

# The Long-Run Effects of Childhood Exposure to a Private School Voucher Program\*

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## 1. PROJECT OVERVIEW

School vouchers are an increasingly popular policy tool – at the time of writing, 17 states have implemented universal school voucher programs – but to date we know almost nothing about the medium-run consequences of school voucher programs, and nothing about the long-run consequences of voucher programs, in the United States. The only US study that can look as far as college graduation is Chingos, Figlio, and Karbownik (2025), who present evidence that Ohio’s EdChoice voucher program led to increases in both college attendance and graduation.

In this project, we will study the long-run effects of childhood exposure to a private school voucher program. The policy setting is the Florida Tax Credit (FTC) scholarship program, a voucher program in Florida that began in 2002, and was initially called the Corporate Tax Credit Scholarship Program. Our project will leverage variation on three margins: first, students living in neighborhoods that had more private schools prior to the policy are more exposed; second, students who began their primary education closer to 2002 (as opposed to earlier) are more exposed; and third, students who entered elementary school at different times during the program’s rollout are differentially exposed. This design builds upon and is validated by prior work that finds positive short-run effects of exposure to vouchers for students attending public schools in Florida (Figlio and Hart, 2014; Figlio et al., 2023). Existing work has been limited to studying short-run effects on test scores and has focused on the competitive effects of the voucher program felt by students enrolled at public schools. Our work will use restricted Census Bureau microdata along with school location data to measure, for millions of children in Florida, their exposure to the voucher program based on year of birth and location during childhood alongside their long-run outcomes including income and labor force participation, educational attainment, family formation, geographic mobility, and more. Our work will produce novel insights in two regards: first, ours will be the first to study long-run effects of childhood exposure to a voucher program beyond shorter-run impacts on test scores or college attendance/graduation, which will be crucial for assessing the overall effectiveness of such policies; second, the effect we estimate will correspond to the total policy effect of the voucher program for those exposed combining any direct effects of increased private school enrollment with indirect competitive effects for students attending public schools, which is the reduced form parameter that reflects overall policy effectiveness and which can be used to compute a Marginal Value of Public Funds (MVPF).

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\* This document is intended to outline the key questions and analytical framework involved in this project while proposing a set of specific methods of analysis as a reference for continued refinement. Researchers may pursue analyses that represent modified versions of those described here and which are within the spirit of this proposal.

## 2. RESEARCH DESIGN

### 2.1. MAIN SPECIFICATION

We will estimate regressions of the following form:

$$Y_i = \beta(Exposure_{c(i)} \times Dosage_i) + \delta Exposure_{c(i)} + \gamma Dosage_i + \theta' X_i + \epsilon_i$$

where  $Y_i$  is a long-run outcome of interest for individual  $i$ ;  $Exposure_{c(i)}$  measures the number of individual  $i$ 's school-age years during which the voucher program was in place, defined as  $\min\{\max\{(c(i) + 17) - 2002, 0\}, 12\}$  with  $c(i)$  being the birth year (cohort) of individual  $i$ ;  $Dosage_i$  is a measure of local exposure to private schools based on individual  $i$ 's residential location prior to 2002, for example the number of private schools within a 5 mile radius from one's residence by crow's flight distance, or other similar measures; and  $X_i$  is a vector of controls.  $\beta$  is the parameter of interest, which reflects any difference in the relationship between long-run outcomes and years of school-age exposure to vouchers for individuals in locations that had a higher versus a lower concentration of private schools prior to the voucher program. We will further exploit the nonlinear rollout of the program, an empirical strategy validated in Florida by Figlio et al., 2023.

### 2.2. ROBUSTNESS

Our design leverages shift-share variation, so we will assess the validity of the design following recent insights related to shift-share designs (Goldsmith-Pinkham et al., 2020; Borusyak and Hull, 2023; Borusyak et al., 2025).

We will further assess the validity of our design by estimating pre-trends, which can be done by replacing  $Exposure_{c(i)}$  with a full set of birth cohort fixed effects in our main regression specification, whereby we can test whether there is a differential pre-trend in long-run outcomes for individuals facing higher versus lower  $Dosage_i$  among cohorts that reach age 17 prior to the voucher program. Moreover, we will study within-family variation by comparing siblings who are more and less exposed to the voucher policy based on their age of birth, which will help assess whether our main estimates are confounded by a changing composition of families over time in more and less exposed regions among cohorts of children who are exposed to vouchers.

### 2.3. HETEROGENEITY

#### *Age Effects*

We will break out variables measuring the specific age of exposure, which will allow us to assess whether the impact of an additional year of exposure to vouchers has a different impact based on the child's age.

#### *Mechanisms: Direct vs. Indirect Effects*

The voucher policy may affect outcomes of exposed children through two main channels. First is the “direct” channel that operates through increasing private school enrollment. Second is the “indirect” channel whereby public schools increase quality to retain students in response to competitive pressures from private schools.

To investigate these two channels, we will consider differential effects for children in families with income below or above the voucher eligibility threshold. In the earlier years of the voucher program, eligibility was restricted to families with income below a threshold. Children in eligible families are potentially affected by both the direct and the indirect channels; however, children in ineligible families can only be affected by the indirect channel. We will compare children from families of varying income levels – especially focusing on income levels around the eligibility threshold – to help separate the contributions from direct versus indirect channels.

### *Mechanisms: Other Regional Interactions*

To better understand channels through which any main effects operate, we will consider interactions with various regional factors including those described below.

First, we will consider whether effects vary based on measures of local public school spending and quality. Children in regions with lower quality public schools may benefit more from vouchers as their outside option is weaker. At the same time, these regions may face more competitive pressure and thereby may observe a greater indirect effect than regions with higher quality public schools. We will interact our main measures of exposure with regional variation in school quality as well as school spending, where spending can be measured via surveys conducted by Figlio in the period immediately before the voucher program was implemented. We will extend this analysis to compare children on either side of school district boundaries, as students can easily attend private schools across district boundaries but can rarely attend public schools across boundaries, and many other local regional factors will be similar on either side of a boundary.

Second, we will consider whether effects vary by local concentration of religious versus non-religious private schools, as well as variation in the range of religious denominations of religious schools. These variables can be measured in the pre-policy census of private schools provided to Figlio by the State of Florida.

Third, we will consider whether there are interactions between the voucher program and school accountability. Florida had a very well-known school accountability system, introduced in 1999, that affected public school quality and behaviors (see, e.g., Figlio, 2006; Figlio and Getzler, 2006; Rouse and Figlio, 2006; Rouse et al., 2013), as well as capitalization of school quality measures into housing prices (Figlio and Lucas, 2004) and public financial support for local public schools (Figlio and Kenny, 2010). We can exploit changes in the school grading system over time to study the differential effects of school voucher exposure and school accountability exposure, as well as the interaction between vouchers and accountability.

Fourth, we will consider whether effects vary by local labor market quality.

### *Demographics*

To understand who is most affected by vouchers, we will study heterogeneity of effects by various demographic characteristics including family income, race, educational attainment of parents, and more.

## **2.4. COMPUTING MVPF**

We will compute the overall MVPF for the voucher program in two ways. First, we will use our main estimates to generate an estimate of the average effect, by combining the dosage-based effect we measure with information on actual dosage. We will then relate this to a measure of average cost, which is based on total program cost and the total number of students. Our second method will directly relate regional variation in exposure to regional variation in voucher expenditures.<sup>1</sup> This is a two-stage least squares approach, where the first stage regression uses variation in dosage to fit variation in voucher expenditures, and the reduced form is our main regression.

We will also separate portions of the total MVPF due to the direct and indirect channels described above. To do this, we will compute an MVPF that compares any differences in total effects of vouchers for families just below versus just above in income eligibility threshold against differences in voucher expenditures for the same. This MVPF would isolate only the direct effect channel. We can compare this with the total MVPF described above (and a similar version restricted to families similar to those used to estimate the direct-effect MVPF) to then infer the remaining contribution from the indirect channel.

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<sup>1</sup> This approach is pending availability of external data that Figlio has used in past research, and which can be merged to Census data at aggregated levels that do not require any linkages at the individual or family level.

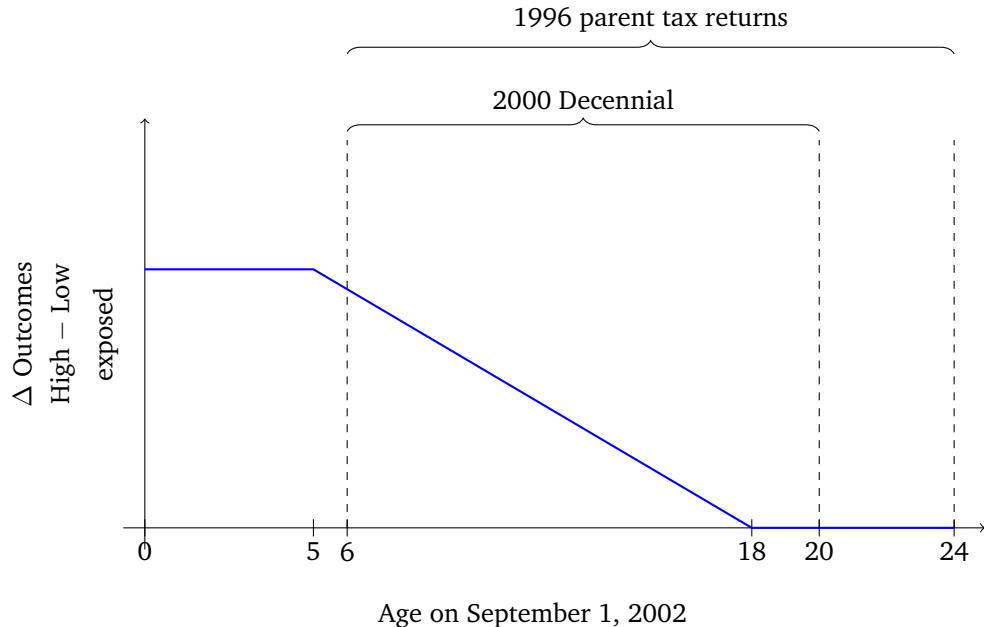
## 2.5. DATA PROCESSING

### *Matching Locations and Measurable Effects*

Our dosage variable measures an individual's geographic proximity to private schools, meaning we must measure two types of locations: (1) the location of private schools in Florida; and (2) the location of an individual's residence. We currently have data measuring latitude and longitude of all private schools in Florida that was previously constructed by Figlio. Therefore, we must define how we will measure an individual's residential location in the Census data.

Our approach to measuring an individual's residential location depends partly on how availability of various sources over time relates to the timing of the policy and which effects we can measure as a result. Figure 1 illustrates this. Florida's voucher policy began in 2002, and our research design identifies the effects of the policy by comparing differences in outcomes for individuals who face a higher versus a lower dosage based on residential proximity to private schools interacted with years of schooling age exposure to the policy. The blue line plotted in Figure 1 shows a potential path of effects, measured as the difference in outcomes for those with high versus low dosage and plotted over an individual's age as of September 1, 2002, the start of the school year in which the policy began. Individuals who were 18 and older in 2002 already completed primary and secondary schooling before the policy began, and so were not exposed to the policy. As a result, there should be no effect for these individuals. Individuals who were age 5 and younger in 2002 had not yet begun primary school when the policy began, so they were exposed to the policy for the entirety of their schooling, and as a result these individuals would face the full effect due to residing in a high versus a low dosage location. In the middle, for individuals between ages 5 and 18 in 2002, the policy was implemented during the schooling years for these individuals, meaning that each subsequent cohort (i.e. a cohort that was one year older in 2002) faced one year less of exposure to the policy, and as a result would face a marginally smaller total effect due to residing in a high versus a low dosage location, with the slope reflecting the effect of one additional year of exposure.

**Figure 1: Potential Effects and Data Coverage**



We can estimate a specific subset of these effects depending upon our data. First, we address how data availability affects the youngest cohort for which we can estimate effects, i.e. how far to the left of the effects in Figure 1 we can estimate. Suppose we are measuring W2 earnings as an outcome. Currently, W2 earnings data is available through 2024. Suppose further that we measure W2 earnings when an individual

is 28 years old. Then, the youngest cohort for which we can estimate effects would be 28 years old in 2024, meaning they are 6 years old in 2002. Therefore, with current Census data and focusing on outcomes measured at age 28, we can measure effects as far left in Figure 1 as those occurring for the cohort that is age 6 in 2002. Of course, this can be extended to younger cohorts both with more recent outcome data becoming available at Census and by measuring outcomes at a younger age. For example, when W2 data is available for 2025, and if we measure an outcome that is W2 income at age 26, then we would be able to estimate effects for cohorts as young as age 3 in 2002.

Next, we address how data availability affects the oldest cohort for which we can estimate effects, i.e. how far to the right of the effects in Figure 1 we can estimate. The limiting factor on this side is primarily not availability of outcomes data, but rather availability of residential location data. In our main specification, we will measure residential location when the child is of schooling age (i.e. we measure location at a time when the child was age 17 or younger as of the immediately preceding September 1), so that it corresponds to a schooling-relevant residence and is more likely to reflect residence with a parent or guardian. Our goal for the main specification is also to measure a precise address – specifically, a latitude and longitude – rather than a larger region like a county. As a result, we have two primary data sources available: the 2000 Decennial, and 1996 parent tax returns.<sup>2</sup> With the 2000 Decennial, we can measure residence for cohorts as old as those age 17 as of September 1, 1999, meaning we can estimate effects as far right in Figure 1 as for those age 20 as of September 1, 2002. The primary advantage of using the 2000 Decennial to measure residential location is its comprehensiveness; as a specific contrast, the 2000 Decennial does not rely on whether an individual's parents file taxes in a given year. The primary disadvantage of using the 2000 Decennial is that we cannot measure effects beyond those who are age 20 as of September 1, 2002. Measuring effects at ages 19+ is important because these estimates serve as a “pre-trends” robustness check; using the 2000 Decennial to assign residential location, we will have three cohorts that are not exposed to vouchers during their schooling, which gives us two pre-trend estimates to test (age effects at 19 and 20). If we instead use 1996 parent tax returns, we can measure residence for cohorts as old as those age 17 as of September 1, 1995, meaning we can estimate effects as far right in Figure 1 as for those age 24 as of September 1, 2002. The primary advantage of using 1996 parent tax returns to measure residential location is the ability to estimate effects over a longer span of ages in the 19+ range, which allows us to better assess “pre-trends”. The primary disadvantage of using 1996 parent tax returns is that the sample is limited to those individuals whose parents filed taxes in 1996. Our approach will be to estimate effects separately using each of these two measures of residential location, with the two sets of results complementing each other in terms of strengths and weaknesses so that the combination of results from both specifications will provide a more robust view of the effects of the voucher policy.

We will consider additional definitions of residential location as further robustness checks. First, we will use latitude and longitude from the 2000 Decennial and include cohorts that are 18+ as of September 1, 1999, in addition to those that are 17 and younger. In this case, we would be able to extend our estimated effects up to the point where we become limited by availability of outcome data. For example, if one outcome is W2 earnings at age 28, and since W2 data begins in 1999, then we would be able to measure effects for cohorts as old as the cohort that is 28 in 1999, meaning they are 32 in 2002. Second, we will incorporate a crosswalk recently developed by Martha Stinson and Laura Weiwei that matches dependents to parents for cohorts born between 1964 and 1979. As an example, in this case we can measure parent location – latitude and longitude – from 1979 tax returns for children born between 1964 and 1979, meaning they are between ages 24 and 39 in 2002 (at which point, we may become limited by outcome data availability, as noted previously).<sup>3</sup> While this only covers “pre-trend” years and is from a sample that differs from that in our main specifications, it is still informative: despite these factors, testing whether the slope of estimated effects over ages is at or near zero is still a useful test of pre-trends. Finally, we will measure an individual's residential location as their county of birth using Numident records.

### *Outcome Variables*

Key outcomes include the following:

<sup>2</sup> Here, we use “parent” loosely to refer to a guardian who claims the child as a dependent on tax forms. We focus on tax forms as of 1996 because this is the earliest year in which dependent claiming required listing the dependent's SSN.

<sup>3</sup> This will require first confirming whether latitude and longitude are recorded for addresses on tax returns as far back as the 1970s.

1. Income, Earnings, and Employment

- W2 wages (1999 onward)
- 1099s (2010 onward)
- 1040s (1989, 1994, 1996 onward)

2. Educational Attainment

- Education variable from ACS

3. Other outcomes

- Safety net participation (SNAP/TANF data for FL)
- Other ACS outcomes – homeownership, occupation, marital status, kids, and more
- Other outcomes observed in tax data – migration from hometown, tax filing, marriage, kids, and more