The Dissertation Committee for Jiwon Park
certifies that this is the approved version of the following dissertation:

Essays in Applied Microeconomics

Committee:

Leigh Linden, Supervisor

Stephen J Trejo

Richard Murphy

Raissa Fabregas

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by

Jiwon Park

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Dedicated to my parents.

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Essays in Applied Microeconomics

Jiwon Park, Ph.D.
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Supervisor: Leigh Linden

The first chapter examines whether funding for public schools affects parents' decision to send their children to private schools in the US. In the wake of the Great Recession, funding for public K-12 education fell precipitously and stayed low for several years. Exploiting the fact that states with greater reliance on state appropriations and states with no income tax experienced larger cuts, I 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.

In the second chapter, I investigate the effect of Facility-Based Childbirth Policy (FBCP) to promote facility-based child delivery (FBD) and prenatal care in Rwanda. To identify the causal effect on childhood mortality

rates, I utilize the geographical variation of FBD in the baseline period and the timing of the policy in a difference-in-difference framework. The reform has a substantial effect on infant (under one year) and child (under five years) mortality, with reductions of 12 and 25 deaths per 1,000 live births, respectively. However, the overall reduction in newborn (seven days) neonatal (30 days) mortality is not statistically significant despite a large increase in FBD. I also show that other policy interventions like performance-based financing schemes can strengthen the treatment effect on newborn and neonatal mortality, implying the importance of multiple approaches to reduce mortality rates.

The third chapter explores whether the increase in service outsourcing to India has reduced the employment of the occupations with greater exposure to Indian service imports. To account for endogeneity, I instrument for the growth of the US's service import from India, exploiting the change in Indian import in European countries. The occupation-level analysis produces a mixed result. An increase in service imports reduces the total employment from 2000 to 2006; however, the effect attenuates in the later period of 2006 to 2016. The change is skill-biased: the reduction in employment is smaller for college-educated workers in the first period, and the sign reverses later.

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Chapter 1

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

1.1 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, Forthcoming). 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, Forthcoming; 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 (Forthcoming) 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, Forthcoming; 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 (Forthcoming). 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.

1.2 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, Forthcoming; 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 1.1). The magnitudes of the funding cuts differ substantially across states in Figure 1.2. 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 1.3 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

[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.

effect and estimate the causal impact of public school funding.

First, states that relied more on the state appropriations to fund K-12 before the Great Recession experienced deeper cuts, first used by Jackson, Wigger and Xiong (Forthcoming) 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 1.4. 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) before the Great Recession, which becomes the first identifying variation.[3] On average, 47 percent of

[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.

[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,

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 1.5. 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 1.4.3. 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 1.5).

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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 1.5).

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 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

[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 the robustness check, I include these two states as no income tax states as well. The results are very similar (See Table 1.5).

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

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 1.6 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.

[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.

1.3 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)

[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.

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 the Census and ACS do not provide information on survey month.[16] See Appendix Section A.1 for the further details of the crosswalk.

In Table 1.1, 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 in-

[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.

[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).

come 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 1.4.

1.4 Econometric Model and Validity

1.4.1 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 1.2.

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

$$Private_{ipst} = \delta \widehat{Rev}_{pst} + X_{ipst}\pi + P_{pst}\kappa + \mu_p + \theta_t + \varepsilon_{ipst}, \tag{1.1}$$

$$Rev_{pst} = \sum_{k \neq 2007} [\, \beta_k S_s \times \mathbb{1}(k=t) + \gamma_k NT_s \times \mathbb{1}(k=t)] + X_{ipst}\lambda + P_{pst}\psi + \rho_p + \tau_t + \nu_{ipst}, \tag{1.2}$$

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 ($\frac{State\ \ Rev_{s,2006}}{Total\ \ Rev_{s,2006}}$),

[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.

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 1.1), 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 generalizable for other funding changes.

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

1.4.2 First Stage

In this section, I present the first stage result to confirm the relevance of the instrumental variables using Equation 1.2. 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 1.7. 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,

[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.

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 1.6. 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 1.5.).

1.4.3 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 revenue. 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 1.2, 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 1.8 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 1.2. 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.

1.5 Results

1.5.1 Main Results

I begin by estimating the main model presented in Equations 1.1 and 1.2 in Table 1.3, 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]

1.5.2 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 dis-

[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.

trict, 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.

1.5.3 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

[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).

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 (Forthcoming), 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 1.4, I regress the level of spending in each category: expenditure on the total operation, instruction, capital, and student support. There are statistically significant increases in all spending categories, except for capital spending. The impacts on total operational, instruction, and student support are all somewhat proportionate to the change in revenue ($1,000 or 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, Forthcoming), which finds a large effect on capital spending reduction in the same period, because I rely on different specification.[25]

[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.

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

[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.

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.

1.6 Robustness Checks

1.6.1 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 1.5, 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 1.5, 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

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

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 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 esti-

[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.

mate 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.

1.6.2 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 1.6, I test whether the impact of public school funding varies by migration status and show it does not.[29]

[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.

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 1.7, 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

[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.

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.

1.6.3 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 1.7, 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

[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.

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.

1.7 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 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.

1.7.1 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 1.8, 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.

[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.

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 1.8. 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 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

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

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 baseline private school enrollment

[36] In Appendix Section A.3.3, 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.

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.

1.7.2 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 1.9, 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 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 1.9.

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

[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 different 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.

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 1.8. 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 1.9, 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, Wigger and Xiong, Forthcoming; 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.

1.8 Conclusion

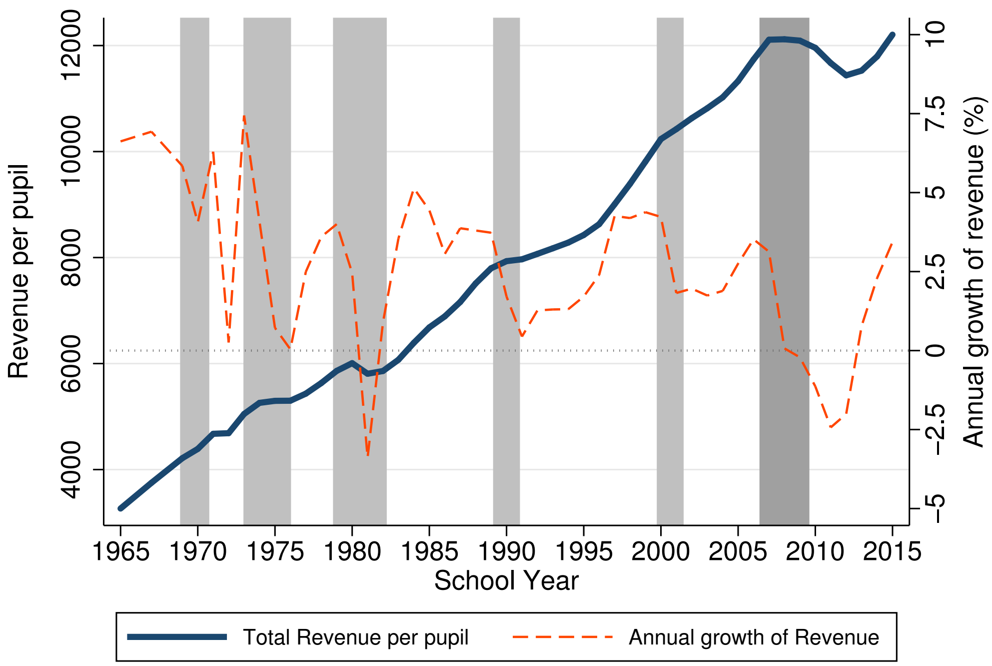
Private schools serve a significant portion of students in K-12 and play an essential role in improving education quality by providing an alternative and inducing competition. Parents often choose private schools because they believe private schools are better resourced than public schools. Considering 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.

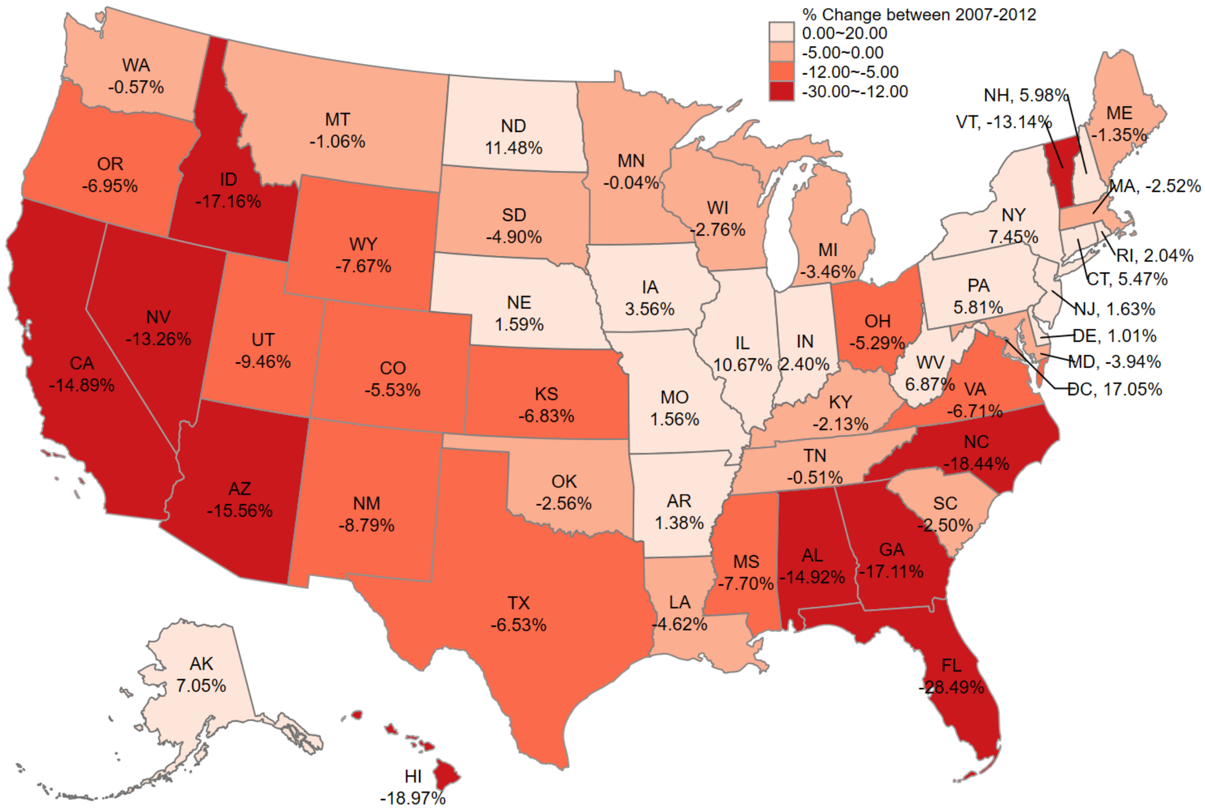
Finally, the Great Recession has an important lesson in handling the current economic crisis caused by COVID-19. We may experience another financial shock for K-12. It has been only a few years since the schools have fully recovered from the Great Recession, and another cut may result in even larger 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.

Figure 1.1: Real Total K-12 Revenue Per Pupil and Growth Rate



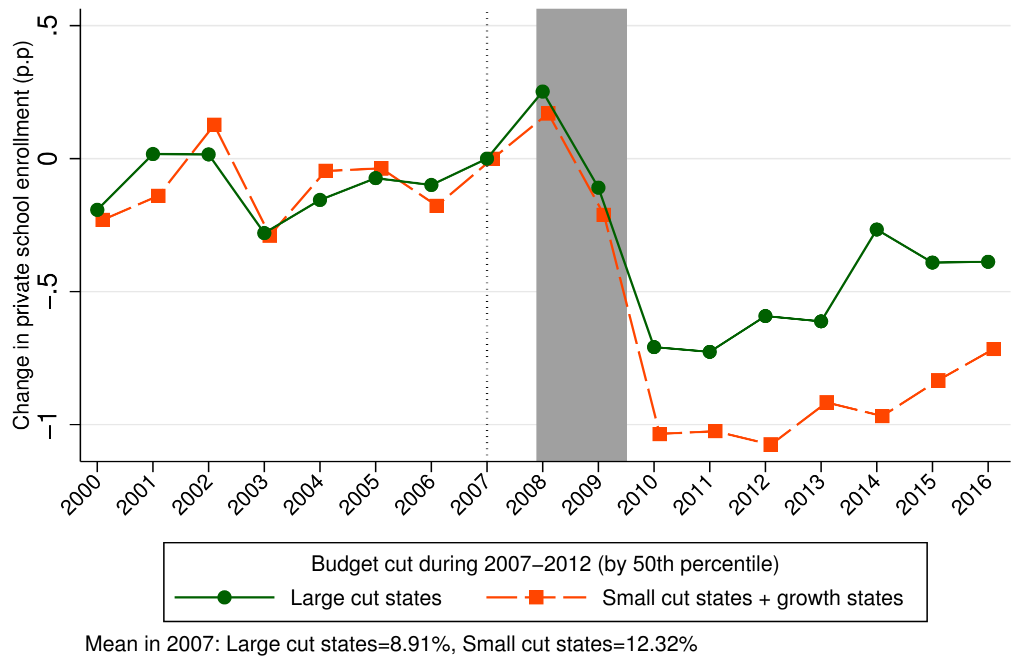
Notes: Data from the Common Core of Data (CCD) of the National Center for Education Statistics (NCES). Data aggregates the K-12 revenue in 50 states and divides by the full-time equivalent enrollment. The revenue per pupil is adjusted for inflation (in 2010 dollars). The orange dash line depicts the 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 1.2: Change of Revenue Per Pupil from 2007 to 2012



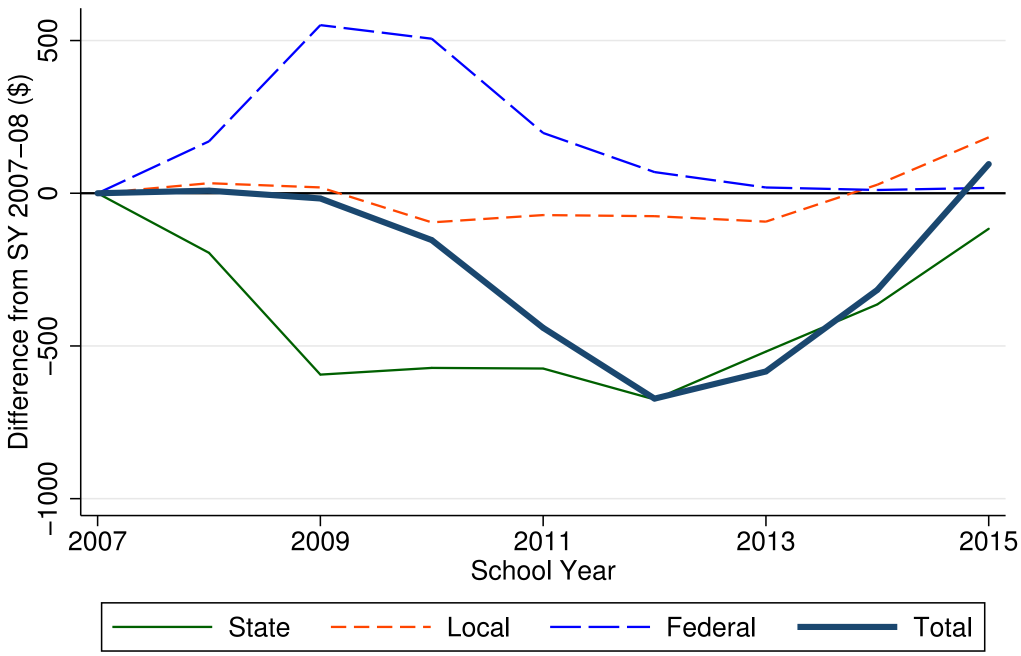
Notes: This figure shows the variation in funding cut across states induced by the Great Recession from 2007 to 2012. The percent change is calculated using real value of revenue per pupil in 2010 dollars. Darker shade means larger cuts and the 16 states with the brightest shade are states with growths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

Figure 1.3: 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.

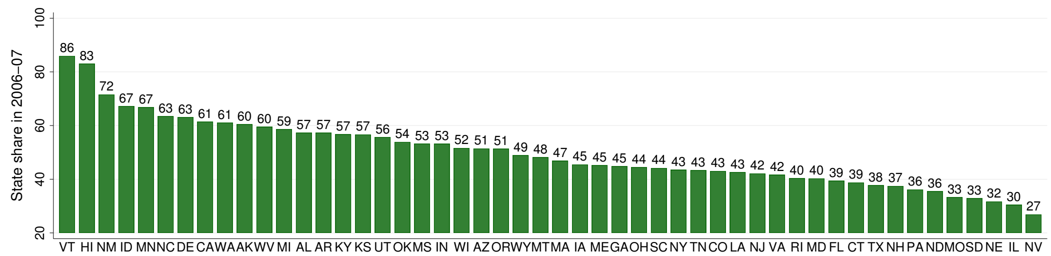
Figure 1.4: Trend of Revenue Compared to 2007, by Sources



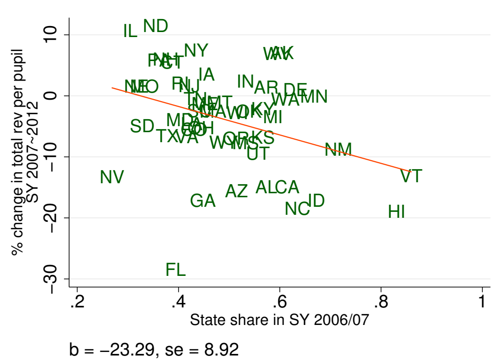
Notes: This figure shows the trend of the change in public education revenue by source. I calculate the dollar difference from the school year 2007-2008 level by sources to show how the budget had changed over time since the start of the Great Recession. All monetary values are in 2010 dollars.

Figure 1.5: Share of State Appropriations and Relation to Total Revenue and Funding Cut

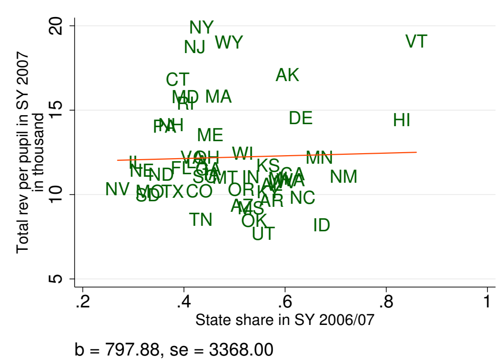
(a) Variation in Share



(b) Relation to Funding Cut



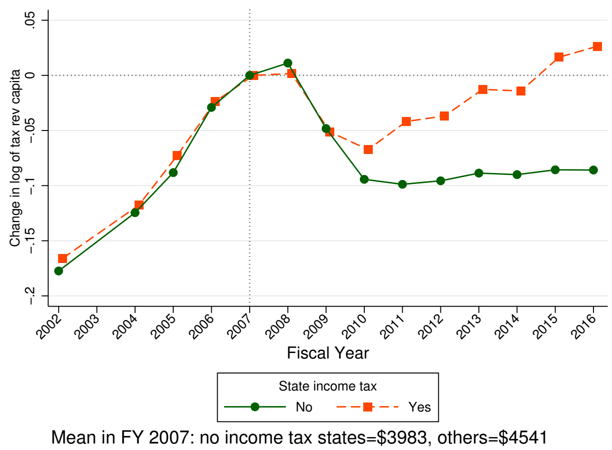
(c) Relation to Total Revenue



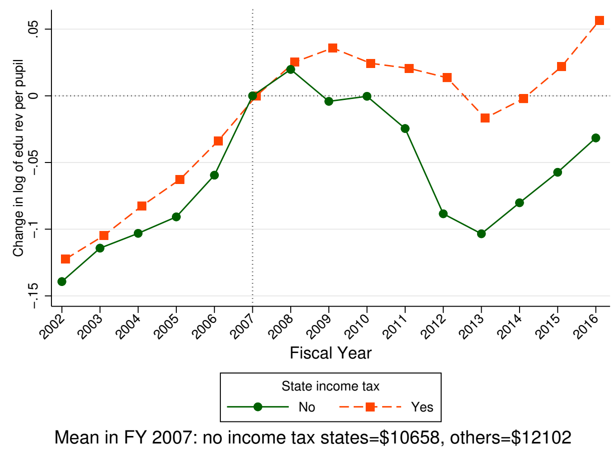
Notes: Panel A displays the variation in state share ($S_s = \frac{State\ rev}{Total\ rev}$) in SY 2006-2007, a year before the Great Recession in the 50 states. The numbers above the bars indicate the state share in 2006. Panel B shows the relationship between state share and total K-12 revenue per 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 1.6: Trend of Real Tax Revenue and K-12 Funding Compared to 2007, by State Income Tax Status

(a) Real Tax Revenue (in 2010 dollars)

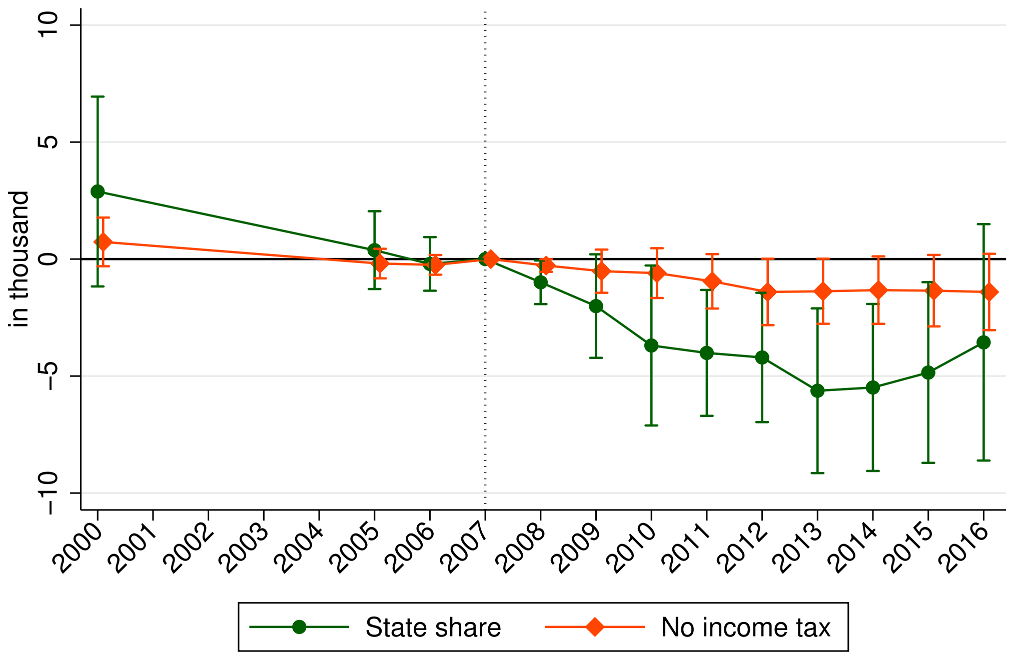


(b) Real K-12 Funding (in 2010 dollars)



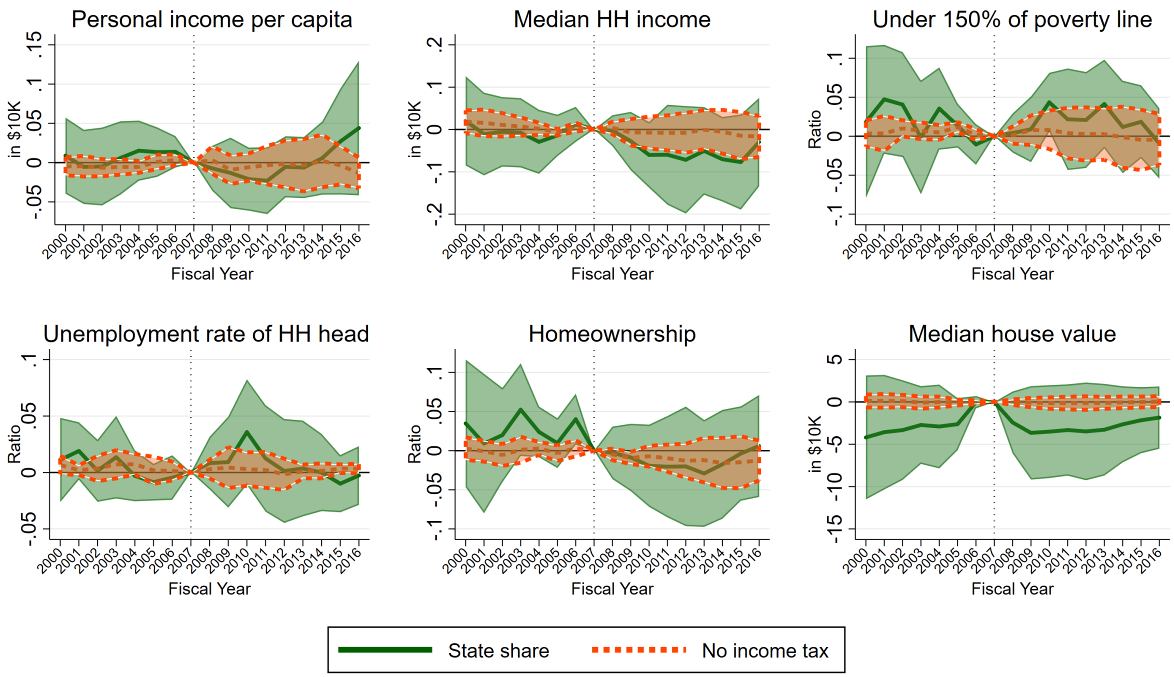
Notes: Panel A shows the trend of the mean tax revenue per capita relative to FY 2007 in two groups of states (states with and without an individual income tax). The mean of each group is the weighted mean with state population in 2000. Panel B shows the trend of mean K-12 funding per pupil relative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

Figure 1.7: 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 1.3 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 1.8: 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.

Table 1.1: Summary Statistics in the Pre-Recession Period

		Year≤2007		Year≥2008	
		Mean	SE	Mean	SE
		(1)	(2)	(3)	(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 1.3. Total education revenue is larger in the post-period because it was increasing before the recession. All monetary values are in 2010 dollars.

Table 1.2: Placebo Test in 2SLS

	Personal Income per capita ($10K) (1)	HH Income ($10K) (2)	Under 150% Poverty (3)	HH head Unemployed (4)	Home Ownership (5)	House Value ($10K) (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 1.3: 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 1.1. 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 1.2 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 1.4: Impact on Staff and Expenditure Categories

	(1)	(2)	(3)	(4)
Panel A. Expenditure per pupil				
	Total Operational	Instruction	Capital	Student Support
Rev per pupil (in thousand)	732.5***	477.9***	58.09	43.80***
	(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 (in thousand)	1957	3404***		
	(1571)	(944.6)		
	62,944	*18,854*		
Panel C. Staff per 100 students				
	Teacher	Aides	Guidance Counselor	Library Staff
Rev per pupil (in thousand)	0.175**	0.207***	0.007	0.020
	(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 1.5: 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 (1)	5-yr avg state share (2)	2000 state share (3)	1990 state share (4)	Add NH,TN in NT states (5)	State rev (6)	Expenditure (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 1.3. 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 1.6: 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 1.3. 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 1.7: 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 1.3. 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 1.8: 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 1.3. 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 1.9: 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). Other details are same as in Table 1.3. 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%.

Chapter 2

Does Facility-Based Delivery Save Lives? Evidence from Rwanda

2.1 Introduction

Despite the substantial reduction in the mortality rate for children in developing countries, it is still high compared to the developed world. To prevent premature deaths, one of the most critical goals in developing countries, governments have designed various policy interventions. Most developing countries have at least one public program to induce safe birth (skilled attendant at birth) to meet their Sustainable Development Goals (SDGs) set by the United Nations by 2030 (Doctor, Nkhana-Salimu and Abdulsalam-Anibilowo, 2018).

One of the most known programs is promoting safe birth and facility-based delivery (FBD).[1] Sound clinical studies prove that a skilled attendant's presence during labor significantly reduces the chance of maternal and neonatal deaths provided that the quantity and quality of the birth attendants are high enough (De Bernis et al., 2003; De Brouwere, Tonglet and Van Lerberghe, 1998; Kumar et al., 2008). This is largely true for developed countries where

[1] Other similar interventions include educating midwives and nurses and banning traditional midwives.

high-quality facilities are abundant. However, it is not clear whether FBD helps reduce mortality in developing countries (Fadel et al., 2015). There are several reasons why FBD can be ineffective. First, the quality of health facilities might be poor, or such countries lack proficient health assistants equipped with skills to deal with emergencies during labor. Second, traveling to facilities could increase the risk of deaths if the facility is too remote or transportation is poorly prepared. Finally, being surrounded by other patients may also cause other infections that lead to death (Graham, Bell and Bullough, 2001). Therefore, whether FBD and related policies in developing countries reduce newborn mortality is an empirical question.

This paper examines the causal relationship between FBD and childhood deaths by exploiting a policy intervention in Rwanda. Rwanda achieved its Millennium Development Goals of safe childbirth by increasing FBD from 35 percent in 2005 to 96 percent in 2015 after implementing a series of health policies. The most relevant policy is the Facility-Based Childbirth Policy (FBCP), which, in 2006, provided a full package of prenatal care and FBD for all pregnant women free of charge. A rapid increase in FBD rate follows the policy intervention as Figure 2.1 shows.

To evaluate the effect of this policy, I exploit the spatial variation in the program exposure represented by the extent of FBD in the baseline period and construct a difference-in-difference estimator. The assumption is that the regions with lower baseline FBD rates have more scope to increase FBD and prenatal care utilization by the reform in 2006. I use the Rwandan Demographic and Health Survey (RDHS), which contains rich information on the birth history and socioeconomic and demographic variables of mothers and households. I define the low FBD districts in which the historical FBD rate is relatively low and show those areas have a greater increase in FBD by 9.9 to 13 percent.

Despite a substantial increase in FBD in the treatment districts, I find weak evidence on any reduction in the newborn (death in seven days) and neonatal (death in 30 days) mortality. The newborn and neonatal mortality rate is 5.1 and 4.9 lower per 1,000 live births in the low-FBD districts after the reform, respectively, in the most conservative specification, although they are not statistically significant. I also expand the analysis to the later mortality rates. The difference-in-difference estimator suggests that the pre-period low FBD use is associated with a decline of 12 deaths and 25 deaths lower infant death (in one year) and child death (in five years) per 1,000 live births, which is equivalent to 15 and 20 percent reduction, respectively. The results are robust to using alternative treatment definitions, such as using continuous treatment or adjusting the threshold of defining low FBD districts.

Next, I examine whether the treatment effect varies depending on the exposure to other health policies. The Rwandan Ministry of Health implemented universal health insurance (Community-Based Health Insurance, CBHI) and Performance-Based Financing (PBF) scheme in a similar period, which was intended to improve access to and the quality of health services, respectively (Bucagu et al., 2012). The treatment effect does not vary by exposure to the CBHI scheme. However, the impact on mortality rates is stronger in districts exposed longer to PBF.[2] This result implies that the quality of a facility quality plays a vital role in increasing the impact of FBD and prenatal care.

This paper has two important contributions. First, it contributes to the scant literature investigating the causal effect of FBD on childhood mortality rate. Because it is difficult to find an exogenous variation in FBD, very few papers attempt to identify a causal effect without a policy intervention (Fadel et al., 2015; Okeke and Chari, 2018). Most prior studies have evaluated the impact of certain policies. A notable example is the Janani Suraksha Yojana (JSY) program in India, a conditional cash transfer program that rewards mothers and health providers for FBD. This program has successfully increased the FBD rate, especially among poor and rural households in India; however, there is scarce evidence that it helped reduce NMR (Lim et al., 2010; Powell-Jackson, Mazumdar and Mills, 2015; Randive et al., 2014). Studies in other countries tell a similar story: a financial incentive program in Nepal (Powell-Jackson et al., 2009) and the ban on the traditional birth attendants in Malawi (Godlonton and Okeke, 2016). The results of my paper are consistent with these previous studies. On the other hand, some studies find a meaningful reduction in neonatal mortality through government intervention (Feng et al., 2011; McKinnon et al., 2015). However, there are very limited studies on FBD

[2]The Ministry of Health started the CBHI and PBF pilot programs in 1999 and expanded nationwide in 2006. Thus, pilot districts had been exposed longer to CBHI and PBF.

and mortality rates beyond the neonatal period. My paper fills this gap.

Second, this paper is one of the few studies examining FBD in Rwanda. Rwanda has experienced a surprising increase in FBD during the last decade, but its causal effect is not well-known. Thus, it is important to confirm whether the seemingly successful free FBD policy has achieved its ultimate goal of reducing mortality rates. To my knowledge, there is only one more paper studying Rwanda. Chari and Okeke (2014) use the staggered roll-out of the performance-based financing program in Rwanda, where its performance determines the budget of each facility, and also find no effect of FBD on NMR. My paper focuses on a different policy, the free FBD policy, exploiting the pre-period prevalence of FBD in each district. Also, I expand the focus to later life mortality and find an improvement in infant and child mortality rates. Given that early life intervention potentially impact health in later childhood, it is crucial to examine the effect on other mortality measures. My paper implies that Rwanda was successful in reducing child deaths through promoting FBD and prenatal care.

2.2 Institutional Background

The Rwandan genocide in 1994 destroyed most of the health facilities and workforce, leading to a surge in mortality and morbidity rates. Furthermore, the health care utilization rate had severely dropped after the genocide with extreme inequality across the population. Wealthier, more educated, and urban households had a much greater propensity to pursue health care and have safe child delivery (Comfort, Peterson and Hatt, 2013).

Since 1999, at the end of the civil war, the Rwandan government had implemented a variety of health policies and public programs to improve people's health status. There are three notable policies: (1) Facility-Based Childbirth Policy (FBCP), which provides a full package of maternity care and delivery service free of charge, (2) community-based health insurance (CBHI) scheme, a universal health insurance plan eligible to the entire population,[3] and (3) performance-based financing (PBF), which determines the budget of each facility based on its performance in the previous year (Bucagu et al., 2012). Note that (1) and (2) are intended to improve access to health service while (3) is to improve the quality of the care. Table 2.1 shows the timeline of these events. It is noteworthy that most of the changes and expansion occurred together in 2006. While all of the three policies played a significant role in improving facility access and quality, I focus on the increase in facility-based delivery (FBD) in Rwanda induced by (1). The FBCP emphasized the benefits of FBD and provided pregnant women with free prenatal care and delivery service at health facilities regardless of insurance possession (Rwanda Ministry of Health, 2017). FBD rate had increased from 35 percent in 2005 to 95 percent in 2010 thanks to this effort (Figure 2.1). This increase is notable compared to other sub-Saharan African countries with similar health

[3]The premium and co-payment varies depending on the income and wealth of the household. Poorest households are exempt from premium or co-pay, depending on the region (Bucagu et al., 2012).

initiatives (Doctor, Nkhana-Salimu and Abdulsalam-Anibilowo, 2018).

Under the Rwandan health system, Health Centers handle prenatal care and normal childbirth (vaginal delivery). Complicated pregnancies are referred to the higher-level health facilities such as District Hospitals and Provincial Hospitals that are generally well equipped and capable to perform surgical procedures (Bucagu et al., 2012).[4] Most of the prenatal and normal delivery service is provided by midwives and nurses (Lundeen et al., 2019).

2.3 Data

2.3.1 Data Sources

The main dataset is the Rwandan Demographic and Health Survey (RDHS). I use the birth history data of 2005, 2008, 2010, and 2014/2015 waves of RDHS and stack them according to birth year to create a continuous set of births from 2000 to 2014. The birth history data includes all birth happened to the respondent (woman of reproductive age, 15–49 years old) retrospectively regardless of child's survival. For the children who have died, the respondents provide detailed information on the age of death (in months when they died before 60 months). The RDHS also collects information on the place of birth

[4]The healthcare system in Rwanda reaches from community-level care to the national hospitals. At the most basic level, community workers visit households and identify each household member's healthcare needs. Health posts and centers are the primary care unit. Health posts are smaller than centers, reaching out to the most remote portions of the country. The more complicated illness that cannot be treated in primary care units is referred to higher-level facilities, such as district hospitals (secondary) and provincial and national referral hospitals (tertiary).

and prenatal visits for children under five years old. The data also provides useful information on households and mothers' socioeconomic status (SES), such as household wealth and mother's education.

I use four measures of mortality rates: Newborn Mortality (death in seven days, NMR7), Neonatal Mortality (death in 30 days, NMR), Infant Mortality (death in one year, IMR), and Child Mortality (death in five years, CMR). Following the traditional definition, the mortality rates are scaled per 1,000 live births.

The Rwandan Government reformed its administrative areas in 2006 from 12 provinces and 106 districts to five provinces and 30 districts. Because of this change, it is difficult to compare the spatial change of FBD and other health outcomes across time. Fortunately, RDHS provides GPS coordinates of the primary sampling units (PSU, or clusters),[5] making it possible to identify the old district of each PSU in the later surveys. I match 2008-2014 PSUs to the 106 old districts to have a greater variation. Because the DHS displaces the GPS coordinates for privacy issues,[6] some measurement error still exists.

For additional information on the district characteristics, I use the Integrated Household Living Condition Survey (EICV) of Rwanda, 2005 and 2014 waves. This survey provides information on changes in people's well-being, such as economic conditions, education, health and housing conditions, house-

[5] Available from 2005 wave.

[6] The coordinates are displaced with some error; zero to two kilometers for the urban clusters and zero to five kilometers for rural.

hold consumption, etc. I use it as a supplementary data set to calculate the average insurance coverage rate and total population by district and survey year that RDHS does not provide. When calculating the average insurance coverage by district, I restrict the EICV cample to women 15-45 years old as the RDHS sample consists of reproductive-aged women.

2.3.2 Descriptive Statistics

Table 2.2 presents the summary statistics of the resulting data. I present the pre- and post-period relative to the free FBD policy. Panel A and B show the birth characteristics and mother's and household's characteristics, respectively. I have data on 58,660 births after removing missing variables.

Two things are noticeable in Panel A. First, the Facility-Based Childcare Policy seems to be effective in terms of service utilization. FBD rate had increased significantly, from 31 percent to 76 percent. Prenatal care utilization did not increase substantially; however, the number of visits increased, and the month of pregnancy at the first visit decreased. Second, the overall mortality rates had declined during this period. For example, CMR had declined the most, from 124 death to 66 death per 1,000 birth.[7]

Panel B shows that the overall socioeconomic status of the household

[7] When defining mortality rates, I only include the births that passed the threshold periods.For NMR and NMR7, I include births that happened at least one month before the survey. For IMR and CMR, births that occurred one year and five years before the survey date are included, respectively.

had improved during this period. Especially, the fraction of households with piped water had increased significantly, from 17 percent to 34 percent. Furthermore, Rwandan women become more educated and literate over time, consistent with the government's effort to promote female education in most developing countries.

2.4 Empirical Strategy

2.4.1 Basic Specification

To identify the causal effect of the FBCP in 2006 on facility use and mortality rates, I exploit spatial and temporal variation in the 'intensity of exposure' to the policy in a difference-in-difference framework. I use the baseline district-level home delivery rate (1-FBD rate) as the proxy for the intensity of exposure. In other words, I use the fact that districts with low FBD rates experienced a greater increase in FBD rates following the old literature.[8]

The geographic unit I use is old districts that were used until 2006. I take the districts with a low FBD rate (below median) as the treatment districts and call them low-FBD districts. Figure 2.2 displays how the treatment and control districts are distributed in Rwanda. Kigali area (capital) and other large cities are mostly control treatment; however, some rural areas are classified as control districts as well.

[8]Using the baseline means as exposure measures is found in previous works like Bleakley (2007); Godlonton and Okeke (2016); Osili and Long (2008).

Specifically, the main regression equation is as follows:

$$y_{idpmt} = \beta_1 Low\ FBD_d \times \mathbb{1}(t \geq 2006) + X_{it} + \tau_d + \eta_m + \theta_t + \alpha_p \times t + \varepsilon_{ipdmt}, \quad (2.1)$$

in which y_{idpmt} is an outcome for child i born in district d in province p in month m of year t. The outcome variables include an indicator for FBD, information on prenatal visits, and mortality rates, such as newborn (deaths within seven days) neonatal (deaths within 30 days), infant (deaths within a year), and child (deaths within five years) mortality rate. $Low\ FBD_d$ is one if the district has a low baseline FBD rate (or high home delivery rate) and $\mathbb{1}(t \geq 2006)$ is an indicator function whether t is greater or equal to 2006. X_{it} is a vector of birth and household characteristics. τ_d is the district fixed effect, and θ_t is the year fixed effect. η_m is the birth month fixed effect, controlling any possible seasonality. I allow province-level time trends to vary by the district to absorb any long-term linear trend in the outcome variables that may vary across provinces. 2005 RDHS is representative at the old district level while rest of the waves are at the new district level. Thus, I use the proper districts to cluster the standard errors.

The coefficient of the interest is β_1, the reduced-form impact of the policy change on the outcome variables, capturing the difference in change in the outcome variables before and after the reform between the districts of high and low FBD.

2.4.2 Validity of Identification Strategy

The identifying assumption for Equation 2.1 is that the high FBD districts are a good control group for the low FBD districts. In other words, the outcome variables should evolve in parallel in treatment and control districts. Women and children in low-FBD districts are disadvantaged, to begin with: low FBD rate is correlated with lower SES and fewer and worse quality health facilities in the district. This discrepancy can cause a differential change in relevant variables and affect the outcome variables. Thus, it is important to examine whether the changes in other factors that could affect FBD and mortality rates are correlated with the treatment status.

In Figure 2.3, I display the trend of FBD and prenatal care in treatment and control districts. The pre-trends in treatment and control districts are very similar for all three variables. Especially in Panel A, there is a much larger increase in FBD rate in treatment districts, suggesting that the baseline FBD rate is a good proxy for treatment intensity.

Figure 2.4 graphically presents the coefficients of $Low\ FBD_d \times \mathbb{1}(t \geq 2006)$ with different outcome variables. If the point estimate is statistically significant, it indicates a differential change between two groups. Most of the coefficients are not distinguishable from zero. Birth characteristics such as sex, twin indicator, and whether the mother is under 20 (young mother) are all uncorrelated. Whether the child is a first child and current marital status are statistically significant. I suspect that this stems from a decline in total fertility and an increase in men's survival rates. I also test mother and household

SES, such as an indicator for primary school completion, living in an urban area, possession of a car or motorcycle and piped water, and wealth index, all of which are not associated with $LowFBD_d \times \mathbb{1}(t \geq 2006)$. Overall, results in Figure 2.4 indicate that treatment status balances almost all observable characteristics, and the research design is unlikely to be biased by changes in unobservable variables.

2.5 Results

2.5.1 Effect on Facility-Based Delivery and Prenatal Care

Table 2.3 presents β_1's of Equation 2.1 with different specifications for three outcome variables: an indicator for FBD, number of prenatal visits, and month at the first visit. In all panels, the coefficients are robust to the inclusion of birth-related controls and SES controls in columns 2 and 3. In my preferred specification, column 4, I include province-specific time trends to adjust to the differential time trend of each area. In Panel A, the coefficient means that the FBD rate had increased by 9.9 FBD districts in the preferred specification. Overall, Table 2.3 implicates that the policy changes effectively increased FBD. The policy was also intended to increase prenatal care at health facilities, and Panels B and C show it was effective. The number of prenatal visits increased by 0.113 times, and the month at first visit declined by 0.125 months in my preferred specification (column 4). The results in Table 2.6 are consistent with Figure 2.3.In Appendix Figure B.1, I present the results in the event study framework.

2.5.2 Effect on the Mortality Rates

The previous results show that the health reform in 2006 successfully increased FBD rate and prenatal care use in Rwanda. Here, I examined whether this is associated with a reduction in mortality rates. Since the mortality rates had surged during the Rwandan genocide and thus led to a very different trend in the late 90s, I only include births after 1999 even though the RDHS asks the full history of the birth career.[9] The primary outcome is the binary indicator for deaths. To match the standard definition of the mortality rates in developing countries (deaths per 1,000 lives), I assign 1,000 to the deaths.[10] I tested deaths in seven days (newborn death), 30 days (neonatal mortality), one year (infant mortality), and five years (child mortality).

Figure 2.5 presents the mortality trends in treatment and control districts. The relatively small sample size of RDHS gives the unsmoothed trend of mortality rates. Consistent with Table 2.2, there is an overall reduction in mortality rates in all panels. It is not clear there is a larger decline in newborn and neonatal mortality rates in treatment districts in Panels A and B. However, infant and child mortality rates in treatment and control districts seem

[9] Including earlier birth can be potentially problematic for following reasons as well. First, it is more likely that mothers imprecisely remember the child's information, especially when she did not survive (age at death, year of birth, and even omitting the birth). Second, I need to assume that the birth happened in the same district where the respondent currently lives. When including older births, the measurement error increases.

[10] This is to scale the mortality rates per 1,000 live births. This method is used in Geruso and Spears (2018).

to converge after the health reform (Panels C and D).

The difference-in-difference results with the preferred specification are presented in Table 2.4.[11] Each cell is from separate regressions with Equation 2.1, and dependent variables are described in the column title. There is an insignificant decline in the newborn and neonatal mortality rates in columns 1 and 2, respectively. Infant and child mortality rates declined by 12 and 25 deaths per 1,000 live births, corresponding to 15 and 20 percent, respectively, in low FBD districts. These point estimates are both statistically significant.

2.5.3 Alternative Specification

My main specification defines low FBD districts as the districts whose baseline FBD rate is below the median (of all districts). In Table 2.5, I use alternative specifications to examine the robustness of my result. Overall, Table 2.5 shows my results are robust to using an alternative definition of the treatment.

Panel A uses the home delivery (1-FBD) rate in a continuous form as the treatment variable. FBD rate increased by 47 percent when the baseline FBD rate decreases from 100 to 0 percent. Because of the scale of the treatment variable, the point estimates in Panel A are larger than the results in Tables 2.3 and 2.4. Panel B defines the low FBD districts as districts whose FBD rate is below the top 25th percentile. By defining the treatment variable this way,

[11]See Appendix Figure B.2 for event study results.

I get larger and more precise estimates for most outcome variables. However, limiting control districts to the top 25 percent of the FBD rate results in comparing rural and urban areas. To include rural areas in the control group, a more generous definition of treatment status is necessary.[12]

In Panel C, I define a composite index representing the exposure to FBD and prenatal care in the baseline period. I perform Principal Component Analysis (PCA) with district-level FBD rate, frequency of prenatal visits, and month at the first prenatal visit and take the first principal component (PC1) as the indicator.[13] Similar to the main specification, the treatment variable is one when the composite index is lower than its median (low use of prenatal care and FBD). Because I include the two prenatal care measures as the treatment intensity measure, the point estimates get larger and more precise in columns 2 and 3. The impact on mortality rates is similar to the main specification with slightly smaller point estimates, although not statistically significant.

Prenatal care and FBD may be associated with the overall use of health facilities. Areas with high utilization of health services are often areas with higher accessibility and more well-equipped facilities. Also, the free FBD program happened together with other policies like universal insurance. Thus, I test whether the results are substantially different when using an index for pre-period facility use in Panel D. Included variables are whether the respon-

[12] This specification much more precisely estimates the result. However, there is a differential pre-trend in FBD rates between treatment and control districts, thus, I did not use it as the main specification.

[13] See Vyas and Kumaranayake (2006) for further information on PCA.

dent (mother) visited a facility within 12 months, whether she uses modern contraceptives, whether she got tested for HIV, and whether a child was taken to a facility for diarrhea or fever. I also include FBD and prenatal care variables. Similar to Panel C, I take the PC1 and define the low index status if the index is below the median. The point estimates are similar, however, more precisely estimated. This result suggests that low facility use and low FBD rate before the reform are somewhat correlated.

2.5.4 Impact on Other Health Services Utilization

My results show that promoting FBD successfully reduced mortality rates, although the impact on newborn and neonatal deaths is not precisely measured. Free prenatal care and FBD may induce mothers to visit health facilities for other reasons after birth, as they learn facilities are accessible and affordable. In this case, it is difficult to determine whether more frequent use of health facilities after birth or prenatal care and FBD resulted in reductions in mortality rates. In Table 2.6, I test whether the free FBD policy is associated with an increase in facility visits for other reasons than prenatal care and delivery.

Panel A shows little impact on children's facility visits. Interestingly, children born from cesarean section (C-section) did not increase due to the free FBD program. This result suggests that while the overall FBD rate increases, C-section does not increase accordingly. In Rwanda, C-sections are available at the district or provincial hospitals (secondary or tertiary facilities); however, this process is often not smooth (Harrison and Goldenberg, 2016; Niyitegeka et al., 2017). If complicated deliveries were not successfully transferred in some areas, it might explain why newborn and neonatal mortality rates are relatively less affected. Surprisingly, the use of postnatal care does not increase as well, although postnatal care is included in this free FBD program. In Panel B, there is evidence that women in treatment districts are more likely to use health services. Especially, they tend to visit any health facilities within 12 months and get an HIV test. However, there is an insignificant increase in the tendency to visit fertility planning (column 3) or use modern contraceptives (column 4) conditional to using any birth control methods. To summarize, the FBCP may have increased the overall facility use to some extent; however, it seems that it is not the primary reason for the decline in mortality rates.

2.6 Relation to Other Health Policies

Previously I show there were three critical programs in the 2006 health reform in Rwanda. This section examines whether the treatment effect varies depending on the intensity of two other health programs: free universal insurance (community-based health insurance, or CBHI) and performance-based financing (PBF).

From 2006, insurance coverage had increased significantly in Rwanda thanks to CBHI. In 1999, the Rwandan Ministry of Health started a pilot program of CBHI in three Health Districts.[14] After the success in these three districts, the Ministry of Health decided to expand the program nationwide (Nyandekwe, Nzayirambaho and Kakoma, 2014). Even before the expansion, some districts independently offered CBHI or other public health insurance schemes and some wealthy households were able to possess their insurance. Thus, the intensity of exposure to health insurance also varied across regions. In other words, districts without CBHI experienced a greater increase in insurance coverage after 2006.

Due to the lack of data, I cannot match each PSU to the Health Districts. Thus, I use EICV and estimate the insurance coverage change between 2005 and 2014 in each (new) district and use it to measure the intensity of exposure to CBHI. In columns 1 and 2 of Table 2.7, I compare the treatment effects on outcome variables in districts with larger vs. smaller change in insurance coverage.[15] FBD rate tended to increase much larger in districts with a larger increase in insurance coverage. However, point estimates on the rest of the outcomes are not statistically different in columns 1 and 2. Insurance may have increased access to health services and thus FBD; however, impact on mortality rates is limited.[16]

[14]There are 40 Health Districts in Rwanda, which is a separate administrative area. They largely match to new 30 districts.

[15]Large increase districts are districts with low baseline insurance coverage. Separating the sample by baseline insurance coverage results in a similar result (Results available upon request).

[16]I test the treatment effect of CBHI using Equation 2.1 in Appendix Table B.2. It seems that the expansion of CBHI is not associated with a reduction in mortality rates, except for CMR.

Performance-based financing (PBF) intends to improve health facilities' quality by rewarding them based on their performance in the previous year. Like CBHI, PBF also had a pilot study period in a few health districts and became nationwide in 2006 (Chari and Okeke, 2014). Districts that implemented PBF earlier (i.e., pilot districts) are more likely to have better quality facilities if PBF is effective. In columns 5 and 6, I divide the sample by the timing of PBF implementation (earlier or later). The impact on FBD and prenatal care is mixed. The treatment effect on FBD is similar; however, effects on the number of prenatal visits are statistically different in columns 5 and 6. On the other hand, there is a consistent pattern in effects on mortality rates. The decline in mortality rates is greater in districts that implemented PBF earlier, although it is significantly different only for newborn mortality. While PBF itself may not have increased the treatment effect of free FBD policy, my results imply that it may have improved health facilities' quality and thus reduce mortality rates.[17]

2.7 Discussion

Despite the remarkable success in promoting FBD in Rwanda, there is limited evidence that FBD reduces newborn and neonatal mortality in this paper. It is hard to believe that FBD reduces infant and child mortality

[17]Papers like Basinga et al. (2011); Chari and Okeke (2014) finds that PBF itself increased FBD rate. However, they both did not find PBF reduced mortality rates.

without reducing newborn and neonatal mortality rates because FBD is more directly related to earlier life deaths (Moyer, Adanu and Engmann, 2013). However, prenatal care and FBD may reduce infant and child mortality by improving the child's long-term health status. For example, prenatal cares play an essential role in promoting women's health literacy (Lori et al., 2014) and positively affect child health.

There are several possible explanations why the impact on newborn and neonatal deaths is limited. First, the quality of the facilities or the care provided may not have been sufficient enough to have a meaningful impact on newborn and neonatal mortality rates. Very basic causes of death could be prevented by primary care in the health facility. However, more resources are required to deal with complicated causes. In fact, about 84 percent of health centers did not have an ambulance in 2014. The majority of health centers own a motorcycle as the main means of transportation, not appropriate for patient transfer. Almost none of the health centers have an X-ray or anesthesia machine. Even in the provincial and district hospitals, 15 percent did not own anesthesia machine (Rwanda Ministry of Health, 2015). Although both devices may have little to do with safe delivery (especially X-rays), it implies how low the overall quality of Rwandan health facilities is. Because of the data limitation, I cannot examine the differential treatment effect based on the quality of the nearby health facility. The negative and significant estimates on the newborn and neonatal mortality rates in column 5 in Table 2.7 suggest the quality matters, although the coefficients are not statistically distinguishable from column 6.

Second, the increase in the use of prenatal care and family plan may have made high-risk women successfully deliver their children. In other words, without the reform, some of the children might not have been born alive, not contributing to the death rates. Alternatively, a high-risk woman who would not have been able to conceive may have got pregnant as she became healthier. Either way, the chance of high-risk children being born increases, and the overall neonatal mortality does not go down. In Figure 2.4, the average birth order in the treatment group increases (because the likelihood of being the first birth decreases), supporting that there has been more birth in treatment districts after the reform.

2.8 Conclusion

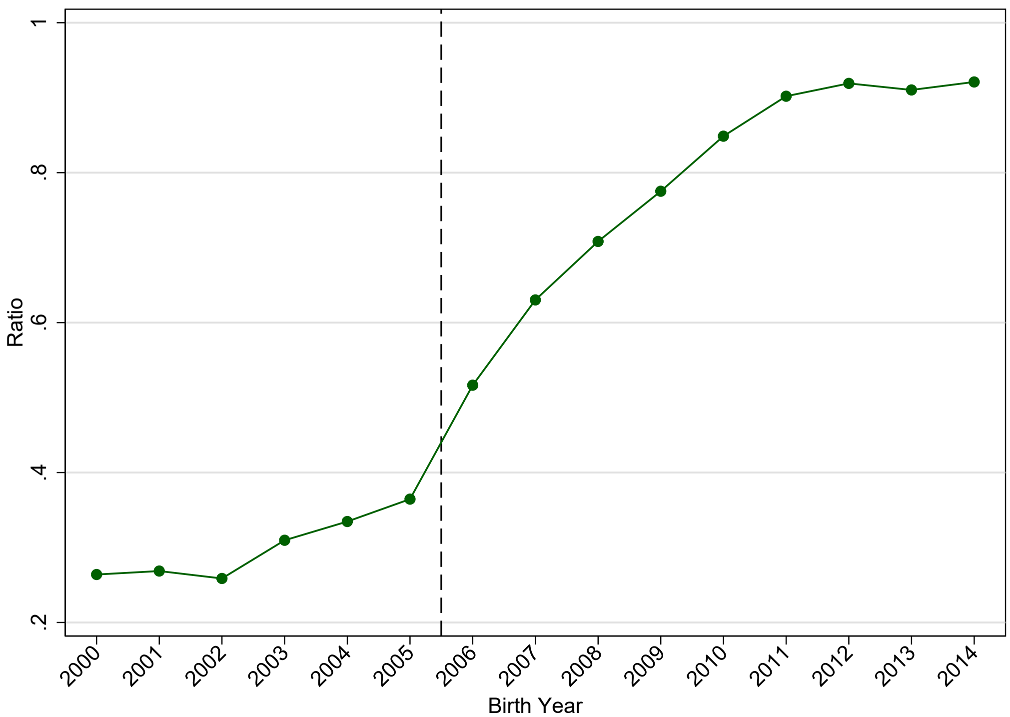
Today, still more than 2.5 million children die within a month after birth every year. Many developing countries have made enormous efforts during the last thirty years, but premature deaths are still far more frequent than their developed counterparts. Because a health professional can readily treat some causes of deaths during labor, FBD and safe delivery very important. However, several empirical studies do not support the positive impact of FBD on neonatal mortality.

Rwanda is recognized as one of the most successful countries promoting FBD. Rwanda's experience is unique in that only a few countries went through such a rapid surge in FBD. Today, most Rwandan children are now born in a health facility, and women have access to pre-and postnatal care thanks to FBCP initiated in 2006. By leveraging variation in the district-level FBD rate in the baseline period in a difference-in-difference framework, I find evidence that the policy effectively improved FBD and prenatal care utilization.

The estimates show that the policy had reduced the infant mortality rate and child mortality rate by 12 and 25 deaths per 1,000 births. I also find that newborn and neonatal mortality declined, although not statistically significant. My results are robust to using alternative treatment definitions. The size of the treatment effect of free FBD and prenatal care programs is not related to other health policies in this period, such as universal health insurance or PBF.

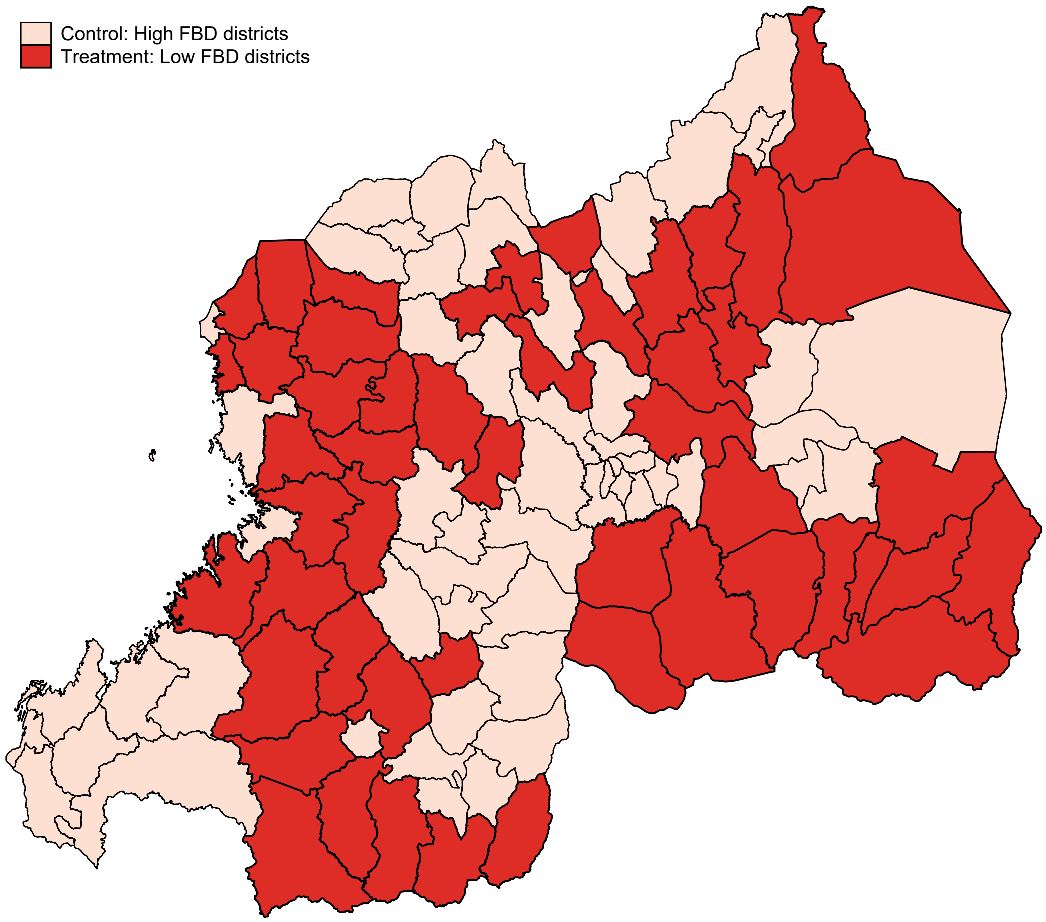
It seems Rwanda has a little problem with access to maternal health services. However, anecdotal evidence tells the quality of Rwandan facilities is far below the standards in developed countries. Even secondary and tertiary level facilities lack basic equipment in the developed countries' standards. For a country like Rwanda, where its quantitative success is widely known, it is time to seek qualitative improvement in the health sector.

Figure 2.1: Trend of Facility-Based Delivery in Rwanda



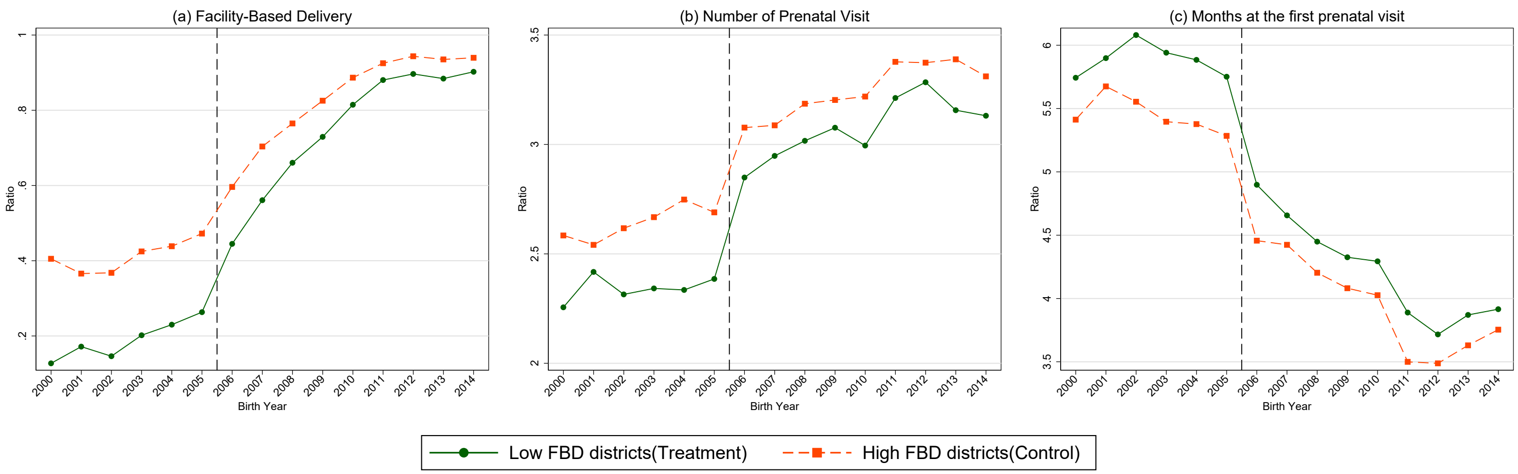
Notes: Data from the Rwandan Demographic and Health Survey (RDHS). I combine multiple rounds of RDHS to show the trend of FBD. The Facility-Based Childbirth Policy (FBCP) was implemented in 2006, and the dashed line separates before and after the policy intervention. The figure presents a steep increase in FBD rate after the policy implementation.

Figure 2.2: Treatment/Control Assignment



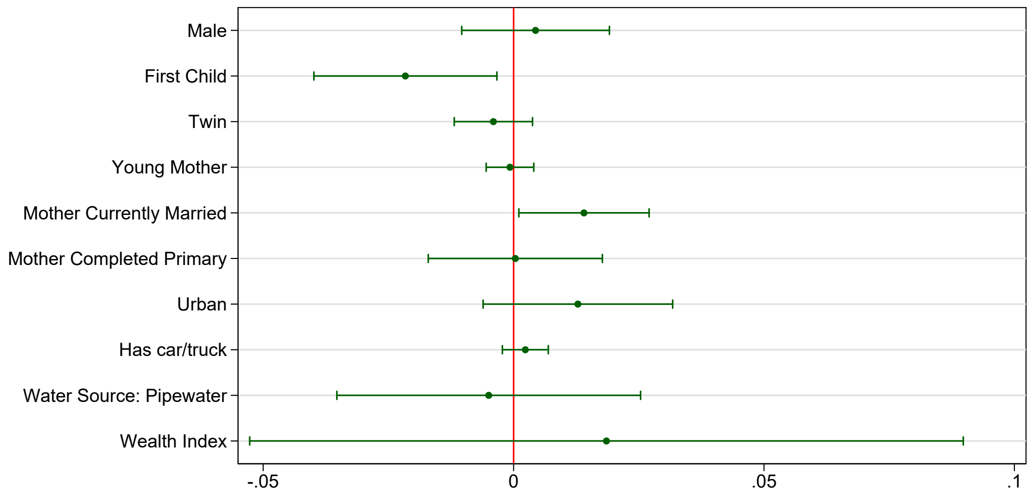
Notes: This figure shows the map of Rwanda with its administrative districts in 2005. The treatment districts or the low FBD districts are depicted in the darker shade. The treatment districts are districts whose baseline average FBD rates are below the 50th percentile. High FBD or control districts are concentrated on the Kigali area and other urban cities, however, not entirely.

Figure 2.3: Trend of Facility-Based Delivery and Prenatal Care



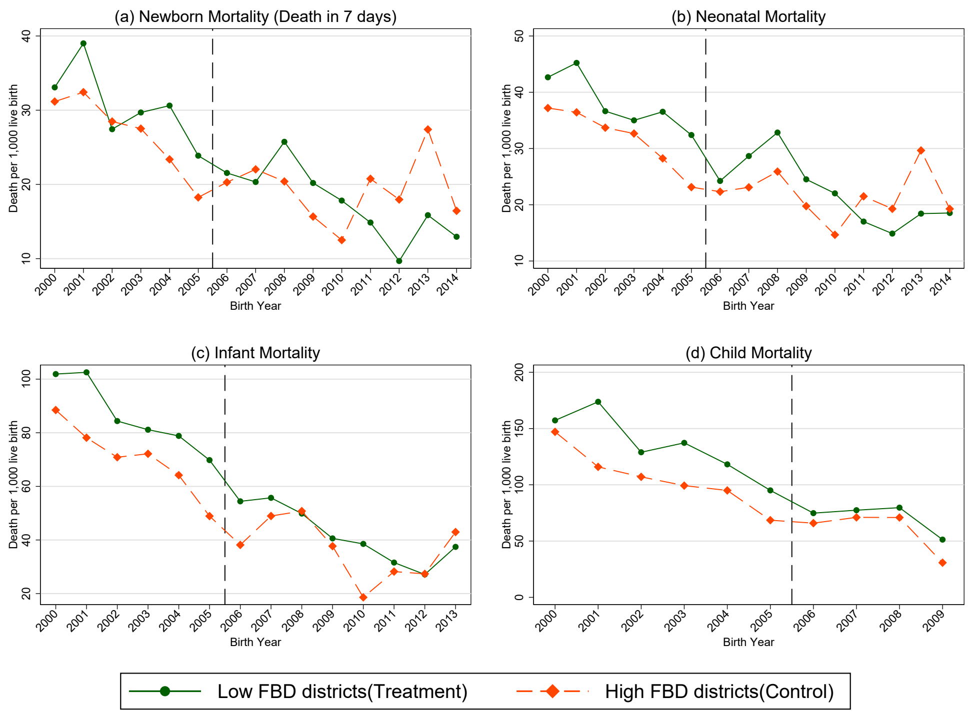
Notes: This figure shows the trend of FBD (Panel A), the number of prenatal visits (Panel B), and the month at the first visit (Panel C) in treatment and control districts. Treatment and control status is defined as the same in Figure 2.2. The solid green line and dashed orange line represent treatment and control districts, respectively. Three figures all show that the difference between treatment and control districts got smaller after 2006, implying the policy was effective.

Figure 2.4: Balance Test



Notes: This figure graphically shows the result of the balance test. I separately run regressions using Equation 2.1 with the dependent variables described in the vertical axis and plot the difference-in-difference estimates (β_1 with 95% confidence interval. Controls are not included. Standard errors are clustered at the proper district level. (Old districts for the 2005 wave and new districts for the rest of the data.)

Figure 2.5: Trend of Mortality Rates



Notes: This figure shows the trend of the newborn (death in seven days), neonatal (in 30 days), infant (in one year), and child (in five years) mortality rate by the treatment status. Treatment and control status is defined as the same in Figure 2.2. The solid green line and dashed orange line represent treatment and control districts, respectively. Because I cannot determine the survival status of children who have lived less than the threshold period, I drop the observations when estimating the mortality rates. For example, babies born less than a year ago are all omitted when estimating the infant mortality rate. Therefore, the sample size is smaller when estimating infant mortality and child mortality. While it is not clear for the newborn (Panel A) and neonatal (Panel B) mortality rates, it seems that infant and child mortality rates relatively declined in treatment districts.

Table 2.1: Rwanda Health Policy Events, 1999-2010

Year	Policy Description
1999	Pilot project on community based health insurance (CBHI)
2001	Performance-based financing contracts (PBFC) pilot projects
2005	Rwanda Health Sector Policy (including Sexual and Reproductive Health)
2006	**Facility-Based Childbirth Policy** **PBFC introduced in all districts** **CBHI becomes mandatory** National family planing policy
2007	Government declares family planning a development priority
2008	Health facilities made autonomous Community health program enhanced Maternal death reviews institutionalized

Source: Bucagu et al. (2012)

Table 2.2: Summary Statistics

	Year≤2005			Year≥2006		
	Mean	SD	Observation	Mean	SD	Observation
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Birth Characteristics						
Male	0.51	0.50	33559	0.50	0.50	25363
Birth Order	3.62	2.30	33559	3.31	2.24	25363
Twin	0.03	0.17	33559	0.03	0.17	25363
FBD	0.31	0.46	11279	0.76	0.43	18778
Prenatal Care	0.95	0.22	6430	0.98	0.13	14261
Number of Prenatal Visit	2.52	1.15	6421	3.13	0.97	14257
Month at First Prenatal Visit	5.64	1.50	6096	4.15	1.48	14021
Newborn Mortality (NMR7)	28.75	167.10	33455	19.39	137.88	25205
Neonatal Mortality (NMR)	35.07	183.96	33455	23.22	150.59	25205
Infant Mortality (IMR)	79.40	270.36	31810	42.86	202.56	21179
Child Mortality (CMR)	123.60	329.13	22097	65.68	247.74	6531
Panel B. Mother and Household Characteristics						
Age at Birth	28.22	6.31	33559	28.41	6.28	25363
Currently Married	0.85	0.36	33559	0.84	0.36	25363
Some Education	0.74	0.44	33559	0.81	0.39	25363
Urban	0.13	0.34	33559	0.14	0.35	25363
Has Pipewater	0.31	0.46	33559	0.34	0.47	25363
Has Car/Truck	0.01	0.09	33559	0.01	0.10	25363

Notes: This table shows the summary statistics of the entire data used. In Panel A, I present the variables related to children or births. The unit of observation is each birth. Pre- and post-policy is divided by birth year. There is a considerable variation in the observation number because some detailed birth information is only available for the births in recent five years. The place of birth (facility or not), prenatal care, and vaccination information are available for recent births only. Survival status, gender, birth order (from the same mother), twin indicator, and the mother's age at birth is available for the whole sample. Because the observation unit is birth, some children (births) in the sample are from the same mother. Panel B presents the summary statistics of the variables of the mothers and households. Like Panel A, the unit of observation is birth, so some mothers are duplicated if she has more than one child.

Table 2.3: Effect on Facility-Based Delivery and Prenatal Care

	(1)	(2)	(3)	(4)
Panel A. Facility Based Delivery				
Low FBD District	0.135***	0.143***	0.133***	0.0985***
× Post	(0.0156)	(0.0155)	(0.0168)	(0.0159)
Observations	30,057	30,057	30,057	30,057
Panel B. Number of Prenatal Visit				
Low FBD District	0.151***	0.157***	0.149***	0.113**
× Post	(0.0419)	(0.0417)	(0.0439)	(0.0438)
Observations	20,678	20,678	20,678	20,678
Panel C. Month at the First Prenatal Visit				
Low FBD District	-0.194**	-0.209***	-0.202**	-0.125*
× Post	(0.0769)	(0.0763)	(0.0788)	(0.0755)
Observations	20,117	20,117	20,117	20,117
Birth Controls		Yes	Yes	Yes
SES Controls			Yes	Yes
Province Time Trend				Yes

Notes: This table presents the treatment effect on FBD and prenatal visits. Dependent variables are presented as the panel title. Each column presents β_1 of Equation 2.1 with different specifications described in the bottom part of the table. The point estimate is interpreted as following: in column 4 (preferred specification) of Panel A, the FBD rate increases by 9.9 percentage points in treatment districts. SES controls include maternal education, household wealth, urban indicator, and an indicator for possession of a car and piped water. Birth controls include gender, twin indicator, a full set of dummies of birth order, marital status, and mother's age at birth and its square term. Because Rwanda changed its administrative boundaries in 2006 (from 106 to 30 districts) and the RDHS is representative at the district level at the survey time, I use the districts appropriate to the time to cluster standard errors, i.e., the old districts in the 2005 survey and the new districts in 2008-2014 surveys. Regressions are weighted using sample weights from the RDHS. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 2.4: Effect on Mortality Rates

	NMR7 (1)	NMR (2)	IMR (3)	CMR (4)
Low FBD District	-5.121	-4.895	-12.15*	-25.06**
× Post	(3.205)	(3.401)	(6.573)	(10.98)
Observation	58,660	58,660	52,826	28,628

Notes: This table shows the treatment effect on different mortality rates. Dependent variables are presented as the column title. Each column presents β_1 of Equation 2.1 with the preferred specification (column 4 in Table 2.3). Because I cannot determine the survival status of the children who did not pass the thresholds for each definition, I drop all the children who did not reach the threshold at the survey. This makes the number of observations smallest for CMR. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 2.5: Alternative Definitions of Treatment

		Prenatal Care		Mortality Rates			
	FBD	Frequency	First Month	NMR7	NMR	IMR	CMR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Continuous Home Delivery (1-FBD) Rate							
Treatment	0.472***	0.584***	-0.652**	-9.479	-10.08	-29.41*	-63.09**
× Post	(0.0488)	(0.150)	(0.261)	(9.117)	(10.08)	(16.60)	(29.97)
Panel B. Home Delivery Rate Above 25th Percentile							
Treatment	0.120***	0.166***	-0.239***	-5.392*	-6.887**	-11.94**	-23.86**
× Post	(0.0204)	(0.0513)	(0.0880)	(3.128)	(3.017)	(5.290)	(11.02)
Panel C. Composite Index of FBD and Prenatal Care							
Treatment	0.0824***	0.216***	-0.299***	-3.734	-3.584	-7.337	-20.80
× Post	(0.0165)	(0.0429)	(0.0756)	(3.502)	(3.809)	(6.642)	(12.84)
Panel D. Composite Index of Facility Use							
Treatment	0.0833***	0.165***	-0.179**	-4.824	-6.316*	-10.91**	-21.35**
× Post	(0.0162)	(0.0411)	(0.0730)	(2.958)	(3.392)	(5.301)	(9.507)
Observation	30,057	20,678	20,117	58,660	58,660	52,989	28,628

Notes: This table shows the treatment effect on different outcome variables using alternative definitions of treatment. Dependent variables are presented as the column title. Each column presents β_1 of Equation 2.1 with the preferred specification (column 4 in Table 2.3). Panel A uses the continuous home delivery rate (1-FBD) of each district as the treatment variable. Panel B defines low FBD districts as those whose home delivery rate is above the 25th percentile (FBD rate below the 75th percentile). I define a composite index combining FBD and prenatal care utilization in Panel C. In Panel D, I add other facility utilization measures, such as whether the mother visited a facility in the last 12 month, utilizing modern contraceptive, whether got an HIV test, and whether the child got treatment for diarrhea and fever at the health facility. In Panels C and D, I construct the index using the Principal Component Analysis (PCA) and taking the first component (PC1). The treatment variable is defined as the same in the main specification: low use if the index is below the mean. See the main text for further information. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 2.6: Effect on Other Facility Utilization

	(1)	(2)	(3)	(4)
Panel A. Children				
	C-section	Postnatal care	Treated Diarrhea	Treated Fever
Low FBD District	-0.00414	-0.0258	-0.0348	0.0061
× Post	(0.00690)	(0.0228)	(0.0345)	(0.0283)
Observation	27,517	17,974	3,686	9,638
Panel B. Mothers				
	Visited Facility	HIV Test	Fertility Planing	Contraceptive
Low FBD District	0.0694***	0.0868***	0.0238	0.0310
× Post	(0.0223)	(0.0211)	(0.0172)	(0.0209)
Observation	22,772	22,744	22,764	16,700

Notes: This table shows the treatment effect on children's and mother's health facility utilization. Dependent variables are presented at the top of each column. Each column presents β_1 of Equation 2.1 with the preferred specification (column 4 in Table 2.3). Panel A shows the health service utilization of children. Treated diarrhea and fever are one when the child was taken to a health facility for treatment conditional on having symptoms within a week. In Panel B, I present treatment effects on mothers' facility utilization. Visited Facility is one when the mother had visited a facility within 12 months. Contraceptive is one when the mother uses modern contraceptive methods conditional on the intention of birth control. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 2.7: Heterogeneity by Exposure to Other Policies

	Change in Insurance Coverage		Difference		Performance-Based Finance		Difference	
	Large	Small	(1)-(2)	P-value	Earlier	Later	(5)-(6)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. FBD and Prenatal Care								
Facility-Based Delivery	0.1216***	0.0489**	0.0726	0.0324**	0.1045***	0.0988***	0.0058	0.862
	(0.0240)	(0.0242)			(0.0270)	(0.0197)		
Number of Prenatal Visit	0.0914	0.0893**	0.0021	0.9794	-0.0185	0.1618***	-0.1803	0.0798*
	(0.0689)	(0.0410)			(0.0922)	(0.0488)		
Month at the First Prenatal Visit	-0.1043	-0.0256	-0.0786	0.57	-0.1722	-0.0986	-0.0736	0.6652
	(0.1078)	(0.0892)			(0.1504)	(0.0850)		
Panel B. Mortality Rates								
Newborn Mortality (7 days)	-5.0103	-4.0353	-0.975	0.8819	-15.4270***	-1.6214	-13.8057	0.0370**
	(4.1166)	(5.1760)			(5.1853)	(4.1828)		
Neonatal Mortality (30 days)	-3.6721	-4.4702	0.7981	0.9057	-11.8010**	-3.1236	-8.6774	0.2331
	(4.8196)	(4.7809)			(5.7794)	(4.4927)		
Infant Mortality (1 year)	-15.4167	-7.1691	-8.2476	0.5216	-28.9349**	-5.2105	-23.7244	0.1469
	(11.4747)	(5.9042)			(14.6791)	(7.4832)		
Child Mortality (5 years)	-22.4734	-29.4191***	6.9457	0.7608	-37.6575	-21.1826*	-16.4749	0.5615
	(20.4609)	(10.4823)			(25.7400)	(12.6050)		

Notes: This table shows the heterogeneity in the treatment effect by the exposure to other related policies. In 2006, the Rwandan Ministry of Health implemented universal health insurance (CBHI) and Performance-Based Financing (PBF) nationwide. Because the government earlier started these programs as pilot programs, the exposure to the program varies by district. In columns 1 and 2, I divide the sample by districts with large and small changes in insurance coverage. The district-level insurance coverage is estimated with EICV data. I estimate the insurance coverage change between 2014 and 2005 and separate them into two groups. The cutoff is an 80 percent increase, which is the median. In columns 5 and 6, I split the sample by the timing of the implementation of PBF. The PBF pilot districts are identified by Rusa and Fritsche (2007). Early indicates districts where PBF was implemented before 2006 and later is after. PBF intends to improve the health facility's quality, so in principle, the early implemented districts are expected to have better quality facilities. The districts are also in the new administrative boundaries. The differences between the two groups are presented with the p-value. Panel A shows the effect on the outcomes of children, and Panel B shows the women's outcomes. The joint-significant test is also conducted and presented with the p-value at the bottom part of the panels. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Chapter 3

Outsource to India: The Impact of Service Outsourcing to India on the Labor Market in the US

3.1 Introduction

Service trade has dramatically increased since the 1980s, thanks to technological advances. A fraction of service imports is considered outsourcing or offshoring. Outsourcing is a common and well-known practice in firms to save costs by transferring certain tasks to a third party that can produce them at cheaper costs. While the impact of outsourcing in manufacturing has been studied extensively (Ahmed, Hertel and Walmsley, 2011; Bhagwati, Panagariya and Srinivasan, 2004; Hummels, Munch and Xiang, 2018), studies on service import are still limited and nascent.

Service outsourcing has a different implication to domestic workers than merchandise trade. The traditional outsourcing literature documents the collapse of manufacturing industries in high-income countries as unskilled domestic workers start to compete with cheaper labor force overseas (Bhagwati, Panagariya and Srinivasan, 2004). In fact, technological advances have brought occupations that traditionally have not been threatened by globalization at risk (Blinder, 2009). Thus, for the first time in history, skilled workers in developed countries are now competing with the skilled labor force in low-income countries, where the skill level is very high with a significantly lower hourly pay (Liu and Trefler, 2019). Analogous to manufacturing outsourcing, some people blame service outsourcing for taking jobs away from domestic workers and argue that outsourcing is harmful to the labor force. While service outsourcing may improve the productivity of firms (Amiti and Wei, 2009*b*), impact on the labor market is controversial and ambiguous (Amiti and Wei, 2009*a*,*b*; Amiti et al., 2005).

This paper examines the impact of the substantial increase in US service import from India on the US labor market. In order to estimate the causal effect of the service import, I exploit the substantial increase in the service export from India stimulated by technological advances and expansion of the Business Process Outsourcing (BPO) market since the late 1990s. The growth in service export from India stems from the advance of high-speed internet (broadband) in the early 2000s (Choi, 2010; Freund and Weinhold, 2002) as well as the country's massive effort to promote the BPO sector. I follow Autor, Dorn and Hanson (2013) and instrument for the service trade from India to the US utilizing India's export to the 15 European Union countries in this paper.

I construct an occupation level import penetration measure following Ebenstein et al. (2014); Liu and Trefler (2019), and examine the impact on occupational employment and median wage during 2000-2016. Results in this paper suggest a non-linear and multidirectional impact of service import on employment. The overall effect of service import on occupational employment is negative: a one standard deviation increase in import penetration decreases total employment by 0.25 percent annually during 2000-2016. However, when I break the sample into two periods, 2000-2006 and 2006-2016, the impact is concentrated in the earlier period only. In fact, the point estimate is positive (but statistically insignificant) in the later period.

My results suggest there is a skilled-bias change in employment. The earlier period's negative impact is smaller for college-educated workers (-0.282) than the overall impact (-0.403). More importantly, the employment impact is positive and large in the later period for college-educated workers, increasing the employment by 0.47 percent. The increase in service import changes the composition of workers within occupations toward skilled workers. Skill-biased change is found across occupations as well. The negative impact on employment in the earlier period is stronger for low-skilled (occupations with a lower share of college-educated workers) and high-routine occupations. In fact, the negative impact on these low-skilled and high-routine jobs continues to the later period, where the overall impact was small and positive.

I find a positive effect on occupation-level median weekly wages. The impact on wages is consistent over time, without a large difference unlike impact on employment. An increase in import penetration by one standard deviation raises the median weekly wage by 0.13 percent annually. The impact on wage should be interpreted with caution because of the compositional change suggested by the effect on employment. Considering the skill-biased change in employment, the positive impact may represent the compositional change, not an increase in productivity.

This paper is closely related to small literature studying the impact of service import on the labor market. Service trade is more difficult to research than merchandise trade because services are not measured at the border like tangible goods. There are very limited harmonic datasets across countries, most of which are available only recently after 2010. Despite the difficulties, there are a few valuable studies. Liu and Trefler (2019) find service import from India and China induces job switching of affected occupations in the US, both upward and downward in terms of average earnings. Crinò (2010*b*) finds a skill-biased change in employment resulting from service import in the US along with his other papers in European countries (Crinò, 2007, 2010*a*, 2012).

The impact on wages is partially supported by Geishecker and Görg (2013). They find service import polarizes the income distribution by rewarding high-skilled workers and penalizing low-skilled workers with similar import penetration in the UK. If service import from India has a similar impact, the positive impact on wages is amplified by the compositional change toward skilled workers and the positive impact on wages itself.

My paper has three major contributions. First, to my knowledge, this is the first paper studying the impact of service imports from low-income countries on the domestic labor market in the US beyond 2006. Most of the research uses the service trade data in the balance of payment (BOP) from The Bureau of Economic Analysis (BEA); however, the data structure had changed in

2006, making it difficult to study beyond this year. Because the data structure change is not problematic for India as much as in other countries, I overcome this problem by focusing on service import from India.[1] Although I focus on a single country, India's BPO market is the largest globally and accounts for a significant fraction of service outsourcing in the US (Burange, Chaddha and Kapoor, 2010). It is crucial to include beyond 2006 because the service trade has increased significantly since then, with a steeper growth than before.

Second, this paper provides evidence that the impact of service outsourcing on total employment may not be single-directional. I find that the employment of skilled workers even increases in the later period (2006-2016) as the service import grows exponentially. My result supports the existence of skill-biased change in employment, which even increases the total employment in the end. My paper's result is consistent with previous works before 2006, like Amiti and Wei (2009*a*); Crinò (2007, 2010*a*,*b*); Liu and Trefler (2019), showing a reduction in employment and a skill-biased change, and I find that this pattern may flip in the later period.

Finally, my paper captures both affiliated and unaffiliated imports from India. Because of the BEA data structure, most of the literature studying the US focus on unaffiliated trade only (Amiti et al., 2005; Crinò, 2010*a*; Liu and

[1]BEA has aggregated affiliated and unaffiliated trade together for each country since 2006. Until 2005, information on unaffiliated trade was only available for each country. This structural difference makes it difficult to research beyond 2006 together with the previous years. Because affiliated trade with India was minimal in the late 1990s, I could extend the study period beyond 2006 by ignoring affiliated trade in the earlier period. See Section 3.3 for further information.

Trefler, 2019). Although there is a measurement error caused by ignoring affiliated trade in the late 1990s, I estimate the impact of aggregated service import, unlike other research. Affiliated import accounts for more than 40 percent of total service import in 2006 (Koncz, Mann and Nephew, 2006). Thus, omitting affiliated trade may underestimate the impact of service outsourcing. I attempt to avoid this problem by focusing on a single country.

3.2 Empirical Strategy

3.2.1 Defining Import Penetration

To examine the impact of service import on occupation level employment and wage, I define occupation level import penetration following Acemoglu et al. (2016); Ebenstein et al. (2014); Liu and Trefler (2019). The previous literature uses the industrial composition of each occupation to capture the relevancy to each service. Next, I estimate the industrial composition of each occupation to measure the importance of each service. A large proportion of computer scientists, for example, are working in the computer and information service industry because the service is relevant to the task they perform.

The formal definition is:

$$\Delta IP^{US}_{kt} = \sum_{s} \omega_{sk,00} \times \frac{\Delta IMP^{IND-US}_{st}}{Y^{US}_{s,96} + IMP^{US}_{s,96} - EXP^{US}_{s,96}}, \tag{3.1}$$

$$\omega_{sk,00} = \frac{N_{sk,00}}{\sum_s N_{sk,00}},$$

where $\omega_{sk,00}$ is the share of workers of occupation k working in industry s in 2000.[2] In this equation, the change in occupation level import penetration is a weighted average of the change in each service import normalized by the initial size of the sector $(Y^{US}_{s,96} + IMP^{US}_{s,96} - EXP^{US}_{s,96})$.

Table 3.2 displays the 20 year ΔIP of top 35 occupations. Because growth in computer and information service is the greatest among all tradable services, the top 2 occupations are computer and data related jobs. Scientists and researchers are also on the top list because they are overrepresented in the R&D sector. Note that the top 35 jobs are not necessarily high-skilled occupations. For example, data entry keyers (19th), typists (34th), and proofreaders (35th) can be considered a low-skilled service occupation. The full list is available in the Appendix Table C.1.

3.2.2 Instrument Variable

Import penetration is endogenous as, in part, it reflects domestic shocks to US industries and occupations. In this section, I explain the instrument variable strategy to address the endogeneity of import penetration. The main idea is coming from Acemoglu et al. (2016); Autor, Dorn and Hanson (2013) that studies the impact of an increase in Chinese import in manufacturing industries on labor market outcomes in the US. In these papers, the authors instrument for the growth in Chinese imports in the US exploiting the Chinese export to other high-income countries in the same period. Analogous to this, I

[2]The base year is 2000 because I use decinnial Census to obtain this share.

instrument the increase in service import penetration from India with India's export to 15 EU countries (EU member countries before the enlargement in May 2004). The underlying assumption of the identification is that the common rising of Indian service imports comes from the technological shock that made certain services tradable (high-speed internet) and the massive investment in the BPO service industry in India.

The instrument variable is defined as following.

$$\Delta IP_{kt}^{EU} = \sum_s \omega_{sk,90} \times \frac{\Delta IMP_{st}^{IND-EU}}{Y_{s,92}^{US} + IMP_{s,92}^{US} - EXP_{s,92}^{US}}. \tag{3.2}$$

It is similar to the endogenous variable in Equation 3.1, the numerator replaced with the Indian import of EU. The denominator and industrial share in 3.2 are constructed using data in the previous period. By using the share defined in the previous decade, the instrument can mitigate the problem coming from the concurrent change in the industrial composition of each occupation.

3.2.3 Data

I obtain service trade data between the US and India from the Bureau of Economic Analysis (BEA). I use the official balance of payment (BOP) data of the US by combining the Survey of Current Business (SCB) October report from 1997 to 2017. The BOP provides payment and receipt of various services between the US and major countries in dollars.[3] While the exact structure

[3]Unlike physical goods, service trade is not anchored in any observation of physical movement. Thus, there is no single standard to measure service trade, making the figures

of the reported data varies over time, SCB provides both unaffiliated and affiliated imports of private services.

Similarly, I use Eurostat data on the trade between the European Union (EU) and India. Eurostat reports service trade between India and the entire EU in detail; however, the trade between individual European countries and India is not available at the detailed level until 2010. Using trade between the entire EU and India is problematic because the EU member countries are not consistent during my study period (1997-2017).[4] Fortunately, the countries that joined later to the EU did not actively trade with India. The sum of total Indian service imports of these ten countries accounts for less than 0.5 percent of India's total EU import in 2007. Thus, I consider 15 EU countries equivalent to 25 EU countries in 1997 and 2007.[5]

The outsourcing services I consider in this paper are often called tradable white-collar services, including finance, insurance, telecommunication, computer and information, management and business consulting, research and development, advertisement, construction and architecture, accounting, legal

vary by the reporting agency. Moreover, the measurement in a single agency is often not consistent over time, and it is difficult to expand the study period. Most of the previous research uses BOP data in monetary terms.

[4]The EU had expanded from 15 (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, and the United Kingdom) to 25 (adding Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia) countries in 2004.

[5]Starting from 2010, detailed trade data at the country level is available. Assuming the trend of trade between the ten countries and India has remained constant from 2007 to 2010, I adjust the values of imports in 2007. For example, if the 15 original countries accounted for 95 percent of legal service imports in 2010, I assume that the share remained the same in 2007.

services, and other business professionals, and technical services, following the literature (Amiti and Wei, 2009*a*; Crinò, 2010*b*; Liu and Trefler, 2019).[6,7] While the BOP data from US BEA and Eurostat is complete at the most detailed level after 2006, each service's exact trade amounts are sometimes ambiguous before then. The aggregate import and export of each service sector are necessary for 1992 and 1996 as they are used in the denominator of the definitions of import penetration. Because the 1992 or earlier trade data is very unreliable, I obtain the total production and trade data using the benchmark Input-Output(I/O) table of 1992.[8]

Table 3.1 shows the change in US import from India by sector. There is a substantial growth in importing all types of services in the table, especially computer and information service by 140,000 percent. In addition, other sectors that are less known, such as accounting (1,000 percent), man-

[6]The name of services is used in BEA and Eurostat. These services are a subset of other private services in BEA classification.

[7]Note that accounting, legal, and management services were reported together as "professional and management consulting services" until 2005. Thus, it is impossible to know the exact amount of trade between the US and India for each service with the official data. To utilize more variation in service trade, I estimate the import of the three services using the share for which each sector accounts for the total unaffiliated professional and management consulting services trade from India to the US. Total unaffiliated trade data is available at the most detailed level. For example, if legal service accounts for 20 percent of total professional and management consulting services, I assume the same share for trade between the US and India. See Appendix C.1 for the details.

[8]The I/O table provides the monetary value of the input and output of the entire economy along with foreign import and export. The advantage of using the benchmark I/O table is that the total output and trade data are available in detail. However, the import is only available if it is used as an intermediate good, so the total amount of import is not available. Fortunately, the tradable services on which this paper focuses are mostly used as input in various industries, not as final goods for consumers. Nonetheless, measurement error exists for the trade data in the benchmark I/O table.

agement (20,225 percent), and R&D (14,133 percent) services, had significantly increased. Telecommunication, one of the most well-known services outsourced to India besides IT service, had not increased much since 1996. In fact, telecommunication outsourcing started in the late 1980s, and outsourcing this service was already prevalent in 1996.

The occupation level data is constructed with the 1990 and 2000 Census and 2007 and 2017 American Community Survey (ACS) IPUMS data Ruggles et al. (2020). To keep the consistent definition of occupations, I use the occupation crosswalk provided by David Dorn used in Autor, Dorn and Hanson (2013), leaving me 330 consistent occupations over time. To define occupation level import penetration in Equation 3.1, I concord the trade service sectors to industries in the Census. For example, computer and information services in BEA correspond to the "Computer and data processing services" industry (732 in 1990 industry code) in Census. The exact crosswalk is available in Appendix Table C.2.

3.2.4 Estimation Equation

The main specification has the following form:

$$\Delta \ln(y_{kt}) = \alpha_t + \beta_{1t}\Delta IP_{kt}^{US} + X_{kt}\Gamma + \varepsilon_{kt}, \tag{3.3}$$

where $\Delta \ln(y_{kt})$ is the difference of outcome variable of occupation k. The outcome variables examined are the employment and median hourly wage of occupation k. I normalize to the annual change and multiply 100 for interpretation. ΔIP_{kt} represents the annual change in occupation k's import penetration between $t+1$ and t. My data period spans from 2000 to 2016, and I stack the ten-year equivalent first differences for two periods, 2000 to 2006 and 2006 to 2016. I use the Z-score of ΔIP_{kt} for interpretation. X_{kt} is the vector of occupation level controls, including service occupation indicator, employment, college share, weekly wage, average age, sex ratio, and racial composition at the start of the period. The change of IP_{kt}^{US} is instrumented by the variable ΔIP_{kt}^{EU} as described above. Because I use the first difference model, the stacked model is similar to the three periods fixed effect model with a less restrictive assumption made on the error term (Autor, Dorn and Hanson, 2013). The standard errors are clustered at the broader classification of occupations.[9]

Figure 3.2 graphically shows the first stage result. The figure reveals a strong positive correlation between ΔIP_{kt}^{US} and ΔIP_{kt}^{EU}. The coefficient and standard error of the formal first stage regression are denoted in the figure. The F-statistics of the instrument variable is 21.01, which is above the rule of thumb of 10.

3.3 Importance of India in Service Trade

Service import in the US has significantly grown thanks to the development of communication technology since the 1980s. In the interest of expense,

[9]I crosswalk 1990 Census occupations to the 4-digit Standard Occupational Classification (SOC) System. Here, I use the 3-digit SOC codes to cluster the standard errors.

countries with relatively low income and a large English-speaking population became popular outsourcing hubs. These include India, Ireland, the Philippines, China, Malaysia, and a couple of Eastern European countries (Amiti et al., 2005).

India is especially famous for IT and BPO services. The increase in India's service export is primarily due to high-speed internet and its massive growth in the BPO market. First, the commercialization of broadband technology around in 2002 made it easier to offshore complicated service. Also, in the same period, the Indian government's support through several laws and investment accelerated the growth of the BPO industry (Thite and Russell, 2007). The BPO market gained a competitive edge by merging small firms into a mega-firm, completed by 2004. The combination of the effort from public and private entities made a synergy effect, and thanks to high-speed internet, the service export has surged in India. This is presented in Figure 3.1.

In this paper, I focus on the impact of service outsourcing to India. There are three reasons for this. The first reason is relevant to the importance of India, and the other two reasons are to the data issue. To begin with, India has an extensive BPO market: India is best known for its IT consulting and computer programming outsourcing, not limited to those services. Also, India is one of the most popular destinations for outsourcing in the US, accounting for 10 percent of all white-collar service imports and 40 percent of ICT services in the US in 2016 (BEA, 2021). India also exports a large amount of accounting, legal, and financial services. India's BPO market produced 143 billion dollars in 2016, equivalent to about eight percent of India's total GDP (NASSCOM, 2017).

Second, India was not as much engaged in affiliated trade with the US as other countries in the late 1990s. There are two important forms of service trade, trade through affiliated and unaffiliated parties. Affiliated trade is gaining importance over time, especially in the tradable white-collar service sector. Hence, to capture the entire impact of service trade, we must take both affiliated and unaffiliated into account. The BEA trade data had a structural change in 2006. While BEA had reported trade data for each type of service only for unaffiliated trade until 2005, it started to provide the aggregate (affiliated and unaffiliated) trade by type from 2006. This structural change makes it difficult to connect before and after 2006, and most of the previous papers study before 2006. Focusing on India resolves this problem. In 1996, affiliated trade accounted for about 30 percent of the US's total service import; however, less than three percent of Indian service imports came from affiliated trade. Considering that Indian service import was very low in 1996, I assume there was no affiliated import from India in 1996. In this way, I can extend the study period beyond 2006, which is never done in the literature, and where the increase in imports is more rapid.

Third, trade data in both US and EU keep records on India relatively better than other middle- and low-income countries in the 1990s and early 2000s. Although some middle and low-income countries like Ireland and the Philippines are important outsourcing partners, some of the data are limited or confidential in the publicly available BEA trade data. India has relatively complete information both in the US and EU databases, which is a great advantage.

3.4 Results

3.4.1 Main Impact on Employment

In Table 3.3, I present the main impact on the occupational employment during 2000-2016 using Equation 3.3. In Panel A, I estimate the impact on total employment by occupation, and in Panel B, I do with the employment of the college-educated workers. Column 1 estimates the impact of import penetration with the OLS model, and columns 2 to 5 use the 2SLS model using the import penetration in the EU countries as the instrument for the US import penetration. In columns 1 to 3, I stack two periods and estimate the impact of import penetration together, while in columns 4 and 5, I do the regressions separately for two periods.

The OLS estimate in Panel A of column 1 shows a minimal correlation between IP and occupation level employment. However, as I switch to the 2SLS model, the point estimates increase by 90 percent, although not statistically significant (column 2). The point estimate becomes statistically significant with control variables in column 3. The most important variable is the share of the college-educated at the beginning of the period. Some of the occupations with high IP are highly educated jobs, such as computer scientists and workers in the R&D sector, so if these occupations have experienced larger growth over time, the point estimate of the impact of IP is underestimated. The point estimate in column 3 implies that a one standard deviation increase in occupational import penetration decreases employment by 0.25 percent.

When I separately estimate the impact of IP in columns 3 and 4, it appears that the negative effect is concentrated on the earlier period. While there is a strong negative impact of IP on occupational employment during 2000-2006 (column 4), the sign of the point estimate reverses and becomes insignificant in the later period of 2006-2016 (column 5). This is notable because the steeper increase in service imports from India started after 2005 (Figure 3.1).

This result seems to be counterintuitive. However, it may be possible that IP does not work cumulatively. In other words, IP may have hurt employment to a certain point, and the sign of the point estimate reverses after that. When service import first increases, the substitution of tasks happens. The service trade is first concentrated on relatively easier tasks and then progresses to more complicated tasks. After the substitution of task reaches the equilibrium, and together with the technological advance, trade in more complicated tasks begins. If these services are complementary to the service produced domestically, then employment eventually increases. Although not completely comparable, automation has a similar implication. Automation substitutes labor as its intention; however, it also complements labor by increasing output and demand for total labor, leading to the polarization of the workers (Autor and Salomons, 2018; Autor, 2015).

The following results support this hypothesis. In Panel B, I estimate the impact on college-educated workers for all 316 occupations. The overall pattern is similar to columns 4 and 5 in Panel A. While the magnitude (absolute value) of the point estimate in the earlier period is smaller than the main results (-0.282 vs. -0.403), it is larger in the later period (0.467 vs. 0.166). This means that the negative impact on employment in the first period is weaker for college-educated workers, but the positive effect is stronger for them later. The result suggests the employment has moved toward in favor of college-educated workers. It may be true for most occupations considering the increase in college enrollment and graduation in the given period; however, the significant coefficients imply it is stronger for more affected occupations by the Indian service import.

Next, I estimate the impact of IP by age group in Table 3.4. The table shows there is a clear distinction across age groups. The impact on the youngest (25-34 years old) workers is analogous to the overall result: negative effect during the first period and positive for the later period. The magnitude of the impact is much smaller for middle-aged workers, and especially, the effect in the second period is almost zero. Because the youngest and middle-aged workers constitute as twice as oldest workers, these two groups drive the overall impact. The oldest workers are experiencing a substantial increase in employment in both periods. Especially, the employment effect of IP is positive for these ages, even in the first period, where the overall impact in Table 3.3

is negative. This table implies a compositional change in employment; switch to the older, experienced, and educated workers.

In Table 3.5, I divide the occupations into two groups by share of college-educated workers and routine tasks defined by (Autor, Dorn and Hanson, 2013). I separately estimate the impact of import penetration by a period as in the previous tables. Table 3.5 shows that low-skilled workers were more affected by the increase in import penetration. When comparing columns 1 and 2, there is a much larger decrease in employment of occupations with a low share of college-educated workers. A one standard deviation increase in import penetration results in a 0.9 and -4 percent decline in total employment, for high and low college-educated occupations, respectively, during 2000-2016. While the overall impact on employment disappears in the later period (column 5, table 3.3), it gets even stronger for low-skilled occupations. The point estimate for 2006-2016 (column 4) is about twice as large as in 2000-2005 (column 2).

Next, I divide the occupations by how much the tasks of occupations are based on routine tasks. Consistent with columns 1-4, the impact of import penetration is much stronger for routine occupations than nonroutine ones. However, the pattern of larger point estimates in the later period, like columns 2 and 4, is not observed. An increase of one standard deviation of import penetration decreases employment of high routine jobs by 1.12 and 0.3 percent during 2000-2006 and 2006-2016, respectively.

3.4.2 Impact on Wages

Table 3.6 presents the impact on the median weekly wages. The wage estimates must be interpreted with caution because of the previous results on employment. If the employment impact is concentrated on a particular group within the occupation, the compositional change in occupation may drive most of the effect on wages. Furthermore, the inconsistent pattern of employment over time complicates the interpretation. The negative impact on the employment of college-educated workers is smaller than the overall effect, in Table 3.3, suggesting that high-skilled workers within occupation were less vulnerable from Indian service import. Employment of college-educated workers increases in the later period by 0.5 percent, while the overall impact is smaller and statistically insignificant. The pattern observed here suggests the possibility of upward bias caused by compositional change.

The impact on weekly wage is consistent over time, unlike on employment, in Table 3.6. Overall, a one standard deviation increase in import penetration raises the median weekly wage by 0.13 percent. There is not much difference between the earlier (2000-2006) and later (2006-2016) period, 0.11 and 0.10 percent, respectively. The results here are consistent with my suspicion that the overall wage would increase.

3.4.3 Robustness Check: Using Alternative Definition of IP

There is no single way to define occupation level import penetration (IP). In this subsection, I examine whether my results vary by the definition of the occupation level IP. Liu and Trefler (2019)'s IP definition uses the fraction in each service sector as the weight to calculate the occupation level IP. This is very straightforward; however, it may not represent how important the service is as a task. For example, some accountants are hired in different industries than accounting industries but still producing accounting services. If a large fraction of accountants is directly hired in various industries, then the share ($\omega_{sk,90}$ in Equation 3.1) may not truly reflect how much accounting services matter to accountants.

I emulate Criscuolo and Garicano (2010)'s definition to examine the robustness to the definition of IP. Criscuolo and Garicano (2010) uses the I/O table to define the industry-occupation level exposure to service import. I take a weighted average of this exposure measure similar to Equation 3.1 to define occupation level exposure. To be specific, I define the IP measures as follows:

$$\xi_{sk} = \sum_{j} \frac{\omega_{jk}}{\sum_j \omega_{jk}} \times \frac{Y_{sj}}{\sum_s Y_{sj}}, \tag{3.4}$$

$$\sum_s \xi_{sk} = 1.$$

The first term in Equation 3.4 is the occupation k's fraction in industry j, which is shown in Equation 3.1 as well. The second term comes from the I/O table: service sector s's share of production in industry j. This term represents how much the service sector s (as a task) is important in industry j. For example, a great fraction of computer and information services are produced in the same industry, meaning the certain task plays a crucial role in the industry. Using ξ_{sk}'s, I define a weighted average as in Equation 3.1 to define an alternative IP, and estimate the impact on employment and weekly wages.

Table 3.7 shows the estimation results using the alternative definition of IP. Columns 1 and 2 are the main specification for comparison. Columns 1 to 4 use the narrowly defined occupation level IP (316 occupations), and 5 to 6 use broadly defined occupations (SOC 3-digits, 88 occupations). When comparing columns 1 to 2 and 3 to 4, the overall patterns are consistent regardless of the measure. The results are robust to using broadly defined occupations in columns 5 to 8. The point estimates are larger in this specification (in absolute terms), although not statistically distinguishable.

3.5 Discussion and Conclusion

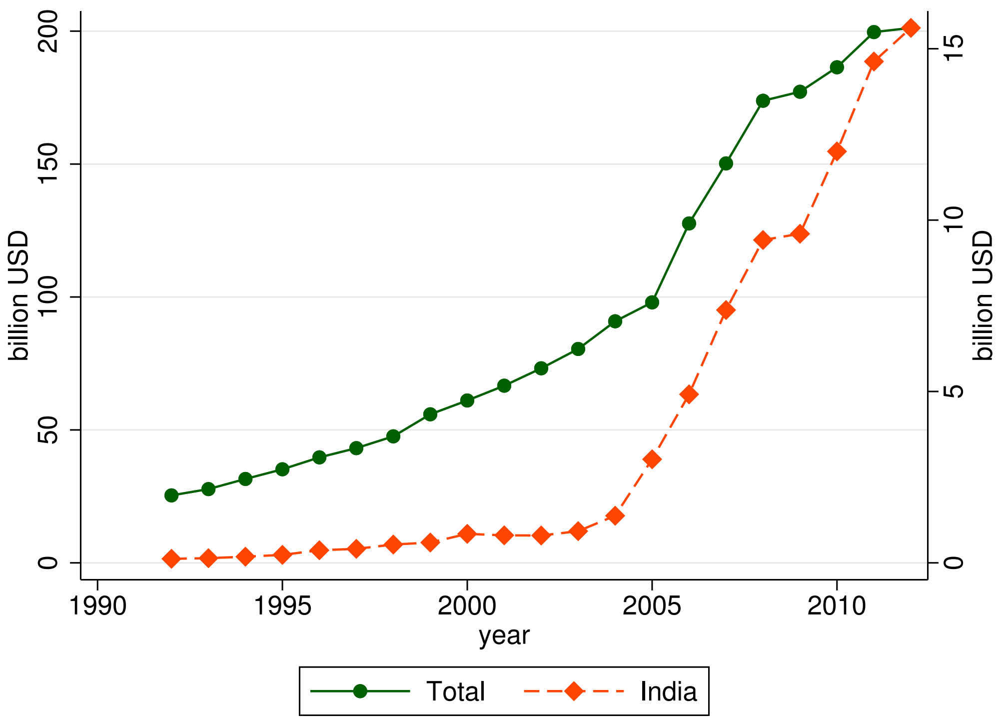
This paper provides evidence that service imports from India had impacted domestic employment in the US. The direction and magnitude of the impact are not consistent over time, unlike Autor, Dorn and Hanson (2013) who find a consistently strong impact of Chinese import penetration in manufacturing. To be specific, the overall impact on total occupational employment is negative. The employment reduces by 0.25 percent during 2000-2016 with an increase in import penetration by one standard deviation. However, when I split the sample into two periods, the impact is concentrated in the earlier (2000-2006) period and becomes positive but insignificant during 2006-2016.

The paper's subsequent analyses suggest that the Indian service import had had a differential impact over time. The monetary value of imports started to grow exponentially from 2005, with a slower but steady growth rate from the 1990s and early 2000s. If the impact of service import is linear or single-directional, there should be a substantial effect in the later period. This paper clearly shows that this is not the case.

There is no sufficient evidence on substitutability and complementarity of service import and outsourcing. This paper shows that while the service purchase from overseas substitutes the domestic workers at first, the role of service import changes as the skill-biased employment change continues. Initially, firms in the US purchase cheaper services from India and other low-income countries. Firms do not substitute the entire service with a cheaper one because certain services are difficult to import, and in-shoring may be more efficient. As the sorting of service continues, firms become more efficient, and they can now hire high-skilled service inshore. As a result, high-skilled employment increases, as in my analysis.

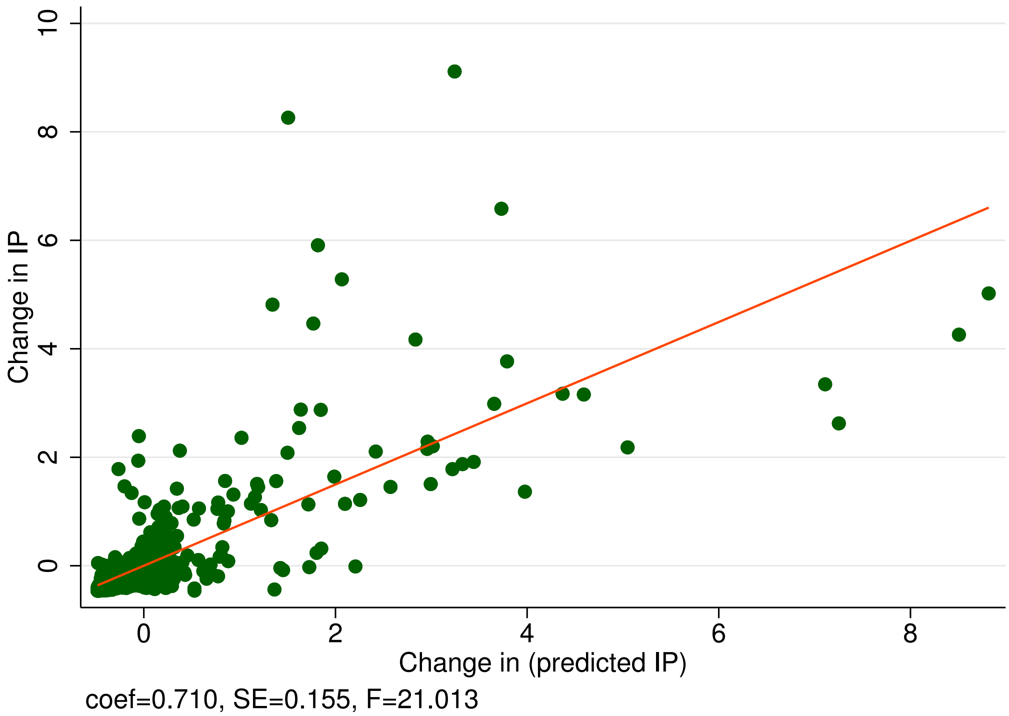
This implies the task composition within occupation moves toward more complicated and sophisticated tasks, especially for more vulnerable jobs in terms of service import. Further work must be done to prove this hypothesis. The economy would find a way to the new equilibrium by sorting less efficient and skilled workers out of the vulnerable occupations.

Figure 3.1: Import of total other private service



Source: Current Business Survey, Bureau of Economic Analysis. A subset of the total other private service is considered as tradable white-collar service in the literature.

Figure 3.2: First Stage Results



Notes: N=316 ×2=632. Two periods of 2000-2007 and 2007-2017 are stacked. The IP measures are normalized in each period (to Z-scores). First stage F-statistics is 21.013.

Table 3.1: US Service Import from India (in million USD)

Service type	1996	2006	2011	2016
Advertising	2	17	61	32
Construction	0.5	127	127	181
Financial	15	104	312	543
Insurance	0.98	15	27	84
Accounting	8.1	81	331	483
Legal	5	14	57	82
Management	4	813	1,057	1,275
Computer	2	2,798	9,395	14,235
R&D	3	427	2,165	3,482
Telecommunication	300	399	302	404
Installation	0.5	7	26	47
Industrial Engineering	2	66	83	234
Leasing	0.5	0.5	0.5	0.5
Other tradable service	18	99	401	545

Notes: Data from the balance of payment (BOP) from the Bureau of Economic Analysis (BEA). From 2006 to 2016, the figures include both affiliated and unaffiliated imports from India. In 1996, the figures included only unaffiliated imports; however, affiliated imports comprised a tiny fraction of total service imports. Thus, the unaffiliated imports were almost the same as total imports. See Section 3.2 and 3.3 for further information.

Table 3.2: Ranking of ΔIP_{kt}^{US}

Rank	Occupation Title	$\Delta IP \times 100$	Rank	Occupation Title	$\Delta IP \times 100$
1	Computer software developers	67.5502	24	Statistical clerks	11.5795
2	Computer systems analysts and computer scientists	49.3654	25	Agricultural and food scientists	11.4366
3	Technical writers	40.6995	26	Office machine operators, n.e.c.	11.2605
4	Physicists and astronomists	38.7210	27	Management support occupations	10.8143
5	Technicians, n.e.c.	34.6463	28	Economists, market and survey researchers	10.6534
6	Physical scientists, n.e.c.	33.0064	29	Sales engineers	10.4142
7	Medical scientists	29.2978	30	Personnel, HR, training, and labor rel. specialists	9.7391
8	Repairers of data processing equipment	25.6408	31	Managers and specialists in marketing, advert., PR	8.6551
9	Biological scientists	25.3492	32	Designers	8.5078
10	Social scientists and sociologists, n.e.c.	24.7227	33	Managers and administrators, n.e.c.	8.1676
11	Mathematicians and statisticians	21.8731	34	Typists	7.3658
12	Lawyers and judges	20.4851	35	Proofreaders	6.7203
13	Computer and peripheral equipment operators	19.8436		⋮	
14	Atmospheric and space scientists	18.5570	303	Barbers	0.0000
15	Chemists	18.5544	303	Mail carriers for postal service	0.0000
16	Geologists	18.2476	303	Hotel clerks	0.0000
17	Legal assistants and paralegals	17.2559	303	Air traffic controllers	0.0000
18	Management analysts	17.0138	303	Primary school teachers	0.0000
19	Data entry keyers	14.3558	303	Food roasting and baking machine operators	0.0000
20	Operations and systems researchers and analysts	13.7050	303	Postal clerks, exluding mail carriers	0.0000
21	Biological technicians	13.5794	303	Secondary school teachers	0.0000
22	Engineers and other professionals, n.e.c.	12.9407	303	Managers of medicine and health occupations	0.0000
23	Electrical engineers	11.6488	303	Purchasing agents and buyers of farm products	0.0000

Notes: This table shows the ranking of change in occupation level import penetration (IP) measure from 2000 to 2016, defined in Equation 3.1. Occupation titles are defined in the Census. See Appendix Table C.1 for the full list.

Table 3.3: Impact of Import Penetration on Employment

Dependent variable: Δln(Employment) $\times$ 100

	OLS	2SLS			
	All period	All period	All period	2000-2006	2006-2016
	(1)	(2)	(3)	(4)	(5)
Panel A. Total Employment					
Z-score of	-0.0166	-0.110	-0.247**	-0.403**	0.166
ΔIP	(0.114)	(0.137)	(0.111)	(0.171)	(0.159)
First Stage F-Statistics		11.35	14.40	19.46	6.36
Panel B. College Employment					
Z-score of	-0.128	-0.236	-0.0804	-0.282*	0.467***
ΔIP	(0.128)	(0.156)	(0.0942)	(0.152)	(0.159)
First Stage F-Statistics		11.35	14.40	19.46	6.36
Observations	632	632	632	316	316
Controls	No	No	Yes	Yes	Yes

Notes: This table reports the estimates of the impact of occupation level IP on employment using Equation 3.3. Each entry is a coefficient from a separate regression. The coefficients are rescaled to represent the annual change in employment in percentage. I also normalize IP to Z-socre for interpretation. In the 2SLS model, the instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. In column 3, the point estimate is interpreted as the following: a one standard deviation increase in IP reduces employment by 0.25 percent annually. Controls include the log of employment, the fraction of college-educated workers, log of median weekly wage, average age and its square term, percent of female, and the fraction of white and black at the beginning of each period. Regressions are weighted using the size of employment in 2000. Robust standard errors are in parentheses clustered by three-digit SOC codes. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 3.4: Impact of Import Penetration on Employment, by Age

Dependent variable: Δ*ln(Employment)*$\times$ *100*

	Age: 25-34		Age: 35-44		Age: 45-60	
	2000-2006	2006-2016	2000-2006	2006-2016	2000-2006	2006-2016
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Total Employment						
Z-score of	-0.502**	0.413	-0.230	-0.0440	0.266*	0.454***
ΔIP	(0.208)	(0.254)	(0.149)	(0.184)	(0.145)	(0.153)
Panel B. College Employment						
Z-score of	-0.695**	0.841*	0.007	0.215	0.378*	0.766***
ΔIP	(0.317)	(0.432)	(0.255)	(0.294)	(0.195)	(0.258)

Notes: N=316. This table reports the estimates of the impact of occupation level IP on employment using Equation 3.3 separately by age. Each entry is a coefficient from a separate 2SLS regression. The coefficients are rescaled to represent the annual change in employment in percentage. I also normalize IP for interpretation. The instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. All regressions include control variables. See the notes of Table 3.3 for the details for the controls. Panels A and B present the total and skilled (college-educated) employment, respectively. I separately estimate the impact in each period. Regressions are weighted using the size of employment in 2000. Robust standard errors are in parentheses clustered by three-digit SOC codes. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 3.5: Impact on Employment by Occupation Characteristics

Dependent variable: Δ*ln(Employment)*$\times$ *100*

	Share of College-Educated				Routine			
	2000-2006		2006-2016		2000-2006		2006-2016	
	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z-score of	-0.905**	-3.961**	-0.194	-7.257**	-1.124**	-0.182	-0.305*	0.159
ΔIP	(0.426)	(2.003)	(0.161)	(2.847)	(0.541)	(0.406)	(0.163)	(0.401)

Notes: This table reports the estimates of the impact of occupation level IP on employment using Equation 3.3 separately by share of college-educated workers and how routine the occupation is. Each entry is a coefficient from a separate 2SLS regression. The coefficients are rescaled to represent the annual change in employment in percentage. I also normalize IP for interpretation. The instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. All regressions include control variables. See the notes of Table 3.3 for the details for the controls. In columns 1 to 4, I use the share of college-educated workers in 2000 to separate the sample (above and below median). In columns 5 to 8, I separate the occupations by how routine the tasks are. For routine measures, I use the measure defined by (Autor and Dorn, 2013). I separately estimate the impact in each period. Regressions are weighted using the size of employment in 2000. Robust standard errors are in parentheses clustered by three-digit SOC codes. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 3.6: Impact of Import Penetration on Median Weekly Wages

Dependent variable: Δ*ln(Median weekly wage)*$\times$ *100*

	OLS	2SLS			
	All period (1)	All period (2)	All period (3)	2000-2006 (4)	2006-2016 (5)
Z-score of ΔIP	0.141*** (0.0413)	0.180** (0.0698)	0.129*** (0.0410)	0.112* (0.0586)	0.100** (0.0390)
Observations	632	632	632	316	316
Controls	No	No	Yes	Yes	Yes

Notes: This table reports the estimates of the impact of occupation level IP on median weekly wage using Equation 3.3. Each entry is a coefficient from a separate 2SLS regression. The coefficients are rescaled to represent the annual change in employment in percentage. I also normalize IP for interpretation. The instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. All regressions include control variables. See the notes of Table 3.3 for the details for the controls. Regressions are weighted using the size of employment in 2000. Robust standard errors are in parentheses clustered by three-digit SOC codes. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table 3.7: Using Alternative Definition of Import Penetration

Dependent variable: $\Delta ln(Employment) \times 100$

	Occupation Definition (Narrow)				Occupation Definition (Broad)			
	LT (2019)		CG (2010)		LT (2019)		CG (2010)	
	2000-2006	2006-2016	2000-2006	2006-2016	2000-2006	2006-2016	2000-2006	2006-2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Total Employment								
Z-score of	-0.403**	0.166	-0.515**	0.0939	-0.624**	0.173	-0.545**	0.221*
ΔIP	(0.171)	(0.159)	(0.226)	(0.188)	(0.289)	(0.144)	(0.242)	(0.125)
Panel B. College Employment								
Z-score of	-0.282*	0.467***	-0.337*	0.351**	-0.403**	0.408***	-0.406**	0.492***
ΔIP	(0.152)	(0.159)	(0.173)	(0.172)	(0.191)	(0.151)	(0.186)	(0.161)
Panel C. Median Weekly Wage								
Z-score of	0.112*	0.100**	0.134**	0.0854**	0.154**	0.125***	0.143**	0.129***
ΔIP	(0.0586)	(0.0390)	(0.0663)	(0.0409)	(0.0663)	(0.0389)	(0.0618)	(0.0384)

Notes: This table reports the estimates of the impact of occupation level IP on employment and median weekly wage using Equation 3.3 using alternative IP definitions. Each entry is a coefficient from a separate 2SLS regression. The coefficients are rescaled to represent the annual change in employment in percentage. I also normalize IP for interpretation. The instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. LT (2019) and CG(2010) stand for Liu and Trefler (2019) and Criscuolo and Garicano (2010), respectively. See Equation 3.1 and 3.4 for the precise definitions of the IP measures. Narrowly defined occupations are occupation codes used in the Census (and modified by David Dron in Autor, Dorn and Hanson (2013)). Broadly defined occupations are three-digit SOC codes. All regressions include control variables. See the notes of Table 3.3 for the details for the controls. I separately estimate the impact in each period. Regressions are weighted using the size of employment in 2000. Robust standard errors are in parentheses clustered by three-digit SOC codes. * significance at 10%; ** significance at 5%; *** significance at 1%.

Appendices

Appendix A

Appendix to Chapter 1

A.1 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.[1] Then, I estimate the average of constitutive PUMAs weighted by population and construct CPUMA level finance information.[2]

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.

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

[2] 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.

A.2 Additional Robustness Checks

A.2.1 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 Medicaid Services), and official unemployment insurance budget data (US Department of Labor). Similar to Figure 1.8, 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 1.6). 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.

A.2.2 Permutation Test for No Income Tax States

In the first stage in section 1.4.3, 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.

A.2.3 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.[3] 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.[4]

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 appropri-

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

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

ate than the traditional difference-in-differences because the treatment effect changes over time (e.g., Figure 1.7).

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.

A.3 Additional Heterogeneity Analysis

A.3.1 Racial Difference in Heterogeneity in Effect by CPUMA Characteristics

This section compares how the heterogeneity by CPUMA characteristics in Table 1.9 differs across races. I redo the analysis in 1.9 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 1.9 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 nonwhite 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 1.8, none of the point estimates in Panel D is statistically significant.

A.3.2 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 1.7. 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.

A.3.3 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.[5]

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

To do so, I estimate the following equations:

$$N_{ipst} = \beta Rev_{pst} + P_{pst}\gamma + \theta_t + \alpha_i + \varepsilon_{ipst}, \tag{A.1}$$

$$\begin{aligned} N_{ipst} = {} & \beta_1 Rev_{pst} \times \mathbb{1}[Catholic]_i + \\ & + \beta_2 Rev_{pst} \times \mathbb{1}[Other\ Religious]_i + \\ & + \beta_3 Rev_{pst} \times \mathbb{1}[Nonsectarian]_i + P_{pst}\gamma + \theta_t + \alpha_i + \varepsilon_{ipst}, \end{aligned} \tag{A.2}$$

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.[6] 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.[7]

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

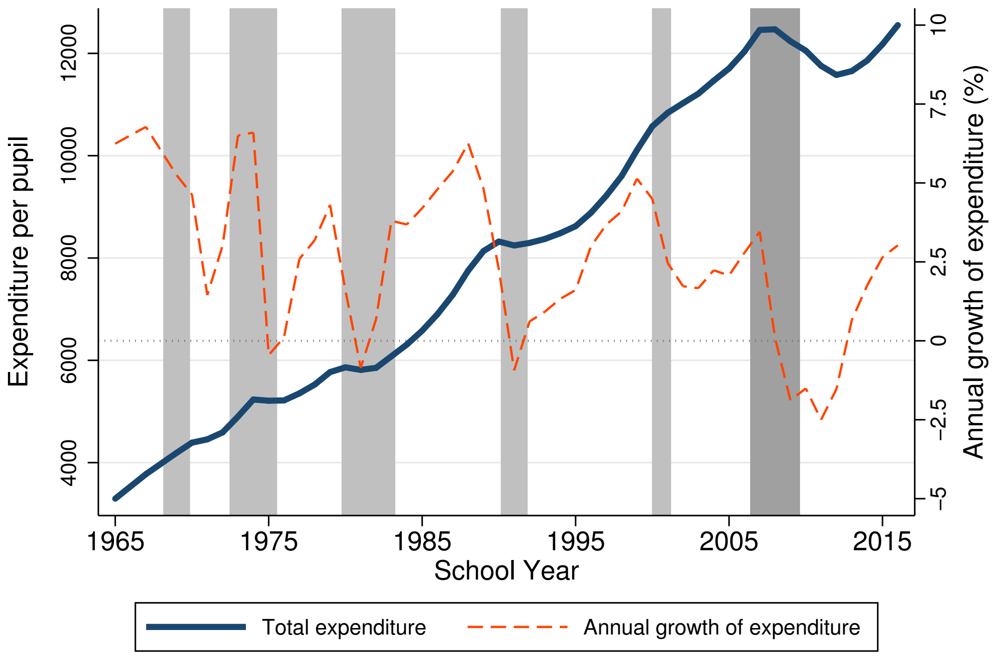
[6]The PSS provides geocoordinates of most of the schools, so I match it to the CPUMA in the Census and ACS.

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

per-pupil revenue in local public schools, which is statistically different from other religious schools (-2.46) and nonsectarian schools (-1.36).

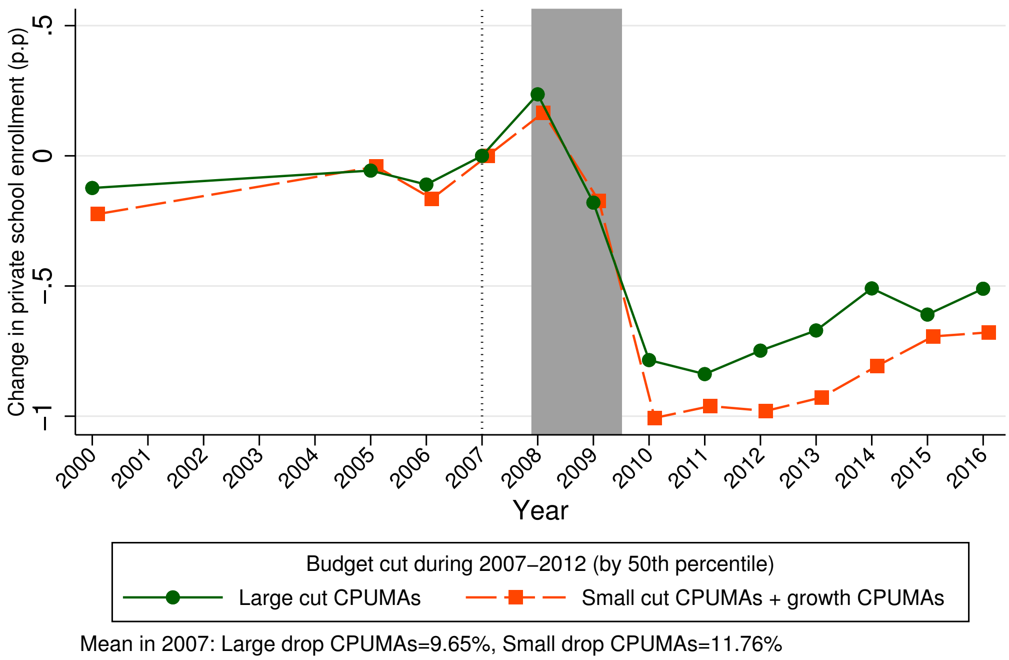
In columns 4 and 5, I test whether this holds for white and Hispanic students. The pattern is similar in column 4 for white students, but not for Hispanics in column 5. However, over 30 percent of schools do not have any Hispanic students, and the median of Hispanic enrollment is only 3. Also, Hispanics 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 1.7 that Hispanics may have switched to Catholic private schools.

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



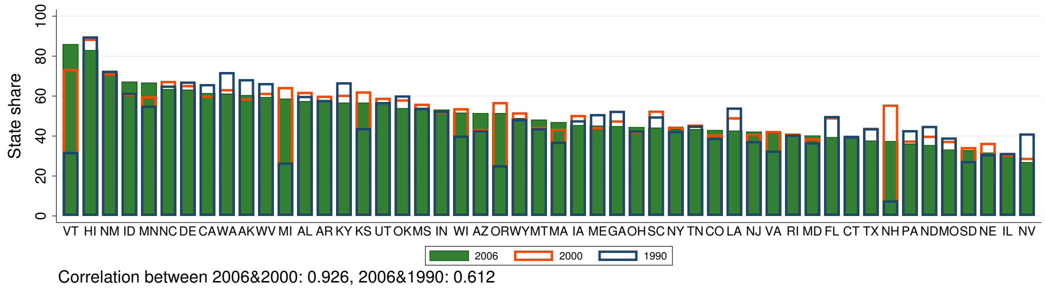
Notes: This figure plots the trend of expenditure per pupil instead of revenue. All other details are the same in Figure 1.1.

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



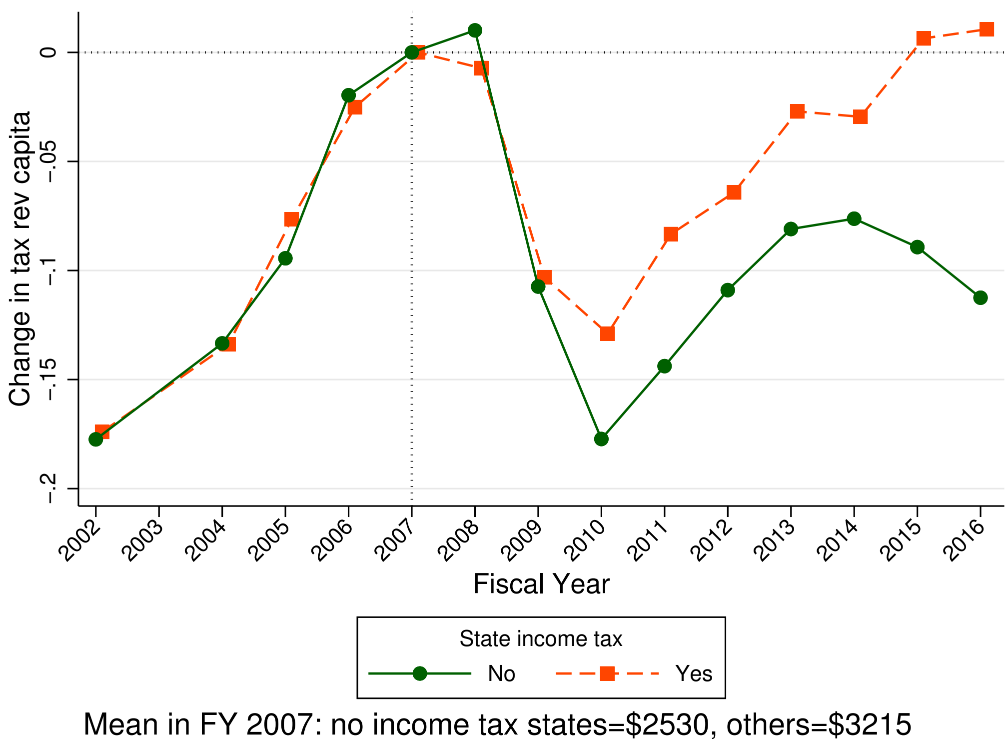
Notes: This figure shows the trend in private school enrollment by large and small budget cut in CPUMA. The mean of private school enrollment is normalized with the value in 2007. All other details same to Figure 1.3.

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.

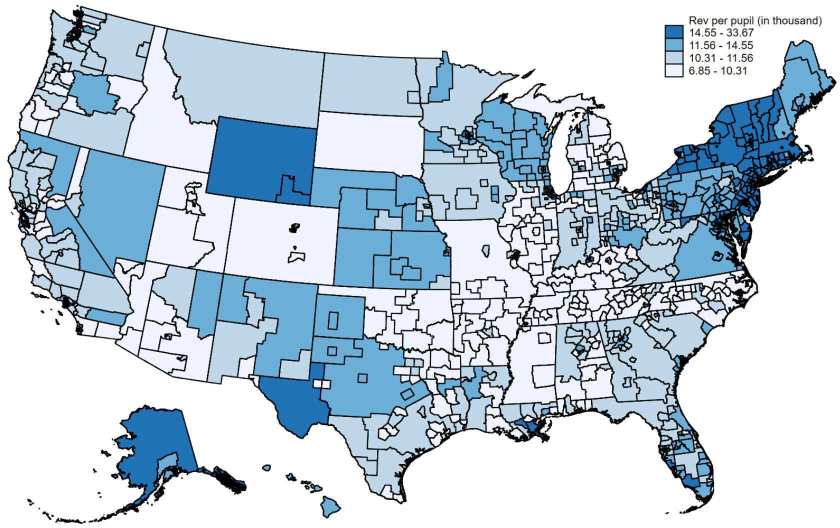
Figure A.4: Trend of Tax Revenue, Property Tax Excluded



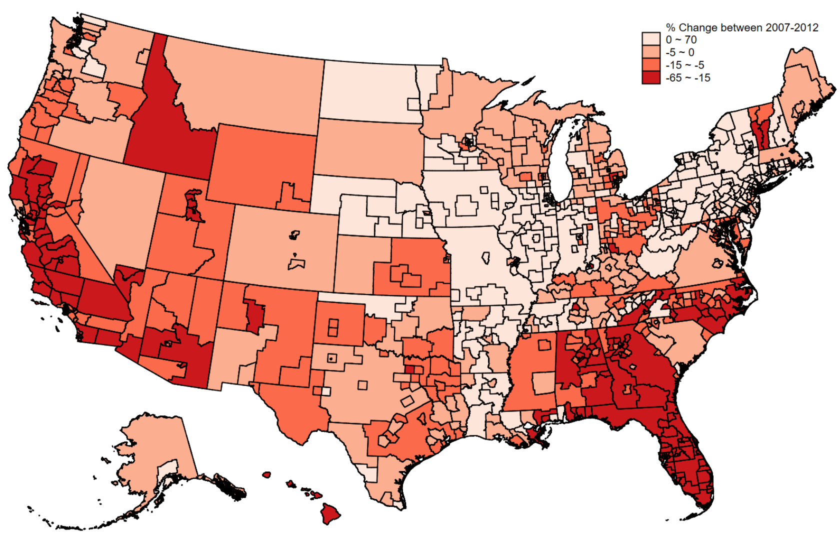
Notes: This figure replicates Figure 1.6 but excludes property tax revenue.

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

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



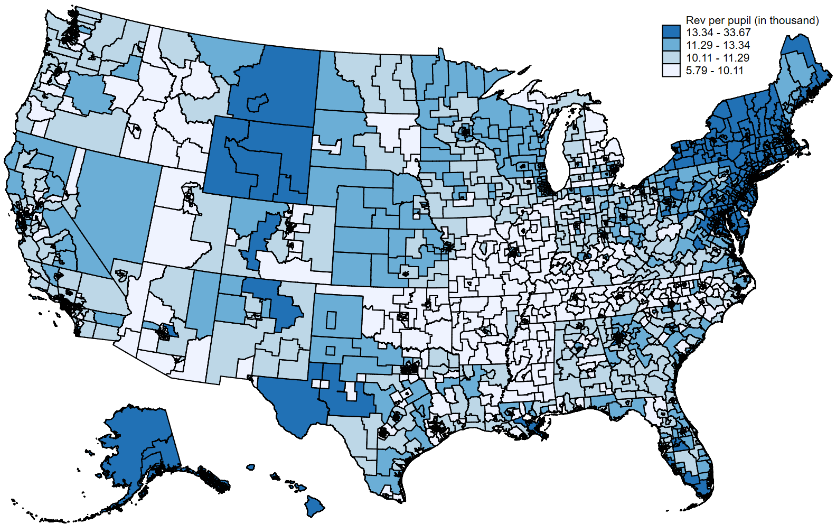
(b) Change in K-12 Revenue Per Pupil in CPUMAs, 2007-2012



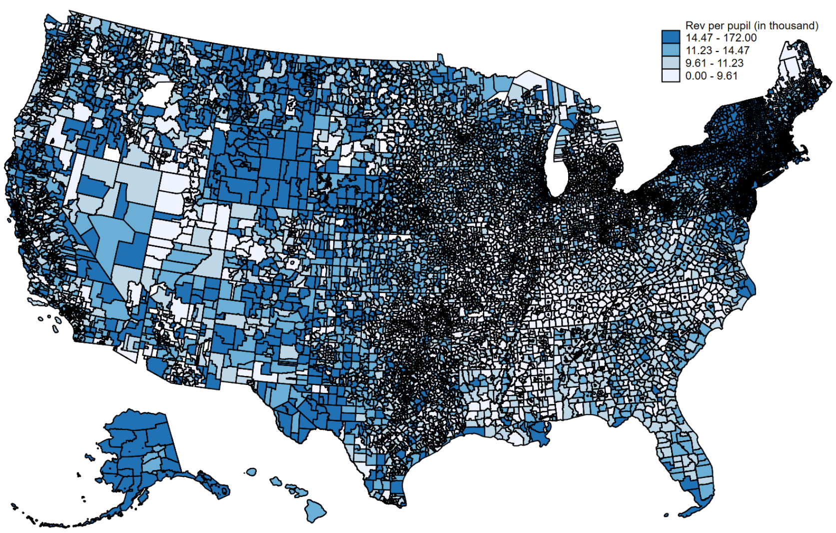
Notes: Panel A shows the K-12 revenue per pupil in CPUMAs in SY 2007-2008. Panel B displays the percent change during 2007-2012. The figures are obtained by matching school districts to each CPUMA.

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

(a) Rev per pupil in PUMA

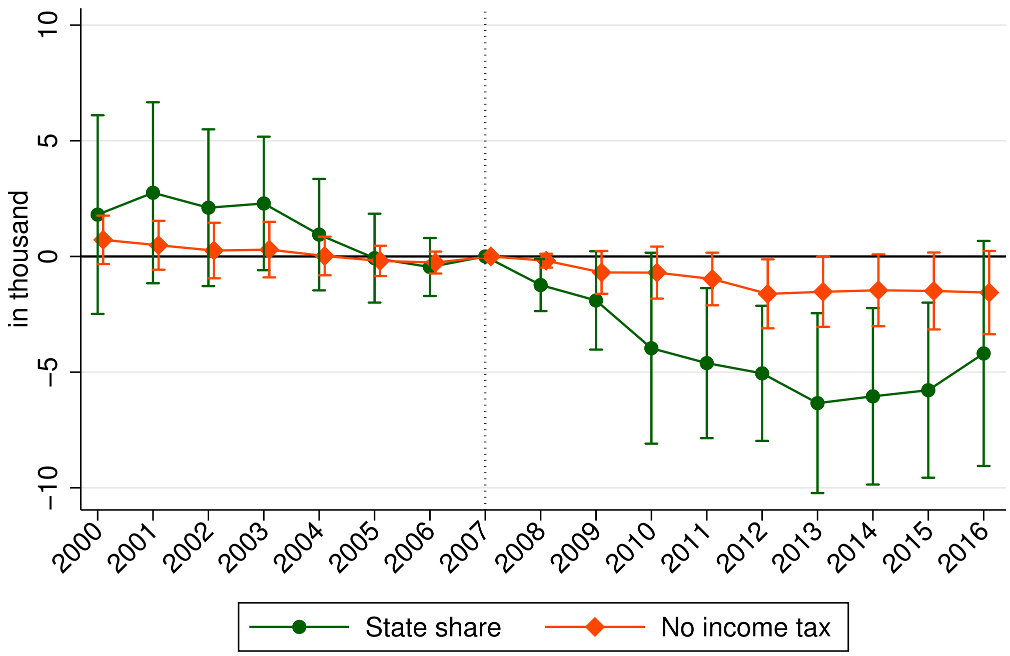


(b) Rev per pupil in school district



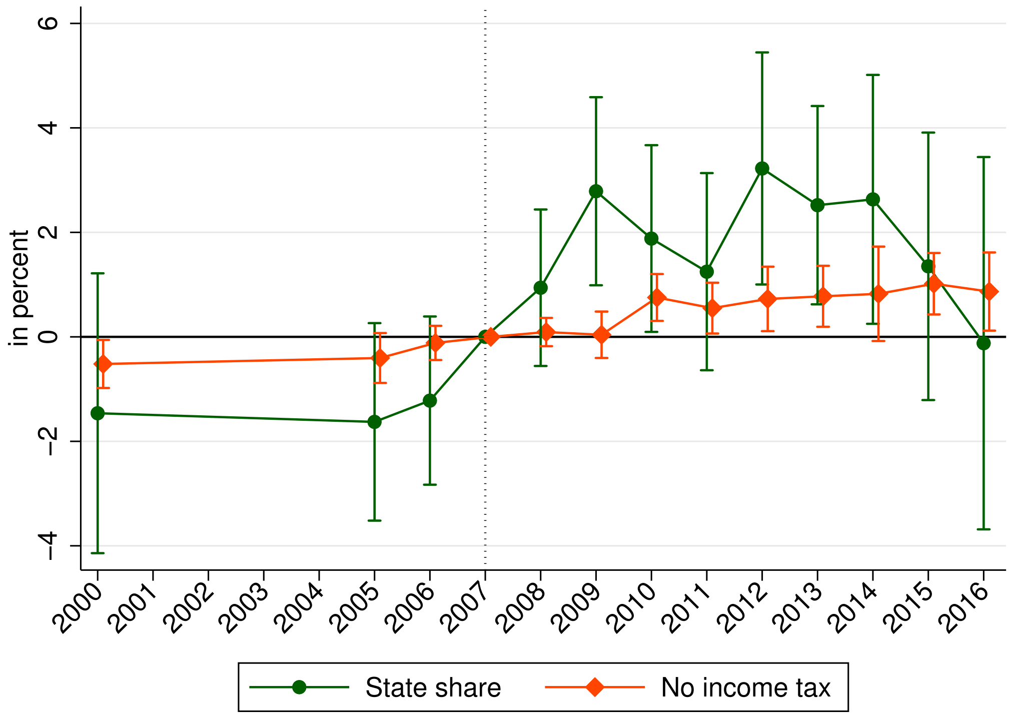
Notes: This figure shows the K-12 revenue per pupil by PUMA (panel A) and school district (panel B) in SY 2007-2008. Panel A is obtained by matching school districts to each PUMA.

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.

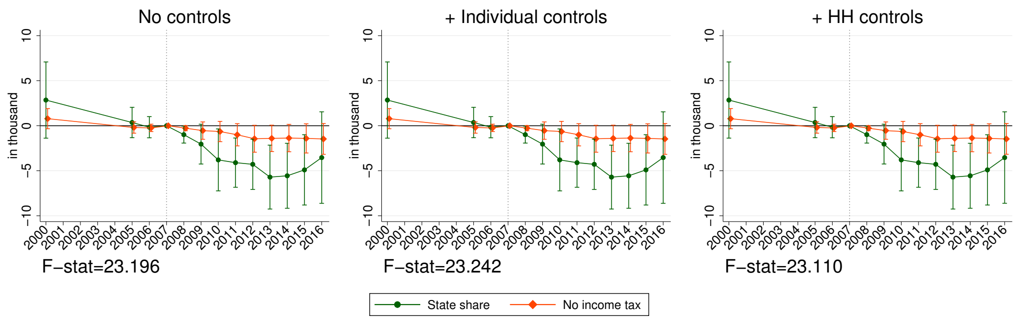
Figure A.8: Reduced Form Result



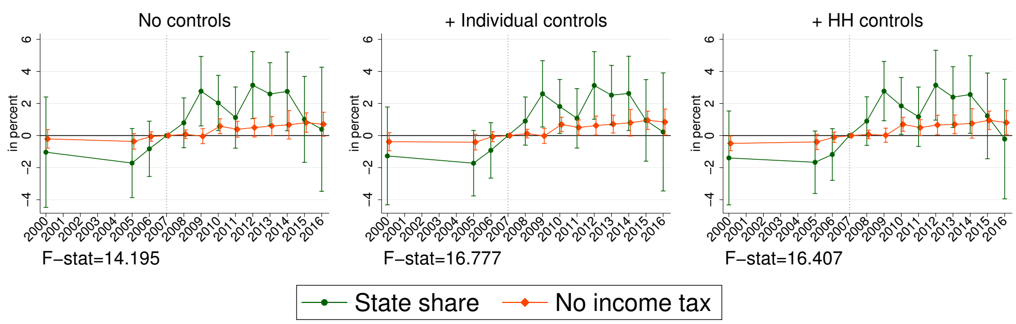
Notes: N=7,744,432. The reduced form result in the most preferred specification (including full sets of controls) is presented in this figure. I display the coefficients of interaction terms of year dummies and state share, and income tax status (β_k's and γ_k's) along with 95% confidence intervals. 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 1.3 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

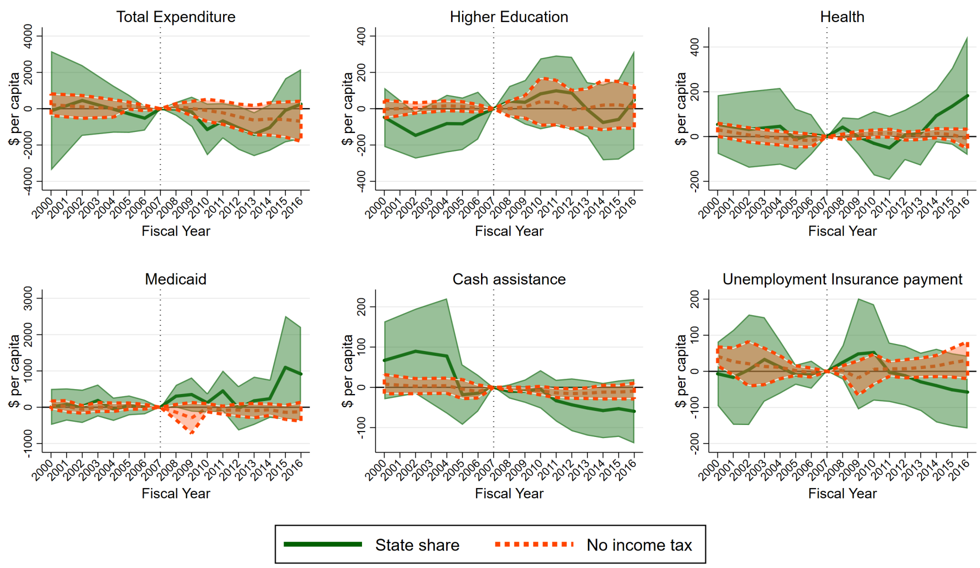


(b) Reduced form: Private School Enrollment



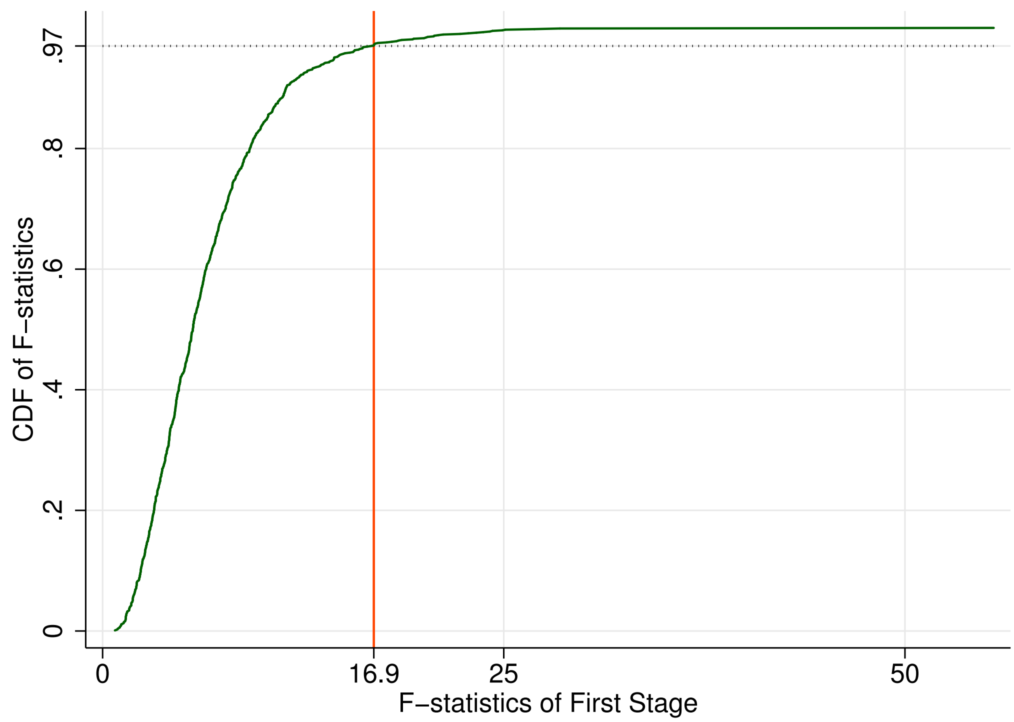
Notes: This figure shows the first stage (Panel A) and reduced form (Panel B) in various specifications. Each figure displays the coefficients for the interaction terms of state share (green dots) and no income tax indicator (orange diamonds) with the year dummies along with 95% confidence intervals 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 1.3 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.

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 1.3 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 1.3. 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 1.3. 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 1.3. 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 (1)	Low (2)	High (3)	Low (4)	High (5)	Low (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 1.3. 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 1.9. Each panel is separately estimated by races. See the notes of Table 1.9 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 1.3. 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 1.3 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%.

Appendix B

Appendix to Chapter 2

B.1 Heterogeneity by Distance to Health Facilities

Proximity to health facilities is an important factor in access to health services. If the health facility is too far away, it is difficult to seek care as needed. The FBCP focuses on increasing utilization by removing the financial barrier. Intuitively, the policy effect may be greater in areas with greater physical access to health facilities. The Rwandan Ministry of Health provides a complete list of the registered health facilities in the country. However, the public data only provides the most recent information.[1] In 2019, there are 48 hospitals (district, provincial, and referral hospitals) and 508 health centers in Rwanda. The geocodes are available for all of the hospitals and 465 health centers. Although the data includes the opening date of each facility, they are overall inaccurate with a lot of missing values. Hence, I ignore the opening dates and use all of the available facilities in 2019. This is potentially problematic because the new opening of health facilities is not exogenous. Nevertheless, I explore whether the treatment effect is stronger in areas with greater health service access.

[1] In this study, I acquire the list of facilities in 2019.

Using the geocodes of the primary sampling units of RDHS, I can identify the distance to the closest facilities. Because the prenatal care and basic delivery services are provided at the health center, and complicated pregnancies are transferred to hospitals, I estimate the distance to the health center and the higher-level facility separately. The median of the minimum distance to the health center is 2.5 km and to district/provincial/referral hospital is 8.8 km. I separate the sample into two groups by the distance to the closest facility, below and above the median distance. I separately evaluate the treatment effect on the outcome variables above.

The results are presented in Table B.1. Panel A contains the impact on FBD and prenatal care utilization, and Panel B contains the mortality outcomes. Columns 1 and 2 depict the differential result by the distance to the health center, the primary level facility. Mothers are more likely to give birth in a health facility and have more prenatal visits when they live closer to the facility. The differences are 9.9 percentage points and 0.2 times for FBD and the number of prenatal visits, respectively, with a very high significance. Despite this differential effect on FBD and prenatal care, this is not leading to a larger effect on newborn and neonatal mortality in the districts with closer proximity to the facility. However, there is a large difference in infant and child mortality rates, a larger decline in areas where facilities are close. These results are largely consistent with my main results: a large decline in infant and child mortality rates, but not for newborn and neonatal mortality rates.

I find similar heterogeneity in effect when I divide the sample by the distance to the closest higher-level facility in columns 3 and 4. Although the higher-level facilities are not the place for most babies' delivery, the treatment effect is much larger in the villages where the hospital is close. This is largely coming from the correlation between the distance to the health center and the hospital. Villages in a denser area are likely to be urban, which makes them closer to everything. However, when I run the regressions only with rural villages, the overall results don't change much (result available upon request). Although not statistically significant, treatment effects are larger in districts where hospitals are close for all mortality rates. The difference in point estimates is substantial for newborn and neonatal mortality: 10.6 and 8.2, respectively. Together with results in Table 2.7 for PBF, it seems that the quality of the facility plays an important role in reducing mortality.

There is concern about using 2019 as the reference year because the number of health facilities has increased substantially since 2006, and thus it would not fully capture the access of health facilities in the baseline period. However, the increase in health facilities is driven mainly by health posts where the child delivery service is not provided, not health centers or district hospitals (or higher level) (Rwanda Ministry of Health, 2009, 2015). The largest increase in the number of health centers from 2008 to 2014[2] is five in Rusizi and Musanze district. Since this number is non-negligible,[3] I estimate the same regressions for the two groups according to the average travel time to

[2] The information available from Rwandan Ministration of Health on the number of facilities in each district starts from 2008.

[3] The median number of health centers in each district in 2014 is six.

the health facility in 2005.[4] The results look very similar. The results change little when I use travel time in 2014 instead (Results available upon request). Although some mothers give birth at a private facility, the fraction is very small (less than two percent).

B.2 Effect of the Expansion of Universal Health Insurance

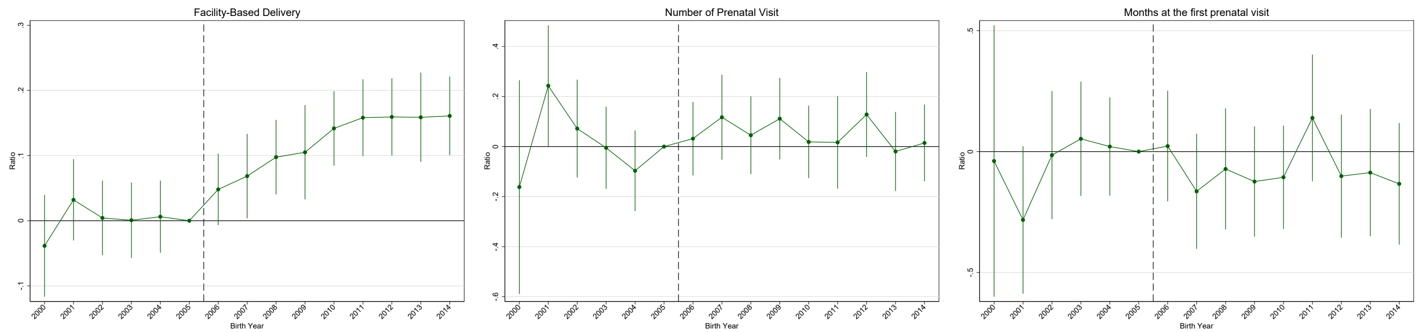
Expansion of universal health insurance is another policy that potentially increased FBD and prenatal care utilization. This section examines how much expansion of the CBHI scheme is associated with FBD and mortality rates. The strategy I use here is similar to the main strategy. I define the treatment districts as those whose baseline insurance coverage is below the 75th percentile. I use the 75th percentile as the threshold because health insurance coverage is more skewed than the FBD rate in the baseline period.

Table B.2 shows the treatment effect of insurance coverage. Unlike Tables 2.3 and 2.4, the effect on FBD and prenatal care is small and statistically insignificant. The point estimates are smaller for mortality rates. Only is the effect on child mortality statistically significant, with a similar magnitude in Table 2.4. This result is consistent with Table 2.7, where an increase in insurance coverage is not associated with the treatment effect of free FBD and prenatal care policy. To summarize, Tables 2.7 and B.2 imply that Facility-Based Childbirth Policy was the main driver promoting FBD and prenatal

[4]This information is available in the EICV 2005 round.

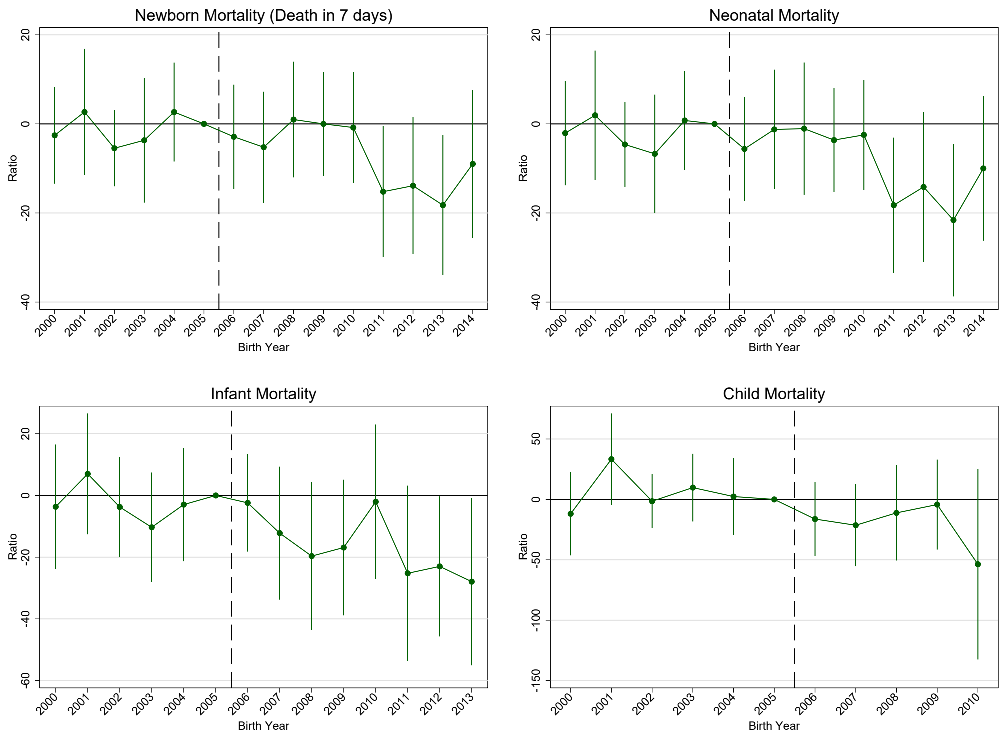
care.

Figure B.1: Impact on Facility-Based Delivery and Prenatal Care, Event Study Framework



Notes: This figure shows the treatment effect on FBD and prenatal care utilization with the event study framework with the preferred specification. I replace $\beta Low\ FBD_d \times \mathbb{1}(t \geq 2006)$ with $\sum_{k\neq 2005} \beta_k Low\ FBD_d \times \mathbb{1}(t = k)$ in Equation 2.1 and plot the β_k's with their 95% confidence intervals. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Standard errors are clustered by the proper district.

Figure B.2: Impact on Mortality Rates, Event Study Framework



Notes: This figure shows the treatment effect on the mortality rates. Other specifications are the same in Figure B.1.

Table B.1: Heterogeneity by Distance to Health Facilities

	Primary facility		Difference		Higher facility		Difference	
	Close	Far	(1)-(2)	P-value	Close	Far	(5)-(6)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. FBD and Prenatal Care								
Facility-Based Delivery	0.1455***	0.0500**	0.0955	0.0007***	0.1360***	0.0358	0.1002	0.0021***
	(0.0223)	(0.0206)			(0.0227)	(0.0238)		
Number of Prenatal Visit	0.2454***	0.0427	0.2026	0.0274**	0.0789	0.0891	-0.0102	0.8976
	(0.0737)	(0.0598)			(0.0538)	(0.0613)		
Month at the First Prenatal Visit	-0.1177	-0.1269	0.0093	0.9475	-0.0769	-0.0895	0.0126	0.9255
	(0.1078)	(0.1066)			(0.1055)	(0.0895)		
Panel B. Mortality Rates								
Newborn Mortality (7 days)	-7.5091*	-4.4298	-3.0793	0.5919	-10.7123**	-0.1203	-10.592	0.1422
	(4.3980)	(3.9913)			(5.3807)	(4.6610)		
Neonatal Mortality (30 days)	-4.9189	-6.785	1.8661	0.7718	-9.3460*	-1.1759	-8.1701	0.2665
	(4.7247)	(4.6034)			(5.4599)	(5.1528)		
Infant Mortality (1 year)	-25.9421***	-5.6041	-20.338	0.0445**	-19.2714**	-4.5328	-14.7386	0.2384
	(8.3285)	(7.9156)			(8.7207)	(9.5099)		
Child Mortality (5 years)	-50.0745***	-18.3917	-31.6828	0.1325	-30.5677***	-17.6239	-12.9438	0.6377
	(14.6058)	(14.0840)			(11.8326)	(22.6410)		

Notes: This table shows the heterogeneity in the treatment effect by distance to health facilities. Because the Rwandan Ministry of Health only provides the most recent list of health facilities, I use the recent data to divide the sample (2019 data in this paper). In columns 1 and 2, I divide the sample by districts with distance to the primary facility (health center) and in columns 5 and 6, with secondary or above hospitals (district or provincial hospital). The median distance to the closest primary and secondary or above facility is 2.5 km and 8.8 km, respectively. Other specification is same with Table 2.7. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Table B.2: Treatment Effect of Insurance Coverage

		Prenatal Care		Mortality Rates			
	FBD	Frequency	First Month	NMR7	NMR	IMR	CMR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low baseline Insurance	0.0304	-0.0389	-0.0263	-2.167	-2.587	-7.597	-25.95**
× Post	(0.0187)	(0.0536)	(0.0908)	(3.399)	(3.420)	(5.633)	(12.02)
Observations	30,057	20,678	20,117	58,660	58,660	52,826	28,628

Notes: This table shows the treatment effect of expansion of CBHI on different outcome variables. Dependent variables are presented as the column title. Each column presents β_1 of Equation 2.1 with the preferred specification (column 4 in Table 2.3). Low insurance coverage means the insurance coverage of district was lower than the 75th percentile in 2005. Controls are as same in Table 2.3. See the notes of Table 2.3 for further information. Robust standard errors are in parentheses clustered by the proper district. * significance at 10%; ** significance at 5%; *** significance at 1%.

Appendix C

Appendix to Chapter 3

C.1 Constructing US's Indian Import Data

The balance of payment (BOP) of the Survey of Current Business (SCB) provides detailed information on service trade between the US and other countries. Unfortunately, the exact amount of trade in 1996 is not available for all service types. The tradable white-collar services are a subset of "other private services" in BEA classification. "Other private services (OPS)" consist of education, finance, insurance, telecommunication, and business, professional, and technical (BPT) services. The total dollar amount of recipient and payment (for all countries) is available for both unaffiliated and affiliated for each sector in 1996. However, at the country level, only unaffiliated trade is available. The trade of OPS is available in Table 5 of the SCB. Fortunately, the total OPS trade (affiliated and unaffiliated) between the US and each country is available through the supplement table. The BPT services are broken into several sectors in Table 7 of the SCB. The BPT services comprise advertising, computer and information, research and development, management, consulting and public relations, legal, construction and architecture, industrial engineering, and installation and maintenance services. In 1996, only unaffiliated BPT trade data is obtainable through SCB for aggregated data and each country.

In Section 3.3, I mention that I ignore affiliated import from India in 1996 as it accounted for only 3 percent of total OPS import. In Tables 5 and 7 of SCB, all types of services are available for India. However, the total import of each sector within BPT services is not available in 1996. Thus, I approximate the total (unaffiliated and affiliated) import and export of each sector of BPT services, assuming that the share of each sector accounting for in the total unaffiliated trade is the same as the share in total affiliated trade. For example, if the unaffiliated import of computer and information services constitutes 10 percent of total unaffiliated import, then this service takes up 10 percent of the total affiliated import as well. This is a strong assumption; however, it seems that this assumption holds well in 2006 when the data is available for the total amount of trade.

C.2 Commuting Zone-Level Analysis

The occupation-level analysis compares the relative change in employment with different levels of import penetration; however, it is difficult to identify the reallocation effects (Acemoglu et al., 2016). This section examines the impact of import penetration on employment in commuting zones (CZ), following Acemoglu et al. (2016); Autor, Dorn and Hanson (2013). The import penetration is defined similarly to equations 3.1 and 3.2, with one more weighted average using occupational composition in each CZ. In other words, CZ-level import penetration is defined as follows:

$$\Delta IP_{zt} = \sum_{k} \frac{L_{kz}}{\sum_k L_{kz}} \times \Delta IP_{kt}, \tag{C.1}$$

where L_{kz} is number of workers in industry k in CZ z. Then, I estimate the impact of trade exposure using a equation similar to Equation 3.3.

Table C.3 presents the impact of CZ-level import penetration on the fraction of employed among the total working-age population (15-65 years old), the log of the number unemployed, and the fraction in risky occupations in the CZ. Risky occupations here are defined as occupations whose ΔIP is in the top 25th percentile of table C.1. Results in Panels A and B are consistent. In the earlier period, there is a positive impact on total CZ level employment, although small and statistically insignificant. The results seem inconsistent with the main result, showing a decrease in employment of highly affected occupations. It may be that the reduction in employment of high IP occupations is not large enough to affect the total employment in a CZ.

This is not necessarily true when combined with Panel C. Panel C's results display the impact on share in risky occupations. While there is a very small and insignificant impact in the earlier period, a one standard deviation increase in import penetration results in a 2.6 percentage points increase in the share of risky occupations in the later period. This result corresponds to the main result that there is an increase in occupation-level employment.

Overall, although not perfectly, the results here imply there is a sorting effect caused by the increase in Indian service imports. While the employment of certain occupations declines in the earlier period, the total local employment even increases. Liu and Trefler (2019) present people with high import penetration occupations are likely to switch occupations or to unemployed. The results of my paper are in line with the literature.

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
1	Computer software developers	67.5502
2	Computer systems analysts and computer scientists	49.3654
3	Technical writers	40.6995
4	Physicists and astronomists	38.7210
5	Technicians, n.e.c.	34.6463
6	Physical scientists, n.e.c.	33.0064
7	Medical scientists	29.2978
8	Repairers of data processing equipment	25.6408
9	Biological scientists	25.3492
10	Social scientists and sociologists, n.e.c.	24.7227
11	Mathematicians and statisticians	21.8731
12	Lawyers and judges	20.4851
13	Computer and peripheral equipment operators	19.8436
14	Atmospheric and space scientists	18.5570
15	Chemists	18.5544
16	Geologists	18.2476
17	Legal assistants and paralegals	17.2559
18	Management analysts	17.0138
19	Data entry keyers	14.3558
20	Operations and systems researchers and analysts	13.7050
21	Biological technicians	13.5794
22	Engineers and other professionals, n.e.c.	12.9407
23	Electrical engineers	11.6488
24	Statistical clerks	11.5795
25	Agricultural and food scientists	11.4366
26	Office machine operators, n.e.c.	11.2605
27	Management support occupations	10.8143
28	Economists, market and survey researchers	10.6534
29	Sales engineers	10.4142
30	Personnel, HR, training, and labor rel. specialists	9.7391
31	Managers and specialists in marketing, advert., PR	8.6551
32	Designers	8.5078
33	Managers and administrators, n.e.c.	8.1676
34	Typists	7.3658

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
35	Proofreaders	6.7203
36	Human resources and labor relations managers	6.6341
37	Urban and regional planners	6.2050
38	Office supervisors	6.1879
39	Architects	6.0182
40	Mail clerks, outside of post office	5.9978
41	Writers and authors	5.9882
42	Bill and account collectors	5.9476
43	Drafters	5.6501
44	Other telecom operators	5.6213
45	Engineering technicians	5.5882
46	Painters, sculptors, craft-artists, and print-makers	5.5765
47	Surveryors, cartographers, mapping scientists/techs	5.3332
48	Inspectors and compliance officers, outside	5.1238
49	Secretaries and stenographers	5.0680
50	Administrative support jobs, n.e.c.	5.0613
51	Purchasing managers, agents, and buyers, n.e.c.	4.9749
52	Accountants and auditors	4.8978
53	Actuaries	4.8825
54	Customer service reps, invest., adjusters, excl. insur.	4.8274
55	Editors and reporters	4.7108
56	Industrial engineers	4.5940
57	Chemical engineers	4.5500
58	Interviewers, enumerators, and surveyors	4.5021
59	Mechanical engineers	4.4394
60	File clerks	4.4030
61	Financial managers	4.3575
62	Broadcast equipment operators	4.2783
63	Aerospace engineers	4.2235
64	Receptionists and other information clerks	3.6779
65	Business and promotion agents	3.6719
66	Records clerks	3.6305
67	Payroll and timekeeping clerks	3.5845
68	Civil engineers	3.5423

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
69	Clinical laboratory technologies and technicians	3.5300
70	Human resources clerks, excl payroll and timekeeping	3.4523
71	General office clerks	3.4107
72	Retail salespersons and sales clerks	3.3869
73	Airplane pilots and navigators	3.3864
74	Other financial specialists	3.2748
75	Chemical technicians	3.2443
76	Other science technicians	3.1957
77	Bookkeepers and accounting and auditing clerks	3.1789
78	Production checkers, graders, and sorters in	3.1080
79	Actors, directors, and producers	2.9593
80	Sales demonstrators, promoters, and models	2.9106
81	Photographic process workers	2.8248
82	Bank tellers	2.6823
82	Financial service sales occupations	2.6823
84	Billing clerks and related financial records processing	2.6745
85	Teachers, n.e.c.	2.6618
86	Psychologists	2.5640
87	Telephone operators	2.5052
88	Material recording, sched., prod., plan., expediting cl.	2.4571
89	Metallurgical and materials engineers	2.4559
90	Messengers	2.4030
91	Machinery maintenance occupations	2.0941
92	Weighers, measurers, and checkers	2.0909
93	Advertising and related sales jobs	2.0341
94	Aircraft mechanics	1.9684
95	Mechanics and repairers, n.e.c.	1.9460
96	Dispatchers	1.9262
97	Supervisors of mechanics and repairers	1.9237
98	Heavy equipement and farm equipment mechanics	1.9078
99	Ship crews and marine engineers	1.8900
100	Art/entertainment performers and related occs	1.8875
101	Crane, derrick, winch, hoist, longshore operators	1.7747
102	Fire fighting, fire prevention, and fire inspection occs	1.7722

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
103	Correspondence and order clerks	1.7712
104	Police and detectives, public service	1.7635
105	Explosives workers	1.7271
106	Archivists and curators	1.6907
107	Librarians	1.6585
108	Telecom and line installers and repairers	1.6283
109	Miscellanious transportation occupations	1.6274
110	Sales supervisors and proprietors	1.6126
111	Industrial machinery repairers	1.6121
112	Health record technologists and technicians	1.6073
113	Plant and system operators, stationary engineers	1.5842
114	Foresters and conservation scientists	1.5805
115	Printing machine operators, n.e.c.	1.5696
116	Helpers, constructions	1.5483
117	Petroleum, mining, and geological engineers	1.5467
118	Personal service occupations, n.e.c	1.5205
119	Packers and packagers by hand	1.4980
120	Supervisors of guards	1.4882
121	Small engine repairers	1.4874
122	Typesetters and compositors	1.4728
123	Precision makers, repairers, and smiths	1.4042
124	Veterinarians	1.3989
125	Construction inspectors	1.3207
126	Salespersons, n.e.c.	1.2999
127	Guards and police, except public service	1.2124
128	Shipping and receiving clerks	1.1564
129	Insulation workers	1.1485
130	Health technologists and technicians, n.e.c.	1.1174
131	Photographers	1.0128
132	Repairers of industrial electrical equipment	0.9723
133	Eligibility clerks for government prog., social welfare	0.9529
134	Hand molders and shapers, except jewelers	0.9384
135	Dressmakers, seamstresses, and tailors	0.9153
136	Repairers of electrical equipment, n.e.c.	0.9130

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
137	Machine operators, n.e.c.	0.9019
138	Vehicle washers and equipment cleaners	0.8587
139	Insurance underwriters	0.8448
140	Programmers of numerically controlled machine tools	0.8429
141	Insurance adjusters, examiners, and investigators	0.8164
142	Laborers, freight, stock, and material handlers, n.e.c.	0.8087
143	Stock and inventory clerks	0.8013
144	Insurance sales occupations	0.7971
145	Subject instructors, college	0.7834
146	Library assistants	0.7829
147	Separating, filtering, and clarifying machine operators	0.7569
148	Dieticians and nutritionists	0.7558
149	Production helpers	0.7528
150	Other plant and system operators	0.7503
151	Repairers of household appliances and power tools	0.7364
152	Taxi cab drivers and chauffeurs	0.6861
153	Guides	0.6719
154	Production supervisors or foremen	0.6365
155	Bus, truck, and stationary engine mechanics	0.6315
156	Repairers of mechanical controls and valves	0.6311
157	Janitors	0.6276
158	Musicians and composers	0.5973
159	Cementing and gluing machne operators	0.5932
160	Assemblers of electrical equipment	0.5543
161	Vocational and educational counselors	0.5462
162	Machine feeders and offbearers	0.5455
163	Boilermakers	0.5374
164	Licensed practical nurses	0.5339
165	Welders, solderers, and metal cutters	0.5222
166	Motion picture projectionists	0.5126
167	Bookbinders	0.4949
168	Furniture/wood finishers, other prec. wood workers	0.4924
169	Registered nurses	0.4863
170	Heating, air conditioning, and refrigeration mechanics	0.4789

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
171	Operating engineers of construction equipment	0.4779
172	Animal caretakers, except farm	0.4698
173	Water and sewage treatment plant operators	0.4649
174	Truck, delivery, and tractor drivers	0.4643
175	Supervisors of motor vehicle transportation	0.4508
176	Upholsterers	0.4409
177	Stevedores and misc. material moving occupations	0.4389
178	Drillers of earth	0.4373
179	Managers of properties and real estate	0.4359
180	Health and nursing aides	0.4326
181	Shoemakers, other prec. apparel and fabric workers	0.4261
182	Dental laboratory and medical applicance technicians	0.4210
183	Miscellanious textile machine operators	0.4085
184	Social workers	0.4024
185	Drillers of oil wells	0.3995
186	Electric power installers and repairers	0.3927
187	Electricians	0.3838
188	Machinists	0.3837
189	Molders and casting machine operators	0.3822
190	Excavating and loading machine operators	0.3716
191	Mixing and blending machine operators	0.3670
192	Other mining occupations	0.3660
193	Millwrights	0.3571
194	Tool and die makers and die setters	0.3491
195	Garbage and recyclable material collectors	0.3490
196	Graders and sorters of agricultural products	0.3434
197	Extruding and forming machine operators	0.3356
198	Supervisors of cleaning and building service	0.2889
199	Announcers	0.2838
200	Transportation ticket and reservation agents	0.2764
201	Physicians	0.2753
202	Sawing machine operators and sawyers	0.2674
203	Radiologic technologists and technicians	0.2600
204	Slicing, cutting, crushing and grinding machine	0.2595

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
205	Therapists, n.e.c.	0.2542
206	Packers, fillers, and wrappers	0.2499
207	Physicians' assistants	0.2484
208	Painting and decoration occupations	0.2461
209	Ushers	0.2459
210	Recreation and fitness workers	0.2357
211	Housekeepers, maids, butlers, and cleaners	0.2325
212	Engravers	0.2301
213	Gardeners and groundskeepers	0.2218
214	Pharmacists	0.2212
215	Baggage porters, bellhops and concierges	0.2159
216	Textile sewing machine operators	0.2154
217	Garage and service station related occupations	0.2152
218	Clothing pressing machine operators	0.2087
219	Other metal and plastic workers	0.2087
220	Pest control occupations	0.2056
221	Painters, construction and maintenance	0.2031
222	Real estate sales occupations	0.2016
223	Misc. construction and related occupations	0.1991
224	Inspectors of agricultural products	0.1934
225	Nail, tacking, shaping and joining mach ops (wood)	0.1894
226	Plumbers, pipe fitters, and steamfitters	0.1861
227	Helpers, surveyors	0.1790
228	Superv. of landscaping, lawn service, groundskeeping	0.1782
229	Paper folding machine operators	0.1713
230	Construction laborers	0.1709
231	Power plant operators	0.1622
232	Crossing guards	0.1613
233	Managers in education and related fields	0.1607
234	Carpenters	0.1556
235	Textile cutting and dyeing machine operators	0.1502
236	Laundry and dry cleaning workers	0.1456
237	Batch food makers	0.1428
238	Farm workers, incl. nursery farming	0.1425

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
239	Automobile mechanics and repairers	0.1397
240	Structural metal workers	0.1248
241	Respiratory therapists	0.1227
242	Supervisors of construction work	0.1198
243	Door-to-door sales, street sales, and news vendors	0.1072
244	Dancers	0.1047
245	Locksmiths and safe repairers	0.0993
246	Punching and stamping press operatives	0.0954
247	Other woodworking machine operators	0.0931
248	Athletes, sports instructors, and officials	0.0884
249	Timber, logging, and forestry workers	0.0878
250	Protective service, n.e.c.	0.0866
251	Food preparation workers	0.0851
252	Physical therapists	0.0786
253	Other precision and craft workers	0.0776
254	Recreation facility attendants	0.0766
255	Concrete and cement workers	0.0765
256	Speech therapists	0.0740
257	Furnance, kiln, and oven operators, apart from food	0.0726
258	Miscellanious food preparation and service workers	0.0715
259	Supervisors of personal service jobs, n.e.c	0.0712
260	Cashiers	0.0697
261	Parking lot attendants	0.0641
262	Public transportation attendants and inspectors	0.0600
263	Farm managers	0.0558
264	Cabinetmakers and bench carpeters	0.0550
265	Glaziers	0.0548
266	Child care workers	0.0545
267	Dental hygienists	0.0537
268	Welfare service workers	0.0529
269	Cooks	0.0505
270	Supervisors of food preparation and service	0.0477
271	Optical goods workers	0.0453
272	Occupational therapists	0.0409

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
273	Masons, tilers, and carpet installers	0.0383
274	Kindergarten and earlier school teachers	0.0331
275	Elevator installers and repairers	0.0326
276	Roofers and slaters	0.0325
277	Waiters and waitresses	0.0301
278	Forge and hammer operators	0.0279
279	Plasterers	0.0276
280	Teacher's aides	0.0262
281	Bakers	0.0246
282	Bus drivers	0.0225
283	Butchers and meat cutters	0.0222
284	Bartenders	0.0215
285	Auto body repairers	0.0200
286	Knitters, loopers, and toppers textile operatives	0.0191
287	Other health and therapy occupations	0.0183
288	Railroad conductors and yardmasters	0.0172
289	Paving, surfacing, and tamping equipment operators	0.0172
290	Dental Assistants	0.0169
291	Locomotive operators: engineers and firemen	0.0169
292	Clergy and religious workers	0.0153
293	Fishers, marine life cultivators, hunters, and kindred	0.0150
294	Miners	0.0092
295	Drywall installers	0.0087
296	Funeral directors	0.0084
297	Special education teachers	0.0075
298	Hairdressers and cosmetologists	0.0060
299	Winding and twisting textile and apparel operatives	0.0058
300	Dentists	0.0056
301	Rollers, roll hands, and finishers of metal	0.0021
302	Optometrists	0.0015
303	Barbers	0.0000
303	Mail carriers for postal service	0.0000
303	Mail and paper handlers	0.0000
303	Hotel clerks	0.0000

Table C.1: Ranking of ΔIP, All Occupations

Rank	Occupation Title	$\Delta IP \times 100$
303	Air traffic controllers	0.0000
303	Meter readers	0.0000
303	Primary school teachers	0.0000
303	Food roasting and baking machine operators	0.0000
303	Buyers, wholesale and retail trade	0.0000
303	Managers of medicine and health occupations	0.0000
303	Podiatrists	0.0000
303	Postal clerks, exluding mail carriers	0.0000
303	Purchasing agents and buyers of farm products	0.0000
303	Secondary school teachers	0.0000

Notes: Extended version of Table 3.2.

Table C.2: Crosswalk Between Census Industry and Outsourcing Service Type

Industry Code (1990)	Label	Service Type
721	Advertising	Advertising
882	Engineering, architectural, and surveying services	Construction & Architecture
700	Banking	Finance
701	Savings institutions, including credit unions	Finance
702	Credit agencies, n.e.c.	Finance
710	Security, commodity brokerage, and investment companies	Finance
711	Insurance	Insurance
841	Legal services	Accounting
890	Accounting, auditing, and bookkeeping services	Legal Services
892	Management and public relations services	Management
732	Computer and data processing services	Computer and Information
891	Research, development, and testing services	R&D and testing
441	Telephone communications	telecommunication
442	Telegraph and miscellaneous communications services	telecommunication
752	Electrical repair shops	Installation and maintenance
760	Miscellaneous repair services	Installation and maintenance
782	Shoe repair shops	Installation and maintenance
731	Personnel supply services(Employment)	Other Business Professionals, and Technical Services
741	Business services, n.e.c.	Other Business Professionals, and Technical Services
742	Automotive rental and leasing, without drivers	Leasing and rental

Notes: This table provides a crosswalk between Census industry codes (defined in 1990) and tradable white-collar services used in the paper. I emulate the crosswalk provided by Liu and Trefler (2019).

Table C.3: Impact of Import Penetration in CZ-Level

	OLS	2SLS			
	All period	All period	All period	2000-2006	2006-2016
	(1)	(2)	(3)	(4)	(5)
Panel A. Share Employed					
Z-score of	0.0297***	0.0350***	-0.0129	0.101	-0.0716**
ΔIP	(0.00879)	(0.00993)	(0.0309)	(0.0643)	(0.0347)
Panel B. Ln(Unemployed)					
Z-score of	0.0031***	0.0030***	0.0006	-0.0129*	0.0086**
ΔIP	(0.000683)	(0.000844)	(0.00340)	(0.00666)	(0.00373)
Panel C. Share in risky occupations					
Z-score of	-0.0309***	-0.0332***	-0.0146	-0.0076	0.0264*
ΔIP	(0.00638)	(0.00596)	(0.0161)	(0.0365)	(0.0157)
Observations	1482	1482	1482	741	741
Controls	No	No	Yes	Yes	Yes

Notes: This table reports the estimates of the impact of CZ level IP on the share of the employed population, log of unemployment, and the share of risky occupations. Risky occupations mean that the increase of IP from 2000 to 2016 is greater than the median. Each entry is a coefficient from a separate 2SLS regression. The coefficients are rescaled to represent the annual change in outcome variables. I also normalize IP for interpretation. The instrument is the IP defined with Indian export to 15 EU countries. See the main text for further information. Control variables include the CZ level share of college graduates, foreigners, women, white, and black, population, average offshorability (routine), and average weekly wage at the beginning of each period. Regressions are weighted using the size of the population in 2000. Robust standard errors are in parentheses clustered by state. * significance at 10%; ** significance at 5%; *** significance at 1%.

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