Competition Between Public and Private Education: Evidence from the Great Recession

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Abstract

This paper asks whether funding for public schools affects parents' decision to send their children to private schools. In the wake of the Great Recession, funding for public K-12 education fell precipitously in the United States and stayed low for several years. Critically, states with greater historical reliance on state appropriations (rather than local or federal appropriations) and states with no income tax experienced larger cuts. These two features were set decades before the Great Recession, changed little over time, and do not predict other impacts of the Recession, such as unemployment, providing plausibly exogenous sources for variation in public school funding. I combine these two sources with the timing of the Great Recession to instrument for local public school funding. I find that students exposed to a $1,000 (9.0 percent) decrease in per-pupil funding are more likely to enroll in private schools by 0.48 to 0.59 percentage points (4.5 to 5.6 percent). I show further that the effect is strongest among high socioeconomic status students living in disadvantaged areas. These findings suggest that reductions in public school resources lead to greater inequality in education and negatively change student composition in public schools through school choice.

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

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I Introduction

Private schools serve 10.3 percent or 5.7 million schoolchildren in the US primary and secondary education (Snyder, de Brey and Dillow, 2019). Besides the size of the market, private schools play an essential role in the education sector, both positive and negative. On the one hand, private schools provide parents with more options in education and compete with public schools, potentially improving the quality of public schools and overall education (Dee, 1998; Hoxby, 1994). On the other hand, private school opponents often argue that such schools increase inequality and reduce intergenerational mobility because they tend to attract high socioeconomic status (SES) students (Davies, Zhang and Zeng, 2005; Glomm and Ravikumar, 1992; Iyigun, 1999).

The fact that public and private schools compete over students means parents consider characteristics of local public schools when enrolling in private schools (and vice versa). In this paper, I investigate the effect of public K-12 education funding on private school participation in the US, a topic that has received limited attention in the literature. There are two primary channels in which public education funding may affect private school participation. First, public education resources may crowd-out household investment in education (Houtenville and Conway, 2008). Thus, when there is a decline in school funding, parents respond by increasing childcare time (Kim, 2001) and providing tutoring (Yuan and Zhang, 2015), implying a potential switch to private schools, another form of education investment. Second, whether school funding improves the quality of education measured by student achievement is ambiguous (Hanushek, 2003); however, it could improve the perceived quality of public schools, such as smaller class sizes and new equipment, which is inversely associated with private school attendance (Brasington and Hite, 2012).

While we expect public school funding would negatively affect private school attendance, the exact causal relationship is difficult to estimate because they are endogenously determined, and large-scale changes in education funding—either from policy interventions or economic downturns—are limited. I utilize states' idiosyncratic characteristics that generated exogenous variation in large funding cuts for public education followed by the Great Recession to overcome these identification challenges. In the wake of the Great Recession, funding for K-12 fell precipitously in many states, on average of 5.3 percent per pupil from 2007 to 2012 and stayed low for several years. Using the Great Recession as a natural experiment seems concerning given the far-reaching impact of the Recession on multiple areas of the economy and society. However, I show that the magnitude of funding cuts depended on two plausibly exogenous characteristics of state tax appropriation which increased the sensitivity of education funding to the Great Recession. This allows me to isolate the changes in school funding from other elements that occurred in the same period. First, states that historically relied more on state appropriations to fund K-12 rather than on local and federal appropriations experienced a deeper cut during the Great Recession (Jackson, Wigger and Xiong, 2021). State tax revenues mostly consist of sales and income taxes, which are more volatile than property tax, a major component of local tax revenues, making states' funding for K-12 volatile as well. Further, unlike local governments, state governments are responsible for meeting increasing demand for other welfare programs, crowding-out spending for K-12. (Evans, Schwab and Wagner, 2019; Jackson, Wigger and Xiong, 2021; Moffitt, 2013). Second, I show that K-12 funding stayed lower after the Great Recession in seven states without an individual income tax. These states lack diversification in their tax portfolio (Cornia and Nelson, 2010), which improves the fiscal health by reducing volatility in the tax revenue during recessions (Jordan, Yan and Hooshmand, 2017; Yan and Carr, 2019). The seven states could not recover their tax revenues as quickly as other states, and consequently, their funding for K-12 in 2016 was still lower than the pre-recession level.

The two factors—funding scheme and tax structure—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 overall wealth of each state before and after the Recession. Thus, these features provide conditions for an instrument by isolating the effects of funding cuts for K-12 from the Great Recession itself. I combine the two sources of variation with the onset of the Great Recession in an event study framework as an instrument to predict the local K-12 education revenue 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 0.48-0.59 percentage points or 4.5-5.6 percent. The estimated elasticity is -0.62 in the most preferred specification, meaning a one percent decrease in public education funding raises private school enrollment by 0.62 percent. This implies that, in response to a 5.3 percent funding cut (the average cut from 2007 to 2012), 162,445 switched to private schools. The results are also robust to a variety of confounding factors, including selective migration and the introduction of government-funded school choice programs like vouchers and tax credits.

To further understand why students switch to private schools, I estimate the impact on spending categories and staff-to-student ratios. My results reveal that areas with larger budget cuts ended up with fewer teachers and instructional aides per student as well as less generous employee benefits for teachers, relevant to the quality of education (Card and Krueger, 1992). I cannot directly connect these changes to changes in private school attendance because my instrument does not allow me to separate the impact on school qualities. However, Jackson, Wigger and Xiong (2021) show that students' test scores had fallen in the same period, supporting that a decline in education quality is the most likely mechanism.

Finally, I test for heterogeneous effects by race and household income: the impacts of public school funding on private school enrollment are not found for black students and are concentrated in middle-income families. Additionally, I divide the sample by high and low SES areas in terms of poverty rate and the share of minorities and immigrants. I find that high SES students (from high-income and white households) are more likely to flee to private schools when they live in low SES regions. My heterogeneity analysis not just shows that certain groups are more responsive than others; it also sheds light on a potential change in the student composition in public schools especially in low SES areas. These results indicate a potential increase in inequality in educational attainment 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 first papers estimating the elasticity of the demand for private school enrollment with respect to the public K-12 education budget. Due to the challenges of identification, few empirical papers examine the causal relationship between public K-12 expenditure and private school attendance. Goldhaber (1999) structurally estimates the relationship between public funding and private schooling, relying on cross-sectional variation across regions for instrumental variables. My paper leverages tighter identification using variation in funding both across regions and over time and obtains more robust results, finding larger elasticity. The closest work is Dinerstein and Smith (2014), finding an increase in public school funding may increase public school enrollment especially for low SES students in New York City. This paper also shows a decrease in private school enrollment is accelerated by private school closures. My paper provides evidence that while the impact of public school funding on private school enrollment is symmetric, the mechanism through which private school enrollment changes is different in case of funding cuts.

Second, I provide evidence of how education funding cuts can deepen the racial gap in educational attainment through school choice. Public school spending has an important role in reducing inequality (Johnson and Jackson, 2019); however, my results complicate this role because some high SES students can avoid funding cuts by switching to private schools, thus exacerbating inequality. My heterogeneity results also indicate that school funding cuts affect student composition, especially in disadvantaged regions, making student composition disproportionately low SES. Thus, without considering this compositional change, the impact of public school spending on student outcomes could be overestimated. Moreover, peer effects may intensify the direct impact of school funding on students' test scores because losing high SES peers can lower the performance of remaining students (Akyol, 2016; Dills, 2005). Either because of compositional change or peer effects, recent papers find a large impact of school funding on student outcomes and heterogeneity by social and ethnic groups (Baron, 2019; Hyman, 2017; Jackson, Wigger and Xiong, 2021; Kreisman and Steinberg, 2019; Lafortune, Rothstein and Schanzenbach, 2018).

Third, I contribute to the identification of education spending cuts driven by the Great Recession initiated by Jackson, Wigger and Xiong (2021). They explore how the K-12 funding cuts after the Great Recession affected test scores and college enrollment by leveraging the variation in funding cuts induced by historical reliance on state-appropriated funds. While being clever, this identification has a weak first stage, so they rely on the fact that the decline in the slope of the K-12 spending was greater in states with higher reliance on state appropriations. This increases the statistical power of the first stage but assumes a specific functional form. I extend their strategy by adding another source of variation: whether a state collects an individual income tax. This strategy improves the precision of the estimates. 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 variations 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 shows the robustness of the results and Section VII presents heterogeneity analysis. Section VIII concludes.

II Background: K-12 Budget and the Great Recession

Funding for K-12 education is not stable over time. A primary factor is the business cycle. This is because tax revenue declines during recessions (income effect), and at the same time, state governments need to spend more money on other social safety net programs like unemployment benefits and food stamps, crowding-out expenditure for K-12 (Jackson, Wigger and Xiong, 2021; Moffitt, 2013). Thus, the growth rate of K-12 revenue per pupil declines during and after recessions.[1] 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 funding since the 1980s recession, and lasted for years (Figure 1). The magnitudes of the funding cuts differ substantially across states in Figure 2. For example, Florida, the state with the deepest cut, curtailed its K-12 revenue by 28 percent during these years, much greater than the national average.

The Great Recession affected parents' demand for private schools in two opposite ways. The Recession pushed students out of private schools by reducing parents' income. (Ewert, 2013; Lamb and Mbekeani, 2017). Separate from this income effect, the devastating funding cuts for public schools may induce some parents to substitute into private schools. Figure 3 clearly shows 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 Re-

[1] K-12 revenue is interchangeable with K-12 budget, funding, or appropriations. This is not the realized spending, but the amount of money appropriated to K-12. From the school district's perspective, appropriations are revenue because they receive it from the governments. This terminology is widely used in the official school district and government documents on school funding.

cession experienced deeper cuts, first used by Jackson, Wigger and Xiong (2021) to examine the impact of K-12 spending on student achievement. K-12 education revenue is funded by three different sources: state, local, and federal governments. This identifying variation utilizes the fact that state-funded revenue had declined more substantially than local and federal revenues in the wake of the Great Recession. To be specific, I visualize the trend of K-12 funding per pupil by the source in Figure 4. There was an immediate and steep drop in state revenues, 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 of state, local, and federal K-12 funding so different? First, state tax revenues experienced both large revenue and crowding-out effects and resulted in an immediate funding freeze for K-12. 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 such as unemployment benefits and food stamps together with the federal government, crowding-out expenditure for K-12. In contrast, local K-12 funds face smaller income and crowding-out effects. Local tax revenues rely heavily on property tax (72 percent (US Census Bureau, 2020)), which is stable during recessions.[2] 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 also true for the Great Recession, although it started from the collapse in the housing market and was followed by substantial foreclosures (Lutz, Molloy and Shan, 2011). 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. Federal funds are mostly earmarked to specific federal programs such as the National School Lunch Program and Title I. During the Great Recession, the federal government substantially increased the funding through the American Reinvestment and Recovery Act, and when the fund ran out, a deep funding cut followed (Evans, Schwab and Wagner, 2019).

Because of this different trend 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 ($S_s = \frac{State\ Rev_s}{Total\ Rev_s}$, state share henceforth)

[2] 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.

before the Great Recession, which becomes the first identifying variation.[3] On average, 47 percent of the total K-12 revenue came from the state government in School Year 2006, varying from 86 percent in Vermont to 27 percent in Nevada in Panel A of Figure 5. 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 Panel B.

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, government 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.[4] I test this more formally by showing the share does not predict several state-level characteristics relevant to the impacts of the recession that may affect private school enrollment in Section IV.3. 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 revenue per pupil before the recession (Panel C in Figure 5).

Along with the education funding structure, 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 individual income tax. While states constantly change the income tax structure and tax rates, whether a given state collects income tax or not was determined decades ago, providing exogenous variation in changes to the education budget.[5] There are seven states with no individual income tax—Alaska, Florida, Nevada, South Dakota, Texas, Washington, and Wyoming.[6] Three factors led

[3]State revenue here is the K-12 revenue "distributed" by the state government. For example, California's Proposition 98 guarantees a minimum spending level for public schools. Proposition 98 dollars are state funds raised primarily through income, sales, corporate taxes, combined with locally raised property tax (EdSource, 2009). This is considered as state revenue in the CCD, although it includes locally raised property tax. Although smaller than California, Texas also redistributes local property taxes through recapturing, and the recaptured property tax is classified as state revenue in CCD. To address a potential problem arising from this, I exclude California and Texas from the sample in the robustness check, and the result does not change much. (See Appendix Table A.5.)

[4]State share had been very stable during 2000-2007 (Appendix Figure A.3). 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).

[5]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).

[6]New Hampshire and Tennessee collect tax on dividend and interest income, but not on labor income. In

to a larger decline in education revenue in these seven states. First, because they lack one tax source with a very wide base, it is difficult for these states to diversify their tax revenue (Cornia and Nelson, 2010). Lack of diversification in their tax portfolio is potentially problematic, especially during recessions, because diversification improves fiscal health by reducing volatility without sacrificing the expected revenue (Jordan, Yan and Hooshmand, 2017; Yan and Carr, 2019). Second, while these states tend to heavily rely on sales tax (Cornia and Nelson, 2010),[7] states with higher reliance on sales tax had suffered longer to recover their tax revenues after the Great Recession Alm and Sjoquist (2014). Finally, states also attempted to recover their tax revenues quickly. One way is to make income tax more progressive, 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.[8] While education funding in other states recovered to the pre-recession level by 2014-15, it was still lagging in no-income-tax states.

Panel A of Figure 6 compares the trend of real total tax revenue per capita in states with and without personal income tax relative to the fiscal year 2007. The tax revenue had increased for years and decreased after the start of the recession in 2008. While states with an income tax have steadily recovered their real tax revenue, the seven states without personal income tax had struggled long, having much slower revenue recovery.[9] The different trends in total tax revenue influenced the total K-12 revenue as well in Panel B. To my knowledge, I am the first to show that having an income tax can affect education funding stability after the Great Recession.

the robustness check, I include these two states as no income tax states as well. The results are very similar (See Table 5).

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

[8]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.

[9]This is even true when excluding property tax revenue. The figure comparing tax revenue without property tax is available in the Appendix Figure A.4.

III Data

My analysis draws data on 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 if 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.[10] I also exclude children living with no parents, about 4 percent of the sample, because students raised by an extended family member or foster parents may have different decision-making processes. I omit 2001-2004 ACS because I cannot identify geographical units smaller than the state in these years.[11] Washington D.C. is also removed from the main sample because the state share is zero by definition and thus D.C. becomes an outlier.[12] My final sample consists of 7,744,432 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. I exclude school districts with no enrolled students, negative total revenue, and only with vocational schools or adult schools. I also restrict to school districts with a valid address because I match school districts to the geographical unit in the Census and ACS using the location address.[13] The total number of school districts varies every year; however, there were 15,187 school districts in the school year 2006-2007 after removing the invalid districts that account for 5-6%.[14] To merge two datasets, I aggregate the school finance data into the Consistent Public Use Microdata Areas (CPUMA) in the Census and ACS.[15] I also take a weighted average of two fiscal years to construct school finance data at the calendar year level because

[10] 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.

[11] ACS in 2001-2004 is also known not to be representative. Nonetheless, I include these years and use the state-level education revenue in the robustness check.

[12] In the robustness check, I estimate the impact of public education revenue including Washington D.C.

[13] I use the address of the school district's main office to assign its CPUMA.

[14] This number changes every year, from 15,000 to 16,000.

[15] PUMA is the smallest geographical unit available in the Census and ACS PUMS files. PUMA boundaries change every ten years., and the Consistent PUMA is an aggregate of some of contingent PUMAs to make the boundary consistent over time. While there are slightly more than 2,000 PUMAs, they are aggregated to 1,078 CPUMAs.

the Census and ACS do not provide information on survey month.[16] See Appendix Section A for the further details of the crosswalk.

In Table 1, I show the summary statistics in the pre- and post-recession period. Slightly more than 10 percent of the total student is enrolled in private school (as opposed to in public school or not enrolled at all). This number marginally decreases after the Great Recession because the income effect of the recession made it difficult for some families to afford tuition. The average inflation-adjusted total revenue per pupil is about $11,139 before the recession. The funding is larger after the recession because it was in an increasing trend from 2000 to 2007. The average composition of the revenue also changes: the share coming from state government decreases, and the share coming form the federal government increases, as seen in Figure 4.

IV Econometric Model and Validity

IV.1 Estimation Equations

Local public education revenue and private school enrollment are endogenously determined, making the Ordinary Least Squares (OLS) biased. While area and year fixed effects control the bias coming from selection to areas and national shocks, they cannot absorb localized economic shocks. A local economic boom may increase both public school budget and private school participation, biasing the OLS estimates upward. Additionally, private school attendance may also influence spending for local public schools while the direction of this reverse causality is indeterminate (Goldhaber, 1999). When students leave the public sector, the local public education funding per pupil mechanically increases because fewer students share the school resource. If the flight to private schools continues, public education funding per pupil may decline because of the political pressure to cut taxes for public schools, as many parents become uninterested in public schools. Thus, OLS cannot identify the causal relationship, and to address these identification challenges, I leverage the Great Recession as a natural experiment for reasons I explain in Section II.

[16] I use fiscal year instead of the school year because it is the 12-month period to which the annual operating budget applies, according to NCES. The results are robust to alternative ways of defining years–using school year level constructed by fiscal year data and the fiscal year matched to the same calendar year (Available upon request).

I estimate the following system of equations using two-stage least squares (2SLS):

$$Private_{ipst} = \delta \widehat{Rev}_{pst} + X_{ipst}\pi + P_{pst}\kappa + \mu_p + \theta_t + \varepsilon_{ipst}, \tag{1}$$

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

where $Private_{ipst}$ is an indicator whether individual i in CPUMA p of state s in (calendar) year t is in a private school (as opposed to in a public school or not in school) and Rev_{pst} is the total K-12 revenue per pupil in thousands (in 2010 dollars).[17] I include a vector of student and household level controls (X_{ipst}) and time-variant CPUMA characteristics (P_{pst}), 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 means of the Census and ACS.

I instrument for Rev_{pst} by combining S_s, the share of total K-12 revenue coming from state-appropriated funds in the school year 2006-2007 ($\frac{State\ Rev_{s,2006}}{Total\ Rev_{s,2006}}$), and NT_s, the indicator for having no state income tax, with the year dummies in an event study setting. I take 2007 as the base year, so all coefficients can be interpreted as changes relative to 2007.[18] 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.6, I show that my results are robust to using alternative instruments such as traditional DiD and using only one source of variation.

The Great Recession-induced funding cuts for public education provides a chance to test the impact of massive funding changes. However, using them as identification raises the question of the extent to which my results are generalizable when public school funding increases or decreases for different reasons than recessions. While it is an area for future investiviation, studies on school finance reforms give suggestive answer. Downes and Schoeman (1998) and Husted and Kenny (2002) find opposite impact on private school enrollment in districts that have and have not benefited from the reforms, implying my results are somewhat generaliz-

[17] 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.3.

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

able for other funding changes.

IV.2 First Stage

In this section, I present the first stage result to confirm the relevance of the instrumental variables using equation 2. When estimating this equation, I scale the per-pupil public education revenue to thousands of 2010 dollars. The first stage result is presented in Figure 7. This figure displays the excluded instrumental variables, the set of coefficients of state share (green dots), and no income tax indicator (orange diamonds) 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 full set of individual, household, and CPUMA controls (preferred specification).[19] The first stage F-statistics is 16.9, which passes the weak IV test threshold. [20]

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 $500 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 $11,048, this is a very large impact. The funding cuts induced by the Great Recession was 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, but not for the no-income-tax indicator, as in Panel B of Figure 6. The figure shows little evidence of pre-trend; however, to address potential pre-trending in education funding, I add CPUMA-specific linear time trend as robustness check, showing very similar results (Table 5.).

IV.3 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 rev-

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

[20]2001-2004 ACS are not included in my sample. To examine the pre-trend including this period, I estimate the event study equation only with school finance data in Appendix Figure A.7. I find little evidence of pre-trend.

enue. 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}). However, in this section, I provide evidence that my instrumental variables are uncorrelated with important state-level characteristics that may affect private school participation. Particularly, 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.

I choose six state-level characteristics that represent the income effects of the Great Recession: personal income per capita, median household income, poverty rate (share under 150 percent of the federal poverty line), the unemployment rate of household heads, homeownership, and the median housing value. The first three variables represent the overall wealth and earnings. The unemployment rate indicates the economic condition in each state. It is also important to check homeownership and median housing value, given the Great Recession's impact on the housing market. Except for personal income per capita, which comes from the Bureau of Economic Analysis, I calculate the state-level variables from the Census and ACS. When calculating the mean, I restrict the sample to households with at least one school-aged children (age 6 to 17) for relevancy.

Using the event study model similar to equation 2, I first test whether these six variables are correlated with my identifying variation. The unit of observations of the regressions is state-year, and I weigh the regressions with the population of school-aged children in each state. Figure 8 displays the coefficients of the event study variables, along with a 95 percent confidence interval. None of the six variables are correlated with state share or no-income-tax status before and after the recession.

Next, to confirm the education revenue per pupil is not related to these characteristics, I estimate the impact of education revenue with the 2SLS model using the instrumental variables described above. I rescale the point estimates and standard errors by multiplying 10,000 for display and present the result in Table 2. Although statistically insignificant, the coefficients are all nearly zero, confirming that they are irrelevant to public education funding per pupil. Therefore, both the reduced form and 2SLS estimates support that my instruments can remove the income effects of the Great Recession and focus on the variation in education funding.

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 CPUMA fixed effects. In columns 1 to 4, the coefficients are consistent and robust to the inclusion of controls, falling within the small range of -0.48 to -0.59 percentage points. When I control for individual demographic characteristics, the point estimate increases in magnitude by 0.06 percentage point or 12 percent (column 3). 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 0.02 percentage points. I add time-variant CPUMA controls in column 4, and the point estimate increases by 0.038 percentage points without losing precision. The magnitude of the impact is much larger (more negative) in 2SLS regressions than OLS results (-0.087 to -0.132, see the Appendix Table A.3.), which means the OLS estimate is biased toward zero.

In column 4, my preferred specification, the coefficient indicates that a $1,000 reduction in public education revenue per pupil in CPUMA increases private school enrollment by 0.59 percentage points. This represents a nine percent decrease in the public education budget and a 5.6 percent increase in private school enrollment, given that the mean of budget and private school attendance was $11,048 and 10.61 percent before the Great Recession, respectively. This implies the elasticity of the demand for private schools with respect to public school funding is -0.62.[21] Using this elasticity, I calculate that roughly 159 in the average CPUMA or 162,455 students in the country leave for private schools in response to a 5.3 percent funding shock, the average funding cut from 2007 to 2012.[22]

[21] $-0.62 = \frac{5.6\%}{-9\%}$. It means a one percent decline in public education revenue increases private school enrollment by 0.62 percent.

[22] The 5.3 percent decline in K-12 revenue implies a 3.29 percent increase (-5.3×-0.62) in private school enrollment. In the average CPUMA, there are 45,638 school-aged children, and 10.61 percent of them are in private schools in 2007. The back in the envelope calculation suggests that 159 students ($= 45,638 \times 10.61\% \times 3.29\%$) transfer to a private school system in this CPUMA. We can do a similar calculation for the total school-aged children in the US, 46,536,645.

V.2 Comparison to Existing Literature

This elasticity estimate is larger than the two only existing estimates. Work by Goldhaber (1999), estimating a structural model, suggests 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, indicating the elasticity of -0.5.[23] A more recent study by Mavisakalyan (2011) estimates the relationship between public education spending and private school enrollment in more than 80 countries. 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, implying the elasticity of -0.34, about half of mine.[24] While they both investigate different periods and regions, the cross-sectional instrumental variables used in these papers may not completely rule out reverse causality and omitted variables, generating a smaller elasticity. If this is the case, then I would expect estimates to be biased toward zero, consistent with the results of my OLS estimation.

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 (observable) quality of public schools. As Hanushek (2003) points out, input-based schooling policy does not necessarily improve school quality because the resources could be distributed inefficiently. In this section, I test whether the funding cut happened for expenditure categories related to the education quality. I also focus on average instructional salary and teacher employment benefits, which may hint at the actual quality of education, and staff-to-student ratio, which is relatively easily observable by parents and children. 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. Overall, my results are consistent with Jackson, Wigger and Xiong (2021), showing a decline in education quality represented by student test scores in the same period because of the Great Recession induced funding cuts.

In Panel A of Table 4, I regress the level of spending in each category: expenditure on the total operation, instruction, capital, and student support. There are statistically significant

[23] 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.

[24] The K-12 education spending accounts for 4 percent of US GDP in 2016 (Snyder, de Brey and Dillow, 2019), so a one percentage point increase corresponds to a 25 percent increase in education spending. The estimated elasticity is -0.34 (-8.5/25).

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 nine percent), from eight to eleven 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 the literature (Jackson, Wigger and Xiong, 2021), which finds a large effect on capital spending reduction in the same period, because I rely on different specification.[25]

Next, I examine teacher compensation, 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 insignificant decrease in real average teacher salary (total instructional salary expenditure divided by the number of teachers) by $1,957 or 3 percent. There is a strong influence on employee benefits for teachers in column 2, about $3,404, or 18 percent. The result seems reasonable; it may be difficult to cut teachers' salaries, 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.[26] I find that a $1,000 decrease in K-12 revenue per pupil led to a significant decline in the teacher-student ratio by 0.175 (column 1). This corresponds to a 2.8 percent decline compared to the mean in the pre-recession period. The impact on instructional aides is much larger, a reduction in the ratio by 0.207 or 16 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 instructional aides to save expenses. Guidance counselors and library staff are also supplementary compared to teachers; however, they often cover the entire school alone, leaving little room to reduce them. Thus, the coefficients are close to zero and not statistically significant in columns 3 and 4.

[25] Baron (2019) finds that an increase in capital spending does not improve student achievement compared to instructional expenditure, which implies that the instructional spending is more critical to the education quality.

[26] I use the staff-to-student ratio instead of the student-to-staff ratio (class size) not to lose observations because some CPUMA don't have any instructional aides or library staff.

VI Robustness Checks

VI.1 Alternative Specifications

In this section, I provide several additional robustness checks to assess the sensitivity of my result, including alternative specification and definitions of instrument and education funding. In column 1 of Table 5, I control 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 1 is essentially the same with a slightly larger standard error, implying that differential trends cannot explain the main finding. In columns 2 to 4 in Table 5, I test whether the point estimate is robust to using an alternative definition of the state share: five-year average share, the share defined in 2000, and in 1990, respectively. Although the share stayed very stable from the school year 2000 to 2006, with a very high correlation coefficient over 0.9.[27] Because several states implemented school finance reform in the 1990s, the correlation between 1990 and 2006 is weaker, but still over 0.6 (0.75 when comparing ranking). Because of the large correlation with the share in 2006, the point estimates stay almost the same in columns 2 to 4. In column 5, 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. When including these two states, the point estimate increases by 0.6 percentage points, suggesting these two states also have experienced a relative increase in private school attendance.

I use state-level K-12 revenue per pupil and include the 2001-2005 ACS in column 6. Some states have a considerable variation in K-12 revenue within the state, so I examine whether using state-level K-12 revenue substantially changes the main result in column 6. The point estimate gets smaller by 17% but not statistically different from the main specification.[28] In column 7, I use the realized expenditure rather than the appropriated funds. If there is a large discrepancy between K-12 revenue and spending, for example, if the states 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 the balanced budget is

[27] See Appendix Figure A.3 to compare across states.

[28] Note that 2001-2004 ACS is considered not to be nationally representative. When I exclude these years, the point estimate is -0.598(0.184), almost identical to the main specification.

highly recommended to school districts and often required by law in some states and cities. However, the budget deficit is sometimes inevitable, especially during an economic crisis. Column 6 gives a consistent result, disregarding this concern. The point estimate is slightly larger by 7% because the unexpected funding cuts may have forced some school districts to accrue debt; however, not statistically different.

In Appendix Table A.5, I estimate using alternative samples such as including DC and removing some states that may respond differently. The results are robust to all alternative samples, showing small differences to the main result. I also use lagged revenue per pupil in Panel B of Table A.6 to test whether students are sensitive to the cumulative experience of funding cuts and show they are.

VI.2 Selective Migration

People strategically migrate due to their preference for public goods (Tiebout, 1956). This is specifically true for education (Barrow, 2002); thus, when observing or expecting budget cuts for education, families with a high preference for public schools may relocate to higher 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 funding cuts for public education rather stimulate competition among public schools than between public and private schools, which is a serious challenge to my results. Thus in this section, I examine whether issues of selective migration confound my results.

To analyze it, I use the migration history within a year available in the ACS. The 2000 Census is excluded in this analysis for consistency because it identifies migration history within five years. 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. The MPUMA here is different from either the regular or CPUMA; it aggregates the regular PUMAs to resemble the commuting zone and is specifically used to collect workplace or migration information. Using this information, I can determine the amount of in- and out-migration in a given MPUMA. In Appendix Table A.7, I estimate the impact of K-12 revenue per pupil on the total number of school-aged children and in- and out-migration, showing public school funding is not correlated with any of them.

Although I show evidence of little selective migration between MPUMAs, this is not sufficient to rule out the possibility of selective migration because MPUMAs or CPUMAs are often larger than school districts, and households may strategically relocate within MPUMA. If migration for education is a prevalent reason for relocation, then migrants' response to the funding cuts for public school would be different than non-migrants. In Table 6, I test whether the impact of public school funding varies by migration status and show it does not.[29]

I first split the households by migration status in columns 1 to 3.[30] From columns 1 to 3, I divide the sample into those who have migrated between 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 0.662 percentage points. However, it is not precisely estimated because of the small sample size. This is very similar to those who have not migrated (column 2) and who have not moved within a year (column 3), implying that the migrants' behavior is no different from stayers.

In column 4, I estimate the impact on children whose household head has lived in the same house for five or more years.[31] The coefficient increases to -0.73, by 16 percent. Although it is not statistically different, the larger point estimate is interesting. These households consist of adults who are on average older and more likely to be homeowners (and therefore have higher SES). As further discussed in Section VII, higher SES families are more likely to respond to the shock by fleeing to private schools than the average population. Finally, in column 5, I use the K-12 revenue per pupil in the state of birth, excluding foreign-born children.[32] 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 larger than the main estimate by 0.09 percentage points, but not statistically different.

[29] 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.

[30] For reference, the point estimate (SE) is -0.629 (0.229) for the sample of the year 2005-2016.

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

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

VI.3 Statewide School Choice Programs

Several states have school choice programs. The programs include (but are not limited to) private school programs like vouchers and tax-credit scholarships, 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 some students (Epple, Romano and Urquiola, 2017). 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).[33] Charter schools and magnet schools are also popular alternatives to traditional public schools (TPS). Thus, the existence of these school choice programs could partially drive the result, regardless of public school funding.

In Table 7, I address this potential problem and show school choice programs do not drive my results. In columns 1 to 3, I add a time-variant indicator for whether a state has any school choice policy (column 1), only a voucher program (column 2), or only a tax credit program (column 3). The point estimates have little difference from the main impact estimated with the main specification (-0.589), suggesting the indicator for the school choice program does not absorb the effects on private school enrollment. Next, I add number of charter schools (column 4), magnet schools (column 5), and total public schools (column 6) in the CPUMA as control variable. The point estimates in columns 4-6 are also not statistically different from the main estimate. Especially, the result in column 4 is consistent with Chakrabarti and Roy (2016)'s findings that charter schools have little impact on private school enrollment.

VII Heterogeneity in Effect

In this section, I present the impact of per-pupil public education funding in different subgroups. The fact that private schools increase inequality implies that the demand for private schools is stronger for high SES households. This does not necessarily mean high SES families are more sensitive to public school expenditure as well. However, a model constructed by Sonstelie (1979) suggests heterogeneity in demand for private schools with respect to public school funding. In his model, households enroll in public schools only when they get greater

[33] 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.

utility than choosing private schools. Funding cuts for public schools reduce the utility from choosing public schools, making parents who marginally prefer public schools leave for private schools.

Sonstelie (1979)'s work implies that parents' preferences for private schools affect how sensitive they are to public education funding. The literature shows that the preference for private schools is related to the demographics and the SES of students and parents (Brunner, Imazeki and Ross, 2010; Long and Toma, 1988). In addition, extensive research exists on the relationship between regional characteristics and private schools attendance, indicating that private school enrollment depends on 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; Cascio and Lewis, 2012; Murray, 2016; Tumen, 2019).[34] In the following subsections, I empirically evaluate how responses vary by these characteristics and examine the types of households that exhibit stronger responses to changes in funding.

VII.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, belief in critical stages and experience in previous (public) schools (Goldring and Phillips, 2008). In columns 1 and 2 in Panel A of Table 8, 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 0.18 percentage points. 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 the public school resources is not evident. I examine heterogeneity by three race categories and present them in columns 3 to 5 of Panel A of Table 8. I find significant effects for whites and Hispanics, but not for African Americans. The impacts on whites and Hispanics' private school enrollment are very similar to one another: a $1,000 increase in public education revenue per pupil decreases private school enrollment by 0.604 and 0.586 percentage points, respectively. When restricting the

[34] 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.

sample to US-born Hispanics, the point estimate increases to 0.682 percentage points (not reported in the Table), even larger than whites. The effect on black students in column 5 is smaller and not statistically significant and statistically different from columns 3 and 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 estimate of -0.604 and -0.586 for whites and Hispanics corresponds to the elasticity of -0.5 and -1.11, respectively. Back in the envelope calculation suggests 100,195 and 26,776 white and Hispanic students were leaving for private schools in the country in response to -5.3 percent funding shock from 2007 to 2012, respectively.[35]

The similar point estimate for whites and Hispanics is interesting, implying it is not just a “white effect”. Previous literature suggests that Hispanics are as sensitive as their white peers to some situations that affect preference for private schools. Fairlie (2002) finds the existence of “Latino flight", similar to “White flight," that Hispanic students transfer to private schools as the black population increases in their neighborhoods, and the impact is no smaller than whites. Also, results of Neal (1997) and Evans and Schwab (1995) indicate that Hispanic students have a high preference for Catholic schools and benefit more from them than whites. These papers suggest Hispanics may have a relatively strong preference for private schools, and the funding cuts for public schools made some marginal Hispanics transfer to private schools.[36]

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 within state and year. In other words, I divide the sample by their relative standing within the state of residence and survey year.[37] The Table shows evident heterogeneity in response to budget cuts across income groups. While middle-income households strongly respond to the education budget, the richest (above 90th percentile) and the poorest (below 25th percentile) are not as responsive. These two groups are not elastic for different reasons. Wealthiest families have a high

[35]The total number of white and Hispanic school-aged children before the Great Recession is 28,216,266 and 8,428,589, respectively.

[36]In Appendix Section C.3, 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 in Hispanic-concentrated CPUMAs tend to switch to Catholic private schools too.

[37]I divide the sample in this way to include all states in each group, as I use the state-level variation as the identifying variation. Results using the national income percentiles are available upon request. The coefficients and standard errors change a little, but the overall patterns–concentrated in the middle-income families–remain the same.

baseline private school enrollment rate, suggesting always-takers of private schools are disproportionately in this group. These people are not sensitive to public school funding because they will never choose it. On the other hand, most of the poorest households are never-takers of private schools, either because of low preference or affordability and stay in public schools no matter what happens.

The point estimates in columns 2 to 4 suggest that a $1,000 increase in public education revenue leads to a reduction in private school enrollment by -0.65, -0.82, and -0.55 percentage points, for the income percentile of 90th to 75th, 75th to 50th, and 50th to 25th households, respectively. The coefficients for the three groups are not statistically different from each other; however, they are all different from the richest (column 1) and the poorest households (column 5).

Overall, heterogeneity analysis indicates that high SES students can avoid the adverse effects of a funding freeze by switching to private schools. Given that private education may increase inequality (Glomm and Ravikumar, 1992), cuts for public school spending can have a broader impact on inequality and intergenerational mobility than expected. While the adverse effects of funding cuts on remaining students could be partially alleviated by high SES students leaving for private schools (Akyol, 2016), public school funding cuts may increase inequality in student outcomes by directly affecting remaining students in public schools (Johnson and Jackson, 2019) and by inducing some students to opt-out from public schools.

VII.2 Heterogeneity by CPUMA Characteristics

While the exodus of high SES students from public schools may increase the overall inequality, it would not significantly affect student composition if it only happens in high SES areas. On the other hand, if high SES students in low SES areas flee to private schools, this would lower the peer quality and remove heterogeneity within the schools. In this section, I examine the potential change in student composition in public schools by exploring the effect of the public education budget by the neighborhood characteristics and household income. I investigate three CPUMA level characteristics: the poverty rate, minority population, and foreign population. In Table 9, I first divide the sample into high and low CPUMA using the state means in 2000.[38] To further assess who is exactly leaving for private schools in disadvantaged

[38]Again, I use the relative standing of CPUMAs within states to ensure each group has all 50 states. Because these three variables have large regional variations within the state, using national-level means does not change my point estimates much. However, it increases the standard errors and makes some point estimates not dif-

areas, I divide the sample one more time by household income. Thus, four groups for each regional characteristics are separately estimated, and the results are presented in Table 9.

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 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 rate CPUMAs, we can refer to the end of the Panel. When comparing the income groups within the same region, the corresponding *p*-value is presented at the bottom of each column.

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 8. On the other hand, the impacts are larger in disadvantaged regions (columns 1, 3, and 5) for both income groups, meaning 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 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 point estimates for high-income households in high areas are all statistically different from low-income families in high areas (end of the columns) and high-income families in low areas (end of Panel A). I observe 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 in Appendix Table A.8. Together with Table 9, high-income and white families in low SES areas tend to opt-out from public schools when exposed to funding cuts for public education.

The results imply that school funding can change student composition in public schools, especially in disadvantaged areas. Consequently, we should take this into account when we interpret the impacts of public education spending on students in public schools; otherwise, they may be overstated. Critically, the adverse effects of funding cuts on student achievement could be stronger in low SES areas even without any direct causal impact because students remaining in public schools would be disproportionately low SES. In line with this result, several recent papers find that K-12 funding increases standardized test scores and college enrollment for students in public schools with a larger effect in high poverty areas (Jackson,

ferent from each other. The result using national means is available upon request. Also, I divide the CPUMAs by their characteristics in 2000 to avoid any endogenous change happening together with the change in the education budget.

Wigger and Xiong, 2021; Jackson, Johnson and Persico, 2016; Kreisman and Steinberg, 2019; Lafortune, Rothstein and Schanzenbach, 2018). 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).

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 highly resourced 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 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.

VIII 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 0.59 percentage points, implying the elasticity is -0.62. A decline in public schools' perceived quality represented by the student-staff ratio and spending per teacher seems to be a likely mechanism. Moreover, the impact of funding cuts is concentrated within white and Hispanic students and 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 may experience another financial shock for K-12. It has been only a few years since the schools have fully recovered from the Great Recession, and another cut may result in even larger impacts. Some families may leave for private schools which are under fewer regulations and have greater resources than public schools. This is especially critical during the current crisis where public schools physically shut down, and if private schools can avoid this, it could lead to a striking learning inequality.

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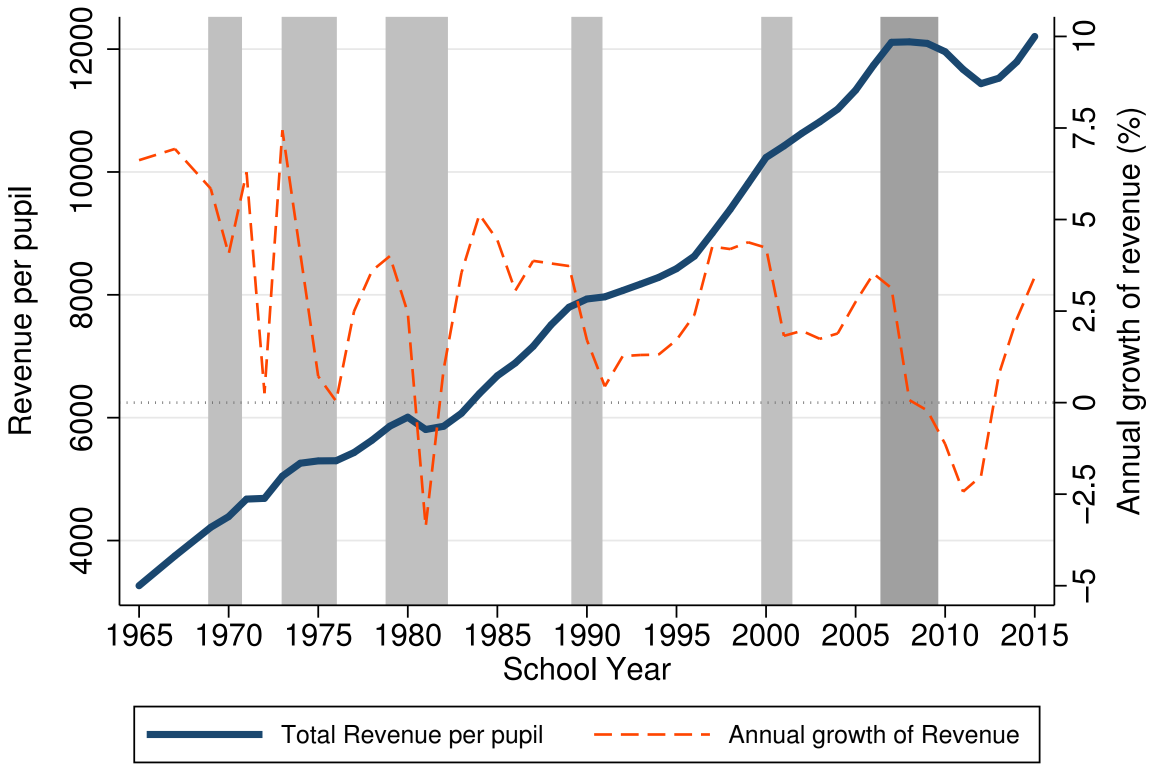
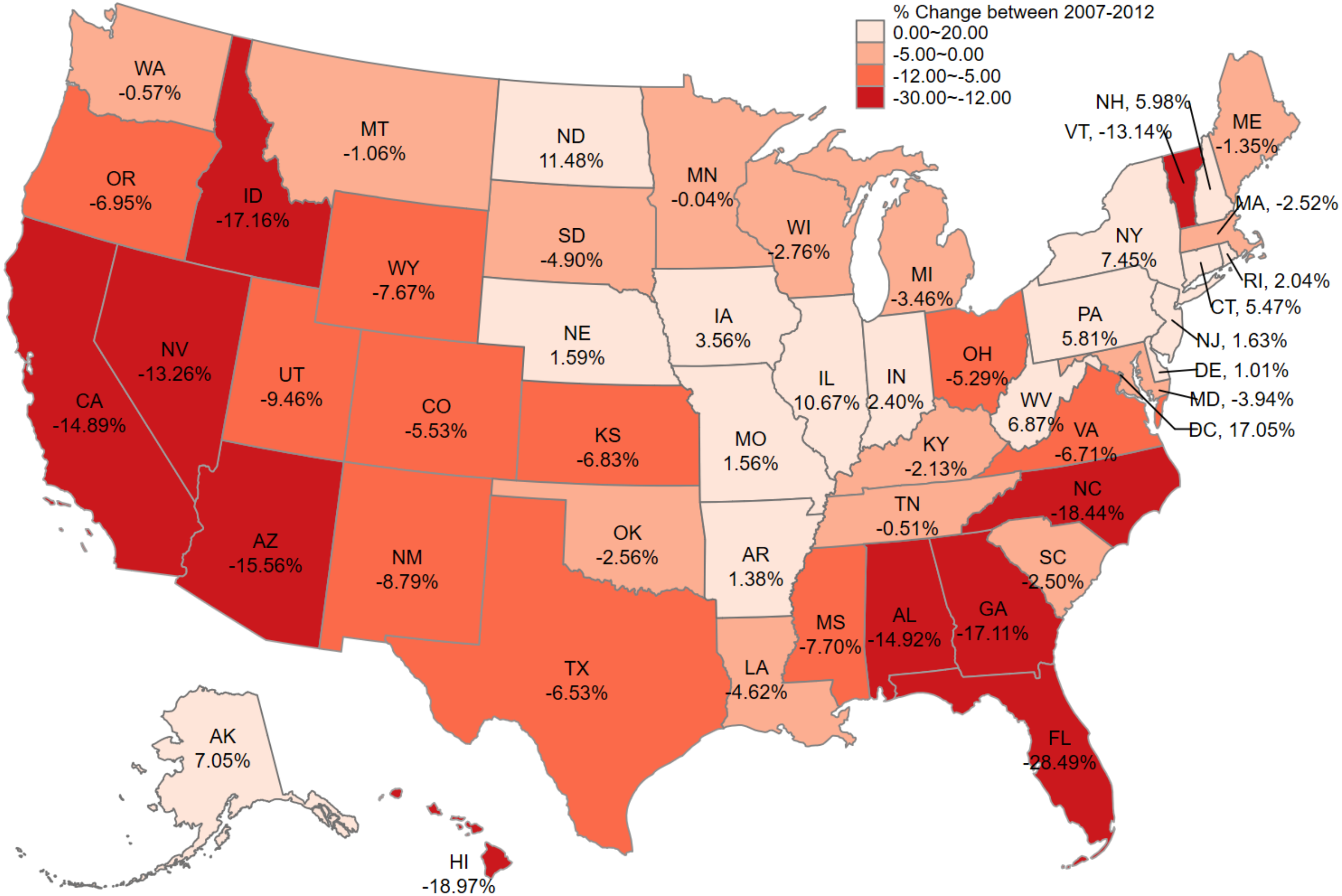
Figures

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



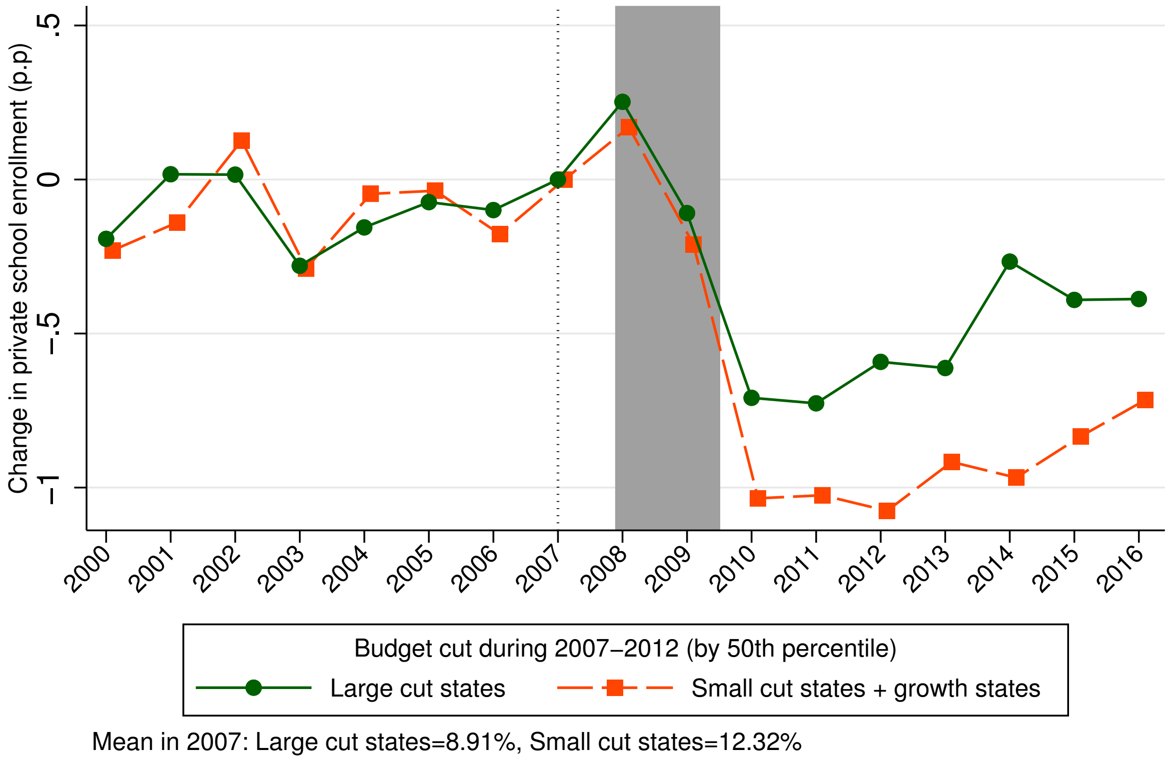
Notes: Data from the Common Core of Data (CCD) of the National Center for Education Statistics (NCES). Data aggregates the K-12 revenue in 50 states and divides by the full-time equivalent enrollment. The revenue per pupil is adjusted for inflation (in 2010 dollars). The orange dash line depicts the annual growth rate of the revenue 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 revenue per pupil decreases during or after recessions, and the Great Recession is followed by an unprecedented revenue cut that lasted for almost a decade.

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



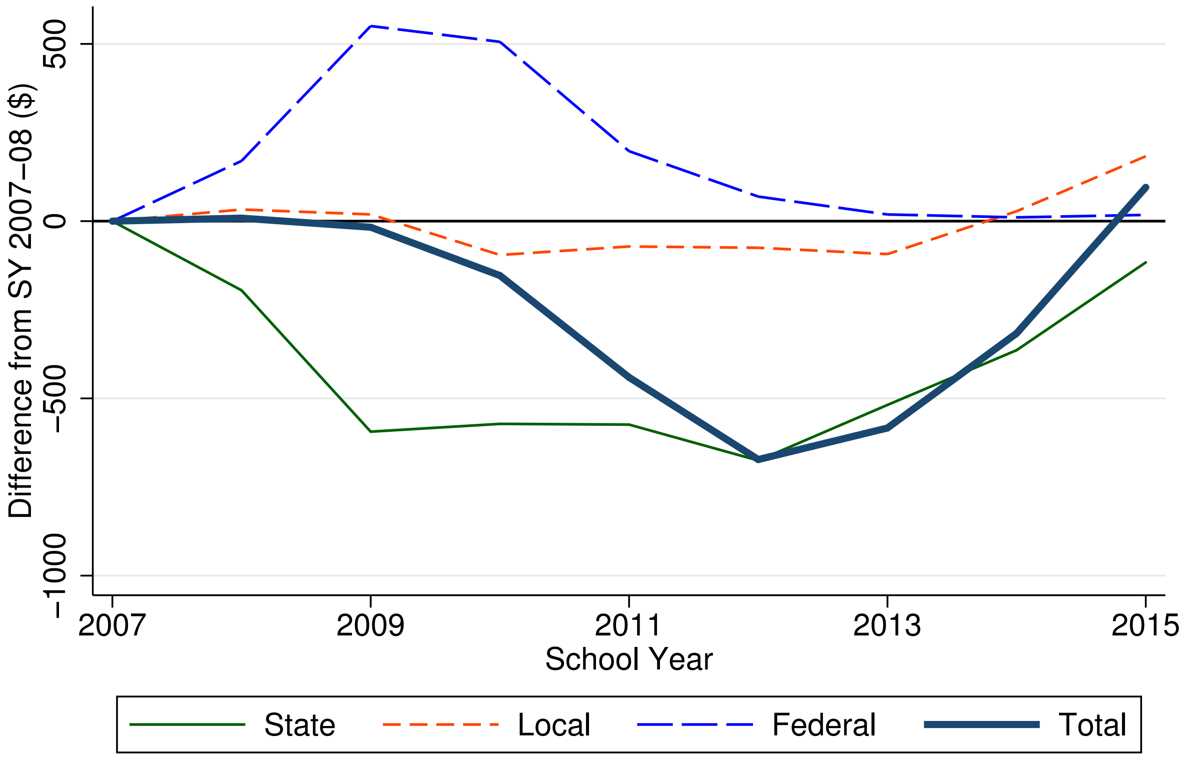
Notes: This figure shows the variation in funding cut across states induced by the Great Recession from 2007 to 2012. The percent change is calculated using real value of revenue per pupil in 2010 dollars. Darker shade means larger cuts and the 16 states with the brightest shade are states with growths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

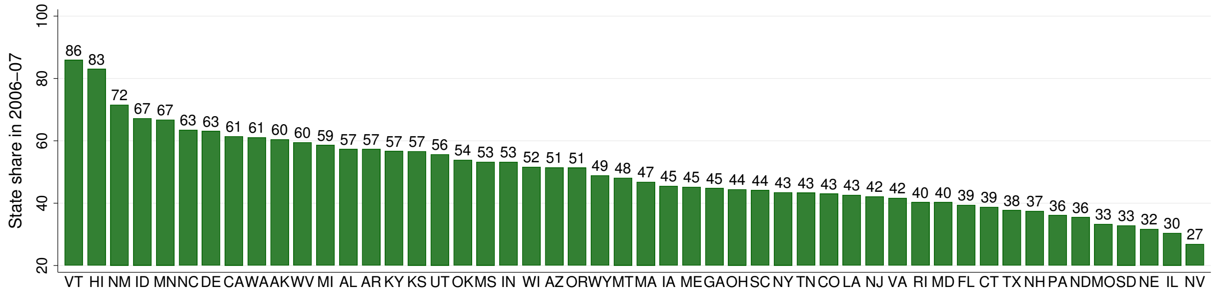
Figure 4: Trend of Revenue Compared to 2007, by Sources



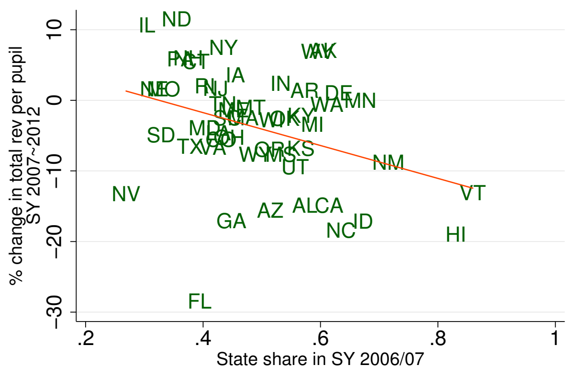
Notes: This figure shows the trend of the change in public education revenue by source. I calculate the dollar difference from the school year 2007-2008 level by sources to show how the budget had changed over time since the start of the Great Recession. All monetary values are in 2010 dollars.

Figure 5: Share of State Appropriations and Relation to Total Revenue and Funding Cut

(a) Variation in Share

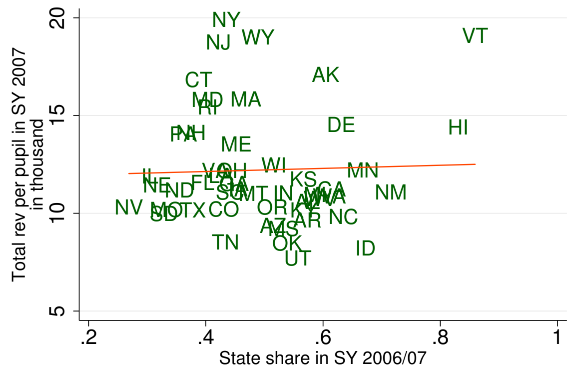


(b) Relation to Funding Cut



b = −23.29, se = 8.92

(c) Relation to Total Revenue

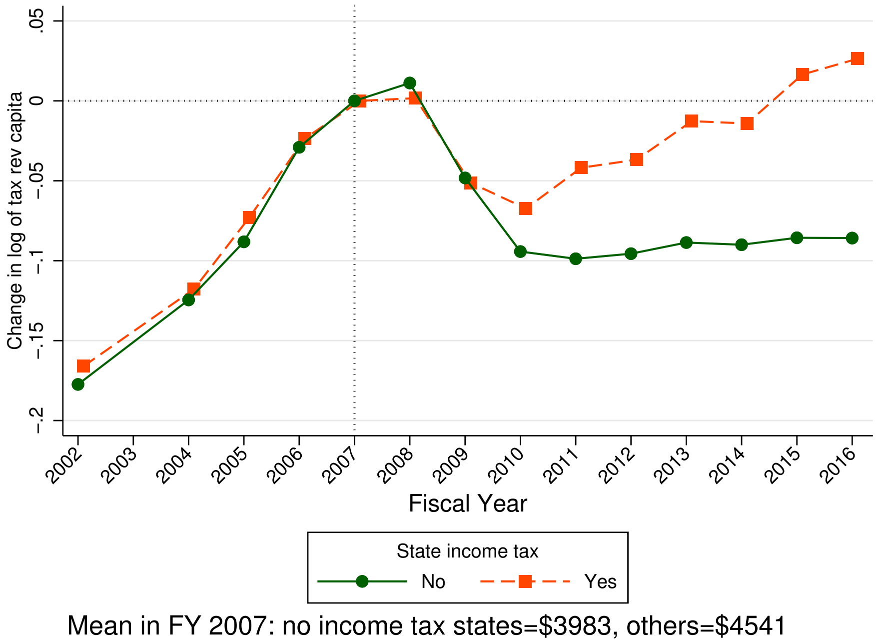


b = 797.88, se = 3368.00

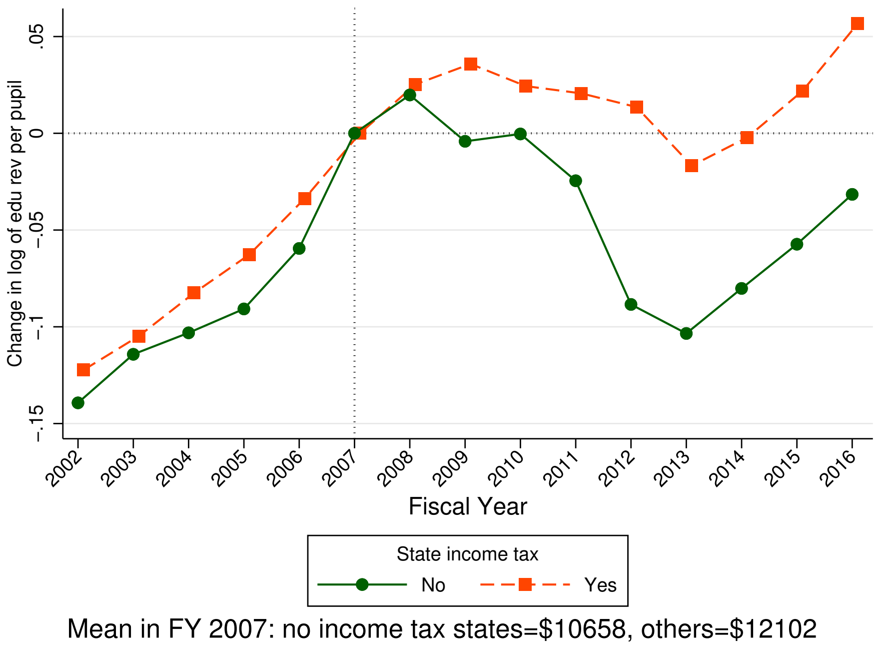
Notes: Panel A displays the variation in state share ($S_s = \frac{State\ rev}{Total\ rev}$) in SY 2006-2007, a year before the Great Recession in the 50 states. The numbers above the bars indicate the state share in 2006. Panel B shows the relationship between state share and total K-12 revenue per pupil before the Great Recession. Panel C shows a negative correlation between the state share in 2006 and the change in revenue per pupil in percent from 2007 to 2012. Coefficients and standard errors of the linear fitted values in Panel B and C are presented below each figure. All monetary values are in 2010 dollars.

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

(a) Real Tax Revenue (in 2010 dollars)

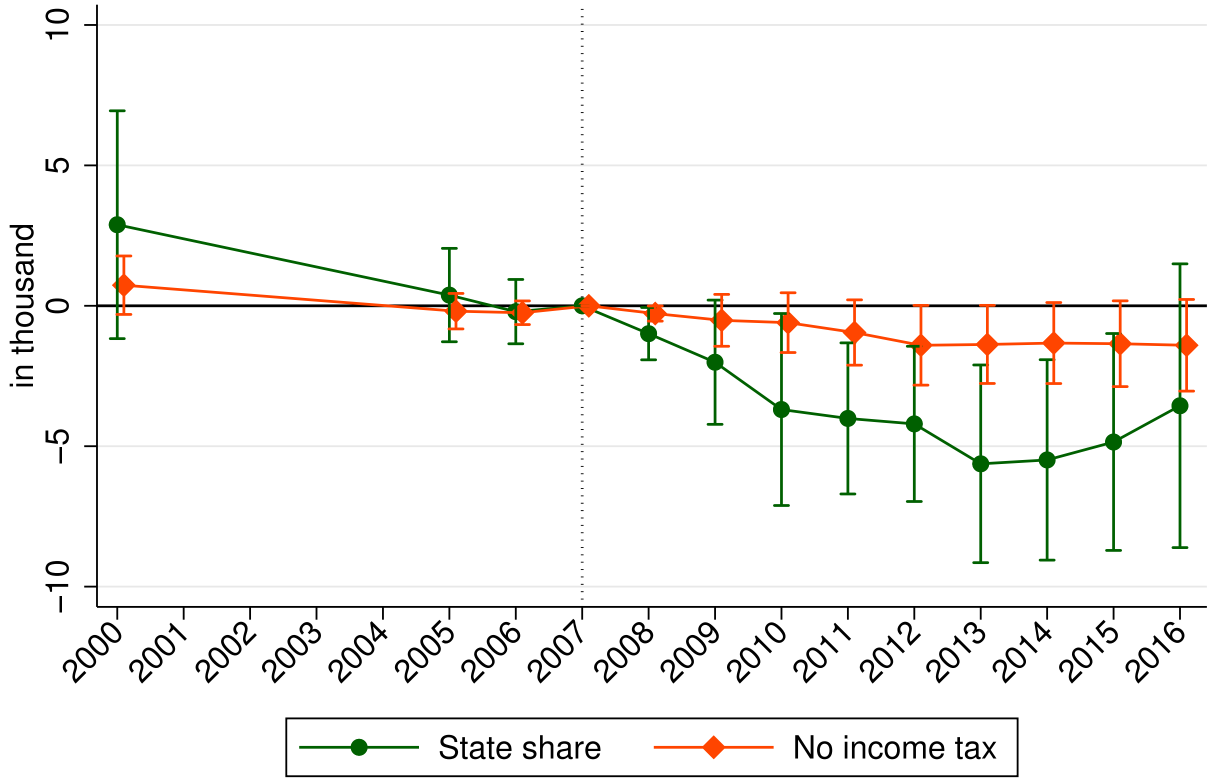


(b) Real K-12 Funding (in 2010 dollars)



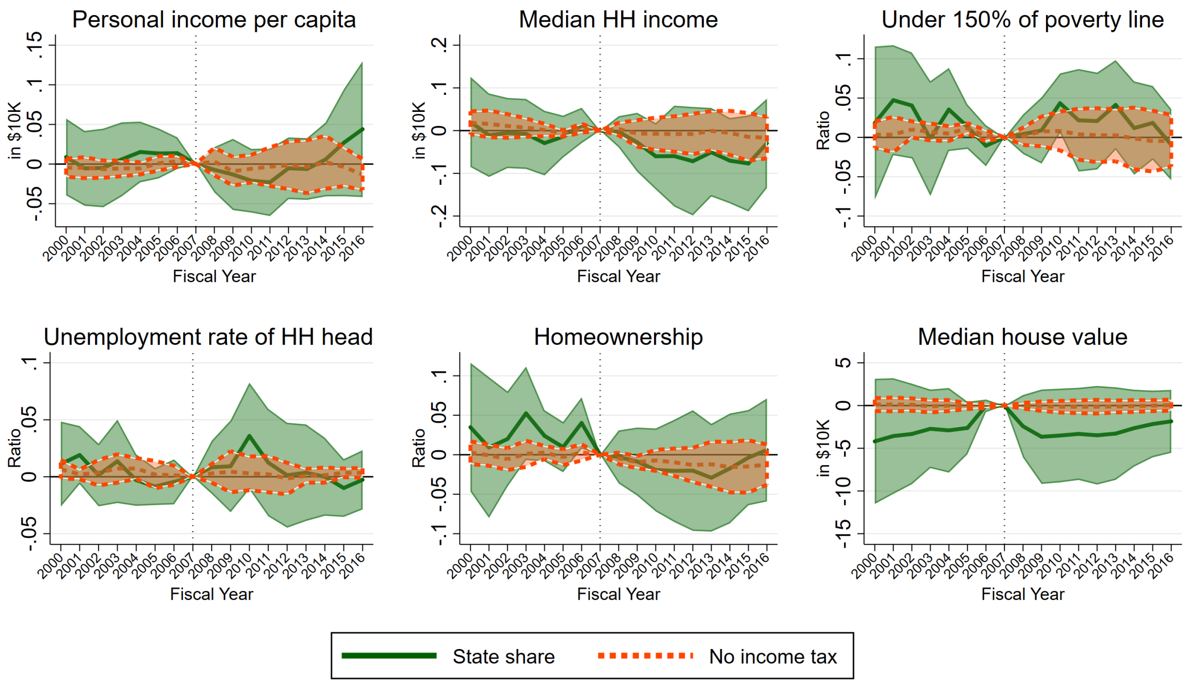
Notes: Panel A shows the trend of the mean tax revenue per capita relative to FY 2007 in two groups of states (states with and without an individual income tax). The mean of each group is the weighted mean with state population in 2000. Panel B shows the trend of mean K-12 funding per pupil relative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



Notes: N=7,744,432. 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. 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 3 for further information on the controls. Standard errors clustered at the state level. F-statistics for 24 excluded instrumental variables is 16.243. See Appendix Figure A.7 for the impact on state-level K-12 revenue per pupil, including 2001-2004. See Appendix Figure A.9 for other specifications. See Appendix Table A.2 for the table version of this figure.

Figure 8: Placebo Test: State and Household Characteristics



Notes: Personal income per capita from the Bureau of Economic Analysis. The other five variables are from the Census and ACS by aggregating the household level characteristics to the state-year level. I only include households with at least one school-aged children to only include relevant households. The solid green line represents the interaction terms between state share (S_s) and year dummies, while the orange dashed line does the interaction terms between no income tax indicator (NT_s) and year dummies. Shaded areas show 95 % confidence intervals calculated using standard errors clustered by the state level. All regressions are weighted with the school-aged population in each state. All monetary values are in 2010 dollars.

Tables

Table 1: Summary Statistics in the Pre-Recession Period

		Year≤2007		Year≥2008	
		Mean (1)	SE (2)	Mean (3)	SE (4)
Private school enrollment	in percent	10.61	[0.016]	10.08	[0.015]
Real revenue per pupil	Total	$11,139	[1.540]	$11,967	[1.860]
(in 2010 dollars)	State	$5,244	[0.972]	$5,462	[1.032]
	Local	$5,039	[1.452]	$5,448	[1.680]
	Federal	$856	[0.288]	$1,057	[0.259]
Composition of Revenue	Total	100%		100%	
	State	47.10%		45.64%	
	Local	45.26%		45.53%	
	Federal	7.69%		8.83%	

Notes: This table presents the mean and standard error of private school enrollment and public education revenue per pupil before and after the Great Recession. The sample for private school enrollment includes children who are not in school as well. The average education revenue per pupil by the funding source is displayed below with the composition. The private school enrollment rate decreased after the Great Recession, consistent with Figure 3. Total education revenue is larger in the post-period because it was increasing before the recession. All monetary values are in 2010 dollars.

Table 2: Placebo Test in 2SLS

	Personal Income per capita ($10K)	HH Income ($10K)	Under 150% Poverty	HH head Unemployed	Home Ownership	House Value ($10K)
	(1)	(2)	(3)	(4)	(5)	(6)
Rev per pupil	0.0791	-0.0306	0.0088	0.0040	-0.0894	-1.031
(in thousand)	(0.076)	(0.127)	(0.060)	(0.031)	(0.097)	(1.166)

Notes: N=850. Coefficients and standard errors are multiplied by 10,000 for display. The dependent variables of the regressions are indicated in the column title and defined in the state level. Unit of observation is state-year. Each entry is a coefficient from a separate 2SLS regression of the dependent variable on real K-12 revenue per pupil in the state (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 the schoolchildren population of the state in 2000. Robust standard errors are in parentheses clustered by state. First stage F-stat is 7.84 for all regressions. * 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)

	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)
Rev per pupil	-0.477***	-0.534***	-0.551***	-0.589***
(in thousand)	(0.173)	(0.172)	(0.177)	(0.177)
First stage F-Stat	23.20	23.24	23.11	16.24
Individual Controls		Yes	Yes	Yes
Household Controls			Yes	Yes
CPUMA Controls				Yes

Notes: N=7,744,432. This table reports the estimates of the impact of K-12 revenue 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 revenue per pupil is adjusted for inflation in 2010 dollars and scaled in $1,000. All specifications include students' age in the full set of dummy variables with CPUMA and year fixed effects and controls described in the table. The point estimate is interpreted as following: in column 4 (preferred specification), a $1,000 increase in revenue per pupil decreases private school enrollment by 0.589 percentage points. Individual controls include race, sex, number of siblings, and an indicator for limited English proficiency and foreign-born. Household controls include log of total household income, parental characteristics such as education, race, foreign-born indicator, and employment status, and the composition of parents (presence of both parents and same-sex parents). CPUMA controls include share of minority, foreign-born, under 150% of the poverty line in the CPUMA level and CPUMA median household income. 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
Rev per pupil	732.5***	477.9***	58.09	43.80***
(in thousand)	(77.28)	(71.02)	(68.03)	(15.55)
	9,248	*5,678*	*1,253*	*452*
Panel B. Expenditure per teacher				
	Salary	Employee Benefits		
Rev per pupil	1957	3404***		
(in thousand)	(1571)	(944.6)		
	62,944	*18,854*		
Panel C. Staff per 100 students				
	Teacher	Aides	Guidance Counselor	Library Staff
Rev per pupil	0.175**	0.207***	0.007	0.020
(in thousand)	(0.087)	(0.059)	(0.006)	(0.013)
	6.166	*1.313*	*0.200*	*0.151*

Notes: N=13,730. First stage F-stat = 11.18. Dependent variables defined at the CPUMA level are indicated above the point estimates. Each entry is a coefficient from a separate 2SLS regression of the dependent variable on real K-12 revenue per pupil in CPUMA (in thousands of 2010 dollars). 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. Regressions are weighted using the schoolchildren population of the CPUMA in 2000. Robust standard errors are in parentheses clustered by state. The sample includes only 2000 and 2005-2016 to match the main sample of the paper. Means of the dependent variables are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 5: Alternative Specifications and Samples

Dependent variable: private school enrollment(in percentage point)

		Alternative definition of state share and NT				Different measure of rev	
	Add CPUMA time trend	5-yr avg state share	2000 state share	1990 state share	Add NH,TN in NT states	State rev	Expenditure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rev per pupil	-0.600**	-0.597***	-0.564***	-0.596***	-0.654***	-0.488**	-0.630***
(in thousand)	(0.294)	(0.177)	(0.160)	(0.219)	(0.194)	(0.220)	(0.205)
First stage F-Stat	9.426	14.72	16.05	19.42	8.790	13.36	13.79
Observations	7,744,432	7,744,432	7,744,432	7,744,432	7,744,432	8,498,386	7,744,432

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. Column 1 includes a linear time trend of CPUMAs ($\eta_p \times t$). In column 2, I use the average state share from 2002 to 2006 instead of the state share in 2006. Columns 3 and 4 use state share in 2000 and 1990, respectively. In column 5, I add New Hampshire and Tennessee to no income tax states. Columns 6 uses state-level revenue, including 2001-2004 ACS as well. The estimate without 2001-2004 is -0.598(0.184). Column 7 uses realized expenditure instead of CPUMA-level appropriated funding. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 6: Selective Migration and Private School Enrollment

Dependent variable: private school enrollment(in percentage point)

	Migration status from last year			5yr+	Funding of
	Different CPUMA	Same CPUMA	Same house	Not moved	State of birth
	(1)	(2)	(3)	(4)	(5)
Rev per pupil	-0.662	-0.632***	-0.648***	-0.728***	-0.672***
(in thousands)	(0.531)	(0.224)	(0.238)	(0.248)	(0.236)
	7.88%	*10.75%*	*11.21%*	*12.68%*	*10.62%*
First stage F-Stat	5.860	11.58	12.08	11.37	8.752
Observations	185,230	5,188,968	4,785,526	3,209,403	7,297,042

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. 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 asked location 5 years ago. The main estimate without the 2000 Census is -0.629 (SE: 0.229). Each regression uses the subsample indicated in the title of each column. Area refers to Migration PUMA (MPUMA, the geographical unit the ACS uses to determine migration status), which resembles the 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. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 7: Private School Choice Policies and Impact of Public School Revenue

Dependent variable: private school enrollment(in percentage point)

	Private school choice program			Number of		
	Any policy	Voucher	Tax credit	Charter schools	Magnet Schools	All Public
	(1)	(2)	(3)	(4)	(5)	(6)
Rev per pupil	-0.604***	-0.615***	-0.601***	-0.630***	-0.576***	-0.602***
(in thousands)	(0.169)	(0.175)	(0.179)	(0.170)	(0.181)	(0.182)
First stage F-Stat	26.72	21.91	10.75	15.97	18.12	16.67

Notes: N=7,744,432. 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. Column 1 adds indicator for any statewide policy helping enrollment of private schools. In columns 2 and 3, I consider statewide voucher program and tax credit, respectively. Theses indicators are time variant as states differentially implement private school programs. Columns 4 to 6 include the number of charter schools, magnet schools, and all public schools as control variable, respectively. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 8: 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
Rev per pupil	-0.647***	-0.474***	-0.604***	-0.586***	-0.138
(in thousand)	(0.213)	(0.151)	(0.215)	(0.160)	(0.226)
	11.27%	*9.30%*	*13.35%*	*5.37%*	*5.94%*
First stage F-Stat	16.66	14.97	15.42	12.78	53.84
Observation	5,139,254	2,605,178	4,835,452	1,382,743	862,474
Panel B. By household income					
	Richest >90	90-75	75-50	50-25	Poorest <25
Rev per pupil	-0.242	-0.645**	-0.821***	-0.552**	-0.0942
(in thousand)	(0.282)	(0.250)	(0.250)	(0.206)	(0.203)
	22.53%	13.33%	*9.87%*	*6.85%*	*4.70%*
First stage F-Stat	10.97	14.95	13.79	9.463	16.56
Observation	1,058,362	1,538,369	2,170,024	1,690,188	1,287,489

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. 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. The percentile is defined within state and year. Thus, the 90th percentile means that a household is at the 90th percentile in the state and year when the household is observed. 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 9: 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						
Rev per pupil	-1.221***	-0.383**	-1.264***	-0.338*	-0.968***	-0.399**
(in thousand)	(0.353)	(0.183)	(0.331)	(0.200)	(0.267)	(0.191)
	14.16%	*13.49%*	*16.48%*	*12.16%*	*16.29%*	*11.89%*
p-value of column difference	<0.01		<0.01		0.017	
First stage F-Stat	16.87	7.604	11.84	15.22	5.056	14.23
Observations	1,810,104	2,956,651	1,562,443	3,204,312	1,735,320	3,031,435
Panel B. Low income households						
Rev per pupil	-0.458	-0.179	-0.550*	-0.117	-0.486**	-0.182
(in thousand)	(0.286)	(0.149)	(0.285)	(0.152)	(0.240)	(0.189)
	5.80%	*6.02%*	*5.90%*	*5.91%*	*6.17%*	*5.69%*
p-value of column difference	0.239		0.039		0.129	
First stage F-Stat	16.96	12.84	14.63	13.74	9.879	13.08
Observations	1,637,378	1,340,299	1,271,212	1,706,465	1,139,620	1,838,057
p-value of difference of panel A and B	0.014	0.392	<0.01	0.403	0.051	0.416

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 state average in 2000. Then, I divide each group by the household income percentile within state and display them in Panels A and B. Thus, each regional characteristic has four subgroups. The *p*-values of the difference in coefficients of same income group in high and low CPUMAs are presented it the bottom of each panel (column difference). *p*-values of the difference between different income groups in same area are also presented in 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%.

Appendix

A School District and CPUMA Crosswalk

The smallest geographical unit identifiable is the PUMA (Public Use Microdata Area) in the publicly available Census and ACS. For consistency, in 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. The Census and ACS aggregate PUMAs to Migration PUMAs (MPUMA) that resemble commuting or living zones and use them to identify the location lived a year earlier (if relocated) and each respondent's workplace location.

There are 15,000 to 16,000 school districts in the US and slightly more than 2,000 PUMAs. Matching school districts to PUMAs or CPUMAs is difficult because 1) school district boundaries 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 PUMAs 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, PUMAs are larger than the school districts and consist of several.

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 district into PUMA level
3. For PUMAs with no matched school district, use the average in the MPUMA level
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 revenue and expenditure and public school enrollment. Most of the PUMAs are matched to at least one school district. Panel A and B of Figure A.6 display the map of PUMA and school districts in the school year 2007-2008, respectively. In most cases, several school districts fit in a single PUMA. This is largely true for less populated areas such as the Mountain States. 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.[39] Then, I estimate the average of constitutive PUMAs weighted by population and construct CPUMA level finance information.[40]

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.

B Additional Robustness Checks

B.1 Balance Test on Other Expenditures in States

My identification strategy utilizes the variation in K-12 funding cut coming from two state-level variables. In this section, I test whether the identifying variation is correlated with spending in other government programs, as it can indirectly affect private school attendance. For example, if the state government cuts funding on cash assistance, some households may drop out of private schools because of the negative income effect.

In Figure A.10, I test six categories of expenditures: total, higher education, total health, Medicaid, cash assistance, and unemployment insurance. I collect the data from the Annual Survey of State and Local Government Finances (US Census Bureau (2020), through Urban Institutes), Medicaid expenditure reports from MBES/CBES (Centers for Medicare and Medi-

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

[40] 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.

caid Services), and official unemployment insurance budget data (US Department of Labor). Similar to Figure 8, I display the event study results. All of the monetary values are in 2010 dollars and normalized with the state's total population (i.e., expenditure per capita). The state expenditure includes both expenditure of both state and local government, excluding intergovernmental transfers.

In the first panel, I show the impact on total expenditure per capita. The no income tax indicator is marginally correlated with a decrease in total spending as the tax revenue declines (Figure 6). The total expenditures are less sensitive because other state revenues and intergovernmental transfer act as a buffer. Other panels do not suggest a decline in expenditure. Although not perfect, this figure supports that change in government expenditures is unrelated to change in private school enrollment.

B.2 Permutation Test for No Income Tax States

In the first stage in section IV.3, I show that states without an income tax have experienced a larger budget cut after the Great Recession. Florida and Nevada are known to be two states with a sharp drop in property value during the Great Recession, which could confound the private school choice results as well. Also, it may be that other (unobserved) common characteristics of the seven states result in slower tax recovery after the Great Recession, not necessarily the income tax status.

I conduct a placebo test as additional evidence that it is not based on spurious correlation. First, I randomly assign seven states to no state income tax states. I then re-estimate the first stage and record the F-statistics. I do this process 1,000 times and compare the re-estimated F-statistics with the original F-statistics. If the original F-statistics is located at the tail of the distribution, we can reject the hypothesis that the first stage is based on a spurious correlation. Figure A.11 shows the cumulative distribution function of the 1,000 F-statistics of the first stage. I display the F-statistics of the original first stage (16.9) together in the figure. The figure shows that the original first stage lies in the tail of the distribution, within the top 3% of the distribution. This test resolves the question of the spurious relationship between no income tax states and education funding cuts.

B.3 Alternative Sample, Instrumental Variables, and Lagged Revenue

Alternative samples—In Table A.5, I estimate the impact of education revenue 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 almost identical to the main model. In column 2, I restrict the sample to children currently in school and get a very similar result. Columns 3 and 4 compare native-born and foreign-born students and find that the impacts are larger for native students, although not statistically different.

Next, in columns 5 to 8, 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. Removing these two states does not change the result much. Next, I remove the two largest states among no income tax states, Florida and Texas. The point estimate declines by one third, although it is not statistically different. I suspect removing these two states from the sample reduces the point estimates because they are two of the largest immigrant-receiving states. The impact is weaker in areas with a high share of immigrants, affecting the coefficient in column 6. In column 7, I remove California from the sample because some of California's state revenue comes from locally raised property tax. California's Proposition 98 guarantees a minimum amount of education funding from the state's General Funds and local property taxes. Thus, California's state revenue is less sensitive to the business cycle because a portion of it comes from stable property tax. Excluding California only slightly increases the point estimates. Finally, I exclude Alaska in column 8 because Alaska not collects neither income nor sales tax.[41] Columns 7 and 8 are both not statistically different from the main estimate. Finally, I remove the top 10 percent CPUMAs in private school enrollment in 2000 in columns 9 to test whether the impact is concentrated in certain areas with high access to private schools. Point estimate in column 9 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.[42]

[41] Some local government collects local sales tax in Alaska. Most of Alaska's tax revenue comes from natural resources

[42] Impact of funding cuts is stronger in high baseline private school enrollment CPUMAs. Results available upon request.

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 an indicator for no income tax state interacted with year indicator, taking 2007 as the base year. I consider that 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 (e.g., Figure 7).

In Panel A of Table A.6, I test whether my results stay consistent with the choice of instrumental variables. In column 1 of Panel A of Table A.6, 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 0.11 percentage points. This could be 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, the point estimate gets smaller and loses statistical power. In column 3, the coefficient is larger when I use the no income tax indicator as to the sole identifying variation. Two columns show that the impact of education revenue driven the by no income tax indicator is stronger than the state share, and the main specification captures the 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 even deeper education funding cut if two variations strengthen each other. The first stage F-statistics explodes with the interaction term's inclusion; however, the point estimates are closer to column 2. None of the point estimates in Panel A is statistically different from the main estimate.

Lagged revenue—I use the lagged value of K-12 revenue per pupil in Panel B. 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 ($Rev_{t-1}, Rev_{t-2}, Rev_{t-3}$), respectively. The first stage F-statistics is reasonably smaller than the main result and decreases over the column as I use more lagged value. The point estimates in columns 1 to 3 are smaller than the main impact of K-12 revenue per pupil; however, they are still large and statistically significant. When using the average of the past three years of revenues, the point estimate is almost identical to the main specification. Overall, results in Panel B suggest the "exposure" to funding cut is as important as the current level of funding.

C Additional Heterogeneity Analysis

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

This section compares how the heterogeneity by CPUMA characteristics in Table 9 differs across races. I redo the analysis in 9 separately by race in Table A.8. Panel A, B, C, and D present the results for all races, whites, Hispanics, and blacks, respectively.

Table 9 shows a larger impact in low SES areas for both high and low-income households. Panel A of Table A.8 shows this is indeed true without dividing the sample by household income. The overall results for whites in Panel B are not different from those in Panel A. All of the point estimates are statistically significant and stronger, where it is in the main results. 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 -0.35 percentage points increase in private school enrollment in CPUMAs with a low minority population, the point estimate is -1.6 percentage points in CPUMAs with high minority population, which is much higher 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 strengthens the white flight from public schools. White students have a stronger preference for private schools when they attend schools with a larger concentration of non-white schoolchildren (Brunner, Imazeki and Ross, 2010), and therefore they switch to private schools more easily when the quality of the schools declines.

The patterns are slightly different for other races. The overall pattern—stronger in low SES areas—is found for Hispanics as well; however, the difference is not as striking as whites. It may be because Hispanics are more likely to be impoverished and immigrants and belong to the minority category as well. It may be that these characteristics do not particularly make Hispanics' preference for private schools stronger. In Panel D, I present the results for blacks. As the overall impact of the K-12 budget for blacks is small and statistically insignificant in Table 8, none of the point estimates in Panel D is statistically significant.

C.2 Heterogeneity in Effect by Parental Characteristics

Studies like Barrow (2002) and Goldring and Phillips (2008) suggest the importance of parental characteristics on school choice. In Table A.9, I compare the impact of educational revenue 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. The results show that there is no heterogeneity in effect by these parental characteristics. The point estimates in columns 1, 3, 5, and 7 are not statistically different from columns 2, 4, 6, and 8, respectively.

It is interesting that parental characteristics do not affect the competition between public and private schools like regional characteristics. High SES parents have a stronger preference for private schools (Goldring and Phillips, 2008), so they should be more sensitive to the funding cut, based on the discussion in section VII. However, the heterogeneity by regional characteristics may cancel out this. Because households sort themselves according to their characteristics and preference, there is a large correlation between individual characteristics and regional characteristics. They may have a stronger preference for private schools, but they tend to live in high SES regions where the overall impact of funding cuts is weaker.

C.3 Heterogeneity in Effect by School Type

Religious affiliation is important private school characteristics when parents consider private schools (Goldring and Phillips, 2008). Hispanics have an especially high preference for Catholic schools, so it would be useful to explore whether Hispanic students leave for Catholic schools because of funding cuts. Also, the average tuition reasonably varies by religion. 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 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. This section shows that Catholic schools have received the most students because of the K-12 revenue shock.

In this section, I look into heterogeneity in effect by the religious affiliation of private schools using an alternative data source. The Private School Universe Survey (PSS) from the NCES is a biennial survey targeting all private schools in the US, containing school level enrollment and characteristics.[43] To do so, I estimate the following equations:

$$N_{ipst} = \beta Rev_{pst} + P_{pst}\gamma + \theta_t + \alpha_i + \varepsilon_{ipst}, \tag{3}$$

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

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, Rev_{pst} is total K-12 revenue per pupil in CPUMA p where the school locates.[44] 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.[45]

Table A.10 presents the estimation results. In column 1, the point estimate means that a $1,000 increase in local public education revenue per pupil reduces private school enrollment by 5.8 students. The point estimate stays stable for the inclusion of CPUMA controls. In column 3, I estimate the impact on different types of schools. While the impacts on other religious and nonsectarian schools are smaller and insignificant, it is much stronger for Catholic schools. The enrollment decreases by 18 students with a $1,000 increase in per-pupil revenue in local public schools, which is statistically different from other religious schools (-2.46) and nonsectarian schools (-1.36).

In columns 4 and 5, I test whether this holds for white and Hispanic students. The pattern is similar in column 4 for white students, but not for Hispanics in column 5. However, over 30 percent of schools do not have any Hispanic students, and the median of Hispanic enrollment is only 3. Also, Hispanics tend to be concentrated in some regions, so it is not logical to include those schools in CPUMAs with very few Hispanics. In column 6, I restrict the sample to schools in Hispanic-concentrated CPUMAs (above the national mean). The results are consistent with column 3. Although the point estimates are much smaller, it is large in percent because of the small baseline enrollment. Overall, the result for Hispanics is consistent with the discussion in section VII that Hispanics may have switched to Catholic private schools.

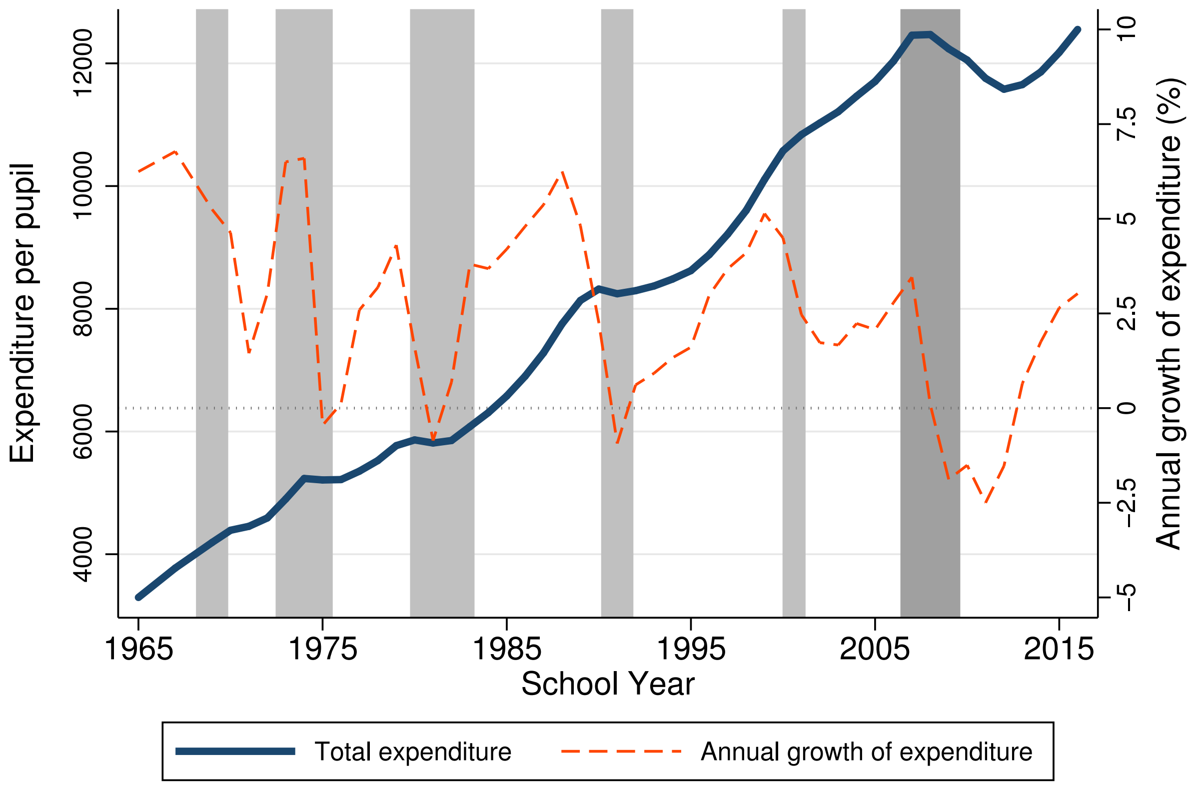
[43] All private schools are in the universe; however, the actual number interviewed depends on the response rate which is on average over 90%.

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

[45] In the analysis, I exclude schools only with ungraded class or whose highest grade offered is pre-Kindergarten level.

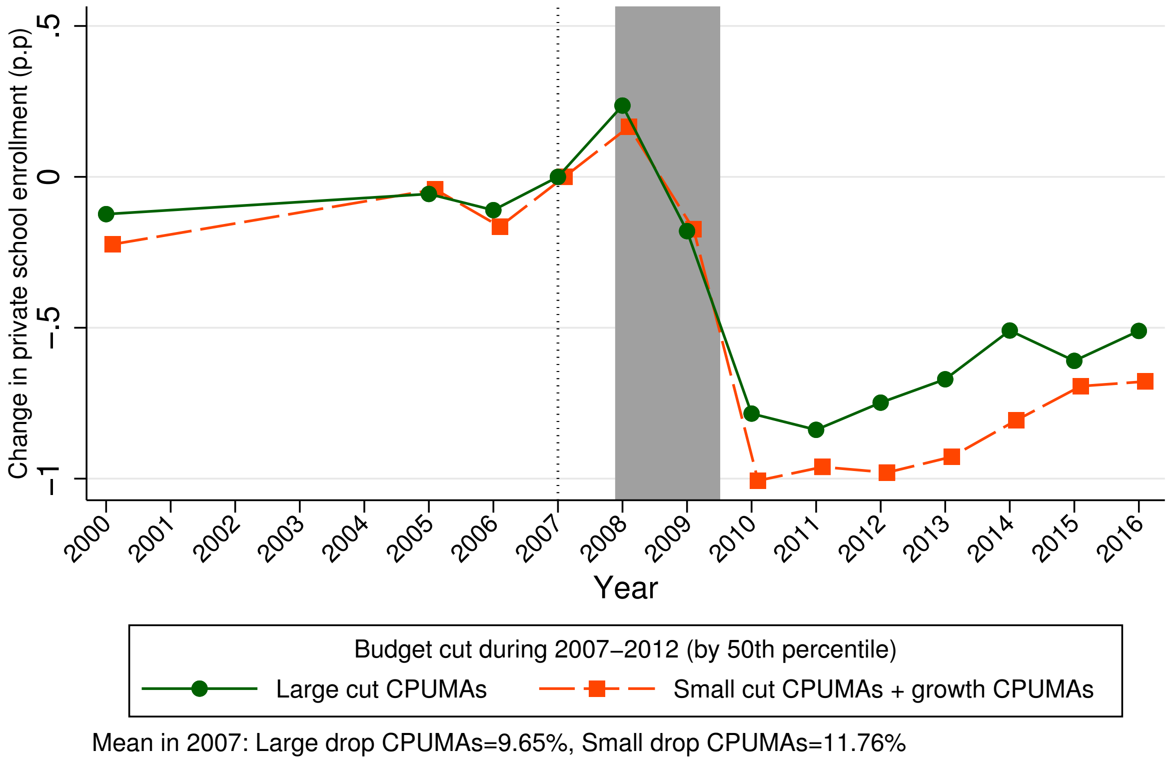
Appendix Figures

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



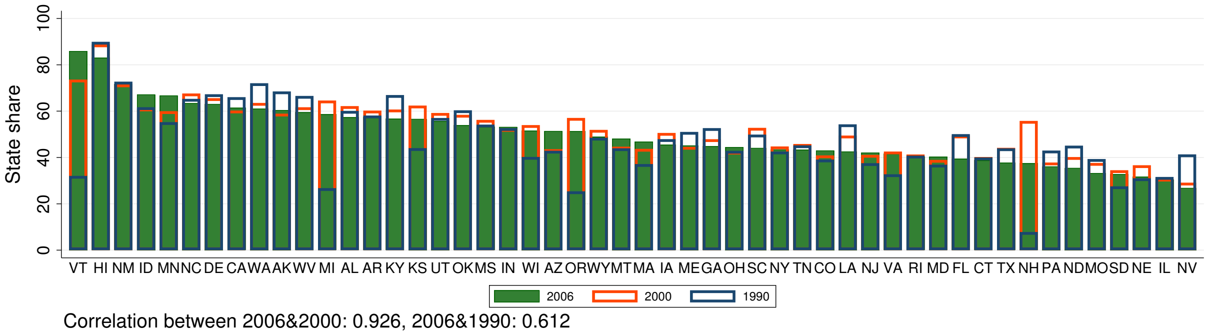
Notes: This figure plots the trend of expenditure per pupil instead of revenue. All other details are the same in Figure 1.

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



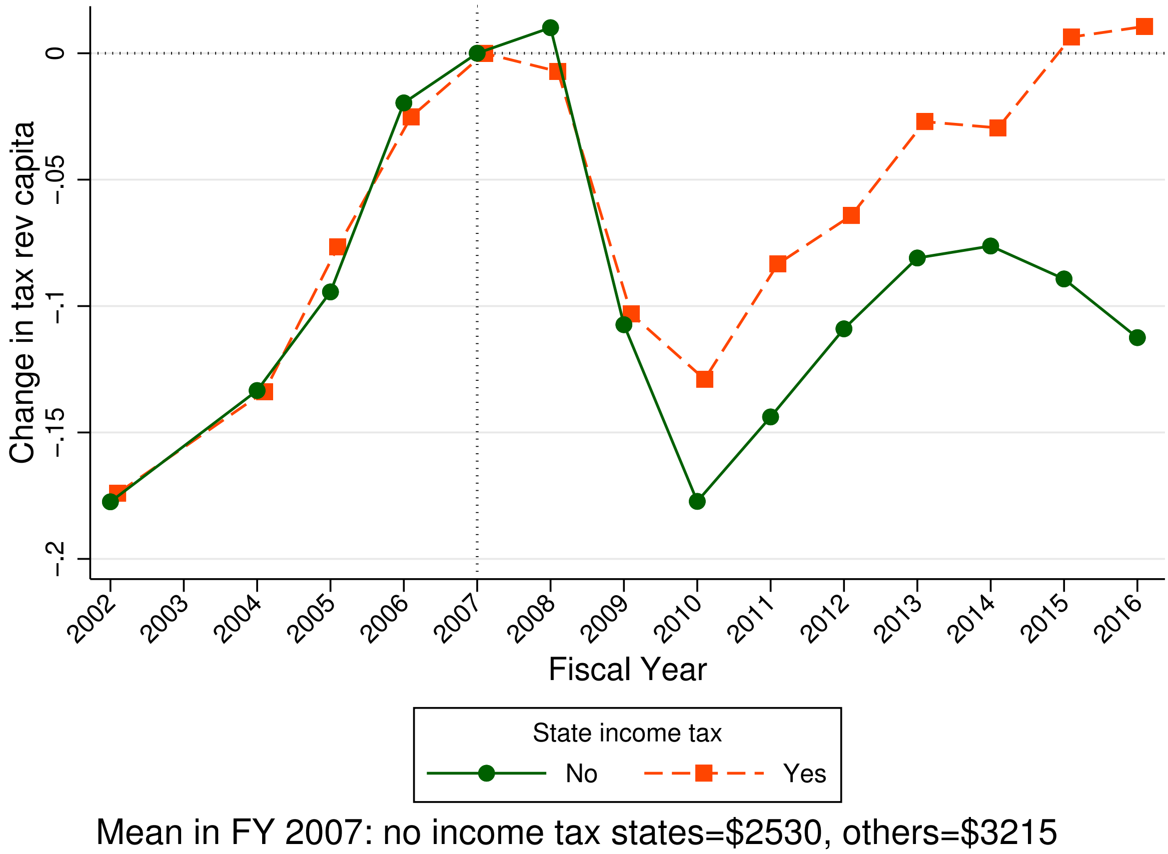
Notes: This figure shows the trend in private school enrollment by large and small budget cut in CPUMA. The mean of private school enrollment is normalized with the value in 2007. All other details same to Figure 3.

Figure A.3: State Share in SY 2006, 2000, and 1990



Notes: This figure compares the state share in 2006 (solid green), 2000 (oragne), and 1990 (navy). The correlation of the shares between 2000 and 2006 is very high—over 0.9. The correlation is weaker between 2006 and 1990, 0.61, but it goes up to 0.75 when comparing ranks.

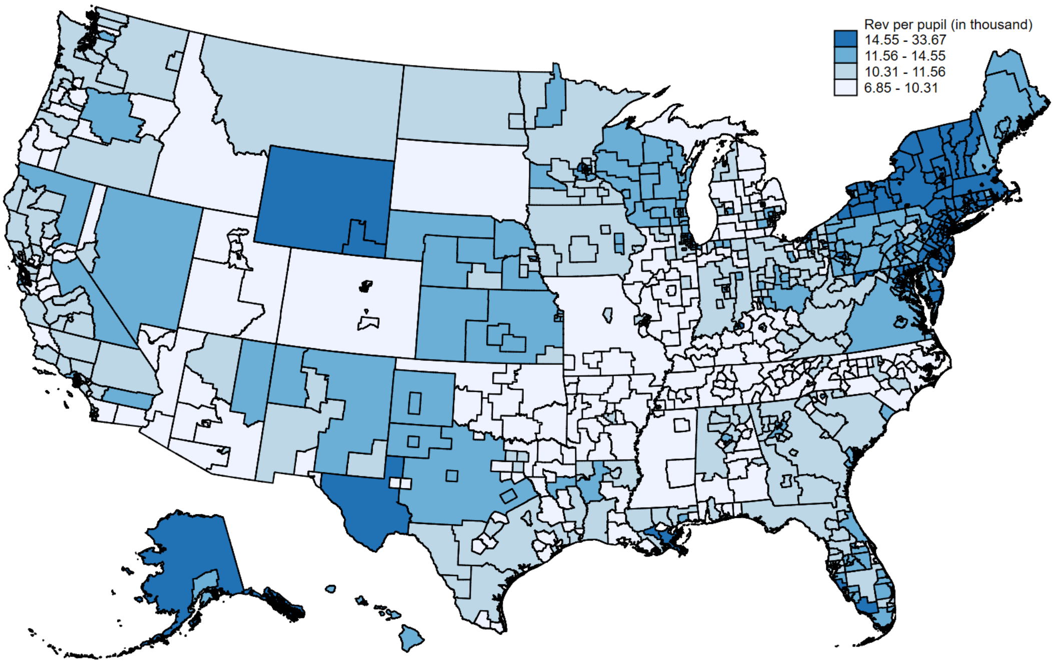
Figure A.4: Trend of Tax Revenue, Property Tax Excluded



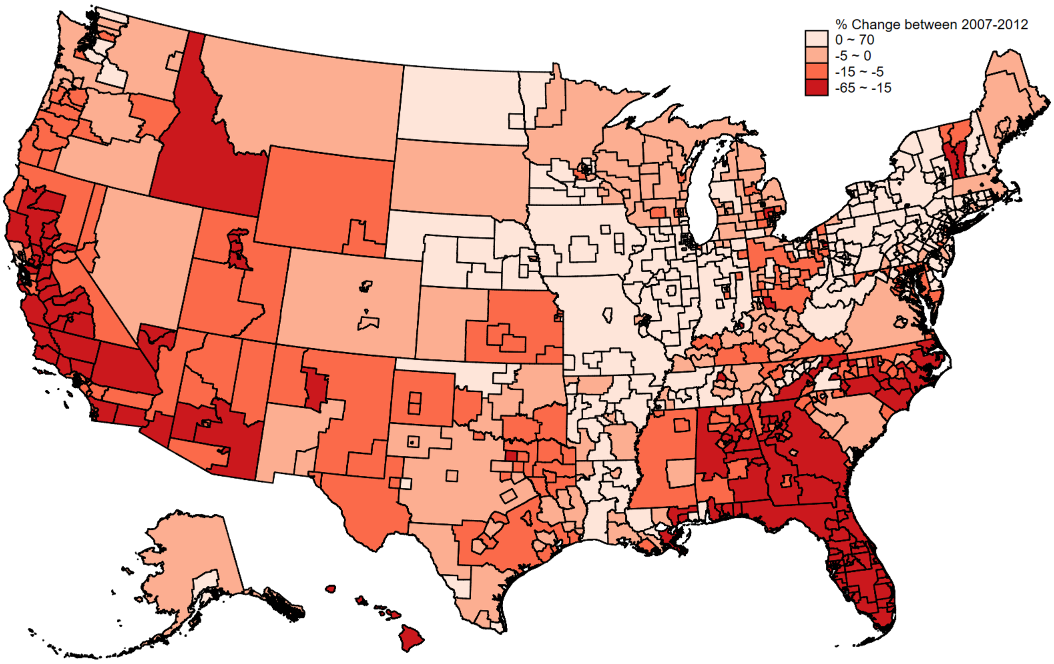
Notes: This figure replicates Figure 6 but excludes property tax revenue.

Figure A.5: K-12 Revenue Per Pupil in CPUMAs in SY 2007

(a) K-12 Revenue Per Pupil in CPUMAs, SY 2007-2008



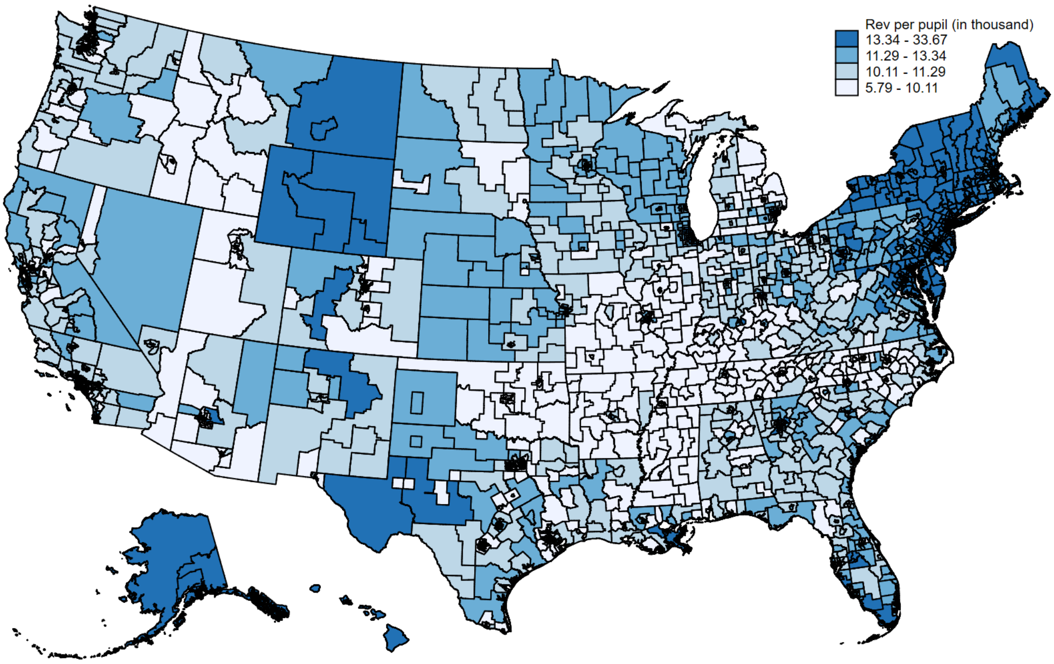
(b) Change in K-12 Revenue Per Pupil in CPUMAs, 2007-2012



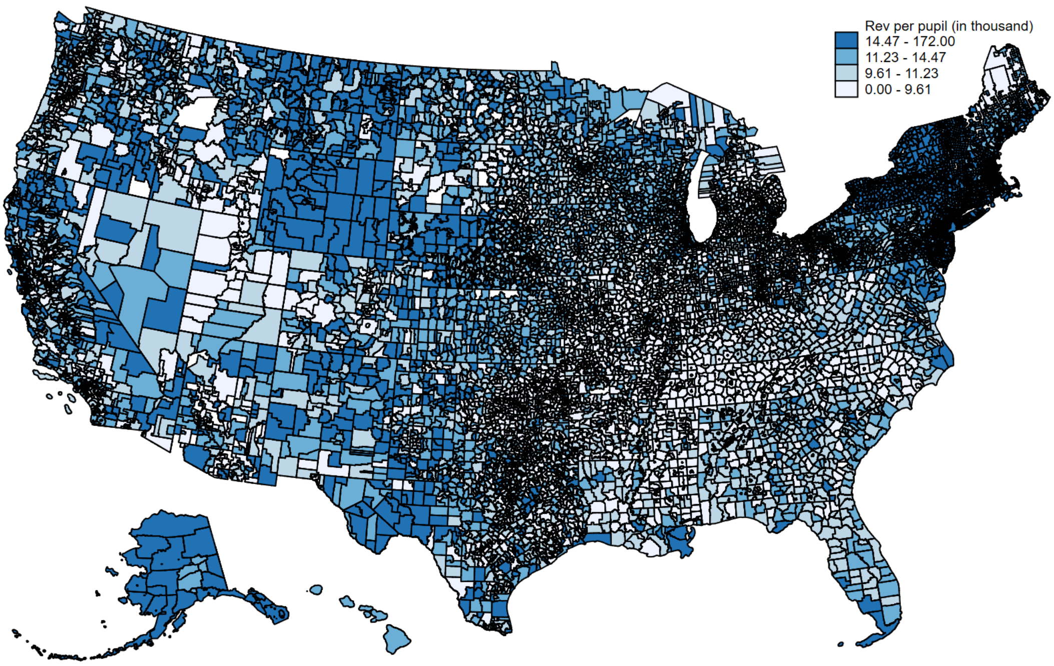
Notes: Panel A shows the K-12 revenue per pupil in CPUMAs in SY 2007-2008. Panel B displays the percent change during 2007-2012. The figures are obtained by matching school districts to each CPUMA.

Figure A.6: Rev per pupil in PUMAs and school districts in SY 2007-2008

(a) Rev per pupil in PUMA

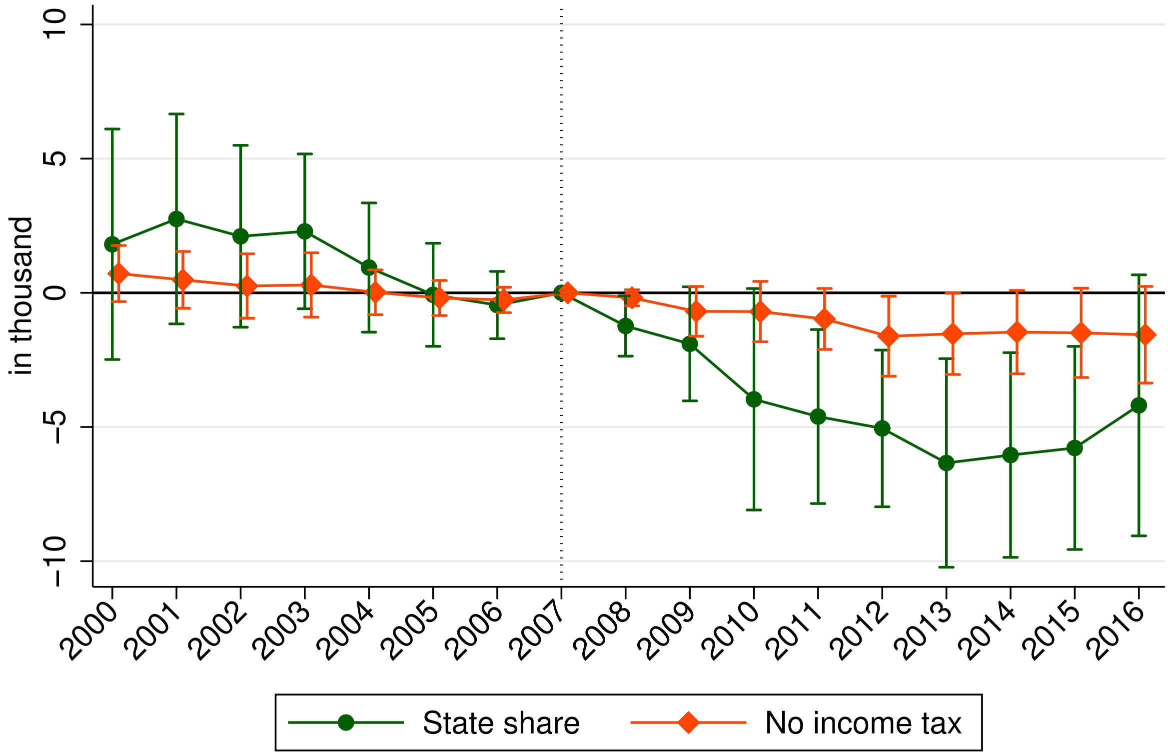


(b) Rev per pupil in school district



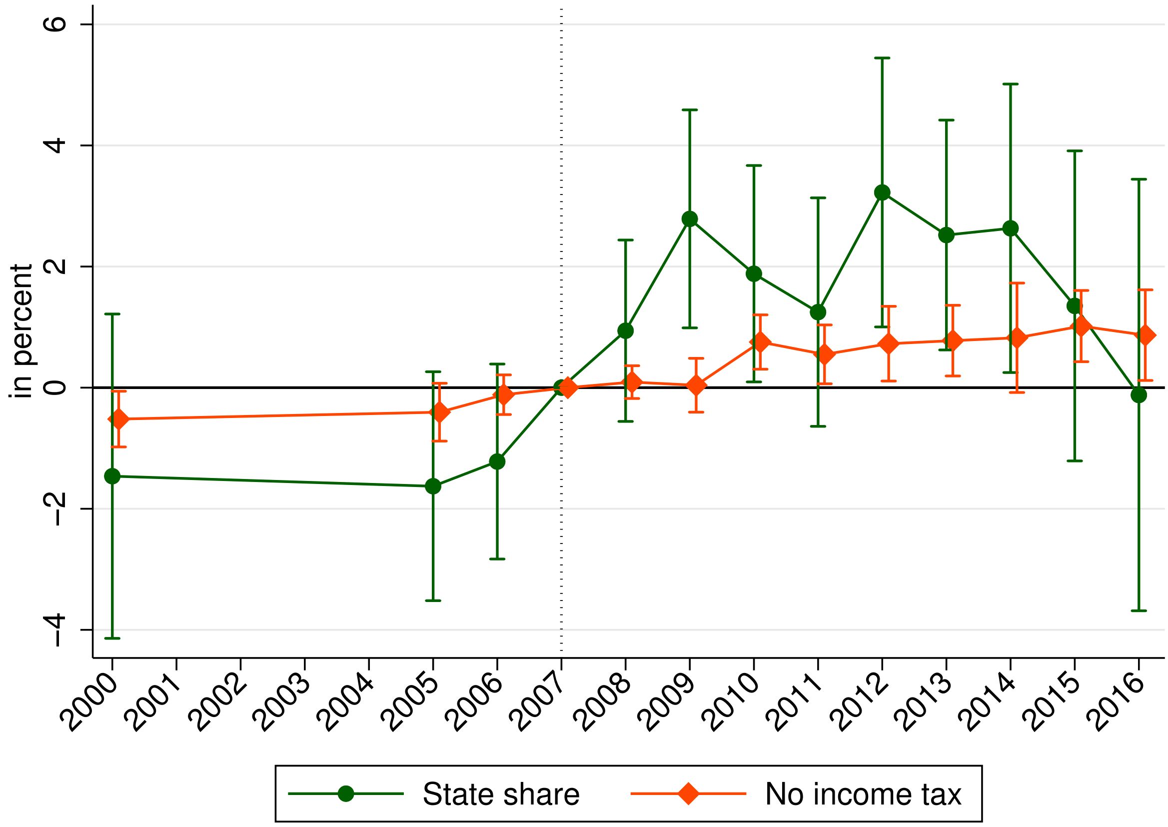
Notes: This figure shows the K-12 revenue per pupil by PUMA (panel A) and school district (panel B) in SY 2007-2008. Panel A is obtained by matching school districts to each PUMA.

Figure A.7: Impact on State Level Education Funding for All Years



Notes: Using state level K-12 funding data, I construct this first stage figure to include the year of 2001-2004. The estimated equation is: $Rev_{st} = \sum_{k \neq 2007} [\ \beta_k S_s \times \mathbb{1}(k = t) + \gamma_k NT_s \times \mathbb{1}(k = t)]\ + \rho_s + \tau_t + \varepsilon_{st}$. F-statistics for the event study variables is 30.89.

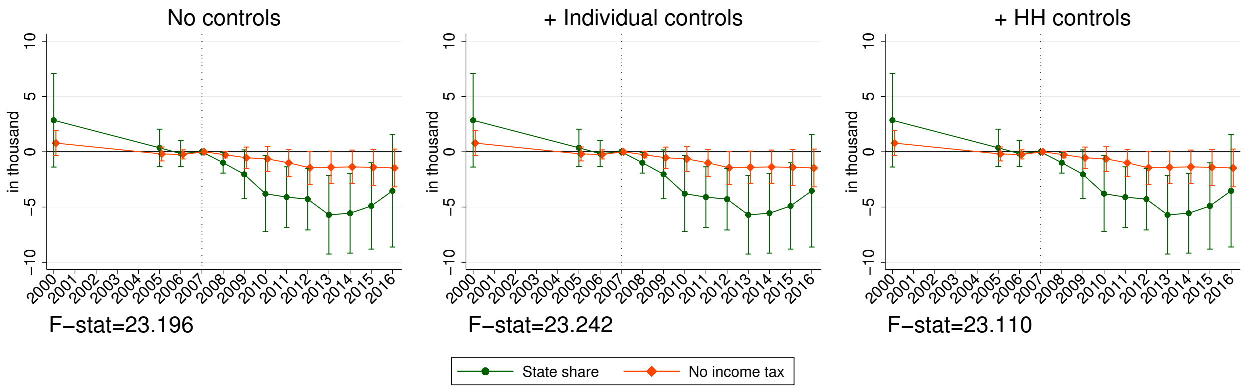
Figure A.8: Reduced Form Result



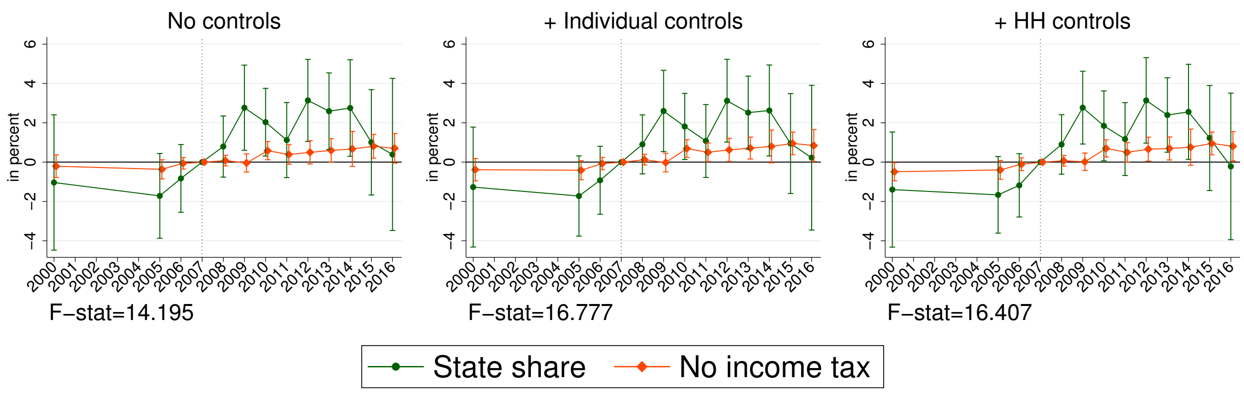
Notes: N=7,744,432. The reduced form result in the most preferred specification (including full sets of controls) is presented in this figure. I display the coefficients of interaction terms of year dummies and state share, and income tax status (β_k's and γ_k's) along with 95% confidence intervals. The coefficients are rescaled to represent private school enrollment in percentage points. 2001-2004 ACS are excluded from the sample because CPUMA is not identified in these years. F-statistics for the event study variables is 15.294. See the notes of 3 for further information on the controls. Regression weighted using the Census and ACS sample weights. Standard errors clustered at the state level.

Figure A.9: Frist Stage and Reduced Form in Different Specifications

(a) First Stage: Revenue Per Pupil

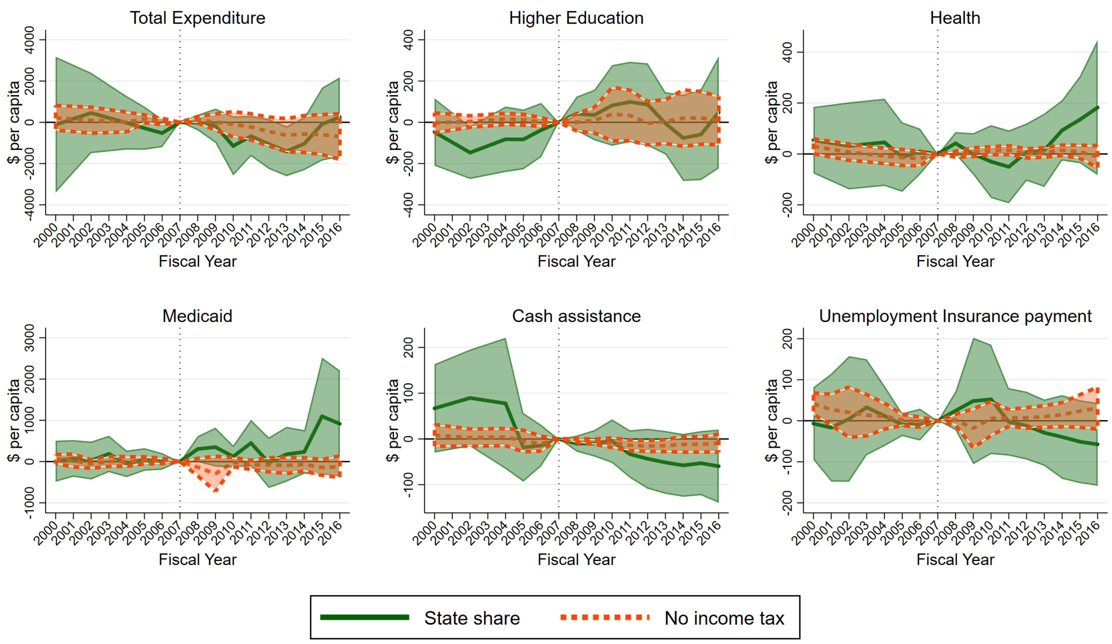


(b) Reduced form: Private School Enrollment



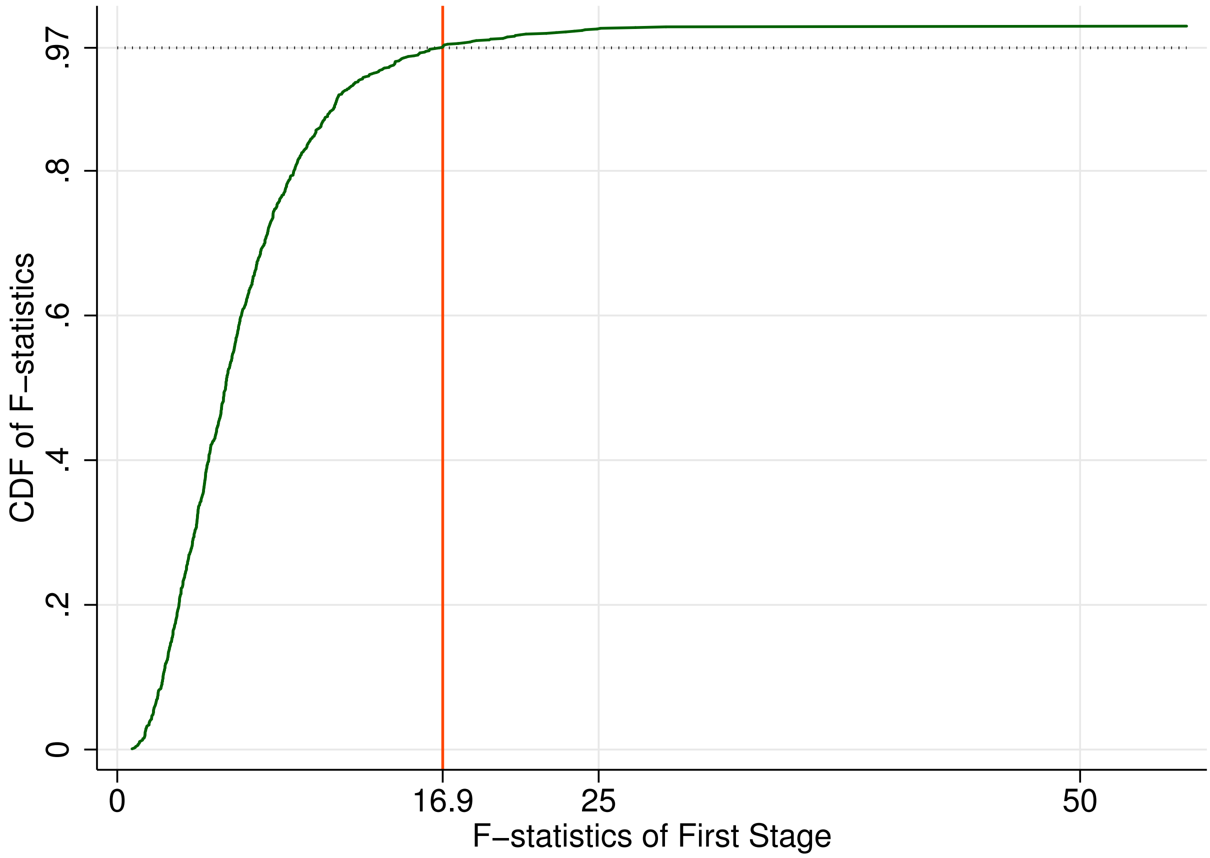
Notes: This figure shows the first stage (Panel A) and reduced form (Panel B) in various specifications. Each figure displays the coefficients for the interaction terms of state share (green dots) and no income tax indicator (orange diamonds) with the year dummies along with 95% confidence intervals calculated using standard errors clustered by the state level. Column 1 includes no control variables. Column 2 adds individual controls, and column 3 includes household controls as well. 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-statistics of the event study variables is presented below each figure.

Figure A.10: Placebo Test: State Expenditure Per Capita



Notes: All monetary values are in 2010 dollars. All variables are normalized with the total population of the state. 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), Medicaid expenditure reports from MBES/CBES (Centers for Medicare and Medicaid Services), and official unemployment insurance budget data (US Department of Labor). The expenditures include those of both state and local governments.

Figure A.11: Permutation Test and F-statistics of First Stages



Notes: This figure shows the cumulative density function of F-statistics of the first stages of 1,000 randomizations with the main specification. The first stage is estimated with the most preferred specification after randomly assigning seven states to no income tax states. The red line (16.9) represents the F-statistics of the first stage of the main analysis. The *p*-value is under 0.01 as well. The F-statistics falls in the tail of the distribution, supporting the validity of the empirical strategy.

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: Rev 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	2.853	0.786	2.853	0.786	2.854	0.786	2.888	0.734
	(2.106)	(0.557)	(2.106)	(0.557)	(2.106)	(0.557)	(2.018)	(0.518)
Instrument × 2005	0.360	-0.196	0.361	-0.196	0.360	-0.196	0.382	-0.191
	(0.839)	(0.314)	(0.839)	(0.314)	(0.839)	(0.314)	(0.827)	(0.314)
Instrument × 2006	-0.163	-0.239	-0.163	-0.239	-0.163	-0.238	-0.207	-0.247
	(0.585)	(0.204)	(0.584)	(0.204)	(0.584)	(0.204)	(0.570)	(0.209)
Instrument × 2008	-0.986**	-0.270*	-0.986**	-0.270*	-0.987**	-0.270*	-0.992**	-0.276**
	(0.469)	(0.140)	(0.469)	(0.140)	(0.468)	(0.140)	(0.464)	(0.134)
Instrument × 2009	-2.040*	-0.539	-2.040*	-0.539	-2.039*	-0.539	-2.009*	-0.517
	(1.099)	(0.481)	(1.099)	(0.481)	(1.099)	(0.481)	(1.101)	(0.460)
Instrument × 2010	-3.790**	-0.643	-3.791**	-0.643	-3.790**	-0.642	-3.693**	-0.600
	(1.712)	(0.561)	(1.712)	(0.561)	(1.712)	(0.560)	(1.701)	(0.530)
Instrument × 2011	-4.099***	-1.001	-4.099***	-1.001	-4.099***	-1.001	-4.011***	-0.951
	(1.362)	(0.614)	(1.362)	(0.614)	(1.362)	(0.614)	(1.339)	(0.578)
Instrument × 2012	-4.282***	-1.446*	-4.282***	-1.445*	-4.282***	-1.445*	-4.205***	-1.405*
	(1.388)	(0.745)	(1.388)	(0.745)	(1.387)	(0.745)	(1.375)	(0.706)
Instrument × 2013	-5.703***	-1.403*	-5.703***	-1.403*	-5.703***	-1.403*	-5.626***	-1.375*
	(1.765)	(0.730)	(1.765)	(0.730)	(1.765)	(0.730)	(1.752)	(0.692)
Instrument × 2014	-5.557***	-1.367*	-5.557***	-1.366*	-5.558***	-1.366*	-5.488***	-1.326*
	(1.796)	(0.756)	(1.796)	(0.756)	(1.796)	(0.756)	(1.776)	(0.718)
Instrument × 2015	-4.900**	-1.405*	-4.900**	-1.405*	-4.901**	-1.405*	-4.849**	-1.351*
	(1.942)	(0.807)	(1.942)	(0.807)	(1.942)	(0.806)	(1.922)	(0.760)
Instrument × 2016	-3.532	-1.462*	-3.533	-1.462*	-3.533	-1.462*	-3.559	-1.406*
	(2.529)	(0.851)	(2.529)	(0.851)	(2.528)	(0.851)	(2.514)	(0.812)
F-stat of excluded IVs	23.2		23.24		23.11		16.24	
Individual Controls			Yes		Yes		Yes	
Household Controls					Yes		Yes	
CPUMA Controls							Yes	

Notes: N=7,744,432. The identifying variation is indicated at the top of each column. The number on the top of the column indicates the specification. 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 omitted. 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: Main Results in OLS and Logs

Dependent variable: private school enrollment (in percentage point)				
	(1)	(2)	(3)	(4)
Panel A. OLS results				
Rev per pupil	-0.087*	-0.112**	-0.122***	-0.132**
(in thousand)	(0.046)	(0.0522)	(0.0442)	(0.0503)
Panel B. OLS with log of revenue per pupil				
ln(Rev per pupil) × 100	-0.0099	-0.0136*	-0.0155**	-0.0173**
	(0.0065)	(0.0071)	(0.0065)	(0.0072)
Panel C. 2SLS with log of revnue per pupil				
ln(Rev per pupil) × 100	-0.0648***	-0.0722***	-0.0746***	-0.0798***
	(0.0237)	(0.0243)	(0.0251)	(0.0259)
First stage F-Stat	23.26	23.29	23.24	17.75
Individual Controls		Yes	Yes	Yes
Household Controls			Yes	Yes
CPUMA Controls				Yes

Notes: N=7,744,432. Each entry is a coefficient from 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 the Table 3. Panels B and C show the result using 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. The instruments for Panel C are the sets of interaction terms of state share and no state income tax status with year indicators dummies. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.4: Alternative Mechanism: Number of Schools

Dependent variable: Number of schools	TPS (1)	Charter (2)	Magnet (3)	All Public (4)	All Private (5)
Rev per pupil	-0.0108	-0.0047	-0.0099	-0.0255*	0.0138
(in thousand)	(0.0185)	(0.0033)	(0.0082)	(0.0130)	(0.0141)
First stage F-Stat	20.52	20.52	20.52	20.52	4.453
Observation	18,324	18,324	18,324	18,324	8622

Notes: Each entry is a coefficient from separate 2SLS regressions. Dependent variable is the number of each type of schools indicated in the column title. 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. CPUMA level control variables are also included. Regressions are weighted using the school-aged population in CPUMA. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.5: Alternative Samples

Dependent variable: private school enrollment(in percentage point)

	Include DC	Drop Dropouts	Native Only	Immigrant Only	Drop FL & NV	Drop FL and TX	Drop CA	Drop AK	Drop top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rev per pupil	-0.601***	-0.597***	-0.604***	-0.408**	-0.541***	-0.396**	-0.636***	-0.574***	-0.400**
(in thousand)	(0.175)	(0.178)	(0.184)	(0.162)	(0.180)	(0.165)	(0.177)	(0.175)	(0.187)
	10.62%	*10.87%*	*10.91%*	*6.10%*	*10.60%*	*10.93%*	*10.62%*	*10.62%*	*9.29%*
First stage F-Stat	16.32	15.73	15.83	18.17	24.73	17.85	13.99	17.39	15.42
Observations	7,754,355	7,590,031	7,316,582	427,850	7,275,774	6,678,736	6,783,015	7,723,844	7,076,346

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. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.6: Alternative Instrumental Variables and Lagged Revenue

Dependent variable: private school enrollment (in percentage point)

	(1)	(2)	(3)	(4)
Panel A. Alternative IV				
	Diff-in-diff	State share only	NT only	Add interaction
Rev per pupil	-0.700***	-0.436*	-0.779**	-0.474***
(in thousand)	(0.256)	(0.251)	(0.387)	(0.126)
First stage F-Stat	4.655	4.059	9.312	>1,000
Observations	7,744,432	7,744,432	7,744,432	7,744,432
Panel B. Using lagged revenue per pupil				
	1-year lag	2-year lag	3-year lag	3-year average
Rev per pupil	-0.524***	-0.478**	-0.464**	-0.559***
(in thousand)	(0.174)	(0.181)	(0.187)	(0.179)
First stage F-Stat	9.424	9.573	5.501	10.32
Observations	7,744,432	7,744,432	7,744,432	7,744,432

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. In Panel A, I use four alternative instrumental variables. 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. Panel B uses the lagged variables of CPUMA education revenue per pupil. Columns 1-3 use $Rev_{t-1}, Rev_{t-2}, Rev_{t-3}$, respectively. In column 4, I use cumulative average of past 3 years of education revenue per pupil. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.7: 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(Rev per pupil)	0.141	-0.579	0.0764
	(0.579)	(0.560)	(0.657)

Notes: N=11,807. Each entry is a coefficient from separate 2SLS regressions of the private school enrollment on the log of real K-12 revenue per pupil in MPUMA (in 2010 dollars). The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. State fixed effects and MPUMA level controls are included. Regressions are weighted using the MPUMA population. Robust standard errors are in parentheses clustered by state. First stage F-statistics is 9.421 for all regressions. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.8: Heterogeneity by PUMA 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						
Rev per pupil	-0.870***	-0.360**	-0.914***	-0.303*	-0.798***	-0.366**
(in thousand)	(0.294)	(0.147)	(0.293)	(0.151)	(0.259)	(0.150)
	9.70%	*11.32%*	*11.29%*	*10.17%*	*11.95%*	*9.72%*
P-value of difference	0.038		0.012		0.041	
First stage F-Stat	20.30	8.289	13.39	14.82	6.101	13.70
Observations	3,447,482	4,296,950	2,833,655	4,910,777	2,874,940	4,869,492
Panel B. White						
Rev per pupil	-0.862***	-0.379*	-1.583***	-0.349*	-1.071***	-0.369*
(in thousand)	(0.320)	(0.207)	(0.461)	(0.203)	(0.237)	(0.198)
	13.03%	*13.55%*	*17.70%*	*11.74%*	*17.42%*	*11.43%*
P-value of difference	0.086		>0.01		>0.01	
First stage F-Stat	20.15	12.77	10.44	19.50	5.702	12.44
Observations	1,955,679	2,879,773	1,206,624	3,628,828	1,403,979	3,431,473
Panel C. Hispanic						
Rev per pupil	-0.669**	-0.415***	-0.648***	-0.320	-0.673***	-0.317**
(in thousand)	(0.283)	(0.122)	(0.228)	(0.200)	(0.198)	(0.145)
	5.14%	*5.63%*	*5.68%*	*4.96%*	*5.57%*	*5.10%*
P-value of difference	0.395		0.317		0.066	
First stage F-Stat	30.64	65.19	39.45	50.37	65.38	26.86
Observations	711,825	670,918	773,712	609,031	773,076	609,667
Panel D. Black						
Rev per pupil	-0.0644	-0.119	-0.176	0.00686	-0.0103	-0.217
(in thousand)	(0.252)	(0.244)	(0.252)	(0.221)	(0.264)	(0.221)
	5.91%	*5.98%*	*6.59%*	*4.66%*	*6.78%*	*5.22%*
P-value of difference	0.792		0.441		0.417	
First stage F-Stat	12.82	20.47	13.67	19.66	10.59	39.88
Observations	517,426	345,048	558,071	304,403	379,930	482,544

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 divided into two groups by CPUMA characteristics presented in each column's title, like Table 9. Each panel is separately estimated by races. See the notes of Table 9 for the other details.
 * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.9: 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)
Rev per pupil	-0.624***	-0.513***	-0.640**	-0.529***	-0.615***	-0.383**	-0.469***	-0.672***
(in thousand)	(0.197)	(0.152)	(0.275)	(0.176)	(0.196)	(0.164)	(0.168)	(0.207)
	12.16%	6.29%	18.92%	6.60%	12.62%	5.44%	8.41%	11.28%
p-value of difference	0.491		0.641		0.186		0.154	
First stage F-Stat	13.88	16.53	13.87	16.70	14.76	16.84	23.96	12.94
Observations	5,849,114	1,895,318	2,763,933	4,980,499	3,305,703	4,438,729	1,747,092	5,997,340

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 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. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table A.10: Impact on Number of Enrolled Students

Dependent variable: Enrolled students in private school

	All Races			Whites	Hispanics	Hispanics in High share CPUMA
	(1)	(2)	(3)	(4)	(5)	(6)
Rev per pupil in CPUMA	-5.792*	-5.459*				
	(3.342)	(3.072)				
Rev per pupil in CPUMA × Catholic			-18.21***	-15.58***	-0.324	-1.259*
			(2.475)	(3.927)	(0.628)	(0.738)
Rev per pupil in CPUMA × Other Relig			-2.456	4.085**	-0.384	-0.345
			(2.421)	(1.916)	(1.086)	(1.211)
Rev per pupil in CPUMA × Nonsectarian			-1.365	3.265	-0.221	-0.123
			(1.744)	(2.052)	(1.178)	(1.385)
CPUMA Controls		Yes	Yes	Yes	Yes	Yes
N	170658	170658	170658	170658	170658	103710

Notes: I use 2001-2015 Private School Universe Survey (NCES) in this table. The unit of observation is school-year. The independent variable of interest is the public K-12 revenue per pupil in the CPUMA at which the school is located. The instruments are the sets of interaction terms of state share and no state income tax status with year indicators dummies. School fixed effects are included in all regressions. Columns 1-3 estimate the impact on school-level enrollment for all races. Column 4 examines white enrollment and 5 and 6 Hispanics. Especially, I only include schools in Hispanic concentrated CPUMA (share of Hispanics above 50th percentile) in column 6. See notes of Table 3 for further information on the control variables. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.