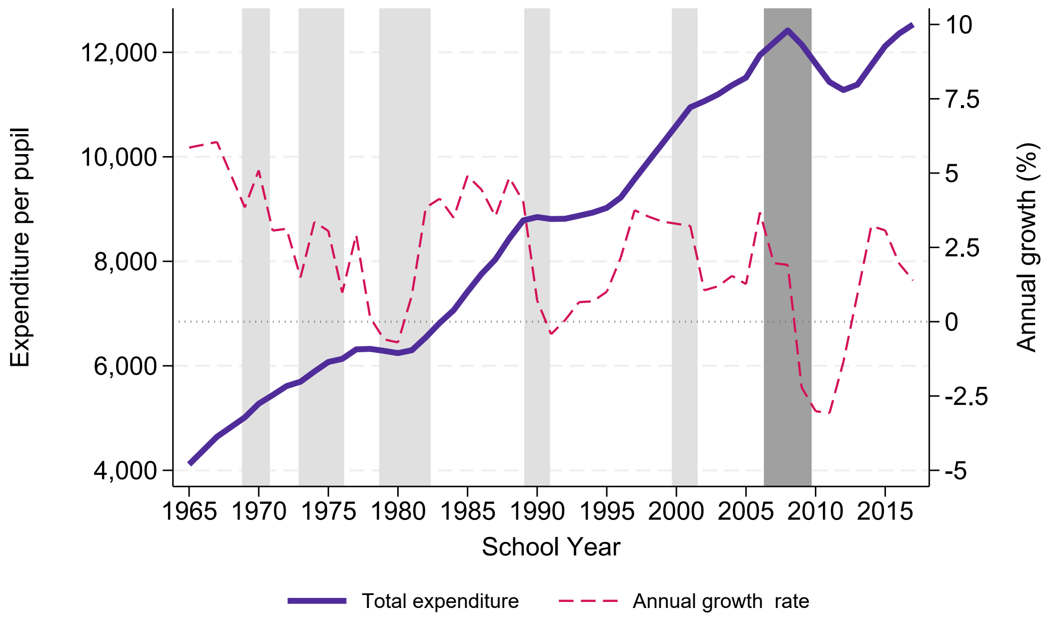
Online Appendix

A Additional Figures and Tables

Figure A.1: Total K-12 Expenditure per Pupil and Growth Rate



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

Figure A.2: State Share in SY 2007/2008

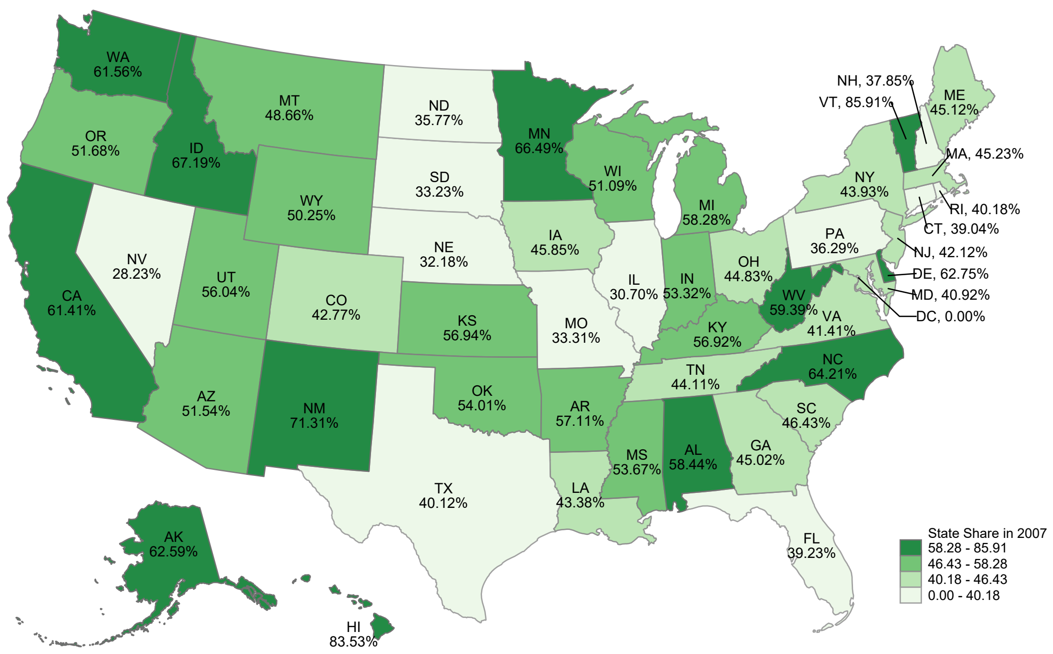
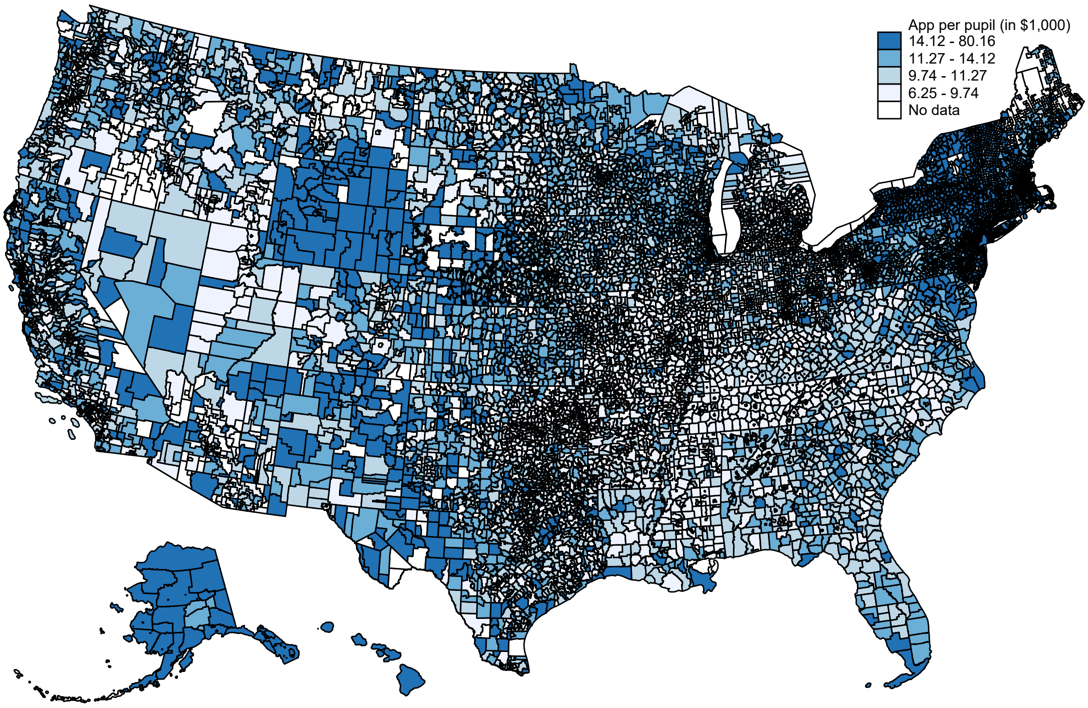


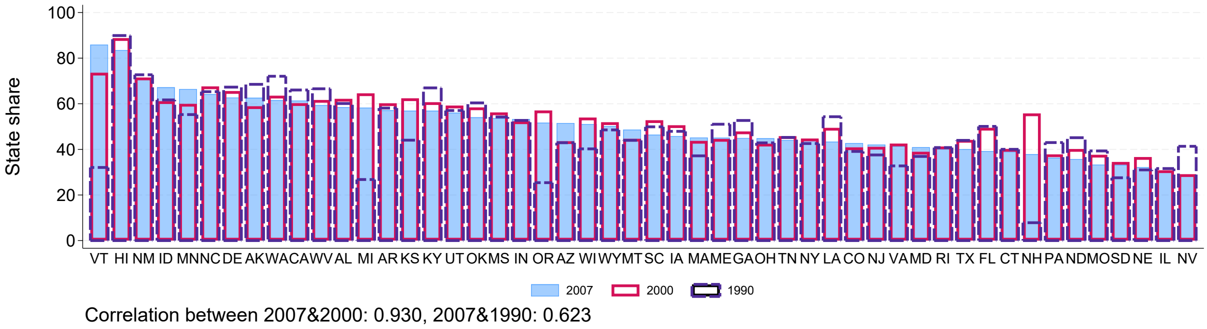
Figure A.3: Appropriation per Pupil in School District in SY 2007/2008



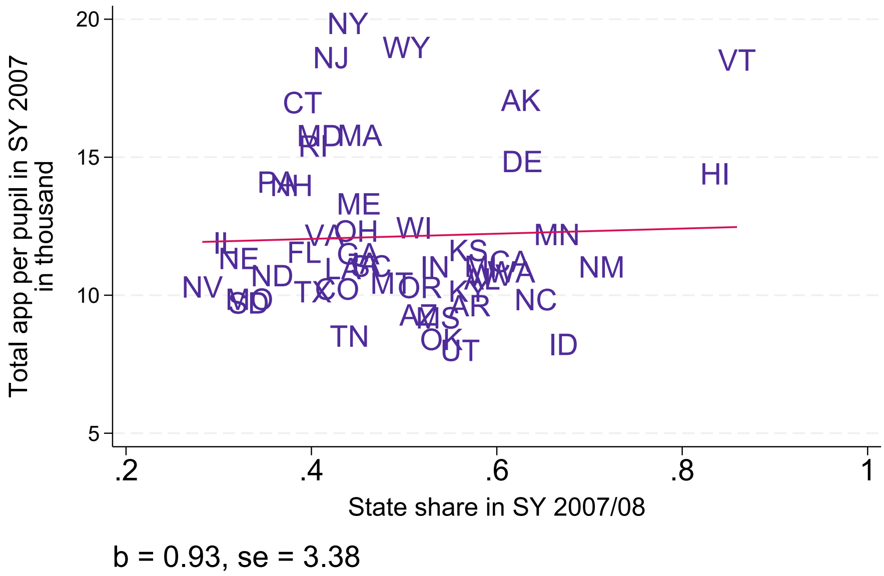
Note: All monetary values are in 2010 dollars.

Figure A.4: Variation in State Share and Relation to Total Funding

(a) Variation in Share



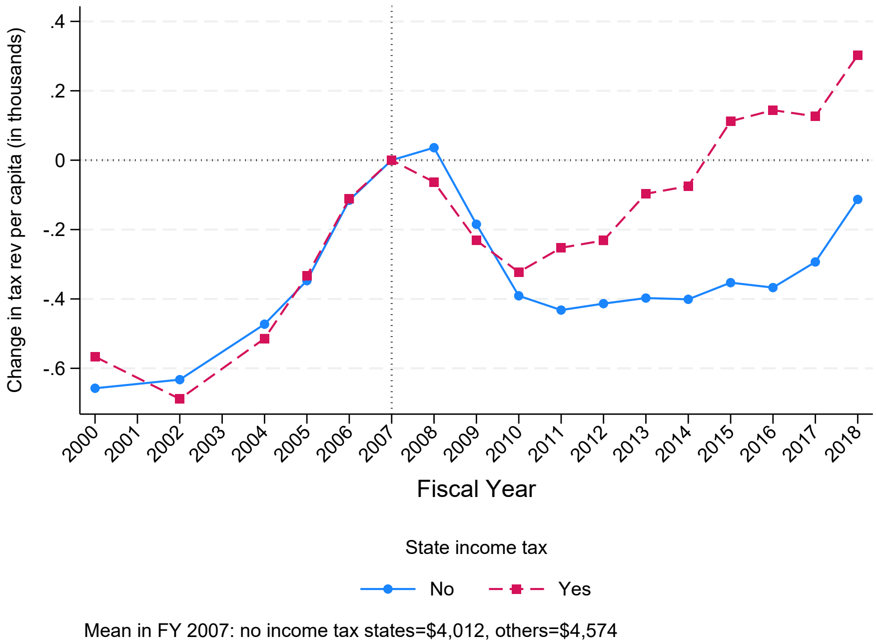
(b) Correlation Between State Share and Total Appropriations per Pupil in SY 2007



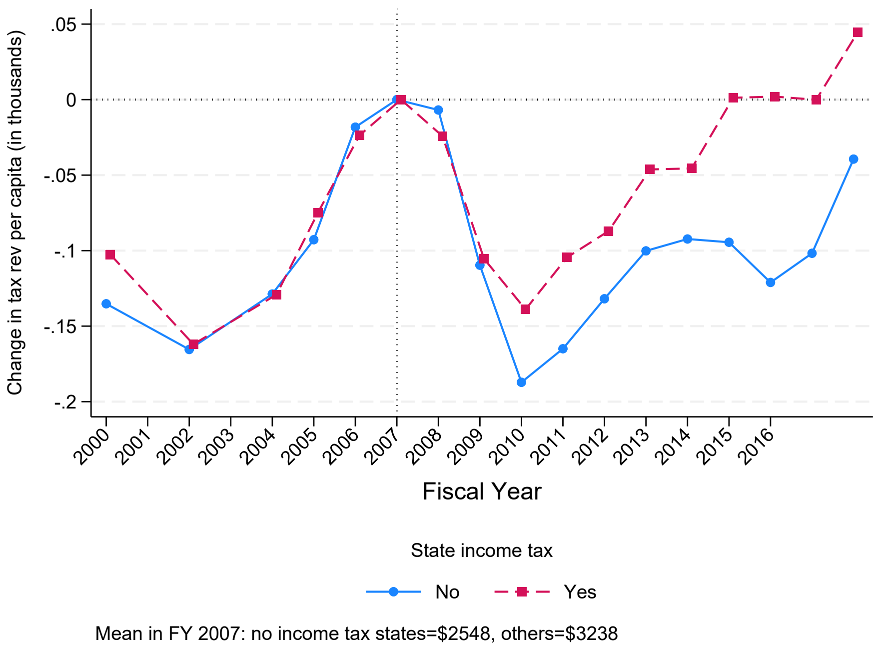
Note: Panel A displays the variation in state share in SY 2007 (blue), 2000 (pink), and 1990 (purple). The correlation of the shares is presented in the bottom of the figure. Panel B shows the relationship between the 2007 state share and total K-12 appropriations per pupil before the Great Recession. The coefficient and standard error of the linear fitted line in Panel B are presented below the figure. All monetary values are in 2010 dollars.

Figure A.5: Trend of Tax Revenue per Capita Compared to 2007, by State Income Tax Status

(a) Total Tax Revenue (in 2010 dollars)



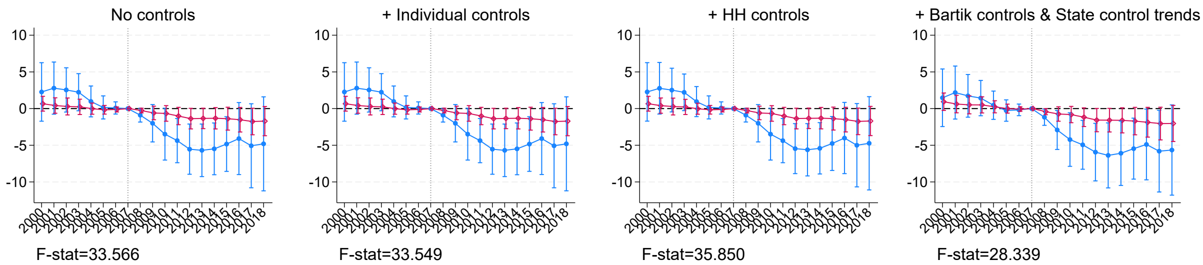
(b) Tax Revenue excluding Property Tax (in 2010 dollars)



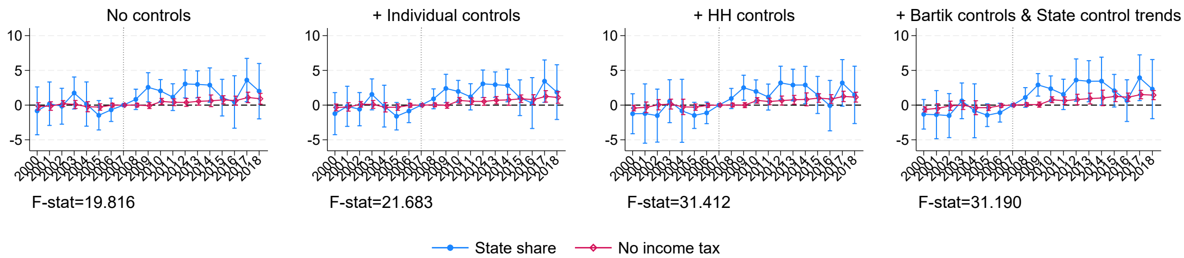
Note: Panels A and B show the trend of the average per capita total tax revenue and tax revenue excluding property tax, respectively, weighted with the state population in 2000, relative to FY 2007 in the two groups of states (states with and without an individual income tax).

Figure A.6: First Stage and Reduced Form Results in Other Specifications

(a) First Stage



(b) Reduced Form



Note: These figures show the coefficients of event study variables along with 95% confidence intervals in the first stage and reduced form regressions in different specifications. The dependent variable is state-level appropriations per pupil for K-12 education (in thousand dollars) and private school enrollment (in percent) in Panels (a) and (b), respectively. See notes of Table 3 for further details.

Table A.1: Tax Revenue in State and Local Governments in the FY 2007 (in thousands)

	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%

Source: Census of Governments and the Annual Survey of State and Local Government Finances, retrieved through State and Local Finance Initiative from US Census Bureau.
Note: All monetary values are presented in thousands of nominal dollars. Income and sales taxes include individual income tax and general sales tax only, respectively.

Table A.2: OLS and Log Specification

Dependent variable: private school enrollment (in percentage point)	(1)	(2)	(3)	(4)	(5)
Panel A. OLS Results					
App per pupil ($1,000)	-0.162*	-0.186**	-0.303***	-0.262***	-0.266***
	(0.0847)	(0.0777)	(0.0509)	(0.0565)	(0.0571)
Panel B. OLS with log of appropriations per pupil					
Ln(App per pupil)×100	-0.0252***	-0.0290***	-0.0405***	-0.0349***	-0.0353***
	(0.00847)	(0.00780)	(0.00987)	(0.00992)	(0.0104)
Panel C. 2SLS with log of revenue per pupil					
Ln(App per pupil)×100	-0.0567**	-0.0674**	-0.0782***	-0.0811***	-0.0921***
	(0.0250)	(0.0258)	(0.0287)	(0.0270)	(0.0291)
Individual Controls		Yes	Yes	Yes	Yes
Household Controls			Yes	Yes	Yes
Bartik Controls				Yes	Yes
Baseline State Char × Time Trend					Yes

Note: See notes of Table 3 for further information. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

B Complier Characteristics

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

In Table B.1, I compare the characteristics of the complier states to the entire sample. Following Angrist and Fernández-Val (2013); Angrist and Pischke (2008)'s methodology, I define two binary instrument variables Z_{1i} and Z_{2i}, high state share (above the median of the share in 2007) and no income tax indicators interacted with post-Recession indicator. Angrist and Pischke (2008) show that the IV estimate with multiple exclusive instruments is a linear combination of the LATE using each instrument. The two instruments in Table B.1 are not mutually exclusive; however, I can redefine three mutually exclusive instrument variables $(Z_1(1-Z_2), (1-Z_1)Z_2, Z_1Z_2)$ and use them as instruments. Binary treatment status D_i equals one if the total K-12 funding per pupil in the state is below the national average funding in the same year, while 0 and 1 denote potential outcomes. Thus, $D_{1i} > D_{0i}$ indicates the compliers. When estimating $E[X_{1i}|D_{1i} > D_{0i}]$, I use Abadie (2003)'s kappa-weighting scheme.[1]

I choose ten covariates in Table B.1. Except for the number of school-aged children, it seems that compliers are very similar to the entire population. This is true for both of the instruments. The complier states seem to be smaller as the relative likelihood presents. The use of per capita variables and state-level controls mitigates the concern of potential bias. Additionally, I present that the change in the size of the school-aged population cannot be the driving mechanism of the main findings through several robustness checks in the later sections.

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

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

where

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

Table B.1: Complier Characteristics

	High State Share ×Post=1 No Income Tax×Post=0			High State Share ×Post=0 No Income Tax×Post=1			High State Share ×Post=1 No Income Tax×Post=1		
	$E[X_{1i}]$ (1)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (2)	Relative Likelihood (3)	$E[X_{1i}]$ (4)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (5)	Relative Likelihood (6)	$E[X_{1i}]$ (7)	$E[X_{1i}\|D_{1i} > D_{0i}]$ (8)	Relative Likelihood (9)
Total school aged children	2,234,401	951,045	0.43	2,234,401	1,023,275	0.46	2,234,401	976,305	0.44
Teacher per 100 students	6.30	6.33	1.00	6.30	6.60	1.05	6.30	6.61	1.05
Share of Hispanic students	0.20	0.15	0.72	0.20	0.15	0.73	0.20	0.14	0.67
Share of black studnets	0.14	0.12	0.84	0.14	0.12	0.85	0.14	0.12	0.83
Share of foreign-born	0.06	0.04	0.70	0.06	0.05	0.80	0.06	0.05	0.79
Below 150% of poverty line	0.30	0.31	1.02	0.30	0.29	0.96	0.30	0.29	0.95
Household Income	61,486	61,402	1.00	61,486	62,113	1.01	61,486	62,360	1.01
Median house value	201,876	174,917	0.87	201,876	177,875	0.88	201,876	178,793	0.89
Unemployment rate	0.07	0.09	1.19	0.07	0.07	0.93	0.07	0.07	0.93

Note: N=950. The unit of observation is state-year. $E[X_{1i}]$ denotes the population means of state level characteristics. $E[X_{1i}|D_{1i} > D_{0i}]$ indicates the means of compliers ($D_{1i} > D_{0i}$). The relative likelihood is defined by dividing the complier mean by the population mean. I define three exclusive instrumental variables as indicated at the top of the table. I also re-define the treatment variable to a binary indicator of whether the total education funding per pupil is below the national median, to use Abadie (2003) and Angrist and Pischke (2008)'s methodologies.

C Parallel-Trend Sensitivity Analysis

The typical event study design relies on the parallel trend assumption, which posits that the trends in the treatment and control groups would remain the same without the intervention. Although it is impossible to directly test this counterfactual, researchers examine whether the trend was parallel before the intervention within the event study framework. This means that if the trend evolved in parallel before the treatment, it is probable that any subsequent change can be attributed to the treatment itself, rather than to pre-existing differences between the treatment and control groups.

Figure 6 and Appendix Figure A.6 reveal that none of the pre-period point estimates in the first stage and reduced form are statistically significant at the 95% significance level, for both the state share and no-income-tax status. Despite the lack of statistical significance, a seemingly downward trend is observed for the state share in the first stage and an upward trend for the no-income-tax status in the reduced form. These trends are concerning because both of these trends align with the direction of the treatment effect, and thus the treatment effect discovered here might be influenced by the pre-existing trends.

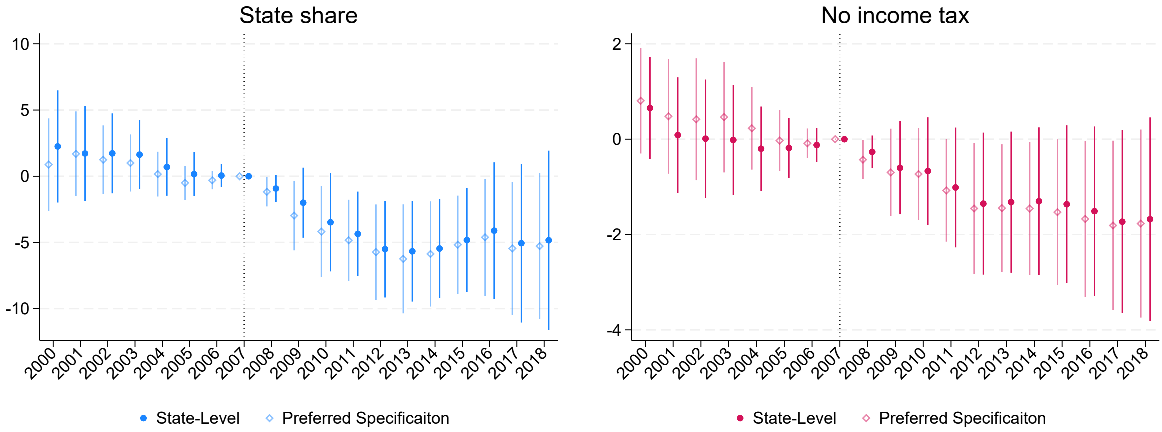
To test the robustness of the estimates, I utilize the robust confidence interval proposed by Rambachan and Roth (2023). This approach provides a robust method for inference and sensitivity analysis, particularly in scenarios where the parallel trends assumption might be violated. Rather than assuming that parallel trends hold exactly, their method imposes constraints on potential post-treatment differences in trends, given the pre-treatment trends. I adopt the smoothness restriction approach, given that parallel trends in K-12 education funding and private school enrollment are likely influenced by smoothly evolving differential secular trends, such as preferences for public or private education.[2]

Given that the methodology proposed by Rambachan and Roth (2023) assumes panel data, I re-estimate the first stage and reduced-form regressions using state-level panel data. It is worth noting that control variables are omitted in these regressions, and the weighting is based on the number of observations in each state-year. Before calculating the robust confidence intervals, I ensure that the new point estimates align with the preferred specification. In Appendix Figure C.1, the post-period point estimates for the state share show minimal dif-

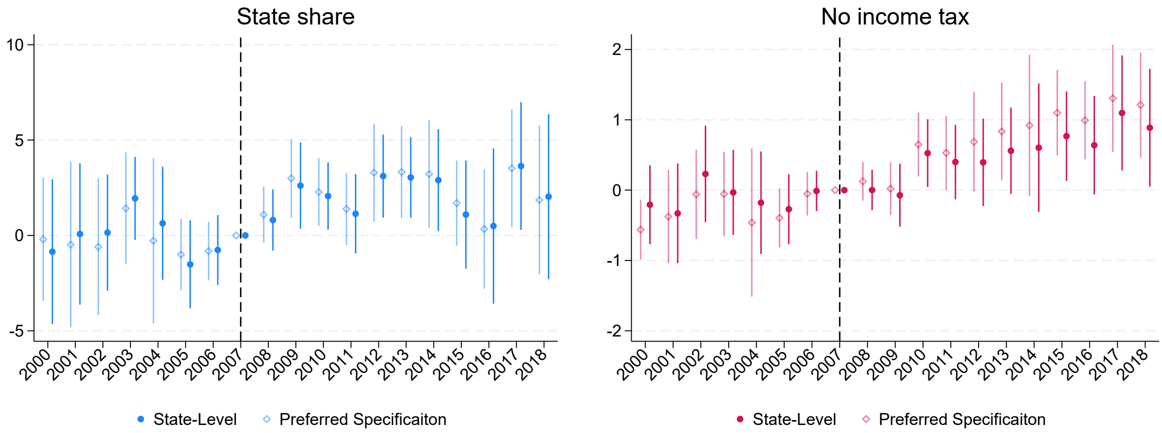
[2]An alternative approach, "bounds on relative magnitude," assumes the presence of differential shocks to the treated and control groups in the pre-and post-periods, with the magnitude of these shocks not significantly different from the pre-period. However, given that the treatment in this study is the Great Recession, this assumption is not highly plausible.

Figure C.1: First Stage and Reduced Form Using State Panel Data

(a) First Stage



(b) Reduced Form



Note: The figures display coefficients and 95% confidence intervals from using the state-level panel data (darker shade) and original preferred specification–using individual-level data (lighter shade).

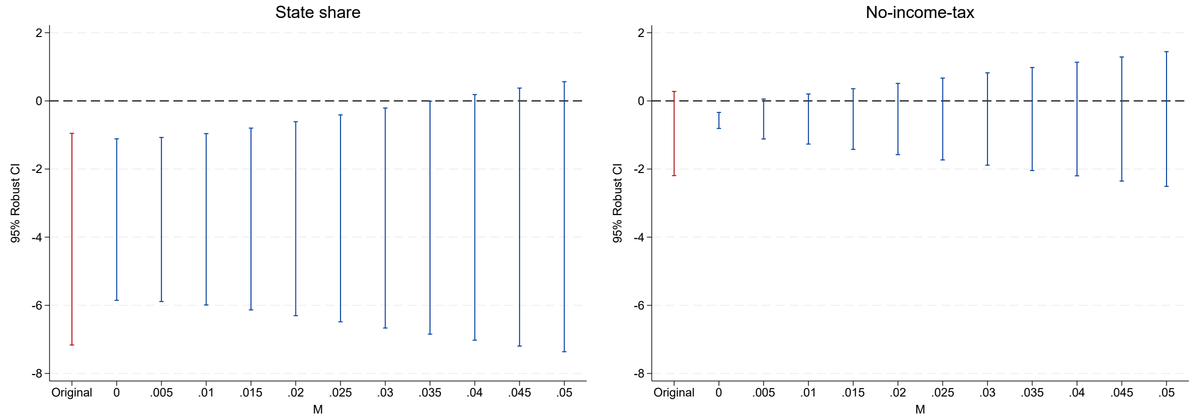
ferences, albeit with larger confidence intervals due to the reduced number of observations. On the other hand, the pre-period point estimates exhibit more disparity when using state panel data. Additionally, the magnitude and precision of the no-income-tax status in the post-period decrease when using state panel data, although potential pre-trends also appear to diminish.

Appendix Figure C.2 illustrates the robust 95 confidence intervals of the average treatment effect (DiD estimator) using state panel data.[3] The original estimator assumes exact parallel trends, with M=0 allowing only linear violations of parallel trends. As M increases, the model relaxes this restriction by constraining the slope of the pre-trend to change by no more than

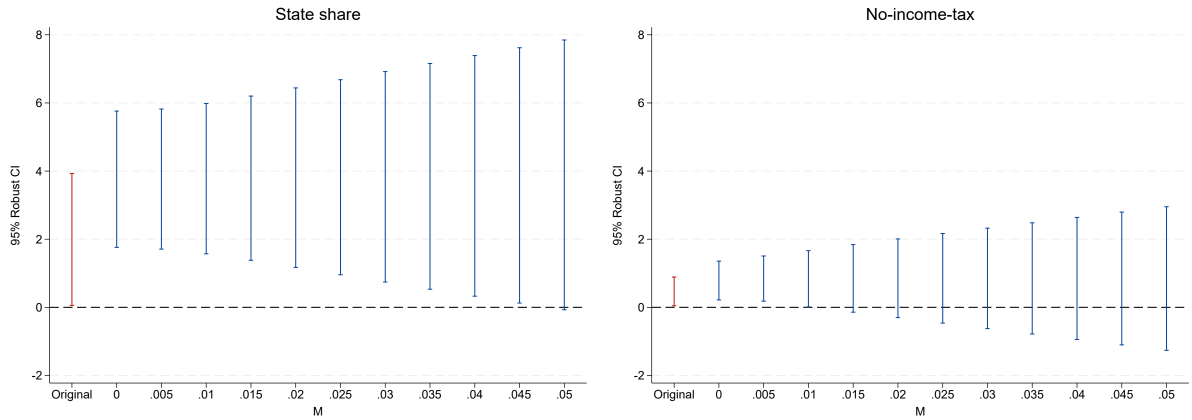
[3]The average treatment effect is estimated by assigning identical weights to all post-period estimators.

Figure C.2: Parallel Trends Sensitivity Analysis with State Panel Data

(a) First Stage



(b) Reduced Form



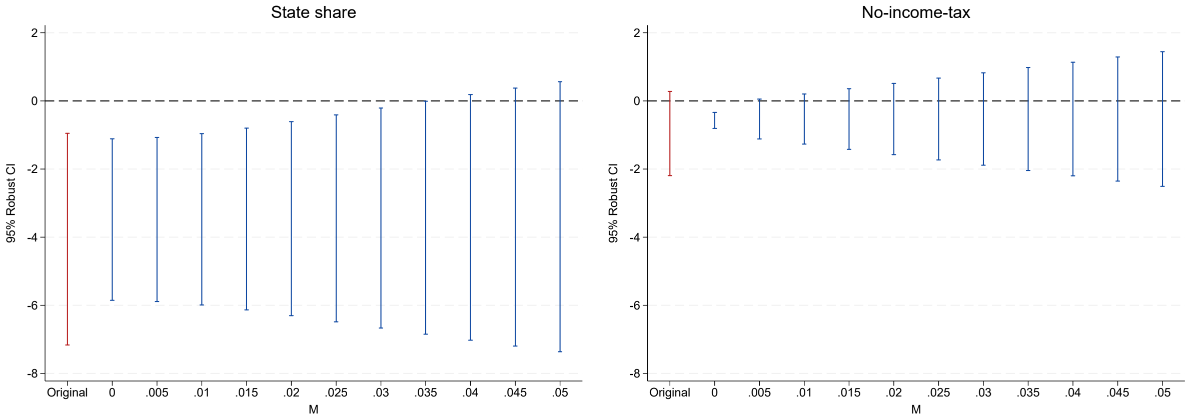
Note: The figures depict the original DiD estimator with the robust confidence interval at the 95% level proposed by Rambachan and Roth (2023), with different values of M. These confidence intervals pertain to the average of 11 post-period estimates. The results are based on state-level panel data.

M. For the state share in the first stage, the breakdown point is 0.04, which is approximately 12.5% of the pre-period slope of 0.32. Similarly, the breakdown point in the reduced form is also 0.04, which is identical to the first stage but accounts for 33% of the pre-period slope due to a smaller slope of 0.12. It's important to note that Rambachan and Roth (2023)'s paper assumes a binary treatment variable only. Thus, the robust confidence interval here should be interpreted with caution and just for reference.

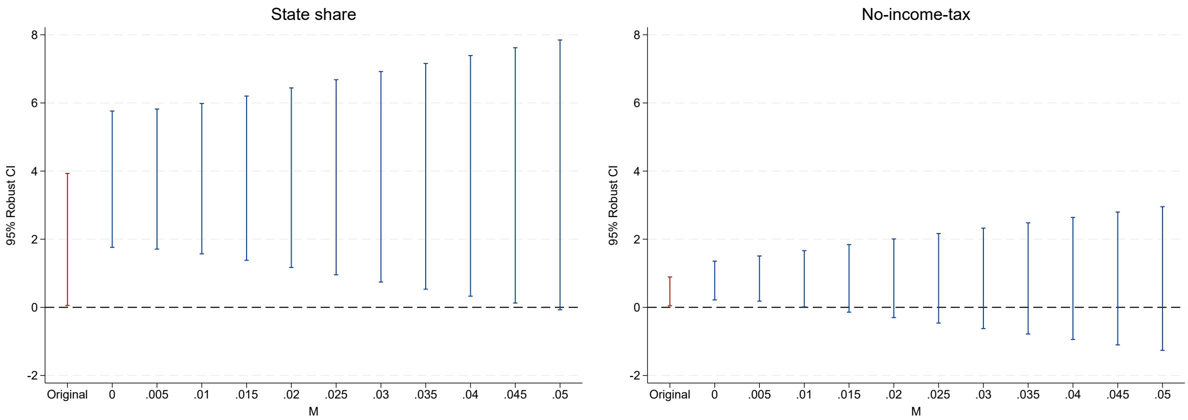
Regarding the no-income-tax status, the original estimator (average DiD estimator) is not statistically significant at the 95% level due to the loss of precision in point estimates with state panel data. However, the estimator is statistically significant when assuming linear pre-trends (M=0). The breakdown value in the first stage is 0.01, which represents about 10%

Figure C.3: Parallel Trends Sensitivity Analysis with Preferred Specification

(a) First Stage



(b) Reduced Form



Note: The figures depict the original DiD estimator with the robust confidence interval at the 95% level proposed by Rambachan and Roth (2023), with different values of M. These confidence intervals pertain to the average of 11 post-period estimates. The results are based on the preferred specification (column 4 of Table 3).

of the pre-period trend of -0.1. In the reduced form, the breakdown value is 0.015, which is approximately 53% of the pre-period slope of 0.028. While the breakdown value for the no-income-tax status is smaller, allowing only a 10% deviation from the linear pre-trends, this still adds robustness to our results. This indicates that our findings are not overly sensitive to minor deviations from the parallel pre-trends.

For reference, I estimate the robust confidence intervals using the preferred specification with the individual-level data in Figure C.3. The results are very similar to those in Figure C.2; however, the confidence intervals are smaller due to the larger number of observations.

D Other Potential Mechanisms

D.1 Selective Migration

People may strategically migrate due to their preference for public goods, a concept known as the Tiebout model (Tiebout, 1956). This phenomenon is especially relevant in the context of education (Barrow, 2002). When budget cuts for education are observed or anticipated, families with a high preference for quality public schools may choose to relocate to districts with higher education spending. 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 areas with higher spending, potentially overestimating my results. On the other hand, students might switch to private schools while relocating to relatively cheaper neighborhoods, where education funding is also low. This could also overestimate the true effect of the decrease in education funding. However, it's important to note that my specification is robust to migration. This is because I utilize state-level variation, and between-state migration is rare. After the recession, only 1.6 percent of households relocated between states. Nevertheless, I examine whether issues of selective migration could confound my results.

To analyze this, I test whether state-level education appropriations have affected interstate migration using migration history data from the ACS. The ACS asks whether a household migrated within a year and records the departure location, including the state and Migration PUMA (MPUMA).[4] Using this information, I calculate the state-level in- and out-migration. In Appendix Table D.1, I estimate the impact of K-12 appropriations per pupil on the state-level

Table D.1: Potential Mechanism: Migration of Children (in thousands)

	Total number (1)	In-migration (2)	Out-migration (3)
App per pupil ($1,000)	-89.70	0.0482	1.403
	(75.07)	(0.914)	(2.273)
	2,232	*32*	*36*

Note: N=900. The year 2000 is excluded because of data limitations. All regressions include year and state fixed effects, Bartik controls, and baseline state characteristics interacted with time trends. Regressions are weighted using the state-level school-aged population in 2000. Robust standard errors are in parentheses clustered by state. The mean of the dependent variables in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

[4]The MPUMA here is different from either PUMA or CPUMA; it aggregates the regular PUMAs to resemble the commuting zone and is specifically used to collect workplace or migration information.

total number of school-aged children, in-migration, and out-migration. The results show that public school funding is not correlated with any of them, which supports the conclusion that migration is not the primary mechanism driving the findings of my study.

D.2 Access to Schools

Numerous states have implemented diverse school choice programs to enhance the accessibility to quality education. These programs encompass alternative public schools such as charter and magnet schools, and private school choice programs including vouchers, tax-credit scholarships, and education savings accounts (ESAs). By 2018, 44 states and Washington DC had passed public charter legislation (National Alliance For Public Charter Schools), and 11% of public school students were in charter or magnet schools (Snyder, de Brey, and Dillow, 2019). The funding cut after the Great Recession may have negatively affected the number of alternative and traditional public schools (TPS), thus leading parents to opt for private schools. As for private school choice programs, the most notable is the voucher, with a large literature suggesting that vouchers can increase private school enrollment for certain students (Epple, Romano, and Urquiola, 2017). To meet the increasing demand for school choice, states have implemented a variety of school choice programs over the study period. While only 12 states and DC had any school choice program in 2007, this number rose to 27 states (plus DC) by 2018 (EdChoice, 2020).[5] If states with lower funding for public schools offer generous private school vouchers or tax credits, parents may choose private schools due to the availability of these private school programs rather than due to concerns over public education funding.

Table D.2 investigates whether changes in the number of schools and the implementation of school choice policies could be the underlying mechanisms driving the findings of this paper. Panel A estimates the effect of education appropriations on the number of schools per 1,000 students by type, finding no effect on any type of school. Panel B analyzes the effect on private school choice policies, revealing that state-wise private school support programs are not correlated with changes in total public K-12 education funding. This, combined with the findings in Table 5, which demonstrate that expenditures on private school tuition and programs decrease due to education funding cuts, suggests that changes in private school programs and funding are unlikely to be the mechanisms for the shift to private schools.

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

Table D.2: Potential Mechanism: Access to Schools and School Choice Policies

	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Variable: Number of Schools per 1,000 Children					
	All Public	TPS	Charter	Magnet	Private
App per pupil ($1,000)	0.000255	0.0406	-0.0131	-0.0250	0.0220
	(0.0353)	(0.0310)	(0.00914)	(0.0163)	(0.0315)
	3651.508	*3412.826*	*135.852*	*102.830*	*1496.341*
Observations	950	850	950	850	950
Panel B: Dependent Variable: Policy Indicators					
	Any Policy	Voucher	Tax Credit	ESA	
App per pupil ($1,000)	0.0633	0.0502	0.0137	-0.0351	
	(0.0614)	(0.0525)	(0.0235)	(0.0373)	
	0.248	*0.120*	*0.073*	*0.000*	
Observations	950	950	950	950	

Note: All regressions include year and state fixed effects, Bartik controls, and baseline state characteristics interacted with time trends. Regressions are weighted using the state-level school-aged population in 2000. Robust standard errors are in parentheses clustered by state. The means of the dependent variables in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

E Additional Robustness Checks

E.1 Sensitivity to Other State Characteristics

State share and the no-income-tax status may exhibit correlations with other state-specific attributes that impact K-12 appropriations per pupil and private school enrollment. For example, political affiliation is a significant determinant of the optimal tax structure (Hettich and Winer, 1988), thereby exerting an influence on K-12 education funding. Furthermore, teachers' unions can influence school finance structures and student achievement (Brunner, Hyman, and Ju, 2020; Ju, 2023), consequently affecting private school enrollment. Moreover, the religiosity of a state is notably linked to private school attendance (Goldring and Phillips, 2008) and may also be correlated with the state's share and the absence of income tax.

Figure E.1 presents the correlation between state share, no-income-tax status, and state characteristics that may lead to bias. The state attributes considered here are the average share of the Republican vote in the 2000 and 2004 presidential elections (Federal Election Commision, n.d.), the bargaining power of teachers' unions,[6] and the proportions of Evangelical, mainline Protestant, and Catholic populations (Association of Religion Data Archives,

[6]The original measures of teacher union power are provided by Winkler, Scull, and Zeehandelaar (2012). Brunner, Hyman, and Ju (2020) make an index out of them, which is used here. Alaska and Hawaii are excluded.

2000). The figure suggests that the overall correlations between the treatment variables and state attributes are minimal. In Panel (a), we observe a correlation solely between state share and the share of Catholics. The low correlation also ensures the validity of continuous treatment variable in the difference-in-differences setting (Callaway, Goodman-Bacon, and Sant'Anna, 2024). Panel (b) compares the means of these characteristics in states with and without income tax. Notably, none of the variables exhibit substantial differences; indeed, the 95% confidence intervals of the means suggest that the disparities are not statistically significant.

The findings from Figure E.1 indicate that while the correlation between the treatment and potential confounders is low, it does not guarantee that these variables do not influence the effect. It is plausible that confounders may still exert a direct influence on the dependent variable and alter the treatment effect. Although isolating the Local Average Treatment Effect (LATE) from these state characteristics is challenging, I conduct a sensitivity analysis by examining whether the inclusion of these characteristics affects my main specification. In particular, I augment my main specification (column 4 of Table 3) by introducing the event-study variables of these state characteristics (i.e., $\sum_{j\neq 2007}$ State Char $\times$ (year $= j$)) to assess whether the effect of appropriations per pupil remains significant.

Table E.1 provides the outcomes when accounting for other confounding characteristics. Columns 1-5 include each of the five state attributes discussed previously, while column 6 encompasses all five state characteristics together. Despite the loss of statistical precision, the estimates in the table remain significant. Furthermore, the point estimates diminish compared to the main specification (-0.567) in all columns, except for column 4, indicating a correlation with private school enrollment. However, all differences are marginal, and therefore, the null hypothesis that the difference is zero cannot be rejected.

Table E.1: Including Other State Characteristics

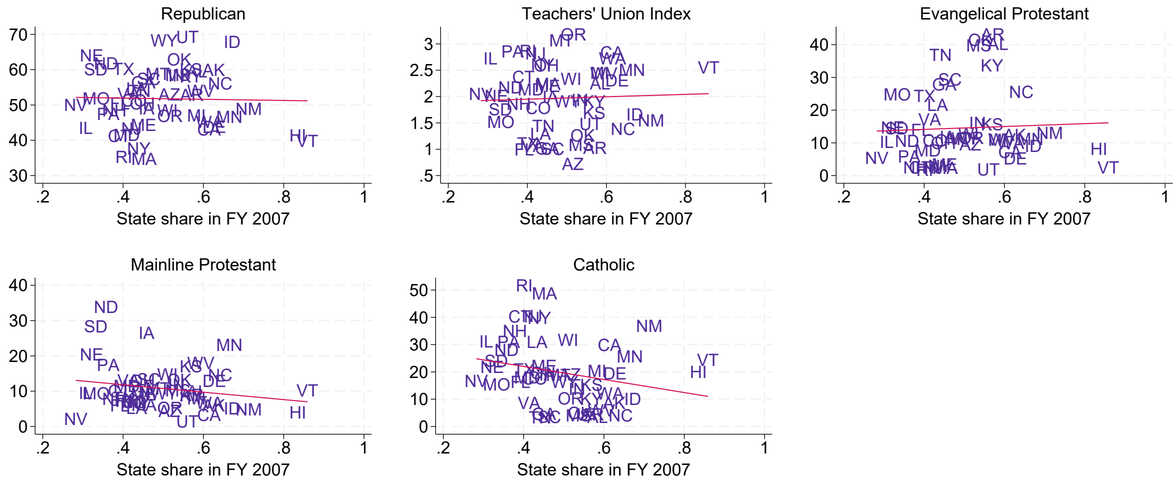
Dependent variable: private school enrollment (in percentage point)

	Republican share (1)	Teachers' Union Index (2)	Evangelical Protestant (3)	Mainline Protestant (4)	Catholic (5)	All (6)
App per pupil ($1,000)	-0.459*	-0.450**	-0.510**	-0.638***	-0.483**	-0.409*
	(0.229)	(0.191)	(0.214)	(0.212)	(0.216)	(0.217)

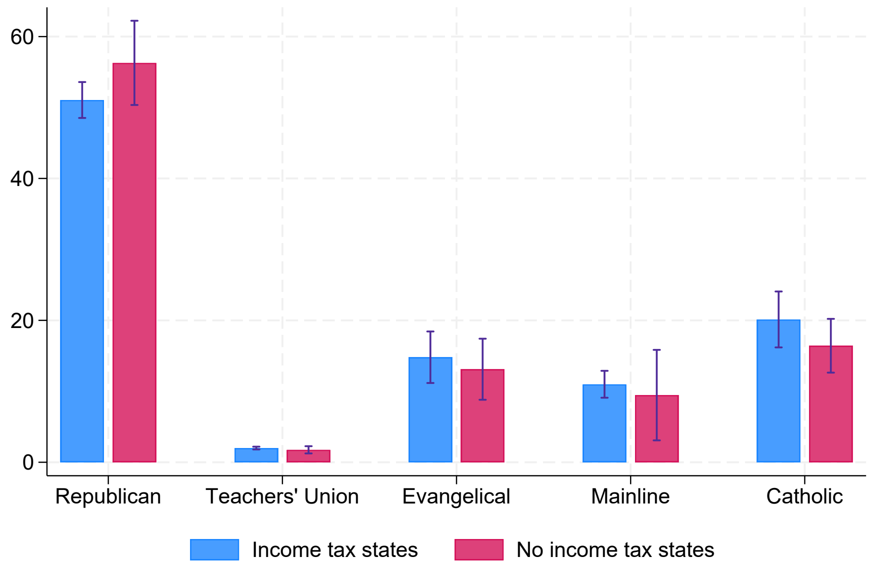
Note: Each column includes the interaction terms between the state characteristics denoted in the column title and year dummies. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

Figure E.1: Correlation Between State Share, No-income-tax Status and Other State Characteristics

(a) Correlation with state share



(b) Correlation with no-income-tax status



Note: Panel (a) displays the correlation between state share and five state attributes. Panel (b) offers a comparison of the means of these five attributes based on the income tax status. The 95% confidence intervals of these means are also presented.

E.2 Alternative Sample

In Table E.2, I estimate the impact of education appropriations per pupil in different samples. First, I include Washington DC in the sample in column 1. My main sample excludes DC because DC's state share is zero by definition. Although DC constitutes about 0.13 percent of total observation, including DC may change the result because it is such an outlier. The point estimate is almost identical to the main model. In column 2, I restrict the sample to 2000-2012 to examine whether the effect is driven by the later years that are less affected by the Great Recession. The point estimate is smaller by 0.08 and less precisely estimated. The effect grows stronger as time goes on, which is consistent with the first stage and reduced form results. In column 3, I remove the dropouts, and because this group is less likely to enroll in private schools, the point estimate slightly increases. Columns 4 and 5 compare native-born and foreign-born students and find that the impact is driven by native-born students. Next, in columns 6 to 9, I remove some states that may respond differently to the funding shock. First, I exclude Florida and Nevada because they are two states without income tax known to have a very large decline in property values during the Great Recession. In column 7, I remove California and Texas from the sample because of their unique funding schemes. California's Proposition 98 guarantees a minimum amount of education funding from the state's General Funds and local property taxes, making education funding less volatile. Texas state education agency redistributes the locally raised tax from wealthy to poor school districts under Chapter 49 (well known as the Robin Hood Plan). Thus, state-appropriated funding may be more stable because part of it is coming from local tax revenue. I also exclude Alaska in column 8 because Alaska does not collect either income or sales tax.[7] None of the estimates in the columns are statistically different. Finally, I remove the top 10 percent states in private school enrollment in 2000 in column 9 to test whether the impact is concentrated in certain areas with high access to private schools. The 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.

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

Table E.2: Alternative Samples

Dependent variable: private school enrollment (in percentage point)

	Include DC	Years of 2000-2012	Drop Dropouts	Native Only	Immigrant Only	Drop FL and NV	Drop CA and TX	Drop AK	Drop top 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
App per pupil ($1,000)	-0.583***	-0.487*	-0.584***	-0.576***	-0.223	-0.529**	-0.727**	-0.546***	-0.338**
	(0.186)	(0.256)	(0.189)	(0.192)	(0.210)	(0.222)	(0.274)	(0.178)	(0.160)
	10.35%	*10.34%*	*10.34%*	*10.65%*	*5.82%*	*10.33%*	*10.71%*	*10.35%*	*8.51%*
Observations	9,808,166	7,064,634	9,572,565	9,246,536	546,180	9,201,505	8,454,587	9,761,231	7,606,625

Note: All regressions are in the preferred specification (column 4 of Table 3). Robust standard errors are in parentheses clustered by state. The 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%.

E.3 Alternative IVs

I explore alternative instrumental variables to assess the robustness of the IVs utilized in the paper. In the main analysis, the instrumental variables are the state share and the no-income-tax indicator interacted with year indicators, with 2007 as the base year. I posit that the event study framework, which is more flexible than the traditional difference-in-differences approach, is appropriate due to the treatment effect changing over time (as depicted in Figure 6).

In Table E.3, I test whether my results stay consistent with the specification of instrumental variables. In column 1, I use traditional difference-in-differences variables, $S_s \times Post_t$ and $NT_s \times Post_t$, as the IVs. The point estimate is almost identical to the main analysis. However, the first stage F-statistics becomes much smaller because of the dynamic effect of the funding cut. When I use the event study variables of state share only in column 2, as in Jackson, Wigger, and Xiong (2021), the point estimate gets smaller and insignificant with much smaller F-statistics. In column 3, the coefficient is larger when I use the no income tax indicator as the sole identifying variation, with losing precision. Two columns show that the impact on education appropriations driven the by no-income-tax indicator is stronger than the state share, and the main specification captures some weighted average of the two.[8] In column 4, I add the interaction term of state share and no income tax indicator interacted with the year dummies. A state with a high state share and no income tax may have been through an even deeper education funding cut if two sources of variation strengthen each other. The point estimates get smaller by 0.15 percentage points with very large F-statistics. None of the point estimates in the table is statistically different from the main estimate.

[8]The 2SLS estimate using multiple instrumental variables is a linear combination of 2SLS estimates using each of single instrument variable (Angrist and Fernández-Val, 2013).

Table E.3: Alternative Instrumental Variables

Dependent variable: private school enrollment (in percentage point)

	DiD (1)	State share (2)	NT (3)	+Interaction (4)
App per pupil ($1,000)	-0.570**	-0.373	-0.730**	-0.414***
	(0.217)	(0.351)	(0.320)	(0.129)

Note: All regressions are in the preferred specification (column 4 of Table 3). In column 1, I use difference-in-differences estimations—$S_s \times Post_t$ and $NT_s \times Post_t$. $Post_t$ 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. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

E.4 Lagged revenue

I use the lagged value of K-12 appropriations per pupil in Panel B of Table E.4 as the independent variable. This helps to examine the cumulative impact of the funding cut. Parents may not perceive the funding cut immediately and make a decision based on cumulative experience. If the lagged appropriations have a much smaller impact than the concurrent appropriations, then it would raise a question of the true impact of appropriations for education. In columns 1 to 3, I use 1, 2, and 3-year lagged education appropriations per pupil ($App_{t-1}, App_{t-2}, App_{t-3}$), respectively. The point estimates in columns 1 to 3 are also smaller than the main result, however still large and statistically significant. When using the 3-year moving average (average of $t-1$, t, and $t+1$), the point estimate is almost identical to the main specification. Overall, the results in the table suggest the "cumulative exposure" to funding cuts is as important as the current level of funding.

Table E.4: Lagged Appropriations and Moving Average

Dependent variable: private school enrollment (in percentage point)

	1-year Lag (1)	2-year Lag (2)	3-year Lag (3)	3-year MA (4)
App per pupil ($1,000)	-0.534***	-0.509***	-0.479**	-0.565***
	(0.183)	(0.182)	(0.197)	(0.191)
Observations	7,316,181	7,111,529	6,928,221	9,792,716

Note: All regressions are in the preferred specification (column 4 of Table 3). Columns 1-3 use $App_{t-1}, App_{t-2}, App_{t-3}$, respectively. In column 4, I use the 3-year moving average ($t-1$, t, and $t+1$) of appropriations per pupil. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

E.5 Selective Migration

Although I show evidence of little selective inter-state migration in Section D, this is not sufficient to rule out the possibility of selective migration because households may strategically relocate within the state and stay in public schools, generating a downward bias. If within-state migration for education is a prevalent reason for relocation, then migrants' response to the funding cuts for public schools would be different than non-migrants.

In Table E.5, I examine whether the effect of public school funding varies by migration status and show that it does not. First, I split the households by migration status for the sample of 2001-2018.[9] From columns 1 to 3, I divide the sample into those who have migrated between MPUMAs (column 1), those who have stayed in the same MPUMA (including those who have moved to a different house within the same MPUMA) (column 2), and those who have lived in the same house (column 3) compared to 12 months ago. In column 1, the point estimate shows that a $1,000 increase in K-12 appropriations per pupil decreases private school enrollment of migrated households by 0.442 percentage points. However, it is not precisely estimated due to the small sample size. This is smaller than those who have migrated within the MPUMA (column 2) and those who have not moved (column 3) but is not statistically significantly different, implying that the migrants' behavior is not significantly different from that of stayers.

In column 4, I estimate the impact on children whose household head has lived in the same house for five or more years. The coefficient increases to -0.604, which is similar to the main specification. Finally, in column 5, I use the K-12 appropriations per pupil in the state of birth, excluding foreign-born children.[10] Using the birth state instead of the current resident state should be more robust to selective migration because it is determined before the educational choice. The point estimate in column 5 presents an almost identical estimate to the main specification.

E.6 Sensitivity to School Access

In Section D, I present that the number of schools or the introduction of statewide school choice programs are unlikely to be the mechanisms behind the main findings. In this section, I test whether the main result is robust to the inclusion of confounding variables. In Table E.6,

[9]For reference, the point estimate (SE) is -0.563 (0.190) when using the same sample period.
[10]From 2000-2007, 82% of native-born children stayed in the birth state.

Table E.5: Selective Migration and Private School Enrollment

Dependent variable: private school enrollment(in percentage point)

	Migration status from last year			5yr+	Funding of
	Different MPUMA	Same MPUMA	Same Residence	Same Residence	State of birth
	(1)	(2)	(3)	(4)	(5)
App per pupil ($1,000)	-0.442	-0.569***	-0.570***	-0.604***	-0.568***
	(0.388)	(0.186)	(0.188)	(0.192)	(0.198)
	6.99%	*10.70%*	*10.91%*	*12.29%*	*10.35%*
Observations	326,485	6,989,696	6,490,332	4,361,052	9,222,935

Note: All regressions are in the preferred specification (column 4 of Table 3). 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 2001-2016 because the 2000 Census lacks this information. Region refers to the 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. In column 5, I use the funding per pupil in the state of birth, excluding foreign-born children. Robust standard errors are in parentheses clustered by state. The 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%.

I demonstrate that school choice programs do not drive my results. In Panel A, I incorporate the number of schools per 1,000 children as a covariate to the main specification. The point estimates increase in columns 1 and 3 when including all public schools and charter schools, respectively. However, assuming that the number of public schools reduces private school attendance, the differences are not statistically significant. Next, in Panel B, I add a time-variant indicator for whether a state has school choice policies (columns 1-4) and the public spending on private school programs, which is examined in Table 5. The point estimates are not statistically different from the main result, suggesting that private school programs are not a significant confounder.

F Additional Heterogeneity Analysis

F.1 Heterogeneity in Effect by State Characteristics

Regional SES can influence the extent of the treatment effect because it is related to the accessibility to or preference for private schools. Table F.1 compares the heterogeneity in effect by three regional characteristics—the proportion of people in poverty, the proportion of minority residents, and the proportion of residents of foreign origin. When dividing the sample, I utilize the average of the CPUMA where the households reside to ensure more variation. It is important to consider these local neighborhood characteristics rather than just state averages because households make decisions based on their immediate environment.

Panel A of Table F.1 demonstrates a more substantial impact in high SES areas for all

Table E.6: Considering Number of Schools and School Choice Policies

Dependent variable: private school enrollment (in percentage points)

	(1)	(2)	(3)	(4)	(5)
Panel A. Inclusion of Number of Schools per 1,000 Children					
	All Public	TPS	Charter	Magnet	Private
App per pupil ($1,000)	-0.756***	-0.555***	-0.794***	-0.532**	-0.542***
	(0.282)	(0.188)	(0.211)	(0.248)	(0.184)
Observations	9,792,716	8,887,319	9,792,716	8,887,319	9,792,716
Panel B. Inclusion of Policy Indicators					
	Any Policy	Voucher	Tax Credit	ESA	Spending on Private Schools
App per pupil ($1,000)	-0.607***	-0.604***	-0.575***	-0.541**	-0.651***
	(0.181)	(0.182)	(0.190)	(0.204)	(0.226)
Observations	9,792,716	9,792,716	9,792,716	9,792,716	9,792,716

Note: All regressions are in the preferred specification (column 4 of Table 3). All regressions additionally include a variable denoted in the column title as a covariate. Robust standard errors are in parentheses clustered by state. The means of private school enrollment in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

households. The point estimates for low poverty and low minority areas are statistically different from those in high SES areas. Panels B and C indicate that the effects are primarily driven by high-income households (whose income is greater than the national median). This finding is consistent with the results in Table 4, which indicate that high SES households are more inclined to opt for private schools. Thus, high SES households residing in high SES areas exhibit greater responsiveness.

F.2 Heterogeneity in Effect by Access to Private Schools

in school access across states. It would be reasonable to expect that households living in states with better access to private schools are more responsive. In Table F.2, I divide the sample based on: 1) whether the states had any private school choice policy (voucher, tax credit, or ESA), 2) the number of private schools, 3) the number of TPS, and 4) the number of charter or magnet schools per 10,000 children in 2007.

In column 1, the point estimate is larger in states with any private school policy, although not statistically distinct from column 2. However, there is a marked disparity between columns 3 and 4 when dividing the sample by the number of private schools per 10,000 children. Notably, the effect is primarily driven by states with a greater number of private schools. Consequently, actual access to private schools holds more significance than mere eligibility for

Table F.1: Heterogeneity by State Characteristic 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. All households						
App per pupil ($1,000)	-0.291	-0.635**	-0.0518	-0.435*	-0.362	-0.479***
	(0.229)	(0.279)	(0.268)	(0.253)	(0.357)	(0.147)
	10.02%	*11.39%*	*10.42%*	*10.14%*	*10.39%*	*10.23%*
Observations	5,481,086	4,311,630	4,520,278	5,272,438	4,055,014	5,737,702
Panel B. High income households						
App per pupil ($1,000)	-0.252	-0.738**	0.0981	-0.601**	-0.390	-0.733***
	(0.341)	(0.331)	(0.370)	(0.298)	(0.359)	(0.209)
	13.51%	*13.39%*	*14.00%*	*12.27%*	*13.69%*	*13.00%*
Observations	2,916,265	3,075,928	2,559,359	3,432,834	2,492,279	3,499,914
Panel C. Low income households						
App per pupil ($1,000)	-0.273	-0.251	-0.184	-0.0429	-0.272	-0.0693
	(0.224)	(0.200)	(0.244)	(0.185)	(0.329)	(0.201)
	5.53%	*6.59%*	*5.54%*	*6.25%*	*5.56%*	*6.07%*
Observations	2,564,821	1,235,702	1,960,919	1,839,604	1,562,735	2,237,788

Note: All regressions are in the preferred specification (column 4 of Table 3). The sample is divided into two groups based on CPUMA characteristics presented in each column's title. Each panel is separately estimated by household income. Samples from 2005-2018 are utilized due to data limitations. Robust standard errors are in parentheses clustered by state. The means of private school enrollment in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

subsidies. This is plausible given that the effects primarily stem from better-off households, which may not qualify for private school policies. Moreover, the numbers of charter or magnet schools (columns 5 and 6) and TPS (columns 7 and 8) do not appear to exert a substantial impact.

Table F.2: Heterogeneity in Effect by Private School Access

	Any Private School Policy		Private Schools		Charter/Magnet Schools	
	Yes	No	Many	Few	Many	Few
	(1)	(2)	(3)	(4)	(5)	(6)
App per pupil ($1,000)	-0.468***	-0.386	-0.649**	0.115	-0.474***	-0.511*
	(0.0959)	(0.238)	(0.263)	(0.225)	(0.134)	(0.255)
	11.55%	*9.91%*	*11.45%*	*8.43%*	*10.49%*	*10.19%*
Observations	2,576,426	7,216,290	4,493,038	5,299,678	4,905,855	4,886,861

Note: All regressions are in the preferred specification (column 4 of Table 3). Samples are divided by policy indicator and number of schools in the state in 2007. Robust standard errors are in parentheses clustered by state. The means of private school enrollment in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

F.3 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 F.3, I compare the impact of education appropriations by four parental characteristics: the presence of both parents, whether at least one parent holds a Bachelor's degree, high-earning occupation (based on median occupational income in 2000), and immigrant status. The findings reveal no heterogeneity in effect by these parental attributes. The treatment effect appears to be amplified among parents with high-earning occupations, likely due to its strong correlation with household income. Although other characteristics indicative of high SES also bolster point estimates, the differences are not statistically significant.

Interestingly, parental traits do not influence the competition between public and private schools as greatly as race and income do. Parents with high SES tend to prefer private schools (Goldring and Phillips, 2008), hence they should be more responsive to funding cuts, as discussed in Section 5.3. The sole significant divergence is observed among immigrant parents. Parents with immigrant status are 0.35 percentage points more likely to enroll their children in private schools when faced with a $1,000 funding cut.

Table F.3: Heterogeneity by Parental Characteristics

Dependent variable: private school enrollment (in percentage point)

	Both parents present		Has a Bachelor's degree		High earning occupation		Immigrant	
	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
App per pupil ($1,000)	-0.579***	-0.592***	-0.466*	-0.541**	-0.613***	-0.400**	-0.828***	-0.472**
	(0.206)	(0.180)	(0.276)	(0.209)	(0.214)	(0.188)	(0.209)	(0.207)
	12.16%	*6.33%*	*18.97%*	*6.69%*	*12.61%*	*5.49%*	*8.42%*	*11.27%*
Observations	7,082,168	2,282,900	3,418,633	5,946,435	6,547,081	2,817,987	2,108,217	7,256,851

Note: All regressions are in the preferred specification (column 4 of Table 3). Robust standard errors are in parentheses clustered by state. The means of private school enrollment in the pre-recession period are in italics below the standard errors. * significance at 10%; ** significance at 5%; *** significance at 1%.

G Using Alternative Dataset

In this section, I examine the robustness of my results utilizing an alternate dataset known as National Center for Education Statistics. PSS is a biennial survey conducted by NCES, which targets all private schools in the US.[11] PSS provides data on school-level enrollment and school characteristics such as precise location, type of school (affiliation), and year of establishment. I assess whether my findings hold when subjected to a school-level analysis via PSS.

Apart from adding robustness to my results, analysis with PSS offers additional advantages. One crucial attribute parents consider when selecting a private school is its religious affiliation (Goldring and Phillips, 2008). Religious schools generally offer reduced tuition fees to students. Specifically, the average annual tuition for the school year 2011-2012 for Catholic, other religious, and nonsectarian schools were $7,210, $9,100, and $22,570, respectively (Snyder, de Brey, and Dillow, 2019). Given the stark disparity in tuition fees, it would be interesting to identify which types of schools are most elastic to changes in local public school funding. This is especially relevant for relatively low-cost schools, as they might be more sensitive to the significant economic shock triggered by the Great Recession.

In Panel A, I first aggregate the sample at the state level and examine whether the effect using PSS aligns with ACS. The dependent variable is the number of enrolled students. State Bartik controls and baseline controls interacted with time trends are included and the regressions are weighted with the children's population in 2000 in Panel A. The point estimates in columns 1 and 2 are very similar, with 16,565 and 14,926 students using ACS and PSS, respectively. In column 1, an additional 6.5 students are enrolled when there is a $1,000 drop in public education appropriations. In column 2, Catholic private schools see an increase of 10.28 students, which is coherent with Panel A's result that the majority of the effect is driven by Catholic schools.

[11] The universe encompasses all private schools; however, the actual number interviewed depends on the response rate, which averages over 90%.

Table G.1: Impact on Number of Enrolled Students in Private Schools

Dependent variable: Enrolled students in private schools

	ACS	PSS			
		All	Catholic	Other Religious	Nonsectarian
	(1)	(2)	(3)	(4)	(5)
Panel A. State Level					
App per pupil ($1,000)	-16565.4**	-14926.4*	-14210.1***	6801.5	-5212.4
	(6783.7)	(7499.7)	(3898.2)	(7638.2)	(3412.0)
	221,298	*271,232*	*106,053*	*103,811*	*61,368*
Observations	450	450	450	450	450
Panel B. School Level					
App per pupil ($1,000)		-6.549**	-10.28***	1.271	-5.887
		(3.211)	(3.295)	(4.109)	(4.385)
		264	*354*	*208*	*226*
Observations		138,044	48,675	61,431	27,319

Note: Panel A: State Bartik controls and state baseline characteristics interacted with time trends are included in all regressions All regressions are weighted with the state-level children population in 2000. Panel B: School fixed effect is included in all regressions. All regressions are weighted with the sample weights. Robust standard errors are in parentheses clustered by state.* significance at 10%; ** significance at 5%; *** significance at 1%.

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