset of column 2) compared to 12 months ago. In column 1, the point estimate shows that a $1,000 increase of K-12 revenue per pupil decreases private school enrollment of migrated households by 0.315 percentage points, about half of the main result. However, it is not statistically different from the main result. The smaller point estimate may be coming from selective migration to attend public schools; however, the result in Appendix Table A.13 does not support this hypothesis. Rather, migrants are disproportionately low SES and are note sensitive to public school funding, which contradicts the selective migration hypothesis. The point estimate in column 2 is similar to the baseline estimate of -0.687, and it gets larger

[47] The MPUMA here is different from either PUMA or CPUMA; it aggregates the regular PUMAs to resemble the commuting zone and is specifically used to collect workplace or migration information. MPUMA boundary changed in 2012 as well, so I use state-level appropriations to examine selective migration.

[48] For reference, the point estimate (SE) is -0.687 (0.230) when using the year 2005-2016.

in those who did not move. In column 4, I estimate the impact on children whose household head has lived in the same house for five or more years.[49] The coefficient increases to -0.836. Although it is not statistically different, the larger point estimate is interesting. These households are more attached to the house and neighborhood and consist of parents who are on average older and more likely to be homeowners (and therefore have higher SES). As further discussed in Section VI, higher SES families are more likely to respond to the shock by fleeing to private schools than the average population, consistent with column 4. Finally, in column 5, I use the K-12 funding per pupil in the state of birth, excluding foreign-born children.[50] Using birth state instead of current resident CPUMA would be more robust to selective migration because it is determined before the educational choice. The point estimate in column 5 is smaller than the main estimate by 0.07 percentage points, but not statistically different.

E Additional Heterogeneity Analysis

E.1 Racial Difference in Heterogeneity in Effect by CPUMA Characteristics

This subsection complements Table 7 by showing the heterogeneity by CPUMA characteristics and races. In Appendix Table A.15, I redo the analysis in Table 7 separately by race. Panel A, B, C, and D present the results for all races, whites, Hispanics, and blacks, respectively.

Table 7 shows a larger impact in low SES areas for both high and low-income households. Panel A of Appendix Table A.15 shows this is indeed true without dividing the sample by household income, although the coefficients are not statistically different in columns 5 and 6, like in Table 7. The overall results for whites in Panel B are not different from those in Panel A, but the differences are larger. Interestingly, in columns 3 and 4, the difference between high and low baseline minority population share is substantial. While a $1,000 decline in education revenue per pupil leads to -0.332 percentage points increase in private school enrollment in CPUMAs with a low minority population, the point estimate is -1.427 percentage points in CPUMAs with a high minority population, which is much larger than the average impact in Panel A. The two coefficients are statistically different from each other at the 1 percent level. The stunningly large point estimate for high minority CPUMAs shows that whites respond differentially to the budget shock depending on the composition of the population. In other words, education budget shock exacerbates the white flight from public schools. White stu-

[49]The ACS asks when the household head had moved into the residence. This information is only available for the head, so I assume the children in the households also have stayed five or more years when the head has.

[50]From 2000-2007, 82 percent of native-born children stayed in the birth state.

dents have a stronger preference for private schools when they attend schools with a larger concentration of nonwhite schoolchildren (Brunner, Imazeki and Ross, 2010), and therefore they switch to private schools more easily when the quality of the public schools declines.

The patterns are slightly different for other races. The overall pattern—stronger in low SES areas—is found for Hispanics as well. While the regional poverty rate does not play an important role, the share of minority or immigrant population substantially affects the response to the funding shock. In fact, the effect on Hispanic students is mostly coming from high minority or foreign population areas. Research by Fairlie (2002) supports "Latino flight" to private schools to avoid black students, and the finding here hints at the possibility of Hispanics avoiding immigrants, although this is not proven in the literature. In particular, the point estimate is larger for native-born Hispanics living in high foreign population CPUMA (available upon request), supporting the hypothesis. Results for blacks in Panel D are mostly small and insignificant, consistent with Table 6.

E.2 Heterogeneity in Effect by Parental Characteristics

Studies like Barrow (2002) and Goldring and Phillips (2008) suggest the importance of parental characteristics in school choice. In Table A.16, I compare the impact of education funding by four parental characteristics: the presence of both parents and whether at least one parent has a Bachelor's degree, high-paying occupation (using median occupational income in 2000), and is immigrant. I exclude students with no parents in this analysis. The results show that high SES students in terms of parental characteristics are more responsive to funding cuts; however, the point estimates are statistically different only for parents' occupation. Interestingly, parental characteristics do not affect the competition between public and private schools as much as regional characteristics in Panel A of Appendix Table A.15.

F Alternative Dataset: Public School Universe Survey

In this subsection, I use an alternative dataset called the Private School Universe Survey (PSUS) to examine the robustness of my result. PSUS is a biennial survey collected by NCES, targeting all private schools in the US with useful school-level characteristics.[51] By using PSUS, I can provide robustness using a school-level sample and examine the heterogeneity in effect by the religious affiliation of the private school. Religious affiliation is an important private school

[51] All private schools are in the universe; however, the actual number interviewed depends on the response rate which is on average over 90%.

characteristic when parents consider sending their children to private schools (Goldring and Phillips, 2008). Also, religious affiliation is correlated with the average tuition, which is a very important factor when choosing private schools. The tuition for Catholic schools is particularly cheaper, where the average yearly tuition in SY 2011-2012 for catholic, other religious, and nonsectarian schools are \$7,210, \$9,100, and \$22,570, respectively (Snyder, de Brey and Dillow, 2019). Considering the stark difference in the tuition, it would be interesting to find which type of schools are the most elastic to the change in local public school funding, especially whether relatively low-cost schools are more sensitive, considering the massive economic shock caused by the Great Recession.

To estimate whether the effect varies by religion, I estimate the following equations:

$$N_{ipst} = \beta App_{pst} + P_{ps} \times t + \theta_t + \alpha_i + \varepsilon_{ipst}, \tag{3}$$

$$\begin{aligned} N_{ipst} = &\beta_1 App_{pst} \times \mathbb{1}[Catholic]_i + \\ &+ \beta_2 App_{pst} \times \mathbb{1}[Other\ Religious]_i + \\ &+ \beta_3 App_{pst} \times \mathbb{1}[Nonsectarian]_i + P_{ps} \times t + \theta_t + \alpha_i + \varepsilon_{ipst}, \end{aligned} \tag{4}$$

where N_{ipst} is the number of students enrolled in school i in CPUMA p of state s in school year t. Like in the main text, App_{pst} is total K-12 appropriations per pupil in CPUMA p where the school locates.[52] I include year fixed effect (θ_t) and school fixed effect (α_i) to control for macroeconomic conditions and time-invariant school characteristics, respectively. Time-invariant school characteristics include the religious type of the school as well.In the analysis, I include private schools with 50 or more enrollees and that serve beyond kindergarten.

I first start with comparing the estimates in ACS and PSUS. In column 1 of Appendix Table A.17, I aggregate the number of students in private school to the CPUMA level and estimate the impact of per-pupil appropriations with my ACS sample, using only odd years to match PSUS. I do a similar procedure with PSUS in column 2. The point estimate is about 72 percent of column 1, although not statistically significant. Columns 3 to 7 estimate the school-level effect. A \$1,000 decrease in local public school funding per pupil increases private school enrollment by 6.2 students. In column 5, the heterogeneity by religious affiliation is interesting; the treatment effect is concentrated on Catholic private schools, the cheapest ones. Of course, this can be interpreted as households with Catholic faith are more sensitive to educa-

[52]The PSUS provides geocoordinates of most of the schools, so I match it to the CPUMA in the Census and ACS.

tion funding, but the lower expense can be an alternative explanation. This result somewhat resolves the question that private school enrollment could have relatively increased right after the Great Recession despite economic shock; students tend to enroll in relatively cheap Catholic schools. In columns 6 and 7, I test whether this holds for white and Hispanic students. The patterns are the same for both races; the change is concentrated on Catholic schools.

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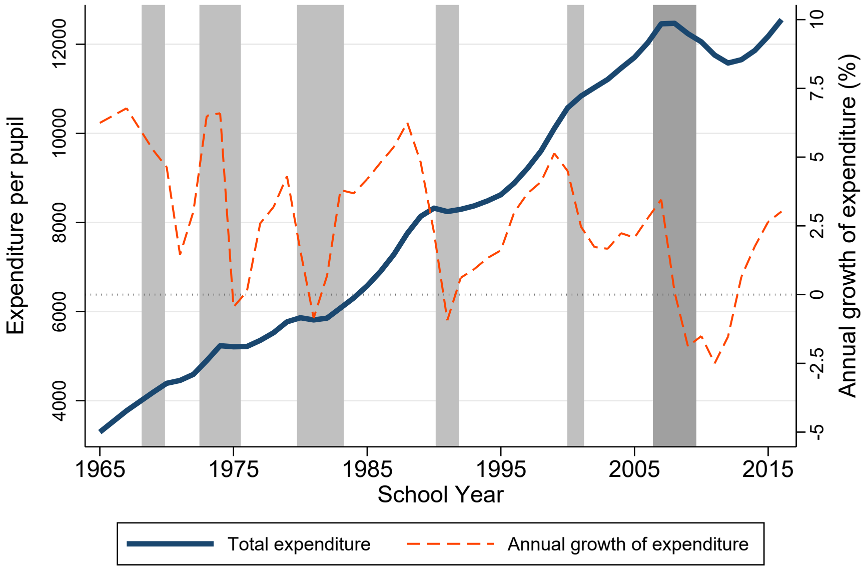
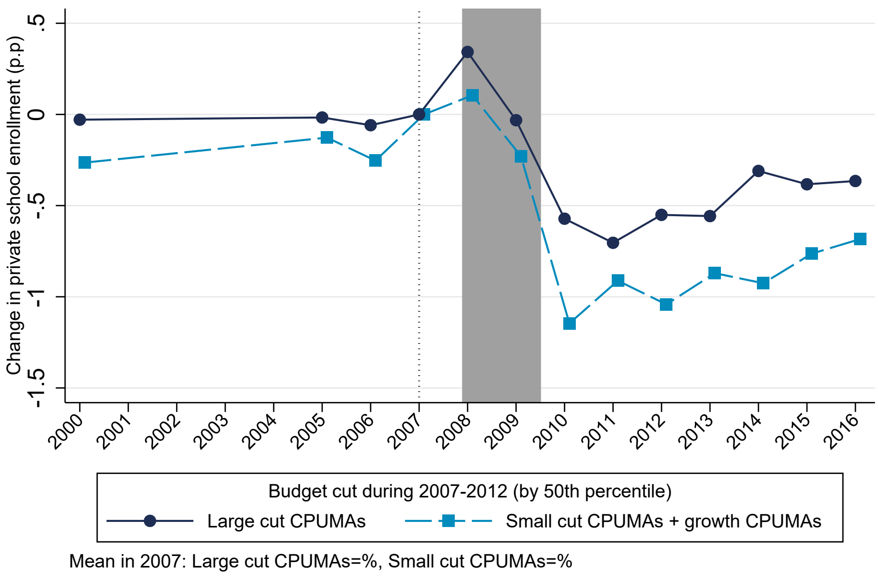
Appendix Figures

Figure A.1: Trend of Total K-12 Expenditure Per Pupil and Growth Rate



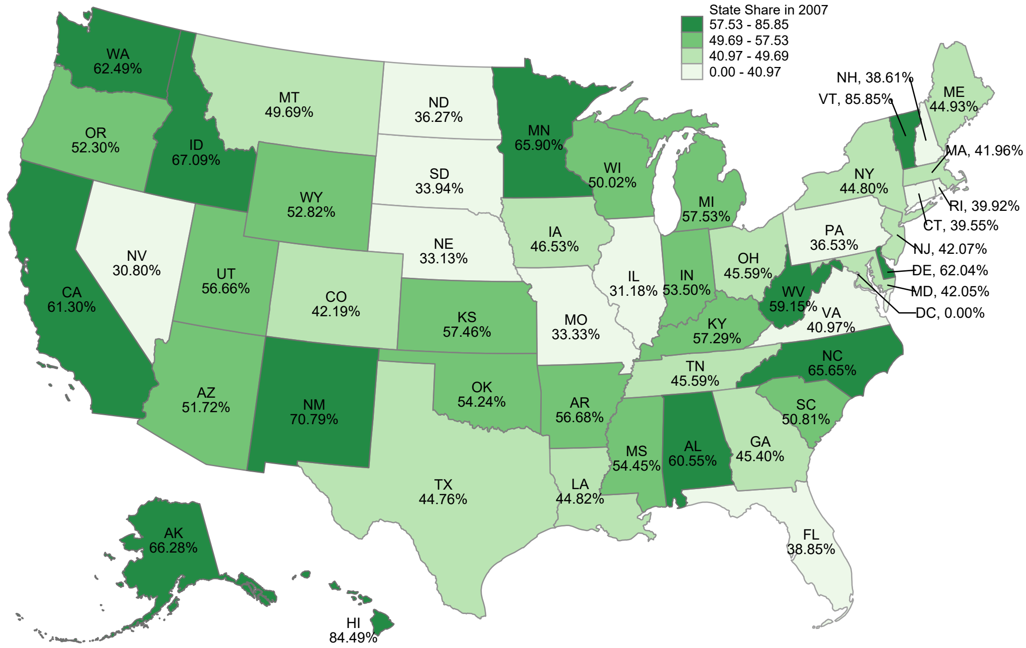
Notes: This figure plots the trend of expenditure per pupil. All other details are the same in Figure 1.

Figure A.2: Trend in Private School Enrollment by Budget Change in CPUMA



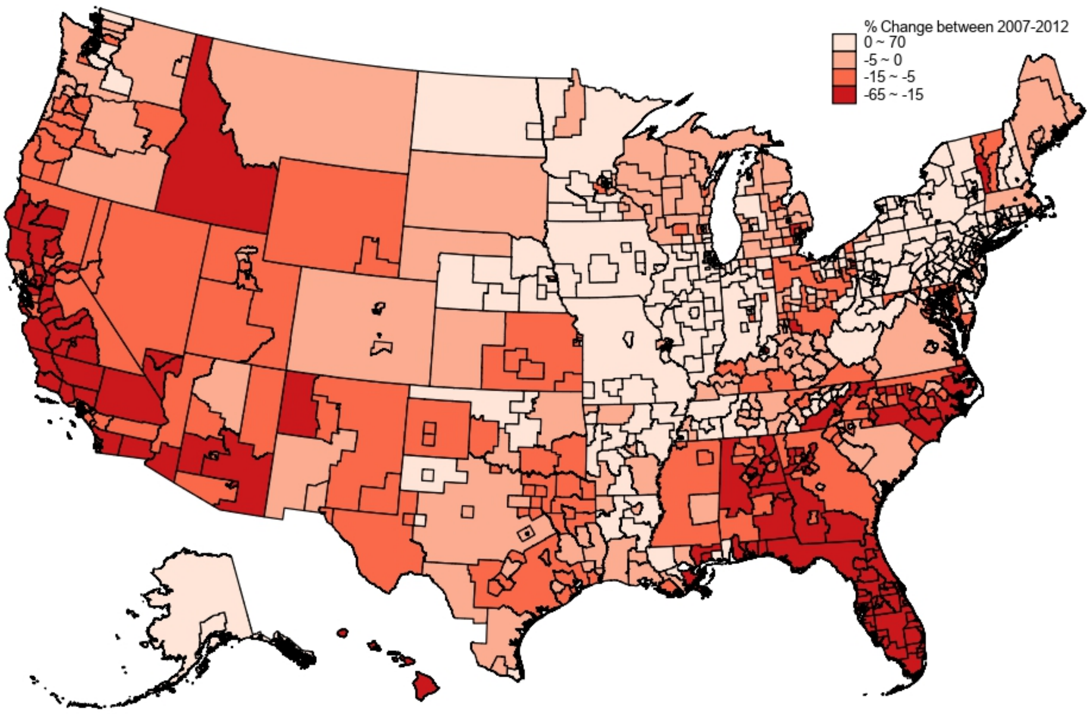
Notes: This figure shows the trend in private school enrollment by the large and small budget cuts in CPUMA. All other details are the same as Figure 3.

Figure A.3: State Share by State



Notes: This map shows the state share in 2007$\left(= \frac{State\ App_{s,2007}}{Total\ App_{s,2007}}\right)$.

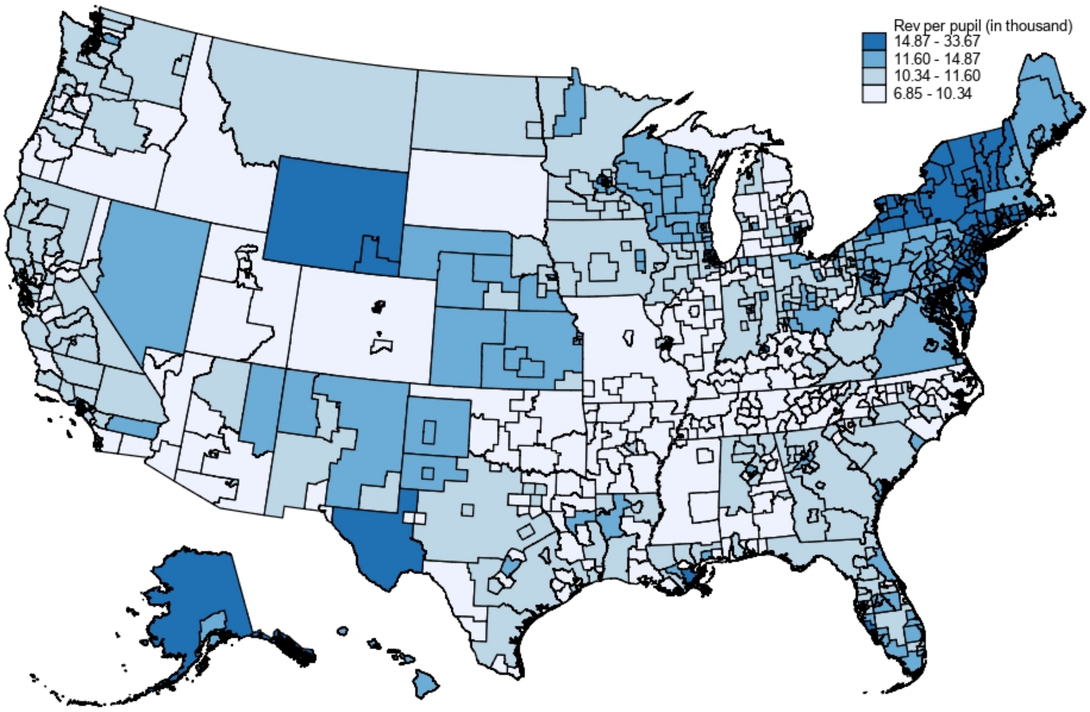
Figure A.4: Change in K-12 Appropriations Per Pupil in CPUMAs, 2007-2012



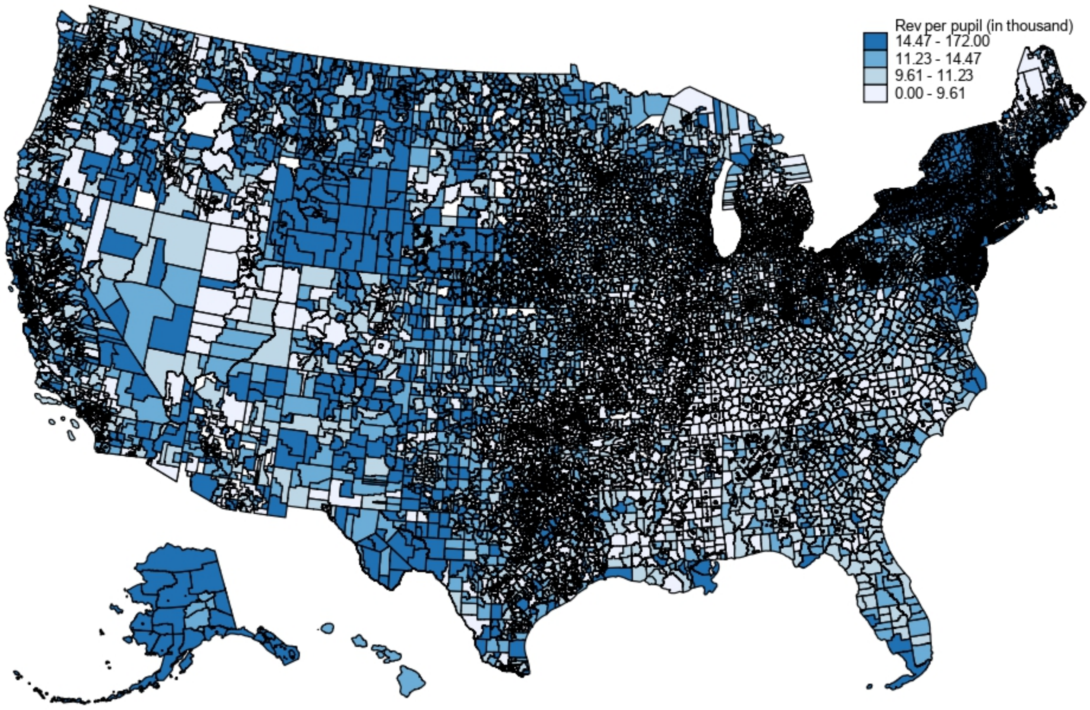
Notes: This map displays the change of education appropriations per pupil from 2007 to 2012 in CPUMAs. Darker shades represent a larger decline.

Figure A.5: Appropriations per pupil in CPUMAs and school districs in SY 2007-2008

(a) App per pupil in PUMA



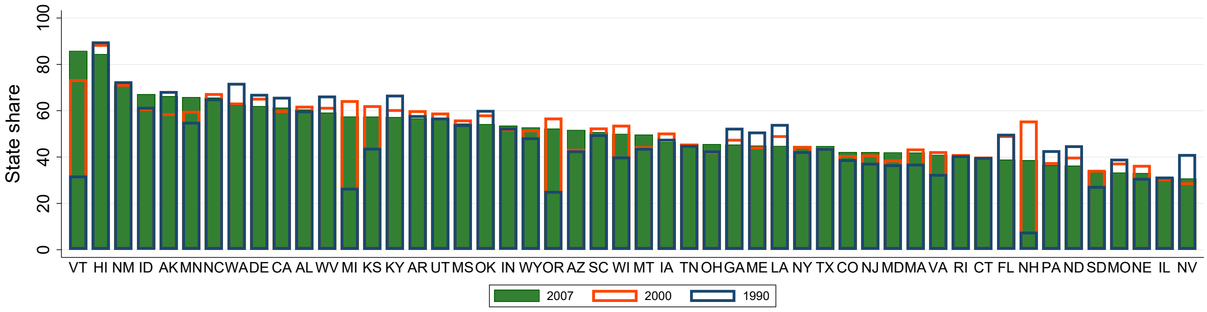
(b) App per pupil in school district



Notes: This figure shows the K-12 appropriations per pupil in CPUMA (Panel A) and school district (Panel B) in SY 2007-2008.

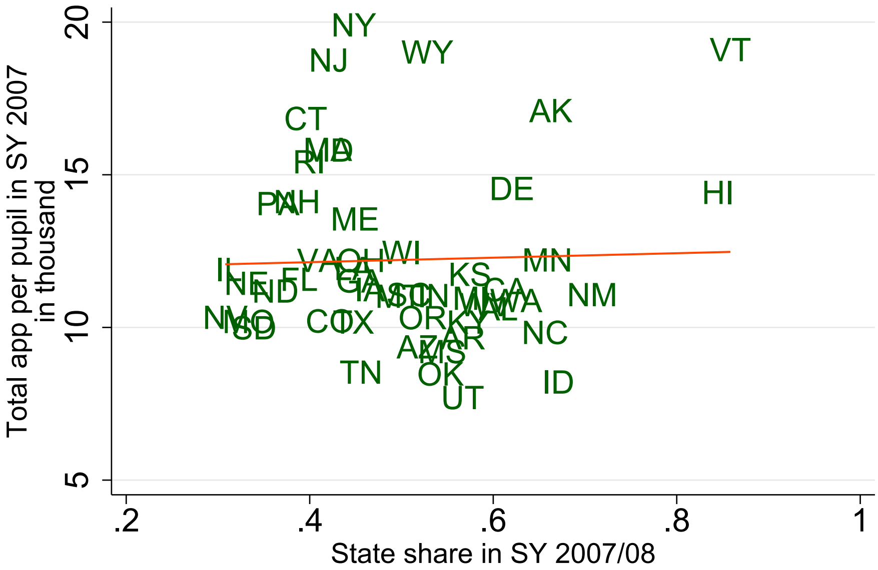
Figure A.6: Share of State Appropriations and Relation to Total Funding

(a) Variation in Share



Correlation between 2007&2000: 0.930, 2007&1990: 0.638

(b) Relation to Total Revenue in 2007

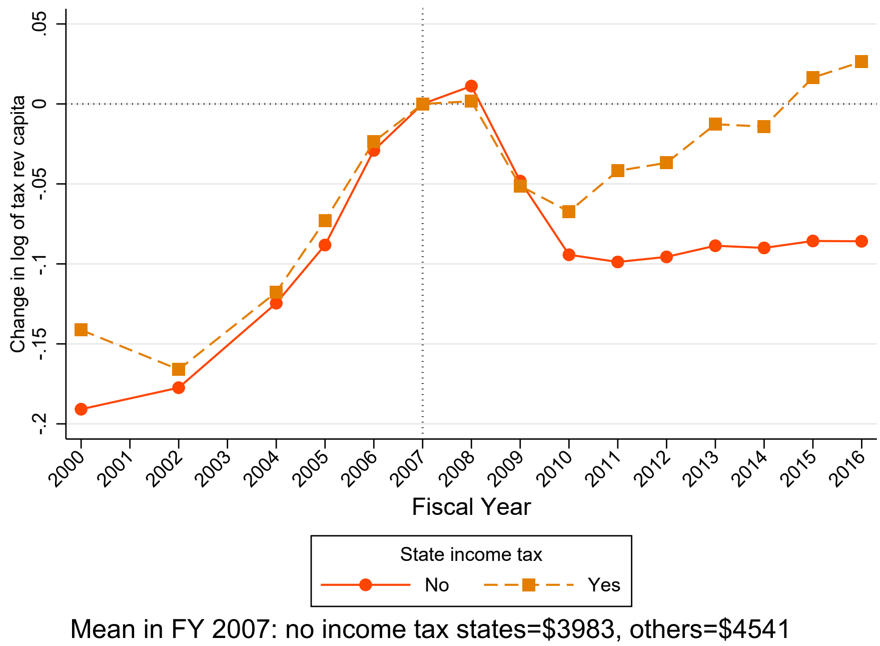


b = 733.87, se = 3417.86

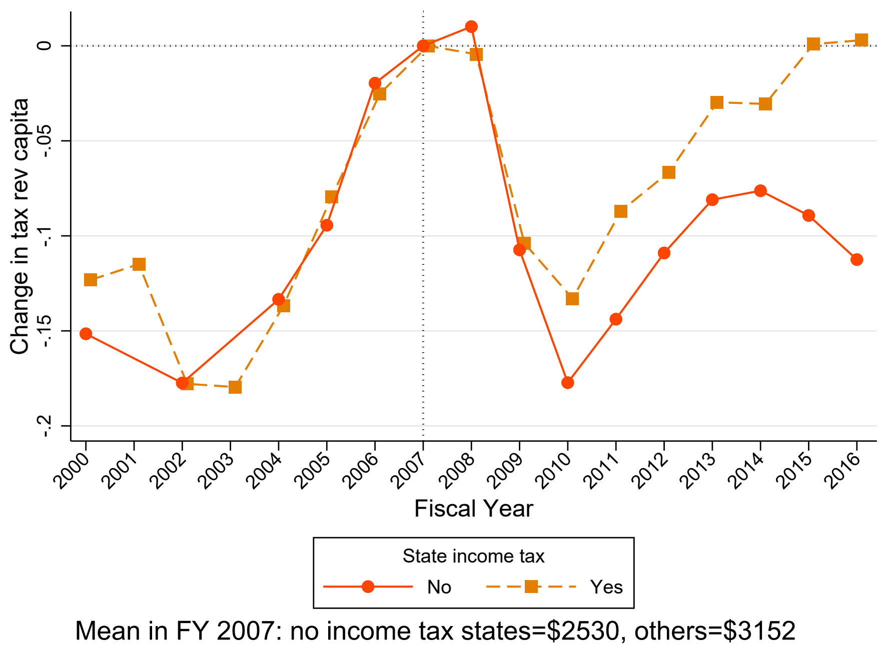
Notes: Panel A displays the variation in state share in SY 2007 (green), 2000 (orange), and 1990 (navy). The correlation of the shares between 2000 and 2007 is very high—over 0.9. The correlation is weaker between 2006 and 1990, 0.64, but it goes up to 0.75 when comparing ranks. Panel B shows the relation between 2007 state share and total K-12 appropriations per pupil before the Great Recession. Coefficients and standard errors of the linear fitted values are presented below each figure. All monetary values are in 2010 dollars.

Figure A.7: Trend of Real Tax Revenue and K-12 Funding Compared to 2007, by State Income Tax Status

(a) Real Tax Revenue (in 2010 dollars)



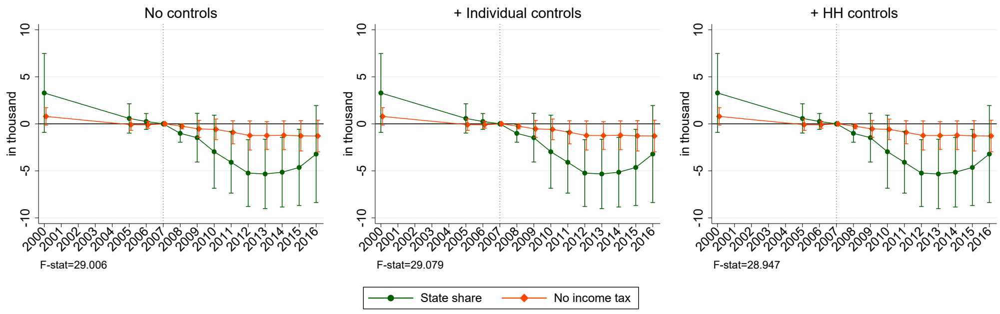
(b) Real K-12 Funding (in 2010 dollars)



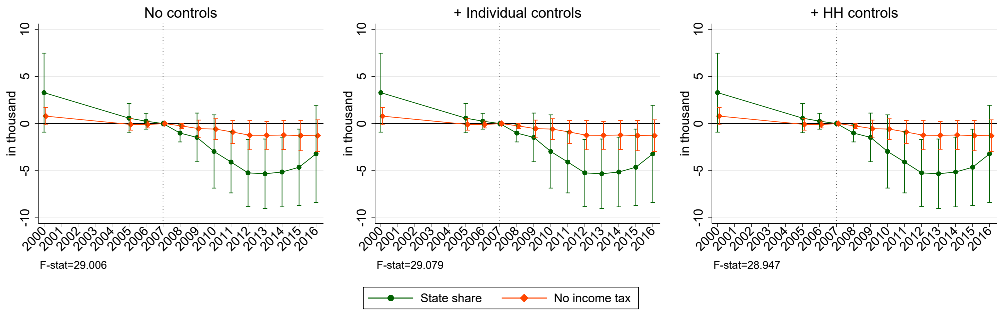
Notes: Panel A shows the trend of the mean tax revenue per capita relative to FY 2007 in two groups of states (states with and without an individual income tax). Panel B shows the trend of the mean tax revenue excluding property tax in the two groups of states. The mean of each group is the weighted mean with state population in 2000.

Figure A.8: First Stage and Reduced Form Results Using State Level Appropriations

(a) First Stage



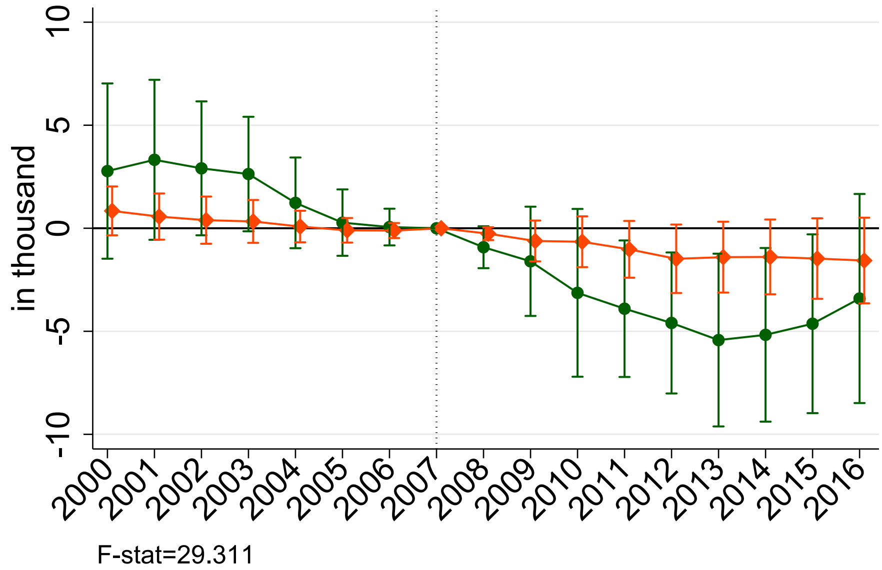
(b) Reduced Form



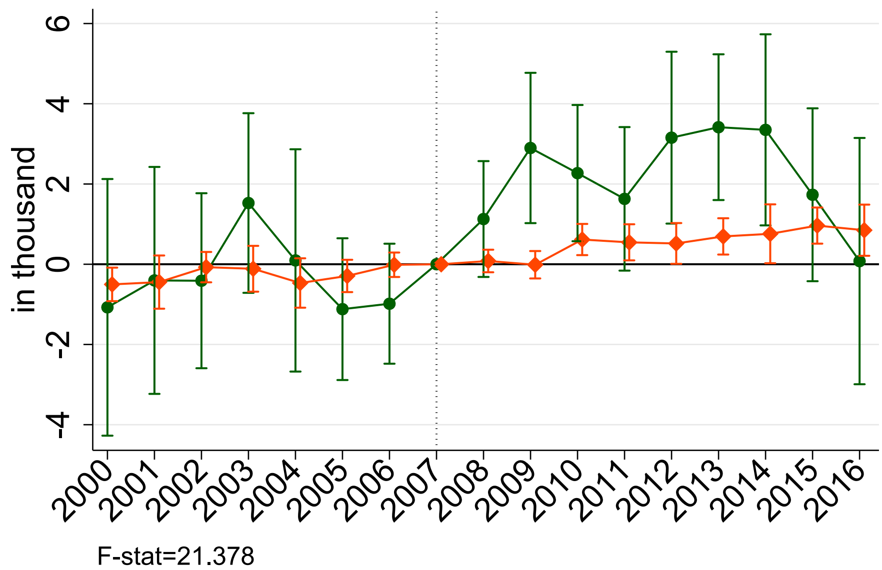
Notes: These figures show the coefficients of interaction terms of year dummies and state share, and income tax status along with 95% confidence intervals. Panels A and B show the first stage and reduced form results with the preferred specification (Column 4 in Table A.7). The dependent variable in Panel A is state-level appropriations for K-12 education in thousand USD. In Panel B, the dependent variable is an indicator for private school enrollment, and I adjust the coefficients to represent percentage points. See the notes of Table A.7 for further information.

Figure A.9: Frist Stage and Reduced Form Using State-Level Appropriations

(a) First Stage: Revenue Per Pupil

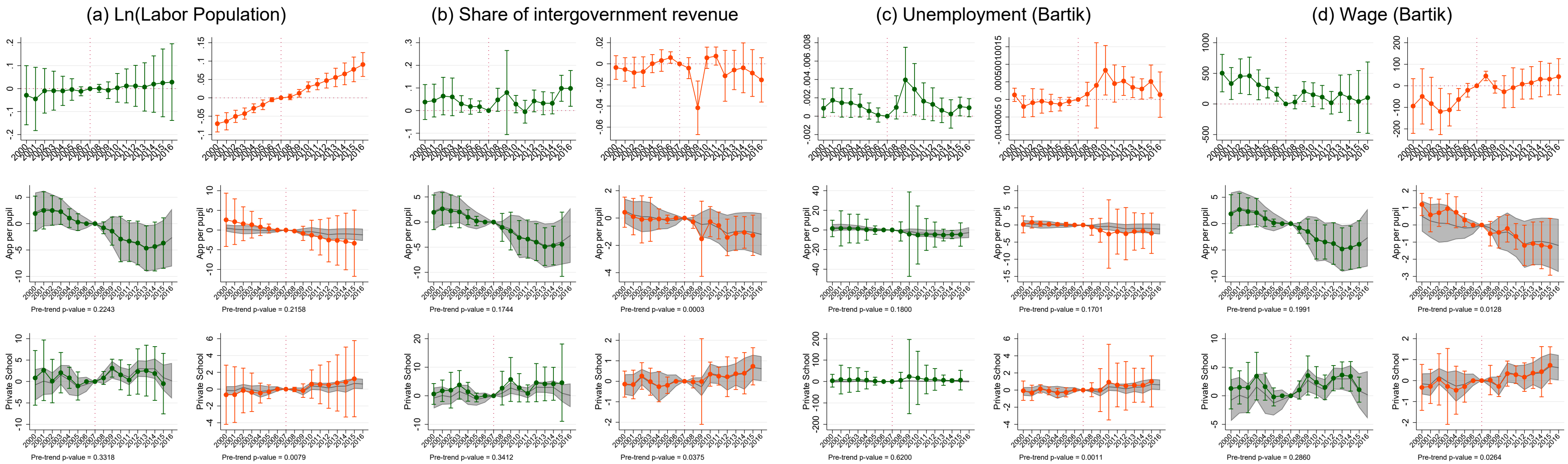


(b) Reduced form: Private School Enrollment



Notes: This figure shows the first stage (Panel A) and reduced form (Panel B) using state-level appropriations per pupil. Each figure displays the coefficients for the interaction terms of state share (green dots) and no income tax indicator (orange diamonds) with the year dummies along with 95% confidence intervals calculated using standard errors clustered by the state level. I follow the preferred specification (column 4 of Table 3, using state characteristics in 2000 interacted with the year trends. See the notes of Table 3 for the details for the control variables. Regressions are weighted using the Census and ACS sample weights. Standard errors are clustered at the state level. F-statistic of the event study variables is presented below each figure.

Figure A.10: FHS Estimator Adjusting Pre-Trends



Notes: I follow Freyaldenhoven, Hansen and Shapiro (2019)'s methodology to adjust pre-existing trends in the event study model with panel data. To exactly follow their model, I aggregate my sample to the state-year panel. The green and orange indicate event study estimates of state share and no-income-tax indicator, respectively. The first row of each panel shows event study figures using state share or no-income-tax as the treatment (policy) variables and the covariate indicated at the panel title as the dependent variable. The second and third rows show the first stage (dependent variable: appropriations per pupil) and reduced-form (dependent variable: percent in private school) using the first lead of event study variable as the instrument for the covariate, respectively, together with the original first stage and reduced form results in the gray shade. *p*-values of the joint hypothesis test of the pre-event coefficients are presented below of each figure. The year 2006, 2007, and 2016 are excluded in FHS's specification. Standard errors are clustered at the state level. All regressions are weighted with the school-aged population of each state in 2000. See Appendix Table A.3 for the exact estimation results.

Appendix Tables

Table A.1: Tax Revenue in State and Local Governments in the Fiscal Year 2007

	Local Government				State Government			
	Total Tax Rev	Income Tax	Sales Tax	Property Tax	Total Tax Rev	Income Tax	Sales Tax	Property Tax
Alabama	$4,642	3%	37%	39%	$8,868	34%	26%	3%
Alaska	$1,256	0%	14%	77%	$3,688	0%	0%	2%
Arizona	$8,925	0%	31%	59%	$14,405	26%	46%	6%
Arkansas	$1,769	0%	49%	40%	$7,392	29%	39%	9%
California	$65,133	0%	14%	71%	$114,737	46%	28%	2%
Colorado	$9,382	0%	31%	60%	$9,217	52%	24%	0%
Connecticut	$8,291	0%	0%	98%	$13,272	48%	23%	0%
Delaware	$749	6%	0%	76%	$2,906	35%	0%	0%
Florida	$34,192	0%	4%	78%	$38,819	0%	59%	0%
Georgia	$14,837	0%	27%	64%	$18,253	48%	32%	0%
Hawaii	$1,470	0%	0%	77%	$5,090	31%	50%	0%
Idaho	$1,199	0%	0%	91%	$3,537	40%	36%	0%
Illinois	$25,006	0%	5%	82%	$30,066	31%	26%	0%
Indiana	$7,606	14%	0%	82%	$14,199	33%	38%	0%
Iowa	$4,442	2%	12%	81%	$6,470	41%	28%	0%
Kansas	$4,460	0%	17%	76%	$6,893	40%	33%	1%
Kentucky	$3,797	26%	0%	55%	$9,895	31%	28%	5%
Louisiana	$6,622	0%	54%	39%	$10,973	29%	32%	0%
Maine	$2,052	0%	0%	99%	$3,696	40%	29%	1%
Maryland	$10,925	37%	0%	48%	$15,094	44%	23%	4%
Massachusetts	$11,424	0%	0%	97%	$20,695	55%	20%	0%
Michigan	$13,247	4%	0%	92%	$23,849	27%	33%	10%
Minnesota	$5,894	0%	1%	92%	$17,768	41%	25%	4%
Mississippi	$2,329	0%	0%	92%	$6,482	22%	49%	1%
Missouri	$8,411	3%	20%	61%	$10,706	45%	31%	0%
Montana	$942	0%	0%	95%	$2,320	36%	0%	9%
Nebraska	$3,107	0%	9%	77%	$4,122	40%	36%	0%
Nevada	$4,141	0%	8%	65%	$6,305	0%	51%	3%
New Hampshire	$2,567	0%	0%	98%	$2,175	5%	0%	18%
New Jersey	$21,937	0%	0%	98%	$29,488	40%	29%	0%
New Mexico	$1,922	0%	40%	49%	$5,527	21%	35%	1%
New York	$70,862	11%	16%	54%	$63,162	55%	17%	0%
North Carolina	$10,647	0%	26%	69%	$22,613	47%	23%	0%
North Dakota	$810	0%	11%	85%	$1,783	18%	27%	0%
Ohio	$19,937	20%	8%	67%	$25,698	38%	30%	0%
Oklahoma	$3,678	0%	39%	53%	$8,141	34%	24%	0%
Oregon	$4,991	0%	0%	79%	$7,743	72%	0%	0%
Pennsylvania	$21,255	18%	1%	70%	$30,838	32%	28%	0%
Rhode Island	$2,021	0%	0%	97%	$2,766	39%	32%	0%
South Carolina	$5,199	0%	3%	82%	$8,689	37%	37%	0%
South Dakota	$1,129	0%	23%	73%	$1,266	0%	56%	0%
Tennessee	$7,297	0%	27%	62%	$11,390	2%	59%	0%
Texas	$41,676	0%	12%	82%	$40,315	0%	51%	0%
Utah	$3,016	0%	20%	68%	$6,076	42%	32%	0%
Vermont	$374	0%	1%	94%	$2,564	23%	13%	35%
Virginia	$13,705	0%	8%	73%	$18,667	55%	19%	0%
Washington	$9,830	0%	23%	58%	$17,706	0%	61%	10%
West Virginia	$1,437	0%	0%	79%	$4,642	29%	24%	0%
Wisconsin	$8,839	0%	3%	94%	$14,483	44%	29%	1%
Wyoming	$1,222	0%	18%	76%	$2,025	0%	34%	13%
US Total	$525,792	5%	12%	72%	$757,470,540	35%	31%	2%

Notes: All monetary values are presented in thousands of nominal dollars. Data source: US Census Bureau's Census of Governments and the Annual Survey of State and Local Government Finances retrieve through State and Local Finance Initiative from Urban Institute (US Census Bureau, 2020). Income and sales taxes include individual income tax and general sales tax only, respectively.

Table A.2: First Stage Results

Dependent variable: App per pupil (in thousand)

	State share	No income tax	State share	No income tax	State share	No income tax	State share	No income tax
	(1)		(2)		(3)		(4)	
Instrument × 2000	3.283	0.790*	3.283	0.790*	3.284	0.791*	3.733*	0.890*
	(2.139)	(0.472)	(2.139)	(0.472)	(2.139)	(0.472)	(2.259)	(0.488)
Instrument × 2005	0.575	-0.0917	0.575	-0.0918	0.575	-0.0914	0.709	-0.0596
	(0.795)	(0.309)	(0.795)	(0.309)	(0.795)	(0.309)	(0.787)	(0.303)
Instrument × 2006	0.252	-0.0985	0.252	-0.0985	0.252	-0.0983	0.316	-0.0829
	(0.431)	(0.186)	(0.431)	(0.187)	(0.431)	(0.187)	(0.417)	(0.183)
Instrument × 2007	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Instrument × 2008	-1.008**	-0.256*	-1.008**	-0.256*	-1.008**	-0.256*	-1.074**	-0.271*
	(0.482)	(0.138)	(0.482)	(0.138)	(0.481)	(0.138)	(0.516)	(0.146)
Instrument × 2009	-1.472	-0.537	-1.472	-0.537	-1.472	-0.537	-1.602	-0.566
	(1.320)	(0.463)	(1.320)	(0.463)	(1.319)	(0.462)	(1.389)	(0.478)
Instrument × 2010	-2.964	-0.587	-2.965	-0.587	-2.963	-0.587	-3.159	-0.632
	(1.982)	(0.559)	(1.982)	(0.559)	(1.982)	(0.559)	(2.085)	(0.584)
Instrument × 2011	-4.084**	-0.900	-4.084**	-0.900	-4.084**	-0.900	-4.345**	-0.960
	(1.673)	(0.624)	(1.673)	(0.624)	(1.673)	(0.624)	(1.803)	(0.658)
Instrument × 2012	-5.235***	-1.238	-5.235***	-1.238	-5.235***	-1.238	-5.558***	-1.313
	(1.813)	(0.788)	(1.814)	(0.788)	(1.813)	(0.788)	(1.970)	(0.829)
Instrument × 2013	-5.328***	-1.246*	-5.329***	-1.246*	-5.329***	-1.246*	-5.716***	-1.337*
	(1.882)	(0.756)	(1.882)	(0.756)	(1.882)	(0.756)	(2.058)	(0.805)
Instrument × 2014	-5.142***	-1.217	-5.142***	-1.216	-5.142***	-1.216	-5.591***	-1.323
	(1.888)	(0.778)	(1.888)	(0.778)	(1.888)	(0.778)	(2.057)	(0.836)
Instrument × 2015	-4.637**	-1.277	-4.637**	-1.277	-4.637**	-1.276	-5.147**	-1.397
	(2.065)	(0.822)	(2.065)	(0.822)	(2.065)	(0.822)	(2.158)	(0.886)
Instrument × 2016	-3.211	-1.290	-3.211	-1.290	-3.211	-1.289	-3.785	-1.426
	(2.630)	(0.859)	(2.630)	(0.859)	(2.630)	(0.859)	(2.611)	(0.930)
F-Stat of excluded IVs	29.01		29.08		28.95		28.4	
CPUMA Fixed Effects	Yes		Yes		Yes		Yes	
Individual Controls			Yes		Yes		Yes	
Household Controls					Yes		Yes	
Baseline CPUMA Controls×Time Trend							Yes	

Notes: N=8,095,073. The identifying variation is indicated at the top of each column. The number on the top of the column indicates the specification as in Table 3. Two columns with the same number are from a single regression. The coefficients are rescaled to represent private school enrollment in percentage points. I use 2007 as the base year and thus omit it. I exclude 2001-2004 in the sample because the CPUMA is not identifiable in the ACS 2001-2004. All regressions include year and CPUMA fixed effects. All regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significant at 10%; ** significance at 5%; *** significance at 1%.

Table A.3: Pre-trends and FHS Estimators

	Main Results					
	State Share			No Income Tax		
	First Stage	Reduced Form	Ratio	First Stage	Reduced Form	Ratio
	(1)	(2)	(3)	(4)	(5)	(6)
year=2000	2.144	-0.777	-0.362	0.513	-0.159	-0.3099
	(1.860)	(1.821)		(0.444)	(0.247)	
year=2001	2.863	0.0981	0.034	0.235	-0.189	-0.8043
	(1.679)	(1.584)		(0.487)	(0.269)	
year=2002	2.57	-0.431	-0.168	0.110	0.207	1.88182
	(1.478)	(1.269)		(0.567)	(0.194)	
year=2003	2.342	1.551	0.662	0.0873	-0.0659	-0.7549
	(1.256)	(1.146)		(0.533)	(0.241)	
year=2004	1.123	0.313	0.279	-0.0694	-0.261	3.76081
	(1.076)	(1.726)		(0.382)	(0.370)	
year=2005	0.303	-1.264	-4.172	-0.176	-0.175	0.99432
	(0.864)	(1.083)		(0.310)	(0.247)	
year=2006	0.0933	-0.652	-6.988	-0.149	0.0366	-0.2456
	(0.483)	(0.864)		(0.193)	(0.146)	
year=2007	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2008	-0.805	0.887	-1.102	-0.175	-0.0572	0.32686
	(0.509)	(0.744)		(0.124)	(0.119)	
year=2009	-1.366	2.723	-1.993	-0.474	-0.268	0.5654
	(1.398)	(1.015)		(0.459)	(0.228)	
year=2010	-2.952	1.749	-0.592	-0.409	0.383	-0.9364
	(2.093)	(0.800)		(0.564)	(0.236)	
year=2011	-3.419	1.020	-0.298	-0.728	0.296	-0.4066
	(1.789)	(0.932)		(0.570)	(0.213)	
year=2012	-3.768	3.008	-0.798	-1.133	0.151	-0.1333
	(2.000)	(0.946)		(0.680)	(0.300)	
year=2013	-4.711	2.948	-0.626	-0.978	0.333	-0.3405
	(2.189)	(0.938)		(0.674)	(0.271)	
year=2014	-4.5	2.985	-0.663	-0.96	0.369	-0.3844
	(2.142)	(1.176)		(0.687)	(0.386)	
year=2015	-3.89	0.910	-0.234	-1.048	0.677	-0.646
	(2.267)	(1.499)		(0.728)	(0.306)	
year=2016	-2.647	0.148	-0.056	-1.198	0.616	-0.5142
	(2.720)	(2.073)		(0.770)	(0.301)	

	Covariate: Ln(Labor Population)					
	State Share			No Income Tax		
	First Stage	Reduced Form	Ratio	First Stage	Reduced Form	Ratio
	(7)	(8)	(9)	(10)	(11)	(12)
year=2000	1.912	0.843	0.4409	2.599	-0.670	-0.2578
	(1.639)	(3.174)		(3.367)	(1.749)	
year=2001	2.501	2.623	1.04878	2.131	-0.654	-0.3069
	(1.754)	(3.520)		(2.884)	(1.646)	
year=2002	2.493	0.104	0.04172	1.608	-0.160	-0.0995
	(1.388)	(2.524)		(2.135)	(1.321)	
year=2003	2.269	2.060	0.90789	1.361	-0.378	-0.2777
	(1.212)	(2.348)		(1.682)	(1.138)	
year=2004	1.044	0.863	0.82663	0.774	-0.467	-0.6034
	(0.876)	(1.224)		(1.074)	(0.588)	
year=2005	0.271	-1.044	-3.8524	0.386	-0.313	-0.8109
	(0.790)	(1.634)		(0.570)	(0.406)	
year=2006	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2007	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2008	-0.796	0.826	-1.0377	-0.247	-0.0394	0.15951
	(0.557)	(0.749)		(0.126)	(0.131)	
year=2009	-1.422	3.116	-2.1913	-0.841	-0.178	0.21165
	(1.324)	(1.036)		(0.897)	(0.401)	
year=2010	-2.924	1.557	-0.5325	-1.295	0.600	-0.4633
	(2.143)	(1.736)		(1.881)	(0.838)	
year=2011	-3.324	0.352	-0.1059	-1.827	0.565	-0.3093
	(1.929)	(1.779)		(2.237)	(1.145)	
year=2012	-3.672	2.339	-0.637	-2.523	0.492	-0.195
	(2.058)	(2.628)		(2.765)	(1.378)	
year=2013	-4.653	2.541	-0.5461	-2.625	0.737	-0.2808
	(2.100)	(2.950)		(3.122)	(1.662)	
year=2014	-4.341	1.873	-0.4315	-2.896	0.844	-0.2914
	(2.304)	(3.120)		(3.578)	(2.069)	
year=2015	-3.688	-0.500	0.13557	-3.345	1.240	-0.3707
	(2.381)	(3.513)		(4.186)	(2.254)	
year=2016	0	0		0	0	
	(.)	(.)		(.)	(.)	

	Covariate: Share of Intergoverment Revenue					
	State Share			No Income Tax		
	First Stage	Reduced Form	Ratio	First Stage	Reduced Form	Ratio
	(13)	(14)	(15)	(16)	(17)	(18)
year=2000	1.941	0.640	0.32973	0.425	-0.137	-0.3224
	(1.718)	(1.811)		(0.549)	(0.317)	
year=2001	2.625	1.761	0.67086	0.101	-0.156	-1.5446
	(1.618)	(1.876)		(0.663)	(0.348)	
year=2002	2.226	1.968	0.8841	-0.0978	0.258	-2.638
	(1.576)	(3.108)		(0.858)	(0.323)	
year=2003	2.017	3.821	1.8944	-0.0991	-0.0202	0.20383
	(1.575)	(2.495)		(0.801)	(0.328)	
year=2004	0.965	1.416	1.46736	-0.0656	-0.261	3.97866
	(0.740)	(1.639)		(0.368)	(0.368)	
year=2005	0.206	-0.590	-2.8641	-0.0979	-0.194	1.98161
	(0.460)	(0.770)		(0.153)	(0.199)	
year=2006	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2007	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2008	-1.061	2.671	-2.5174	-0.277	-0.0321	0.11588
	(1.382)	(2.932)		(0.237)	(0.146)	
year=2009	-1.794	5.712	-3.1839	-1.519	-0.0116	0.00764
	(1.869)	(3.827)		(1.369)	(1.040)	
year=2010	-3.105	2.818	-0.9076	-0.266	0.348	-1.3083
	(1.690)	(2.466)		(0.549)	(0.292)	
year=2011	-3.392	0.828	-0.2441	-0.544	0.250	-0.4596
	(1.888)	(1.482)		(0.489)	(0.218)	
year=2012	-3.996	4.601	-1.1514	-1.420	0.221	-0.1556
	(1.825)	(3.320)		(0.665)	(0.483)	
year=2013	-4.886	4.166	-0.8526	-1.125	0.369	-0.328
	(1.865)	(2.937)		(0.623)	(0.383)	
year=2014	-4.673	4.193	-0.8973	-1.055	0.392	-0.3716
	(1.909)	(2.866)		(0.576)	(0.502)	
year=2015	-4.418	4.597	-1.0405	-1.267	0.730	-0.5762
	(3.179)	(6.738)		(0.733)	(0.453)	
year=2016	0	0		0	0	
	(.)	(.)		(.)	(.)	

	Covariate: Unemployment Bartik Prediction					
	State Share			No Income Tax		
	First Stage	Reduced Form	Ratio	First Stage	Reduced Form	Ratio
	(19)	(20)	(21)	(22)	(23)	(24)
year=2000	1.506	3.676	2.4409	0.164	-0.0733	-0.447
	(4.169)	(14.83)		(1.227)	(0.562)	
year=2001	1.584	9.024	5.69697	0.778	-0.323	-0.4152
	(8.896)	(33.32)		(0.834)	(0.445)	
year=2002	1.493	7.087	4.74682	0.358	0.146	0.40782
	(7.281)	(28.62)		(0.383)	(0.241)	
year=2003	1.288	8.911	6.91848	0.222	-0.0989	-0.4455
	(7.318)	(27.37)		(0.210)	(0.229)	
year=2004	0.277	6.217	22.444	0.201	-0.327	-1.6269
	(5.331)	(20.35)		(0.224)	(0.266)	
year=2005	-0.113	1.640	-14.513	0.188	-0.264	-1.4043
	(2.115)	(9.087)		(0.294)	(0.267)	
year=2006	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2007	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2008	-1.513	5.829	-3.8526	-0.580	0.0422	-0.0728
	(4.863)	(18.49)		(1.206)	(0.406)	
year=2009	-4.279	23.05	-5.3868	-1.547	-0.00469	0.00303
	(21.39)	(86.17)		(1.741)	(1.248)	
year=2010	-5.119	16.87	-3.2956	-2.639	0.929	-0.352
	(14.70)	(61.86)		(4.967)	(2.198)	
year=2011	-4.628	9.454	-2.0428	-1.940	0.593	-0.3057
	(8.072)	(33.23)		(3.261)	(1.270)	
year=2012	-4.721	9.665	-2.0472	-2.542	0.496	-0.1951
	(6.338)	(25.18)		(3.795)	(1.410)	
year=2013	-5.183	6.243	-1.2045	-1.903	0.560	-0.2943
	(2.989)	(11.93)		(2.715)	(0.990)	
year=2014	-4.680	4.241	-0.9062	-1.761	0.565	-0.3208
	(2.114)	(3.418)		(2.533)	(0.981)	
year=2015	-4.670	6.353	-1.3604	-2.420	1.013	-0.4186
	(5.919)	(22.71)		(2.932)	(1.482)	
year=2016	0	0		0	0	
	(.)	(.)		(.)	(.)	

	Covariate: Wage Bartik Prediction					
	State Share			No Income Tax		
	First Stage	Reduced Form	Ratio	First Stage	Reduced Form	Ratio
	(25)	(26)	(27)	(28)	(29)	(30)
year=2000	1.847	1.293	0.70005	1.196	-0.326	-0.2726
	(1.808)	(1.788)		(0.322)	(0.545)	
year=2001	2.667	1.470	0.55118	0.593	-0.277	-0.4671
	(1.536)	(1.445)		(0.494)	(0.406)	
year=2002	2.302	1.435	0.62337	0.709	0.0602	0.08491
	(1.285)	(2.192)		(0.396)	(0.609)	
year=2003	2.072	3.438	1.65927	0.954	-0.278	-0.2914
	(1.337)	(2.074)		(0.434)	(0.901)	
year=2004	0.939	1.598	1.70181	0.742	-0.459	-0.6186
	(0.728)	(1.182)		(0.450)	(0.570)	
year=2005	0.150	-0.197	-1.3133	0.285	-0.288	-1.0105
	(0.350)	(0.787)		(0.189)	(0.370)	
year=2006	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2007	0	0		0	0	
	(.)	(.)		(.)	(.)	
year=2008	-0.823	1.011	-1.2284	-0.508	0.0244	-0.048
	(0.498)	(0.790)		(0.357)	(0.350)	
year=2009	-1.485	3.557	-2.3953	-0.432	-0.278	0.64352
	(1.173)	(1.702)		(0.455)	(0.241)	
year=2010	-3.041	2.370	-0.7793	-0.209	0.334	-1.5981
	(1.829)	(1.363)		(0.391)	(0.298)	
year=2011	-3.485	1.479	-0.4244	-0.667	0.281	-0.4213
	(1.610)	(1.073)		(0.373)	(0.209)	
year=2012	-3.779	3.088	-0.8171	-1.191	0.165	-0.1385
	(1.962)	(1.097)		(0.586)	(0.339)	
year=2013	-4.810	3.635	-0.7557	-1.081	0.358	-0.3312
	(1.995)	(1.144)		(0.623)	(0.366)	
year=2014	-4.561	3.409	-0.7474	-1.185	0.424	-0.3578
	(2.034)	(1.287)		(0.682)	(0.597)	
year=2015	-3.914	1.076	-0.2749	-1.274	0.732	-0.5746
	(2.222)	(1.094)		(0.825)	(0.438)	
year=2016	0	0		0	0	
	(.)	(.)		(.)	(.)	

Notes: N=850 for all regressions. This table shows the FHS estimation results described in Appendix Figure A.10 together with the original results (columns 1-6) using one treatment variable. See the notes for further information. I also present the ratio of reduced form and the first stage to suggest the size of the 2SLS estimator in my main analysis in this paper. Standard errors are clustered at the state level and in the parentheses. All regressions are weighted with the school-aged population of each state in 2000.

Table A.4: Average of FHS Estimators

	State Share			No Income Tax		
	First Stage (1)	Reduced Form (2)	Ratio (3)	First Stage (4)	Reduced Form (5)	Ratio (6)
Panel A. Main Results						
Average of post period	-4.058	2.683	-0.661	-0.789	0.278	-0.352
Pre-trends p-value	0.153	0.337		<0.01	0.0143	
Panel B. Ln (Labor Population)						
Average of post period	-3.103	1.513	-0.488	-1.950	0.533	-0.273
Pre-trends p-value	0.224	0.332		0.216	<0.01	
Panel C. Share of Intergovernmental Revenue (of Total Revenue)						
Average of post period	-3.416	3.698	-1.083	-0.934	0.283	-0.303
Pre-trends p-value	0.174	0.341		<0.01	0.0375	
Panel D. Unemployment Rate, Bartik Predictions						
Average of post period	-4.349	10.213	-2.348	-1.917	0.524	-0.274
Pre-trends p-value	0.180	0.620		0.170	<0.01	
Panel E. Average Wage, Bartik Predictions						
Average of post period	-3.237	2.453	-0.758	-0.818	0.255	-0.312
Pre-trends p-value	0.199	0.286		0.0128	0.0264	

Notes: This table summarizes the results in Appendix Table A.3. I first take the average of post-event estimates of the first stage and reduce form results and take the ratio in columns 3 and 6. p-values of the joint hypothesis of the pre-event coefficients are also presented in the table.

Table A.5: Complier Characteristics with Exclusive Instruments

	High State Share ×Post=1 No Income Tax×Post=0			High State Share ×Post=0 No Income Tax×Post=1			High State Share ×Post=1 No Income Tax×Post=1		
	$E[X_{1i}]$ (1)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (2)	Relative Likelihood (3)	$E[X_{1i}]$ (4)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (5)	Relative Likelihood (6)	$E[X_{1i}]$ (7)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (8)	Relative Likelihood (9)
Rev per pupil (thousand)	11.95	11.35	0.95	11.95	12.504	1.046	11.95	12.683	1.061
Total school aged children	132,795	50,772	0.382	132,795	48,014	0.362	132,795	46, 221	0.348
Teacher per 100 students	6.176	6.169	0.999	6.176	6.279	1.017	6.176	6.273	1.016
Share of Hispanic students	0.208	0.185	0.89	0.208	0.191	0.916	0.208	0.183	0.879
Share of black studnets	0.144	0.131	0.906	0.144	0.138	0.954	0.144	0.137	0.95
Share of foreign-born	0.059	0.055	0.944	0.059	0.06	1.031	0.059	0.061	1.035
Below 150% of poverty line	0.314	0.329	1.046	0.314	0.309	0.983	0.314	0.307	0.977
Household Income	63,360	63,658	1.005	63,360	65,131	1.028	63,360	65,476	1.033
Median house value	215,233	219,327	1.019	215,233	227,816	1.058	215,233	230,996	1.073
Unemployment rate	0.081	0.091	1.119	0.081	0.081	0.992	0.081	0.08	0.989

Notes: N=13,975. The unit of observation is CPUMA. $E[X_{1i}]$ denotes the population means of CPUMA level characteristics. $E[X_{1i}|D_{1i} > D_{0i}]$ indicates the mean of compliers ($D_{1i} > D_{0i}$). I define the relative likelihood by dividing the complier mean by the population mean. I define three exclusive instrumental variables as indicated at the top of the table. I also redefine the treatment variable to binary indicator whether the total education funding per pupil is below the national median, to follow Angrist and Pischke (2008) and Abadie (2003)'s methodologies.

Table A.6: Main Results in OLS and Logs

Dependent variable: private school enrollment (in percentage point)

	(1)	(2)	(3)	(4)
Panel A. OLS results				
App per pupil	-0.120***	-0.151***	-0.154***	-0.138***
(in thousand)	(0.0378)	(0.0419)	(0.0379)	(0.0352)
Panel B. OLS with log of revenue per pupil				
ln(App per pupil)	-1.277**	-1.713**	-1.796***	-1.846***
	(0.608)	(0.676)	(0.651)	(0.552)
Panel C. 2SLS with log of revnue per pupil				
ln(App per pupil)	-6.423***	-7.074***	-7.622***	-8.989***
	(2.153)	(2.236)	(2.390)	(2.568)
First stage F-Stat	23.72	23.73	23.67	22.87
Individual Controls		Yes	Yes	Yes
Household Controls			Yes	Yes
Baseline CPUMA Controls × Time Trend				Yes

Notes: N=8,095,073. Each entry is a coefficient from a separate OLS or 2SLS regression. The coefficients are rescaled to represent private school enrollment in percentage points. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. All regressions include year and CPUMA fixed effects. See the notes of Table 3 for the descriptions of the control variables. All regressions are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. Panel A shows the OLS result of Table 3. Panels B and C show the result using the log of K-12 revenue per pupil in OLS and 2SLS, respectively. Each entry is a coefficient from a separate regression of the private school enrollment. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.7: Main Results with State-Level Appropriations

Dependent variable: private school enrollment (in percentage point)

	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)
State app per pupil	-0.394**	-0.464**	-0.515**	-0.582***
(in thousand)	(0.186)	(0.187)	(0.194)	(0.188)
First stage F-Stat	14.16	14.15	14.15	10.47
Individual Controls		Yes	Yes	Yes
Household Controls			Yes	Yes
Baseline State Controls × Time Trend				Yes

Notes: N=8,887,319. This table emulates Table 3 using state-level K-12 appropriations per pupil, including ACS 2001-2004. See the notes of Table 3 for the descriptions of the control variables. I use the same regional controls with Table 3 but at the state level. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.8: Alternative Samples

Dependent variable: private school enrollment (in percentage point)

	Include DC	Years of 2000-2012	Drop Dropouts	Native Only	Immigrant Only	Drop FL and NV	Drop FL and TX	Drop CA and TX	Drop AK	Drop top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
App per pupil	-0.599***	-0.588***	-0.640***	-0.659***	-0.413***	-0.529***	-0.447***	-0.535***	-0.600***	-0.523***
(in thousand)	(0.146)	(0.162)	(0.163)	(0.167)	(0.141)	(0.126)	(0.117)	(0.189)	(0.154)	(0.172)
	10.35%	*10.34%*	*10.61%*	*10.64%*	*5.97%*	*10.32%*	*10.65%*	*10.70%*	*10.34%*	*9.05%*
First stage F-Stat	25.71	3.958	28.96	26.44	20.80	41.29	58.23	30.98	28.17	23.74
Observations	8,106,020	6,272,388	7,916,186	7,636,564	458,509	7,601,863	6,973,419	6,396,752	8,073,326	7,397,373

Notes: The alternative sample used is indicated in the column title. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.9: Alternative Instrumental Variables

Dependent variable: private school enrollment (in percentage point)

	DiD (1)	State share (2)	NT (3)	+Interaction (4)
App per pupil	-0.690***	-0.509***	-0.794*	-0.464***
(in thousand)	(0.208)	(0.178)	(0.414)	(0.110)
First stage F-Stat	4.866	2.768	2.858	>1,000

Notes: In column 1, I use difference-in-differences estimations—$S_s \times Post_t$ and $NT_s \times Post_t$ —instead of event study estimations to instrument for education revenue per pupil. $Post_t$ indicates after 2007 or the Great Recession. I use the state share only in column 2 and no income tax indicator only in column 3 in the event study framework. In column 4, I add $S_s \times NT_s$, the interaction term of state share and no income tax indicator interacted with year dummies as instrumental variables in addition to the original instrumental variables. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.10: Lagged Appropriations and Moving Average

Dependent variable: private school enrollment (in percentage point)

	1-year Lag (1)	2-year Lag (2)	3-year Lag (3)	3-year MA (4)
App per pupil	-0.535***	-0.425***	-0.284***	-0.687***
(in thousand)	(0.146)	(0.116)	(0.102)	(0.192)
First stage F-Stat	5.728	6.063	3.947	14.10
Observations	5,618,538	5,130,051	4,643,246	8,095,073

Notes: This table uses the lagged variables of CPUMA education revenue per pupil. Columns 1-3 use $App_{t-1}, App_{t-2}, App_{t-3}$, respectively. In column 4, I use the three-year moving average $(t-1, t,$ and $t+1)$ of appropriations per pupil. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.11: Alternative Mechanism: Private School Choice Policies

	Add as covariates			As Dependent Variable		
	Any policy (1)	Voucher (2)	Tax credit (3)	Any policy (4)	Voucher (5)	Tax credit (6)
App per pupil	-0.625***	-0.635***	-0.629***	0.0563	0.0249	0.0086
(in thousands)	(0.158)	(0.161)	(0.162)	(0.0559)	(0.0424)	(0.0215)
				0.248	*0.12*	*0.073*
First stage F-Stat	33.22	27.50	29.51	19.91	19.91	19.91
Observations	8,095,073	8,095,073	8,095,073	850	850	850

Notes: In columns 1-3, the dependent variable is an indicator for private school enrollment (rescaled to percentage points) and indicators for having private school choice policies are added as a covariate. I add an indicator for any statewide private school policy, voucher program, and tax credit program in columns 1, 2, and 3, respectively. These indicators are time-variant because states differentially implement private school programs. Regressions are estimated with 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. In columns 4-6, I regress the indicators for state-level private school policies on education appropriations per pupil. The means of dependent variables are presented below the standard errors in italic. Regressions include state fixed effects and state controls in 2000 interacted with the year trend, and weighted with the state-level school-aged population in 2000. Robust standard errors are in parentheses clustered by the state for all regressions. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.12: Alternative Mechanism: Number of Schools

	TPS (1)	Charter (2)	Magnet (3)	All Public (4)	All Private (5)
Panel A. Add Number of Schools as Covariates					
App per pupil	-0.649***	-0.637***	-0.630***	-0.613***	-0.675***
(in thousand)	(0.161)	(0.158)	(0.158)	(0.162)	(0.251)
First stage F-Stat	30.87	31.17	28.44	32.19	3.521
Observation	8,095,073	8,095,073	8,095,073	8,095,073	2,817,931
Panel B. Number of Schools as Dependent Variables					
App per pupil	-20.18	-1.129	-9.627	-30.93*	12.28
(in thousand)	(19.85)	(1.400)	(8.282)	(15.82)	(13.50)
	114.9	*6.862*	*13.49*	*135.2*	*85.82*
First stage F-Stat	45.58	45.58	45.58	45.58	4.818
Observation	18,273	18,273	18,273	18,273	8,622

Notes: In Panel A, the dependent variable is an indicator for private school enrollment (rescaled to percentage points) and the number of each type of school at the column title is included as a covariate. TPS represents traditional public schools (public schools excluding charter and magnet schools). The number of private schools is from Private School Universe Survey (PSUS) described in Appendix Section F. Regressions are estimated with 2SLS estimation using the preferred specification and weighted using sample weights from the Census and ACS. In Panel B, I regress the number of schools on education appropriations per pupil. The means of dependent variables are presented below the standard errors in italic. Regressions include state fixed effects and state controls in 2000 interacted with the year trend, and weighted with the state-level school-aged population in 2000. Robust standard errors are in parentheses clustered by the state for all regressions. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.13: Impact on Number of School-aged Children, and In- and Out-migration

	ln(Total number) (1)	ln(In-migration) (2)	ln(Out-migration) (3)
ln(App per pupil)	-0.104	1.686	-102.7
	(1.094)	(2.104)	(77.96)
First stage F-Stat		14.82	
Observations		800	

Notes: N=800. First stage F-statistics 14.82 for all regressions. The year 2000 is excluded because of data limitations. The dependent variable is indicated at the column title. State fixed effects and 2000 state controls interacted with time trends are included. Regressions are weighted using the state population in 2000. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.14: Selective Migration and Private School Enrollment

Dependent variable: private school enrollment(in percentage point)

	Migration status from last year			5yr+	Funding of
	Different MPUMA (1)	Same MPUMA (2)	Same Residence (3)	Same Residence (4)	State of birth (5)
App per pupil	-0.315	-0.696***	-0.741***	-0.836***	-0.558***
(in thousands)	(0.303)	(0.231)	(0.245)	(0.258)	(0.178)
	7.55%	*10.47%*	*10.94%*	*12.32%*	*10.34%*
First stage F-Stat	5.617	13.26	14.46	18.54	12.41
Observations	204,575	5,413,963	4,979,663	3,364,390	7,622,046

Notes: I use the ACS question asking where each respondent lived 12 months ago to determine the migration status in columns 1-3. The sample includes only 2005-2016 because the 2000 Census lacks this information. The main estimate without the 2000 Census is -0.717 (SE: 0.251). Each regression uses the subsample indicated in the title of each column. Migration PUMA (MPUMA) is the geographical unit the ACS uses to determine migration status, which resembles commuting zones. Column 3 is a subset of column 2, who lived in the same house for more than 12 months. Column 4 restricts the sample to children whose household head had lived in the same house for more than five years. Because I only know how long the household head had lived in the same house in the ACS, I assume children's migration patterns would be the same as the household head. In column 5, I use the funding per pupil in the state of birth, which is robust to migration. Thus, all foreign-born children are excluded. Means of the private school enrollment in the pre-recession period are in italics below the standard errors. I use birth state controls instead of CPUMA controls in column 5. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.15: Heterogeneity by CPUMA Characteristic and Race

Dependent variable: private school enrollment (in percentage point)

	Poverty Rate		Minority Population		Foreign Population	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All races						
App per pupil	-0.777***	-0.487***	-0.870***	-0.242*	-0.766**	-0.307**
(in thousand)	(0.207)	(0.160)	(0.246)	(0.142)	(0.305)	(0.120)
	9.40%	*11.39%*	*10.58%*	*10.14%*	*10.50%*	*10.23%*
p-value of difference	0.0734		0.0109		0.152	
First stage F-Stat	17.77	30.98	42.08	17.43	147.2	24.78
Observations	25,730,652	23,295,084	21,659,560	27,366,176	18,990,741	30,034,995
Panel B. White						
App per pupil	-1.134***	-0.454**	-1.427***	-0.332*	-0.986**	-0.466**
(in thousand)	(0.410)	(0.197)	(0.426)	(0.182)	(0.453)	(0.177)
	13.36%	*12.92%*	*18.16%*	*11.29%*	*16.62%*	*11.90%*
p-value of difference	0.0756		<0.01		0.204	
First stage F-Stat	18.72	33.02	19.76	22.75	249.1	24.26
Observations	2,169,929	2,819,188	1,220,683	3,768,434	1,167,247	3,821,870
Panel C. Hispanic						
App per pupil	-0.593***	-0.520***	-0.640***	0.0615	-0.634***	0.0881
(in thousand)	(0.205)	(0.170)	(0.201)	(0.148)	(0.154)	(0.235)
	4.91%	*6.12%*	*5.20%*	*5.36%*	*5.095*	*5.65%*
p-value of difference	0.790		0.0106		<0.01	
First stage F-Stat	89.84	21.27	250.4	21.64	267.7	52.92
Observations	1,044,989	409,699	1,099,977	354,711	1,036,130	418,558
Panel D. Black						
App per pupil	-0.0989	0.0372	-0.206	0.388**	-0.134	0.202
(in thousand)	(0.170)	(0.182)	(0.204)	(0.179)	(0.240)	(0.241)
	5.41%	*6.48%*	*6.30%*	*4.46%*	*6.92%*	*5.04%*
p-value of difference	0.438		<0.01		0.294	
First stage F-Stat	28.25	18.97	10.39	32.16	219.6	52.37
Observations	659,173	296,231	654,851	300,553	348,874	606,530

Notes: The sample is divided into two groups by CPUMA characteristics presented in each column's title, like Table 7. Each panel is separately estimated by race. See the notes of Table 7 for the other details. Means of the private school enrollment of each group are in italics below the standard errors. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.16: Heterogeneity by Parental Characteristics

Dependent variable: private school enrollment (in percentage point)

	Both parents present		Has a Bachelor's degree		High earning occupation		Immigrant	
	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
App per pupil	-0.768***	-0.522***	-0.958***	-0.627***	-0.791***	-0.410***	-0.832***	-0.549***
(in thousand)	(0.198)	(0.143)	(0.317)	(0.150)	(0.200)	(0.127)	(0.243)	(0.163)
	12.13%	*6.29%*	*18.93%*	*6.60%*	*12.63%*	*5.45%*	*8.40%*	*11.28%*
p-value of difference	0.128		0.178		0.0187		0.115	
First stage F-Stat	21.42	19.97	18.46	25.35	20.38	18.85	107.0	20.38
Observations	5,849,114	1,895,318	2,761,200	4,983,232	5,646,262	2,098,170	1,745,342	5,999,090

Notes: The sample is divided into two groups by parental characteristics presented in the title of each column. Columns 3 to 8 are 'Yes' if at least one parent satisfies the condition. Means of the private school enrollment of each group are in italics below the standard errors. The dependent variable is the private school indicator rescaled to percentage points. All regressions are estimated with the 2SLS estimation using the preferred specification and are weighted using sample weights from the Census and ACS. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.17: Impact on Number of Enrolled Students in Private Schools

Dependent variable: Enrolled students in private schools

	ACS	PSUS					
			School Level				
			All Races			Whites	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
App per pupil in CPUMA	-732.2**	-527.6	-6.162*	-6.178*			
	(330.5)	(314.8)	(3.355)	(3.347)			
App per pupil in CPUMA × Catholic					-12.81***	-12.95***	-3.410**
					(2.847)	(4.183)	(1.421)
App per pupil in CPUMA × Other Relig					0.514	2.889	1.355
					(3.230)	(4.221)	(3.530)
App per pupil in CPUMA × Nonsectarian					2.893	2.461	1.964
					(4.158)	(4.510)	(3.916)
Mean of Dependent Variable before GR	*12,089*	*12,918*	*272.80*	*272.80*	*272.80*	*193.40*	*52.18*
CPUMA Fixed Effect	Yes	Yes					
School Fixed Effect			Yes	Yes	Yes	Yes	Yes
Baseline CPUMA Controls×Time Trend	Yes	Yes		Yes	Yes	Yes	Yes
Observations	13,974	8,566	130,284	130,284	130,284	130,284	130,284

Notes: In column 1, I aggregate my main sample into CPUMA level and regress the number of students in private schools on CPUMA education appropriations per pupil. CPUMA fixed effects and baseline CPUMA controls interacted with time trends are included and the regression is weighted with the total number of school-aged children in the CPUMA in 2000. In columns 2-7, I use a biennial survey called Private School Universe Survey (PSS) by NCES from 2001 to 2015. The unit of observation is school-year. I include private schools with 50 or more enrollees and serving beyond kindergarten. The independent variable of interest is the public K-12 appropriations per pupil in the CPUMA at which the school is located. In column 2, I aggregate the PSUS sample into CPUMA level and conduct the same analysis with column 1. Columns 3-7 use the school-level data. Columns 3-5 estimate the impact on school-level enrollment for all races. Column 6 examines white enrollment and 7 does Hispanics, respectively. The equations used in columns 3-7 are presented in Appendix Section F. School fixed effects are included and each regression is weighed using the school weights and number of students (in the corresponding race) in the CPUMA in 2000. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.