Supply-Side Responses in School Choice

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Abstract

Despite the growing size of private school voucher programs, our understanding of their effectiveness relies on results from small-scale randomized control trials. In this paper, we show why those results may not translate to programs at scale. First, we examine changes in school quality following the implementation of the Indiana Choice Scholarship Program and find that public schools facing high exposure to the policy increased their school quality while participating private schools decreased their quality. Second, we develop a discrete choice model of household demand for schools to examine counterfactual scenarios where schools cannot respond. We show that the incentive for schools to increase quality is nonlinear with respect to the voucher amount and the income eligibility threshold. Our results suggest that voucher programs only threaten public school enrollment when the voucher amount is large, or when a significant proportion of students are eligible to participate with current voucher amounts. Lastly, we show that when high-income families qualify for a voucher, the incentive to provide quality is first-order different for public and private schools. Policymakers interested in adopting and expanding these programs should consider these indirect and nonlinear effects to understand vouchers' impact on educational outcomes.

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I Introduction

School choice programs have become a popular tool to eliminate inequities in access to schooling. Private school vouchers have drawn increasing attention in this effort. In the last 20 years, the number of state-funded voucher programs has increased five-fold, from 5 in 2000 to 27 in 2021. Furthermore, the scale of these programs has grown significantly over time. The Milwaukee Parental Choice Program had an enrollment cap of 1% of the public school population when enacted as the first voucher program in the United States in 1991. Today, the average program does not have enrollment caps, and around 26% of families qualify to participate (EdChoice, 2021)[1]. Moreover, in states that have these programs, nearly 1 in 10 private school students now use a voucher to attend (EdChoice, 2021; National Center for Education Statistics, 2019).

Despite increases in the size of voucher programs, the literature evaluating their effectiveness has relied on small-scale randomized control trials (RCTs) comparing the outcomes of those offered a voucher to those in the control group for a small subset of the total student population (Mayer et al., 2002; Howell et al., 2002; Witte et al., 2014; Wolf et al., 2010; Abdulkadiroğlu et al., 2018)[2]. While these RCTs provide useful estimates of the average effect of being offered a voucher, their results may not translate to the impact of voucher programs as vouchers are implemented on a larger scale. Specifically, economic theory predicts that as these programs expand, schools have the incentive to respond (Friedman, 1962; Chakrabarti, 2008). It is essential to examine these school responses if we wish to understand the total impact of voucher programs on educational outcomes.

In this paper, we quantify schools' responses by examining changes in school quality following the adoption of the largest voucher program in the United States[3]. Our context centers around the Indiana Choice Scholarship Program (ICSP), which was initially adopted in 2011 and expanded in 2013. We begin with student-level testing data that covers all students in the state between the 2005-2006 and 2017-2018 academic years (AY). We use this data to construct school-level measures of quality by estimating value-added for both public and private schools. We then identify schools facing greater exposure to the voucher policy by calculating the radial distances between each public and private school within the state. Specifically, we distinguish high-exposure public schools as those

[1]The Florida Family Empowerment Scholarship, the Ohio Income-Based and Educational Choice Scholarship Programs, and the Wisconsin Parental Choice Program serve as notable exceptions; however, each program allows for gradual expansion if enrollment reaches the set cap. These caps are still much larger than those in the 1990s.

[2]See Epple et al. (2017) and Rouse and Barrow (2009) for excellent reviews on the topic

[3]There are three main types of voucher programs including tax credit scholarships, education savings accounts and standard private school voucher programs. The Indiana Choice Scholarship Program is the largest standard private school voucher program. Indiana currently ranks sixth in terms of percentage of current educational expenditures spent on voucher programs.

with a private school within five miles of its location. Private schools that accept vouchers students are said to face high exposure to the policy if they are in the top tercile of the distribution of the number of public schools within five miles[4]. The resulting data sets track school quality for those in the constructed high-exposure and control groups both before and after the implementation of the voucher program[5].

Using these datasets, we estimate the causal effects of the implementation of ICSP on schools using a standard difference-in-differences model. Specifically, we compare the change in school value-added in the years before and after the implementation of the policy in schools facing high exposure to the policy versus those in the control group. Our primary analysis focuses on public schools. We find that, on average public schools facing the threat of voucher competition saw a statistically significant increase of 0.023 s.d. in their overall school value-added, an increase of 0.03 s.d. in their math value-added, and an increase of 0.013 in their reading value-added—however, improvements in value-added varied within the high-exposure group. Public schools facing the threat of competition and an above-median share of students qualifying for a 90 percent voucher witnessed the largest improvements in school quality. Specifically, these schools saw increases of 0.039 s.d. in overall school VA, 0.047 s.d. in math VA, and 0.028 s.d. in reading VA.

We further explore the impacts of ICSP on public school quality by disaggregating the results by several baseline characteristics. Specifically, we examine whether the changes in public school quality differ across schools above/below the median in baseline enrollment, overall school value-added, and income of the census block group where the school is located. While we find no evidence of heterogeneous results across enrollment or household income, high exposure public schools with an above-median baseline school value-added saw almost no changes in our outcomes of interest. This result suggests that initially poor-performing public schools facing the potential threat of competition drive the changes we see in quality. Together, we can conclude that the gap in public school quality shrunk following the implementation of ICSP.

We also include results from an event-study specification that allows us to examine whether the adoption and expansion of ICSP had differential impacts on public school quality. We find that the adoption of the policy did not elicit differential changes across high exposure and control public schools in our outcome measures of interest. Instead, increases in school quality for high-exposure public schools are seen only after the program's expansion. This result reveals that despite facing potential enrollment losses as the program is adopted, public schools only responded once there was

[4]The definition of high exposure shifts between public and private schools because while only half of the public schools have a private school within five miles, 98% of private schools have a public school within five miles. In Section VI, I discuss an alternative method for distinguishing high exposure private schools.

[5]We separate the analyses of public and private schools.

a threat that a majority of their students could leave. We take these results as evidence that the total effect of voucher programs at scale may be very different from the partial equilibrium results found in the existing literature.

To understand how high-exposure public schools increased their value-added, we combine a school-level dataset on available teacher data between the 2010-2011 and 2017-2018 AY with the Common Core of Data on Indiana public schools from the National Center of Education Statistics. We do not find strong evidence that following ICSP, high-exposure public schools saw changes in their student-teacher ratios. However, we find that high-exposure public schools saw an increase of 0.7 teachers with a graduate degree and 1.5 teachers with a high-quality certification when compared to the set of control schools. We also find that after the adoption of ICSP, high-exposure public schools saw increases in their attendance and decreases in the percent of students ever suspended or expelled. These findings suggest that as schools respond to ICSP, they are increasing quality such that there are improvements on both the cognitive and non-cognitive dimensions.

Given our results, we pay particular attention to the possibility that changes in the composition of students could generate our findings. To address this concern, we first document to what extent student sorting occurs after the implementation of ICSP. We find that high-exposure public schools see a decline of 2.8 percentage points (p.p.) in the number of White students and a rise of 2.3 p.p. in the number of Hispanic students after the policy is adopted. We also find that students who use a voucher have slightly higher achievement levels than those who qualify for the voucher and remain in the public school system[6]. To understand whether these demographic changes drive our results, we run a difference-in-differences specification using predicted value-added. We find that based only on changes in observable characteristics, high-exposure public schools were predicted to see declines in their school value-added. We present these results as evidence that the improvements in school quality are not due exclusively to student sorting. We address concerns over non-random student sorting on unobservable characteristics by highlighting the advantages of our value-added estimates since they control for prior achievement. Assuming that prior achievement fully proxies for those inputs that affect a student's achievement prior to using the voucher and those inputs are correlated with a student's likelihood of using a voucher, we can mitigate the concerns of this type of sorting.

We also show that the results on public schools are robust to model specification choices and the adoption of other policy interventions that could threaten the validity of our findings. First, in our event-study specifications, high-exposure public schools and those in the control group appear

[6] This phenomenon is often referred to as "cream-skimming" and is one of the main critiques of private school voucher programs. However, our results suggest that high-exposure public schools improve their quality despite this sorting on ability.

to have similar trends in school value-added in all years prior to the program, suggesting that our results are not driven by differential trends between the two groups of public schools. We also show that our results are robust to placebo adoption years. Specifically, we re-run our event-study specification, where we assigned the adoption of ICSP for two years before the program started. We find that changes in school quality occurred only after the actual expansion of the voucher policy, further bolstering our conclusions that pre-trends do not drive our results. To address the concern that high-exposure public schools may be concentrated in a small number of urban districts, we run our results dropping each county in Indiana. Our analysis finds similar results to the entire state sample. Lastly, we argue that no meaningful policy changes were adopted that would have differentially impacted our two sets of schools, which would be needed to explain our findings.

For private schools accepting voucher students (from hereon called choice schools), we use the constructed dataset to present evidence on their responses to the policy. Visually, choice schools see declines in average quality on all dimensions in the program's first year. In our difference-in-differences specification, we compare choice schools surrounded by many public schools to those with fewer options to attract students. We find evidence that high exposure choice schools saw larger decreases in school quality compared to the control group. We discuss the possible theoretical reasons behind the decrease in quality in the following sections. We use the Private School Universe Survey to understand to what extent choice schools alter their school inputs in the years of our sample. Following the adoption of ICSP, high-exposure choice schools see a statistically significant increase of 0.93 (off a base mean of 14.24) in their student-teacher ratios. We also find suggestive evidence that control choice schools increase their instructional time to catch up to high-exposure choice schools once the program is adopted.

The assumption underpinning our reduced-form analysis is that potential changes in enrollment create an incentive for schools to change their quality. However, these results cannot quantify to what extent this threat exists or whether the enrollment threat changes depending on the program's design. We, therefore, develop a simple demand model that allows us to quantify the exact loss (for public schools) or gain (for private schools) in market share that schools would experience absent any change in quality/value-added. We frame our model around the intuition that holding all else constant, increasing voucher eligibility or the voucher amount will reduce product differentiation between the two sets of schools in the market. The reduction in product differentiation reduces the market power of public schools – for which there are now better substitutes – and provides an incentive for them to improve quality to avoid the total loss of market share and corresponding revenue. For the opposite reason, private schools are less incentivized to provide quality. The advantage of our model is that we can estimate the relationship between threats to enrollment and the incentive to change quality for the full range of potential voucher programs with respect to eligibility and voucher size.

Our model provides three important insights: First, small expansions of a voucher program, in terms of either changes in the voucher amount or eligibility, do not threaten public schools' enrollment or influence incentives to provide quality. Second, higher-income households value quality significantly more than lower-income households, suggesting that voucher programs targeting the latter group will affect quality differently than those including both high and low-income households. Third, voucher policies can change competition on two dimensions depending on their design. When these programs target individuals more likely to attend a public school (absent the policy), competition is focused between public and private schools. When they are expanded to include students likely to always attend a private school, competition is focused between private schools. This shift in competition as a voucher program expands creates nonlinearities in schools' incentives to provide quality. Specifically, our results show that public (private) schools only have the incentive to increase (decrease) quality in response to potential changes in enrollment only up to a point. Further expansions reduce (increase) the competition faced by public (private) schools and thus reduce (increase) their incentive to provide quality.

Our paper contributes to the growing economics literature on school choice programs. Many papers specifically examining private school vouchers focus on the direct impact of these policies on the educational outcomes of students offered to participate. One set of papers examines whether participating students experience gains in test scores (Rouse, 1998; Mayer et al., 2002; Howell et al., 2002; Witte et al., 2014; Wolf et al., 2010; Abdulkadiroğlu et al., 2018; Waddington and Berends, 2018) and (Chingos and Peterson, 2015) focuses on the longer-term educational impacts including high school graduation and college enrollment. Our paper complements this prior work by demonstrating that voucher policies implemented at scale affect the educational outcomes of students not participating in the program. Specifically, we show that as ICSP is implemented, both students remaining in the public school system and those continuing in private schools experience changes in school quality. By establishing these indirect effects of ICSP, we can better understand the total effect of voucher policies as they are adopted and expanded[7].

A large body of work evaluates the supply-side responses to school choice programs. A majority of these papers focus on the public school response to the introduction of charter schools (Cohodes and

[7]More broadly, this paper contributes to the literature that calls attention to the limitations of randomized control trials (Lise et al., 2004; Heckman, 1991; Deaton and Cartwright, 2018; Al-Ubaydli et al., 2017). Specifically, our analysis provides an example of how the effects found in a small study may not generalize to the program at scale due to the ability of schools to respond.

Parham, 2021; Imberman, 2011a; Gilraine et al., 2021)[8]. We argue two reasons why understanding voucher programs' specific effects are important. First, current policy discussions often center around the adoption and expansion of voucher policies in particular[9]. Second, our results show that ICSP induces changes in quality for both public and participating private schools, suggesting that the effect of voucher policies may differ from the introduction of charter schools.

The most similar work to ours examines the response of public schools to the voucher programs in Milwaukee, Ohio, and Florida (Hoxby, 2003; Figlio and Rouse, 2006; Chakrabarti, 2008, 2013; Rouse et al., 2013; Chiang, 2009; Greene and Marsh, 2009; Figlio and Hart, 2014)[10]. Overall, these studies investigate the introduction of school voucher programs and find modest positive effects on public school performance. Our context has attractive empirical properties that allow us to abstract from some of the identification issues present within the literature. For example, several papers rely on changes in the degree of private school supply for identification, which may be endogenous to public performance. Other papers identify the effects of voucher programs by leveraging policies that automatically allow students to qualify if their school receives a repeat "F" grade. In those cases, we cannot disentangle the effects of school vouchers from the performance effects of the accountability pressure. Furthermore, we develop a model that both tests the commonly hypothesized mechanism for why schools respond and allows us to extrapolate our reduced-form results to various voucher designs. We believe this has not been done in the previous literature because of extensive data requirements and the need to track students in both public and private schools.

Figlio et al. (2020) also studies the effects of expanding a voucher program by leveraging the growth in Florida Tax Credit Scholarship (FTC) from 2003 through 2018. The authors use variation in the growth of the program and pre-policy levels of local competition to estimate the intensive margin effects of increased competition on public school performance. They find that students in public schools that faced a higher initial level of competitive pressure saw greater gains in test scores as the program matured. We build on their results in several ways, beginning with our identification strategy. Rather than rely on incremental changes in realized voucher enrollment[11], our results are

[8]See Epple et al. (2016) for an excellent review of the effects of charters schools on public school performance.

[9]Since 2021, policymakers from Oklahoma, Nevada, Texas, and Florida have made public announcements supporting the introduction or expansion of voucher policies.

[10]There are several studies examining the specific effect of voucher policies on schools in countries outside of the United States (Hsieh and Urquiola, 2006; Neilson, 2021; Böhlmark and Lindahl, 2015; Muralidharan and Sundararaman, 2015). These papers are similar to ours in that they study programs that serve larger shares of the total student population. However, we might expect different school responses in our context based on differences in baseline private school enrollment and voucher design.

[11]Figlio et al. (2020) uses several measures of growth in their analysis. Their preferred specification relies on the log number of scholarship enrollments.

estimated off legislated changes in eligibility. Understanding the effects based on this dimension may be of particular interest to policymakers as they can set the limits for eligibility and voucher amount and cannot directly control the number of students participating[12]. Furthermore, the estimates from our model show that changes in the voucher design along these dimensions can have important implications for whether and to what extent schools respond. We also are able to examine changes in the quality of participating private schools, which is important because one cannot answer the question of what is the total impact of voucher programs without considering their responses. To the best of our knowledge we are the first to examine changes in private school quality in response to a voucher program within the United States[13].

The rest of this paper is organized as follows. In Section II, we provide background information on the Indiana Choice Scholarship Program. Section III summarizes the data used in this paper and describes our constructed measures of school quality and exposure to the policy. Section IV describes the reduced-form empirical strategy and lays out the regression specifications. Section V contains the main results on public schools, which include our heterogeneity analysis, discussion on student sorting, our validity checks, and a discussion on possible mechanisms. Section VI contains the results for choice schools. Section VII discusses our model, and Section VII offers conclusions from this research.

II The Indiana Choice Scholarship Program

The Indiana Choice Scholarship Program (ICSP) is the most expansive single voucher program in the United States in terms of both participation (36,290 student participants) and eligibility (over 79% of families with children are eligible). Initially, the program capped participation at 5,000 and 7,500 students for the 2011-2012 and 2012-2013 AY, respectively. The expansion of ICSP at the start of the 2013-2014 AY eliminated participation caps. Since the expansion, a student can participate in ICSP if they can meet the income requirements and qualify under one of eight eligibility tracks[14].

Income eligibility for vouchers is based on household size and is set as a percentage of the amount to qualify for the Federal Free or Reduced-Price Lunch (FRPL) Program. Students at or below the

[12] Our results have a slightly different interpretation than those in Figlio et al. (2020). Their results combine the effects of FTC scaling up and maturing over time. Our analysis centers around the first five years after ICSP was expanded, so maturation effects may be less apparent in our context.

[13] Private school responses to voucher programs in the United States is an understudied area. Some papers have studied the effects of these policies on private school enrollment, finances, and school inputs; however, the question of whether and to what extent schools alter their quality is still an open question (Hungerman and Rinz, 2016; Hungerman et al., 2019; Rinz, 2015).

[14] Information on available tracks can be found on the IDOE website (Indiana Department of Education, 2021c).

threshold for FRPL are eligible for a voucher of value up to 90% of per-pupil state funding, while students at or below 300% of the threshold for FRPL are eligible for a voucher of value up to 50% of per-pupil state funding (Indiana Department of Education, 2021b). The actual voucher amount equals the minimum of school tuition and fees or the qualified voucher amount. In the 2020-2021 school year, the average actual voucher amount for students between grades 1-8 was $5,311 for students qualifying for the 90% voucher and $3,094 for students receiving the 50% voucher ($\leq$ 50% of per-pupil public spending) (Indiana Department of Education, 2021a).

For a student to receive a voucher, they must apply and be accepted into a participating choice school. The choice scholarship application is then completed by a parent (or legal guardian) and submitted by the private school. If a student is awarded a voucher, that money goes directly to the school and only an award letter detailing the approved amount of the voucher is given to parents[15]. ICSP vouchers are meant to cover tuition and fees at eligible private schools; however, these schools are allowed to charge additional tuition above the voucher amount so long as they are the same charges non-Choice eligible students pay.

The inclusion of both low- and modest-income families makes ICSP unique. The income eligibility threshold for the 2022-2023 academic year in Indiana is about 1.5 times that of the Florida voucher program (Fla. Stat. § 1002.394), 1.85 times higher than that of the programs in Milwaukee (Wis. Stat. §§ 119.23 and 235), Racine, (Wis. Stat. § 118.60), and Washington D.C. (DC ST § 38-1853), and about 2.2 times higher than the program in New Orleans (La. Rev. Stat. §§ 17:4011 through 4025). This higher income threshold places additional pressure on the public schools of Indiana. Over 79% of public school students qualify for a voucher and participation is not capped at a percentage of public school enrollment as seen in other voucher programs, suggesting that Indiana is a context we might expect to see larger impacts on school quality.

III Data

The data for this project comes from the Indiana Department of Education (IDOE) through a data agreement with the Center of Research on Educational Opportunity (CREO) at the University of Notre Dame. The IDOE-CREO database contains student-level data with information on the membership, test scores, voucher take-up and demographics of all students enrolled in a public, private, or charter school in Indiana[16]. The database covers the 2005-2006 through 2017-2018 AY. We focus on students in schools that serve anyone between the third and eighth grade. Standardized

[15] The distribution of funding to schools rather than households distinguishes ICSP from tax-credit voucher programs or educational savings accounts, which having also become popular over the last 20 years.

[16] We focus on public and private school students in this paper.

testing is consistent between these grades and is required in both public and private schools in order to remain accredited, which allows for a consistent sample across the available years[17].

Depending on whether a student attends a public, private non-choice, or a private choice school (hereon referred to as private and choice schools, respectively) the detail of the demographic information varies. For all students, we have information on race, age, date of birth, free or reduced-price lunch status, section 504 status, zoned school district, and standardized testing accommodations. For students attending either public or private schools, we only have information on whether or not a student would qualify for a 90 percent voucher as it is the same cutoff for free/reduced price lunch. We do not observe whether a student surpasses the cutoff for a 50 percent voucher. We have greater information on students that use a voucher to attend a private school. Specifically, we also have information on these students' home addresses, the tuition they are charged, their voucher status (50 or 90 percent) and the actual amount of the voucher they receive.

We also have access to school directories that outline basic information about the schools in Indiana. This includes data on the opening and closing (if applicable) dates, addresses, school type (public, private, or charter), and lowest/highest grades offered. We construct school-level test scores and demographic information by aggregating individual-level data from students attending each school. Schools must have non-missing test score data for each of the academic years between 2005 and 2017 to be included in the sample. After this restriction, 1,279 public elementary and middle schools and 178 choice schools remain[18].

We create two other school-level measures for our analysis: School value-added, which is used as our proxy of school quality, and our measure of high exposure to the policy, which is used to distinguish schools in our treatment and the control groups. The following sections explain how those measures were created.

III.A School Value-Added Estimates

School value added (VA) is a measure of a school's contribution in a given year to students' test scores. We use it as our proxy for school quality with the assumption that this measure captures

[17] This data set is unique because it includes information on students that attend private schools. This is made possible due to Indiana's accreditation process. For a private school to receive and maintain accreditation, it must administer the ISTEP+ exam to students at the same time that school corporations administer the test and make available to the Indiana Department of Education the results (Indiana Code §20-32-5-17). Furthermore, if a school wants to participate in the Indiana Athletic Association they must have accreditation from the Indiana Department of Education (Indiana Athletic Association, 2021)

[18] This restriction necessarily means the set of schools in our sample are positively selected. We discuss entry and exit into the educational market in Appendix Section B1

how much a school increases students' achievement, controlling for all other relevant variables. This measure of school quality is meant to capture schools' inputs such as teacher quality, infrastructure, school environment and any other school-specific characteristic that improves student achievement, measured as the average test score.

To calculate school VA we run the following OLS regression[19]:

$$testscore_{ist} = \alpha + \gamma_g testscore_{ist-1} + \lambda_g testscore^2_{ist-1} + \mathbf{X}'_i\delta + \beta_{st} + \epsilon_{ist} \tag{1}$$

where $testscore_{ist}$ is the test score for a student i, in school s at year t. Students in the third through eighth grade take both a Math and an English Language Arts exam each year; thus, we have school VA estimates for each subject as well as for the average of both scores. These scores are standardized within grade and year so that estimates can be interpreted as standard deviations. $testscore_{ist-1}$ is the student's test score from the previous academic year and is constructed in the same matter as $testscore_{ist}$. In this specification, we cannot include third graders as they do not have a previous test score. γ_g and λ_g are grade-specific coefficients on lagged test scores and lagged test scores squared. $\mathbf{X}_i$ contains several indicators for student demographics including female, black, hispanic, asian, two or more races, subsidized lunch, special education, section 504 and testing accommodations. Our school value-added measure comes from the school-year fixed effects, β_{st}. The choice of the specification is motivated by that used in Chetty et al. (2014) to measure teacher value-added. Like Chetty et al. (2014), we control for grade-specific effects of lagged test scores to account for selection into particular schools. We also show in Appendix Table A1 that our results are robust to the use of an empirical bayes shrinkage procedure in our value-added estimations (Kane and Staiger, 2008).

Figure 1 depicts the density plots of our school value-added estimates for both the public and choice schools in our sample. Panel A shows the different distributions in the years before the policy was implemented, while Panel B plots our estimates in the years after expansion. For each panel, we report the p-value for the Kolmogorov-Smirnov equality-of-distributions test. In the years before the policy it is visually apparent that choice schools outperform public schools and this is confirmed with Kolmogorov-Smirnov test. After expansion, we cannot statistically distinguish between the distribution of value-added for public and choice schools. The following sections of this paper will separately analyze the changes in public and choice school quality.

[19] In Appendix Table A2, we show that our results are robust to different specifications of this regression. Specifically, we re-run our difference-in-differences where school value-added is estimated using Equation (1) without any demographic controls or prior test scores (Column 2), only including demographic characteristics (Column 3), and including demographic characteristics and linearly controlling for prior test scores (Column 4).

III.B Construction of Exposure Measure

Our main measure for each schools' exposure to the voucher policy relies on the radial distances between the physical addresses of each of the public schools in the sample and all of the eventual choice schools in Indiana. A public school is said to face high exposure to the voucher policy if the nearest eventual choice school is within five miles of its location[20]. We find that around half of the public schools in the sample have at least one nearby choice school[21]. Public schools where the nearest choice competitor is outside the five mile radius are then used as our control group. Nearly all choice schools (over 98 percent) are located within five miles of a public school; therefore we distinguish between high-exposure and control choice schools by their placement in the distribution of the number of public schools within five miles. High-exposure choice schools are those in the top tercile of this distribution with the control group then making up the bottom two-thirds.

Table 1 reports summary statistics for the high exposure and control public schools in the academic year before the policy intervention. Column (1) presents the sample means of the variables for high exposure schools, Column (2) presents those same means for the schools in the control group, and Column (3) presents the results of a t-test for the difference between the two groups. High exposure schools are different from those in the control group on several dimensions. High exposure public schools were larger, with an average of 262 students taking the state exam versus an average of 217 in control schools, had a smaller share of their students identified as White, 65 percent versus 91 percent, had a larger share of students identified as Black, 16 percent versus 2 percent, and had a larger share of students qualify for subsidized lunches, 55 percent versus 42 percent[22].

These differences in demographics, however, do not translate to significant differences in our outcome measures of interest. High exposure public schools had an average overall school value-added estimate of 0.021 in the 2010-2011 academic year versus an average of 0.018 for the schools in the control group. In that same year, high exposure schools had an average school math value-added estimate of 0.025 and an average school reading value-added estimate of 0.009. Schools in the control group had an average of 0.026 and -0.002 in their school math and reading value-added estimates, respectively. We find a similar pattern in the comparison between high-exposure and control choice schools, which are found in Table 2. Importantly, our empirical strategy does not rely on the equality

[20]The results are robust to this definition of having a competitor. Appendix Table A3 illustrates our results using 3, 5, 8, 10 and 15 miles as the required distance.

[21]Appendix Figure A3 shows the distribution of the distance between each public school in our sample and their nearest choice school

[22]The differences in the demographic make-up of the two groups of schools are at least partly explained by their locations within the state. Appendix Figure A2 shows the location of each public school in the sample. Public schools with a nearby choice competitor are often located in the most populous and urban counties in Indiana, while those in the control group are spread out across the more rural parts of the state.

of the pre-policy summary statistics. Instead, identification requires that the change in outcomes for the control group are what those facing high exposure would have experienced had the policy not been put in place. We discuss this assumption in further detail in later sections.

IV Reduced-Form Empirical Strategy

To estimate the effects of introducing (and expanding) private school vouchers in Indiana we use a difference-in-differences model that relies on plausibly exogenous variation in a school's exposure to the voucher policy. We will compare the change in school value-added in the years before and after the implementation of the policy in schools facing high exposure to the policy versus those in the control group. The underlying assumption in this strategy requires that, in expectation, the change in outcomes for the schools in the control group are what those schools facing high exposure would have experienced had the voucher policy not been put in place. While this assumption is ultimately untestable, we address this concern by reporting the results of an event-study specification that allows the effect of voucher program to vary by years since implementation.

We implement this difference-in-differences (DID) strategy using the following regression:

$$VA_{st} = \beta_1 Post_t \cdot HighExposure_s + \sum_{t=2007}^{2018} \Psi_t(\mathbb{1}\{year = t\} * X_s^{2007}) + \alpha_s + \gamma_t + \epsilon_{st} \tag{2}$$

where VA_{st} is our constructed measure of value-added in school s at year t, $Post_t$ is an indicator that equals one in the years after the voucher policy was introduced, $HighExposure_s$ is an indicator that equals one if the public school is identified as having a nearby choice school, α_s is a school fixed effect that removes any time-invariant characteristics about the school that could otherwise bias our results, γ_t is a standard year fixed effect and ϵ_{st} is our idiosyncratic error term. Ψ_t captures the potentially time-varying effects of X_s^{2007}, a vector of initial school-level characteristics[23]. The parameter β_1 is the coefficient of interest and captures the average difference between the high exposure and control schools in the years after adoption of the voucher policy relative to the years before. All standard errors allow for arbitrary correlation in errors at the school level[24].

We visually test the validity of the common trends assumption by presenting a set of event-study

[23]In Appendix Table A4 and Appendix Table A5 we show our results are robust to the exclusion of baseline covariates and the use of a continuous measure of the number of nearby choice schools, respectively.

[24]One may be concerned that our standard errors are incorrect in this specification as we are using an estimated variable as our outcome variable of interest. To address this issue we perform a bootstrapping procedure as described in Appendix Section C1. We find that our estimates are more precise under this procedure most likely due to the fact that clustering at the school level significantly increases our standard errors. We, therefore, continue with our preferred specification.

results that allow the effect of adopting a voucher policy to vary by years since implementation. Specifically, we run the following regression:

$$\begin{aligned} Y_{st} = \sum_{l=-5, l\neq -1}^{6} \theta_l HighExp_s \cdot \mathbb{1}\{t - 2012 = l\} + \sum_{t=2007}^{2018} \eta_t (\mathbb{1}\{year = t\} * X_s^{2007}) \\ + \pi_s + \lambda_t + \mu_{st} \end{aligned} \tag{3}$$

where l represents the lag or lead of interest and 2012 is the year of adoption. Since we omit the year before the adoption of the policy, each θ_l captures the effect of being a school facing high exposure relative to the year before the introduction of the voucher program.

Our estimation strategy bypasses the concerns present in the current difference-in-differences literature because (1) we do not exploit variation across groups treated at different times (Goodman-Bacon, 2021), (2) our main specification relies on a binary measure of treatment (Callaway et al., 2021) and (3) we refrain from the use of time-varying covariates in any of our analyses (Caetano et al., 2022). Furthermore, adding school-level, time-varying characteristics may be inappropriate in this context. Characteristics such as the share of students eligible for subsidized lunches may change in the post-period as a direct result of the policy; hence their inclusion in our specifications would bias our results.

V Effects of ICSP on Public School Quality

We begin by describing the estimated effects of the Indiana voucher program on public schools with a nearby choice competitor. Figure 2 depicts the density plots of our school VA estimates for the public schools in our sample across two periods: Pre-2011, and Post-2013 to align with the policy time horizon. Panel A shows the kernel density plots for the high-exposure public schools and Panel B plots the data for the public schools in our control group. It is visually apparent, for schools facing high exposure, that the distribution of school value-added after voucher adoption is to the right of the distribution before the policy was implemented. For schools in the control group, the distributions are statistically indistinguishable[25]. The p-values for the Kolmogorov-Smirnov equality-of-distributions test confirm this finding. While not a formal difference-in-difference design, Figure 2 provides an instructive graphical preview of our findings.

The results of our main analysis are reported in Table 3. Each cell in the first row of the table represents the coefficient on the $Post_t \cdot HighExp_s$ interaction for separate regressions. In the second

[25] We support this claim by running the difference-in-differences specification on the set of control schools (arbitrarily identifying high exposure as a choice school within 8 miles of its location) and find no changes in school quality. The results are shown in Appendix Table A8.

row, we have included an interaction term to indicate whether or not a school with a nearby choice competitor also had an above median share of its students qualifying for subsidized lunches in the year before the voucher program was introduced.[26] Each column shows the results for an individual outcome of interest. Columns (1) and (2), present the results on overall School VA, while columns (3) and (4) present the results on School Math VA, and columns (5) and (6) present the results on School Reading VA.

Schools with a choice competitor within five miles saw an overall increase in their School VA by 0.023 of a standard deviation in the post-policy period. The estimates in column (2) show that this result is driven by schools having a nearby competitor and an above-median share of students qualifying for subsidized lunch in the year before voucher adoption. Specifically, this set of schools saw an increase in overall School VA of 0.039 (0.030 + 0.009) of a standard deviation following voucher implementation. When we look at the results for math and reading separately, we find that a similar pattern holds. On average, schools with a nearby choice competitor saw an increase in their School Math VA by 0.03 s.d. and an increase in their School Reading VA by 0.013 s.d. in the post-policy period. The inclusion of the interaction terms in columns (4) and (6) shows us that schools with a high share of students who qualify for subsidized lunch saw even larger increases; 0.047 of a standard deviation in School Math VA and 0.028 in School Reading VA[27].

The result that the voucher program induced an increase in school quality experienced by public school students is significant. Increased schooling quality is associated with better educational outcomes including increases in the likelihood of college attainment (Deming et al., 2014) and increases in the likelihood of attending a college with a larger share of STEM degrees (Shi, 2020). Therefore, our results not only suggest that voucher programs at scale can induce responses by schools, but they can do so in such a way that meaningfully changes the educational outcomes of students not participating in the program.

Our findings also complement the results found in Waddington and Berends (2018) that explore the effect of ICSP on the students that use the voucher. The authors use a matched difference-in-differences design to compare students that used a voucher to those that qualified and remained in public schools. They find that voucher students see significant declines in math scores and no changes in reading scores following the switch to a choice school. While the authors do not speculate on the mechanisms that could explain their results, our estimates suggest that the improvements in

[26]This interaction term isolates the impact of the voucher program on the set of schools facing the highest threat of competition. They are located near at least one choice school and have a high share of students that would actually qualify for the voucher.

[27]We also show that our results are stronger when we eliminate public schools that have a choice school within 3-8 miles of their location (Appendix Table A9).

public school quality, particularly in math, can at least partially explain the declines they report.

As discussed in the previous section, ICSP was adopted and expanded in two separate academic years (2011-2012 and 2013-2014, respectively). One might then wonder if the two events could have had differential impacts on public school quality. We answer this question using our event-study specification. The results of Equation (3) allow us to look at the effect (relative to the year before adoption) of facing choice school competition in each year of the sample rather than averaging across the entire post-policy period. We can then compare the results at the year of expansion to that of the year of adoption to get a sense of which event is driving the results. Figure 3 plots the results of Equation (3) for each school quality measure of interest. Years 0 and 2 indicate the years of adoption and expansion, respectively. There is a small and statistically significant jump in School Math VA in the year of adoption of ICSP; however, the effects are largest across our measures of interest in the year after ICSP's expansion. Interestingly, these results suggest that despite facing the threat of losing students as the program is adopted, we do not see schools respond until a much larger percentage of the student body qualifies to participate. This finding suggests that we may not expect voucher programs to have these indirect effects on educational outcomes until these programs are brought to scale.

While these estimates are modest in magnitude, they are statistically significant and indicate a positive relationship between the threat of choice school competition and public school quality. We cannot make exact comparisons between our results and that of the current literature as we are analyzing School VA rather than pure student test scores; however, our results are similar to the aggregated school-by-year estimates shown in Figlio and Hart (2014). We have also estimated models at the student-school-year level and continue to see positive and statistically significant results on the effect of the threat of choice school competition on public school performance. These models are presented in Appendix Table A6 and show that our results are similar in size to those found in the first few years after the Florida voucher program was adopted (Figlio et al., 2020).

V.A Heterogeneity by School Attributes

We have found consistent evidence of modest improvements in School VA when comparing public schools facing the threat of choice school competition to those in the control group. However, these average estimates across all public schools facing competition may either differ or be consistent across various subgroups. Therefore, we disaggregated the results by the following baseline characteristics; enrollment, overall School VA and median income of the census block group where the school is located. We calculated these estimates by introducing interactions of the school subgroup with the

$Post_t \cdot HighExp_s$ indicator in Equation (2)[28].

Table 8 displays the results of our heterogeneity analysis by school subgroup for overall School VA, School Math VA, and School Reading VA, respectively. Panel A displays the differences in outcomes for public schools above and below median enrollment for the 2006-2007 academic year. Across all of the columns, the estimate on the interaction term with above median baseline enrollment is statistically insignificant. This result implies that public schools see similar improvements in quality when facing the potential threat of competition despite having relatively small or large baseline enrollment.

In Panel B, we examine the differences in outcomes for public schools with above and below median overall School Value-Added for the 2006-2007 AY. Across all outcome variables of interest, the estimate on the interaction term with above-median baseline School VA is negative, statistically significant, and almost equal in magnitude to the overall estimate on the $Post_t \cdot HighExp_s$ indicator. These findings implies that the changes we see in school quality are driven by the schools who face potential competition and were originally low-performing. In fact, high exposure schools with above-median baseline school value-added see small or no changes in the outcomes of interest when compared to the control group. The increase in school quality for low-performing schools coupled with the null results for high-performing schools suggests that the gap in public school quality is closing as a result of the program[29].

Panel C reports the effects on quality by the income of the census block group where the public school is located. This specification allows us to capture any differences in the results between public schools located in relatively rich and poor neighborhoods. Similar to the results in panel A, the estimate on the interaction term with above-median neighborhood income is statistically insignificant across all of our outcomes of interest. These findings imply that schools see similar improvements in quality when facing the potential threat of competition despite being located in a relatively poor or rich neighborhood.

We also explore possible heterogeneity by financial incentive. As shown in Figlio and Hart (2014) not all public schools face the same incentives to respond to the implementation of a voucher program. Specifically, public schools on the margin of receiving federal Title I aid may experience a larger reduction in resources as a consequence of losing students to private schools. We, therefore,

[28]We have also considered heterogeneity by initial levels of suspension/expulsions. This analysis addresses a different type of threat public schools could face. Specifically, families may have a desire to leave public schools that they deem unsafe. We do not find any differential effects for public schools that had an above median percentage of their students ever being suspended or expelled. Results are available upon request.

[29]One may be concerned that these results are driven by families wishing to leave low performing public schools; however, as shown in Appendix Figures A6 and A7 there does not seem to be differential student sorting on ability across these two types of public schools when comparing either FRPL (A7) or non-FRPL students (A7)

explore whether high-exposure public schools with Title I funding drive our results. Panel A of Appendix Table A7 reports the differences in outcomes for public schools with and without a Title I program in year before ICSP was adopted. Panel B of Appendix Table A7 includes an interaction term that allows us to identify the differential impact of ICSP on high-exposure public schools that just qualified for Title I funding[30]. Overall, we do not find evidence that public schools facing greater financial pressure respond more to the program.

V.B Potential Mechanisms

V.B.1 Changes in School Inputs

Given the improvements in pubic school quality, we next examine changes in schools inputs that might lead to increases in school quality. In particular, we combine information from the Common Core of Data on Indiana public schools from the National Center of Education Statistics with available teacher data in the IDOE-CREO database to explore changes in student-teacher ratios, the number of teachers with a high-quality (HQ) certification[31], number of teachers with a graduate degree and teachers' average years of experience. Unfortunately, the information on teachers is only available from the 2010-2011 through 2017-2018 academic years, which limits our sample to include only one year of pre-policy data.

Figure 5 separately plots the average of each of these school inputs across the available years of data for high-exposure and control public schools. We do not find strong evidence that high-exposure public schools saw meaningful changes in student-teacher ratios or the average years of experience of their teachers when compared to the control group. However, Panel B shows that while both high exposure and control public schools added around 2 additional HQ certified teachers (either through hiring or certification) in the year ICSP was adopted, control public schools did not retain them. By the end of the sample period, control public schools had returned to their initial levels of HQ certified teachers. Furthermore, Panel C, shows that while both high-exposure and control public schools see declines in the average number of teachers with a graduate degree, control public schools witness faster declines over the sample period.

We confirm these patterns in the data with the results from our difference-in-differences specifi-

[30] Title I funding is allocated based on where a school ranks within their districts' with respect to the share of low-income students they serve. In Indiana, schools that meet or exceed the district's poverty average are eligible to receiving funding. We define "just qualifying" for Title I as being within 5 percentage points above that cutoff for eligibility.

[31] High-Quality certification is determined by standards set by No Child Left Behind. States are allowed to add their own requirements. For the state of Indiana, HQ certification requires passing an additional exam to indicate proficiency in a certain subject.

cation. In Table 4 we report the results of Equation (3) using school inputs as our outcome measures of interest. We find that relative to the year before ICSP was adopted, high-exposure public schools saw increases of around 0.7 teachers with a graduate degree and 1.5 teachers with a HQ certification when compared to the control group[32]. These changes in average teacher characteristics are significant. While the previous literature on the effects of advanced degrees on student outcomes is mixed, recent work shows that subject specific teacher credentials (such as a high-quality certification) are associated with stronger student achievement (Strøm and Falch, 2020).

We also examine the impact of ICSP on students' non-cognitive skill formation in public schools. Table 5 reports the results of Equation (3) where the outcomes of interest are school-level measures of attendance and disciplinary infractions. These two measures have been cited as important indicators for changes in behavior (Imberman, 2011b). After the implementation of ICSP, public schools facing the threat of choice school competition saw increases in attendance and decreases in suspensions/expulsions. Specifically, high exposure public schools saw increases in attendance of 0.3 p.p, or about half a day, compared to those in the control group. The estimate in column (3) suggests that high exposure public schools also saw a reduction of 0.5 p.p in expulsions and suspensions, with the caveat that this estimate is statistically insignificant. Attendance is cited as important determinant of student outcomes including test scores (Goodman, 2014; Fitzpatrick et al., 2011; Gottfried, 2009) and high school graduation (Liu et al., 2021). Using the estimates in Goodman (2014), we can do a back-of-the-envelope calculation that reveals that the increase in attendance by half a day, induced by ICSP, can translate into around a 0.025 s.d. deviation increase in test scores[33]. Overall, we take these results as evidence that in response ICSP, schools are increasing quality such that we see improvements on both the cognitive and non-cognitive dimensions.

V.B.2 Changes in School Financial Resources

ICSP could further have a direct effect on public schools' ability to improve school quality through changes in financial resources. Opponents of school choice policies argue that these programs drain public school finances through direct cuts in state funding (Strauss, 2017). Moreover, losing students eligible for subsidized lunches could result in further resource reductions if schools rely on Title I funding. By contrast, per-pupil revenue may increase in public schools if total federal and local

[32]Our results differ from those in Figlio and Hart (2014). The authors find that schools faced with greater competition shifted their teacher forces to include less qualified teachers. Unfortunately, we lack availability of detailed data on school practices to fully disentangle different school responses under each of these reforms.

[33]One does need to keep in mind that the estimate from Goodman (2014) has a very specific interpretation, as it is identified off of missed classes due to snowfall, that may not translate to our context.

funding remain unchanged[34]. If the latter is the case in Indiana, increases in available school funds could contribute to our results[35].

However, school funding in Indiana heavily relies on state rather than local sources. The state currently ranks 40$^{\text{th}}$ in the percent of public school funding coming from local revenues (just below 30%) (U.S Census Bureau, 2021). Furthermore, the state has provided 100 percent of funds available to support education-related operating costs since 2009. Local funds are used to support other expenses including transportation, capital projects, and debt services (Chu, 2019). This reliance on state-funding suggests that Indiana public schools are susceptible to reductions in revenues as students use the voucher. Anecdotal evidence from statements made by public school boards echo this concern (Gore et al., 2011). Unfortunately, school-level finance data is not available for a majority of our sample period; therefore, we cannot formally test whether changes school funding can explain our results. Future research will explore this question at a greater length.

V.C Student Sorting

The results from the previous section suggest that ICSP implementation improved public school quality; however, it is necessary to distinguish between whether the results we find are due to actual changes in efficiency or are driven by the composition of students that remain in the public schools. In this section, we present several pieces of evidence to suggest that the sorting of students, while apparent, cannot explain all of the gains in School Value-Added we report.

We first investigate this issue by documenting any changes in the demographic composition of students in high-exposure public schools after the implementation of the program. Table 6 reports the results of Equation (3) where the outcomes of interest are school-level measures of demographic variables (Share Female, Share White, Share Black, etc.). After the implementation of ICSP, public schools facing the threat of choice school competition saw statistically insignificant changes of -0.19 p.p in the share of students that are female, 0.27 p.p in the share of students that are Black, and 0.38 p.p in the share of students qualifying for subsidized lunch when compared to the control group. However, as shown in columns (2) and (4), high exposure public schools saw a statistically significant decrease of -2.72 p.p in the share of White students and an increase of 2.27 p.p in the share of Hispanic students.

[34] DeAngelis and Trivitt (2016) show that if Louisiana Scholarship Program was eliminated only 2 to 7 out of 69 school districts would see an increase in financial resources.

[35] There still remains some debate on whether increases in school spending improve educational outcomes (Jackson, 2020). One direct way increased school per-pupil expenditure could directly influence our results is if schools used the extra funds to hire or convert high quality teachers. This is left as an open question as we do not have the data to test this theory.

We next address the concern of student sorting on ability. Figure 4 shows the density plots of standardized test scores for students who eventually use a voucher and those students who remain in the public school system despite qualifying to participate in the program. Specifically, the figure plots the standardized test scores in the years before the program was adopted. We find that eventual voucher students slightly outperformed those remaining at the public schools. This finding suggests that ICSP did induce some "cream-skimming", which has been a major criticism of voucher policies. However, this type of sorting on ability works against the theory that the students leaving the public school system would artificially increase average test scores[36].

Overall, we take these results as evidence that the demographics of students are changing with the implementation of the voucher program. To understand to what extent these changes in demographics drive our results we perform an exercise with predicted school value-added. Specifically, we begin by estimating the following model:

$$VA_{s,2007} = \sigma X_s^{2007} + \epsilon_s \tag{4}$$

where $VA_{s,2007}$ is our estimated school value-added in 2007 (our "base" year) and X_s^{2007} includes all of the school characteristics we observe and their pairwise interactions in that same year. We use the coefficients from this fully interacted model to predict value-added for each school in all years of the sample. We then use these predicted value-added measure to run the following difference-in-differences specification:

$$V\hat{A}_{st} = \beta_1 Post_t \cdot HighExposure_s + \alpha_s + \gamma_t + \epsilon_{st} \tag{5}$$

If changes in observable school characteristics are driving our school quality results, we would expect differential changes in the predicted value-added measures following the implementation of the voucher policy. Table 7 reports the results of this exercise. We find no evidence that high exposure public schools were predicted to improve their quality based off the change in composition of their students. In fact, we find that based solely on changes in observable characteristics, high exposure public schools were predicted to see declines in overall and math value-added. We take this a strong evidence that it is changes in school efficiency that drive the improvements in quality we see. We also recognize that this exercise can only speak to how changes in observable school characteristics

[36]We do not have information on whether students remaining in the public school qualify for a 50% voucher; hence the comparison made in Figure 4 also compares 50% voucher students to FRPL students. Appendix Figure A5 shows the direct comparisons of eventual choice students versus those remaining in the public schools system for both FRPL and non-FRPL groups in Panels A and B respectively. We find almost no sorting on ability when comparing FRPL students and a slight negative selection for non-FRPL students. However one must consider that the non-FRPL comparisons also include high-income students that do not qualify for a voucher.

may have affected our school quality results. There still remains the concern of whether non-random student sorting on unobservable characteristics drive our results.

We can mitigate some concerns of non-random sorting on unobservable characteristics by highlighting the strength of our value-added estimation strategy. In Equation 1, we control for lagged test-scores. Assuming that prior test scores fully proxy for those inputs that affect a student's achievement prior to using the voucher and those inputs correlated with a student's likelihood of using a voucher, we address the concerns for this type of sorting. This is a strong assumption, however it is standard in the school value-added literature.

V.D Threats to Validity

The previous section shows that ICSP implementation is associated with increased school value-added estimates for public schools with a nearby choice school. There remain, however, several potential threats to validity that should be addressed. Specifically, (1) the impact of the voucher policy on high exposure public schools may be driven by differential trends in school value-added across the two groups of schools before program implementation, (2) the results may be sensitive to the exclusion of particular districts that house a large proportion of the students in the state, and (3) there are other policy innovations besides the voucher program that may be driving the results.

To ensure that the findings are not driven by differential trends between the schools facing high exposure to the voucher policy and the control group, Figure 3 plots the event-study results of Equation (3) for each school quality measure of interest. This gives a sense of when school VA patterns changed and if preexisting trends are driving the results. The coefficients are plotted with 95 percent confidence intervals; the omitted category is the schools in the year prior to the program implementation. The expansion of the voucher program is highlighted at Year 2, which corresponds to the 2013-2014 academic year. Prior to implementation, high exposure public schools and the control group appear to have similar trends in school value-added shown by the relatively flat differences between the two groups[37]. In all years before implementation, the 95 percent confidence interval contains zero, which means that in those years, the difference between high exposure and control groups cannot be distinguished from the value in the year before implementation[38].

[37] We further show the robustness of our results using placebo treatment years. Appendix Figure A4 shows the results when we assign the adoption of ICSP to be two years prior to the actual. The figure shows that school quality only improved following the years of actual adoption and expansions (As indicated by the red and blue dashed lines, respectively).

[38] Appendix Figure A4 shows the results of our event-study specification only including those public schools that had an above median share of FRPL students in 2010. We include this specification because this is the group of public schools that drive our main results in Table 3.

The second concern is that the results are sensitive to the exclusion of particular school districts. We, therefore, estimate the main analysis in Table 3 excluding Marion county, home of Indianapolis, as it is the largest county in the state. We find consistent evidence that, regardless of dropping Marion County, the signs and general significance levels of the interaction term of interest are maintained as shown in Appendix Table A10. Appendix Table A11 shows that when we drop any of the 92 Indiana counties, our results remain similar to the full state-analysis. Therefore, it is difficult to believe that some combination of specific counties are driving the general direction of our results.

There is also the concern that other policy interventions beyond the voucher program are driving the results. To address this issue we use year fixed effects in each of our specifications to capture shocks common to both the treatment and control groups. Unaccounted for shocks could still exist, but those shocks would have had to elicit disproportionate reactions from schools with a nearby choice competitor to account for our results. A particular concern is that in 2011 the implementation of the Teacher Evaluations and Licensing Act and the introduction of Indiana's A-F school grading system may have had an effect on school quality. However, since the quality of schools in the high exposure and control groups were statistically indistinguishable in 2010, it is unlikely that either of these reforms differentially impacted the two sets of schools. Moreover, it is not clear if schools felt increased pressure to improve quality as a result of these accountability programs. Prior to the adoption of these specific measures, schools and teachers were held to other accountability metrics. Furthermore, in the 2013-2014 academic year less than 0.5 percent of teachers were cited as "ineffective" and only 4 percent of public elementary and middle schools were given an "F" grade (Indiana Department of Education, 2014a,b).

VI Effects of ICSP on Choice School Quality

Our results thus far have been centered on public schools' responses to the implementation of ICSP. We next assess whether participating private schools also saw changes in school quality as a result of the program. This investigation is necessarily more speculative than our analysis of public schools due to data constraints.[39] However, in this section we present evidence that choice schools are reducing quality after the adoption of ICSP.

We first investigate choice schools' response to the adoption of the voucher program by plotting the averages of our school value-added measures for each year in the sample. Figure 6 plots these averages for our measures of school quality from 2007 through 2018. In the first year of the program,

[39] Specifically, we are unable to compare choice schools to non-choice private schools since non-choice private schools often do not use the ISTEP+ exam, and we are unable to leverage variation in when a choice school starts accepting voucher students as a large percentage adopt in the first year of the program.

there is an immediate drop in average quality on all dimensions. This drop is most apparent for Math Value-Added, but by the following year, the average Reading Value-Added for choice schools saw a similar decline. These school quality measures, while steadily increasing after 2013, remain below the pre-period levels until 2016 for Reading and throughout the sample period for Math. While we do not assert any causal claims from this figure, it does suggest that choice schools saw a decline in quality following the implementation of the program.

Ideally, we would be able to examine choice schools' responses to the implementation of ICSP by comparing them to the set of private schools that never accepted voucher students. Unfortunately, we do not have data on a large percentage of non-choice private schools. We can, however, compare choice schools that pull students from a large pool of public schools to those with fewer public schools in the area. We then learn whether choice schools responded differently to the voucher program based on the potential number of students they could receive[40]. High exposure is now defined as being in the top tercile of the distribution of the number of public schools within a five-mile radius.

Figure 7 shows the density plots of our school VA estimates for these groups of choice schools across two time periods: Pre-2011 and Post-2013 to align with the program's adoption and expansion. Panel A shows the kernel density plots for high exposure choice schools and Panel B plots the data for those choice schools in the control group. Both groups witness a leftward shift in the distribution of overall school value-added following the expansion of ICSP, suggesting that ICSP may not have elicited differential responses across our measure of exposure. Table 9 formalizes this comparison using our difference-in-differences specification (similar to Equation (3)). Column (1) presents the estimates on overall School Value-Added, Column (2) presents the estimates on School Math Value-Added and Column (3) presents the estimates on School Reading Value-Added. After the implementation of ICSP, treated choice schools saw statistically insignificant decreases of around 0.01 s.d. across each of our measures of school quality when compared to control group. This exercise ultimately cannot explain the large drops in school quality seen in Figure 6, but suggest that choice schools with a larger pool of students to pull from saw larger drops in school quality.

To understand what is driving the declines in quality we find, we use data from the Private School Universe Survey to examine changes in choice school inputs. Specifically, we have information on the number teachers, student-teacher ratios and the time spent in school (in hours) every other year from 2006 until 2018. Figure 8 plots the averages of these inputs separately for high exposure and control choice schools. We find that following the adoption of ICSP, there is evidence that high

[40]We also show results in Appendix Table A12 that alter the definition of high exposure for choice schools. Rather than distinguishing between treatment and control based on the distribution of the number of public schools within five miles, we split choice schools by the percentage of public school students that would qualify for the voucher in the schools within five miles of their location. We find similar results under this specification

exposure choice schools experienced an increase in their student-teacher ratios. Panel C shows that while both high exposure and control choice schools increased their average in instructional time, control choice schools saw a more significant rise. We confirm these findings with the results from our difference-in-differences specification shown in Table 10. We find that following the adoption of ICSP, high exposure choice schools saw a statistically significant increase of 0.93 in their student-teacher ratio (off a base mean of 14.24) compared to the control group[41] with the results on the number of teachers and instructional time being statistically insignificant. We, therefore, conclude that once we include baseline controls, the differences in these inputs across high exposure and control choice schools are no longer apparent.

Evidence from Project STAR reveals that changes in student-teacher ratios can have a significant impacts on student outcomes including test scores (Krueger, 1999), high school graduation (Finn et al., 2005), college entrance exam taking (Krueger and Whitmore, 2001), college matriculation (Chetty et al., 2011), criminal activity and teen birth rates (Schanzenbach, 2006). Therefore, our result that students in high-exposure choice schools experience increases in their class sizes further shows that voucher programs at scale can have important impacts on the educational outcomes of students that do not participate in the program.

VII Quantifying the Incentives to Change Quality

Our results so far demonstrate that public schools responded to the implementation of the voucher policy by increasing school quality, while participating private schools reduced quality. However, there remains the question of why schools would alter their quality in response to the voucher program. Prior work argues that potential changes in enrollment or market share serve as a main mechanism (Epple et al., 2017). In this section, we test this commonly hypothesized mechanism and estimate its relationship to changes in schools' incentive to provide quality.

Specifically, we estimate a structural model of household demand for schools with a partial model of how schools choose quality and tuition (public schools do not choose tuition). Our model of demand incorporates rich amounts of heterogeneity in how households trade-off tuition, quality, and distance which allows us to fit flexible substitution patterns from the data. Our model of school incentives (particularly for private schools) is similar in spirit to (Neilson, 2021). In estimation, we exploit variation in the program's eligibility, voucher amounts, and spatial distribution of students

[41] Our results are similar in magnitude (around a 7% increase versus 9% from the authors results) to those found in Rinz (2015) that examines changes in private school inputs following the adoption of voucher programs throughout the 2000s. His analysis includes both traditional voucher programs and large scale tax credit programs, which shows that these two variations of voucher programs may have similar impacts on private school responses.

and schools for identification. A full outline of the model specification and estimation are available in Appendix Sections D1 and E1. We quantify the potential changes in enrollment (market share) for both public and private schools by using our demand estimates to simulate how household choices would change under counterfactual voucher scenarios holding all else constant. We alter the design of the voucher program along two dimensions. First, we change the maximum voucher amount currently eligible students are able to receive. Second, we keep the voucher amount constant while changing the income threshold for eligibility. For high-income households, eligibility amounts to receiving a 50% voucher[42]. As we alter the program's design we estimate the average number of students attending public and choice schools holding fixed other variables in the market including school quality, tuition and distance. We then interpret the changes in market share as the potential loss (or gain) faced by the schools absent any other changes.

We then map this enrollment threat into changes in incentives to provide quality by modeling public and private schools. Importantly, we do not take a direct stance on the objective function of public schools. Instead, we rely on changes in the quality elasticity of demand, while we cannot directly interpret the magnitudes when examining changes, understanding its direction provides a clear picture of how incentives change for public schools regardless of their specific underlying objective function. For choice schools, we are able to quantify changes in their market power by estimating their ability to mark down quality (value-added) below competitive levels.[43]

Counterfactual I: Changing Voucher Amounts

Figure 9 shows how average enrollment at public and private schools changes as we vary the voucher amount from 0% to 200% of the current number. For each of our 15 scenarios, we alter the voucher amount based on a multiplier. For example, a multiplier of zero suggests there is no voucher program, and a multiplier of two suggests students are eligible to up to double the current voucher amount. These counterfactual amounts range from \$0 to $\sim$ \$6,000 and $\sim$ \$10,000 for students eligible for the 50% and 90% vouchers[44], respectively[45]. Importantly, we do not make changes to the eligibility thresholds under this counterfactual.

We find that public schools face a large threat in market share (enrollment) once the voucher

[42] Each of these counterfactual voucher schedules are motivated by current proposals of expansions of ICSP from the Indiana State Legislature.

[43] As we outline in Appendix Sections D1 and E1, the measures we propose to estimate the incentive to provide quality capture how sensitive demand is to changes in quality. The quality markdown, however, is more precisely related to the inverse quality elasticity of demand.

[44] These amounts are based on funding for the 2017-2018 AY

[45] No matter how the size of the voucher changes, students always receive the lesser of tuition and the maximum voucher amount.

amount is large enough to induce households to switch to private schooling. Specifically, Figure 9a shows that the average public school would not see any changes in market share until the voucher reaches 60% of the current amount. As the voucher amount further expands, we begin to see a significant threat to enrollment with the average public school seeing a near a 25% decline in their market share absent any changes in school quality. This potential market share drop is mainly driven by students who qualify for a 90% voucher. Students eligible for a 50% voucher drive the second, smaller drop in market share that occurs at 120% of the current voucher amount. Figure 9b shows the analogous rise in market share that occurs for private schools at each of those jump points.

Figure 10a shows how these changes in market share map into the incentives to provide quality. We find that, all else constant, the quality elasticity of demand increases with the potential outflows of students from public to private schools. This result suggests that public schools' incentives to improve quality are increasing as the voucher amount increases. However, there is a flattening of the quality elasticity of demand after the voucher amount exceeds 150% of the current voucher amount, suggesting the incentive to improve quality is increasing only up to a certain point.

Figure 10b shows that private schools have an incentive to mark down quality as voucher amounts increase. The overall pattern matches that of market share. There is a slight decrease in the magnitude of the quality markdown as the voucher exceeds 150% of the current voucher amount which we argue is driven by 50% voucher students switching to private schools. Our demand estimates suggest that 50% voucher students are more sensitive to changes in quality than their lower-income counterparts; therefore, private schools are not able to markdown quality as much if they wish to attract this group of students.

Counterfactual II: Changing Voucher Eligibility

Figure 11 shows how average market share at public and private schools changes as we vary the income threshold necessary for eligibility. We set the voucher amount households receive equal to that of the current program. We do not change the current program's cutoff to receive a 90%, but instead increase the cutoff to receive a 50% voucher. Therefore, as we increase the income eligibility threshold beyond the current program, newly eligible households would receive a 50% voucher. Households whose income falls below the current program's 90% cutoff will always receive for a 90% voucher in scenarios they meet the counterfactual income threshold. These counterfactual income thresholds range from household incomes of $0 (the voucher program does not exist) to $180,000.

We find that, all else constant, average public school market share decreases (mostly) monotonically as income eligibility increases[46]. The slope of this curve is larger in magnitude when

[46]The step- function-like nature of the plots is a result of the demand specification, and more importantly, the

lower-income households become eligible, suggesting that the threat to public schools increases at a decreasing rate as the program is expanded. Figure 12b shows that private schools face an analogous story in the opposite direction.

Figure 12 shows how these changes in market share map into the incentives to provide quality. For public schools, we find that the quality elasticity of demand increases with the potential outflows of students from public to private schools. This result suggests that public school incentives to improve quality are increasing as the income eligibility threshold increases. However, as the highest-income students become eligible the quality elasticity falls to a level just above the case where a voucher policy does not exist. We argue this is due to three factors: (1) Figure 11a shows that public schools lose fewer students as the income thresholds increases, suggesting there are few high-income individuals that attend public schools[47], (2) our demand estimates show that high-income students value quality more than their lower-income counterparts and (3) public schools have lower levels of quality than private schools. Together, increasing the income eligibility threshold all but ensures that the few high-income students that initially attend a public school will leave, since we hold school quality constant. Public schools then see an overall decline in their incentive to provide quality as the remaining students value quality less.

Figure 12b shows that private schools are able to mark down quality as more students become eligible to receive a voucher. However, the relationship is non-linear. Quality markdowns are increasing in magnitude until the income threshold reaches around $91,000. Beyond that point, private schools' markdowns are slightly smaller. This result is a direct consequence of the fact that higher-income students are more likely to already be attending a private school. Therefore at higher income eligibility thresholds, private schools must compete against each other to obtain new students. The changing nature of competitions faced by private schools shifts the incentive to provide quality up.

Overall, these counterfactuals provide two main insights. First, public schools are in fact threatened by the possibility of students leaving and this threat leads to public schools having incentives to improve school quality. However, this threat only exists when the voucher is significant enough to induce to students to possibly leave. This is a direct result of preference heterogeneity (mainly price sensitivity) of nearby households. We estimate larger quality markdowns for private schools and therefore conclude they have more local market power as vouchers are expanded. Second, there are significant nonlinearities in the incentives to provide quality as income eligibility is expanded. Our results suggest that extending voucher eligibility to higher-income individuals could erode some

voucher program. The voucher amount for 50% voucher students is not large enough to induce significant changes in enrollment even with expanded eligibility for those students. See Appendix Section D1 for further model details.

[47] Lower-income students are by the largest group of students enrolled in public schools.

of the public school incentives to increase quality. However, private school quality markdowns may decrease in magnitude as voucher eligibility is expanded. We estimate that they will still be larger than in the absence of vouchers.

VIII Conclusion

This paper shows that the implementation of an at-scale voucher program can lead to meaningful changes in school quality. We examined the effects of the adoption and expansion of the Indiana Choice Scholarship Program, the largest program in the United States providing private school vouchers to low and middle-income families, and found that both public and participating private schools saw changes in their school value-added.

We found that public schools facing high exposure to the voucher program experienced increases in their school quality, while choice schools witnessed declines. Our estimates were modest in magnitude; however, papers evaluating voucher policies have found relatively small effects on student outcomes ranging from -0.01s.d. to 0.11s.d (Rouse and Barrow, 2009)[48]. Furthermore, Figlio et al. (2020) shows that the impact on public schools grow as voucher programs mature. We analyze the program in the first few years of its adoption, so it is possible to see stronger increases in the future.

Our results complement those found in previous work examining the effect of ICSP on students that use the voucher. Waddington and Berends (2018) shows that students participating in the program saw declines in math performance with no changes in reading. We argue that schools' responses can at least partly explain these student-level results. The results in Waddington and Berends (2018) might overstate the decline in math performance since this is the dimension that high exposure public schools saw the greatest improvements. Our results provide an example of how understanding of a program's effectiveness may change when we take into consideration the indirect effects when the policy is brought to scale.

We then use a model of household demand for schools to estimate the threat to public school enrollment and its relationship with incentives to provide quality holding tuition and quality constant in the market. Our results suggest that 1) the threat to public schools matters if the voucher amount is large enough and 2) significant non-linearities in incentives to provide quality exists when scaling this program up in terms of income eligibility. Therefore, policymakers should caution against using the successes/failures of smaller-scale voucher programs as motivation for expanding/not expanding them to more people due to the presence of these nonlinearities in incentives to provide quality. To our knowledge, these nonlinearities in incentives to provide quality have not yet been documented.

[48] Abdulkadiroğlu et al. (2018) and Waddington and Berends (2018) are notable exceptions.

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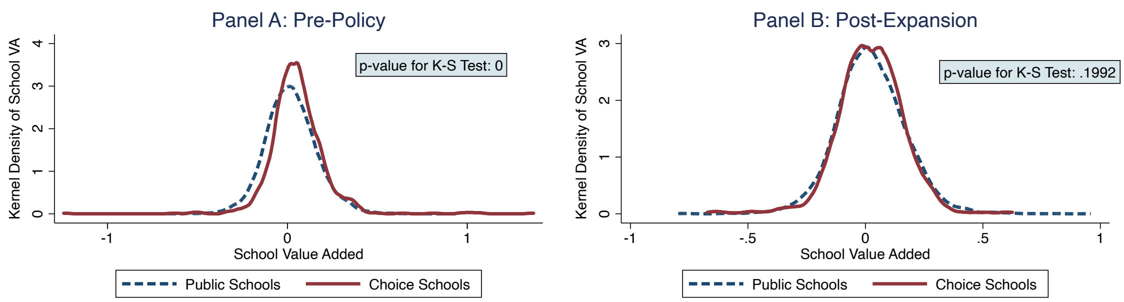
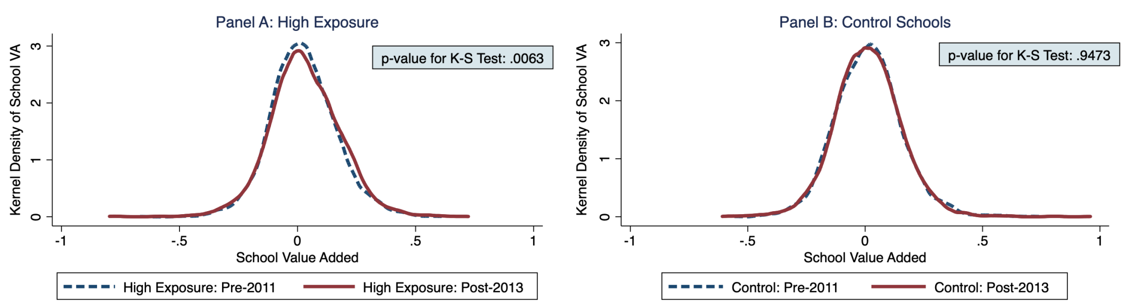
Figures

Figure 1: Kernel Density Plots of School VA - Public and Choice



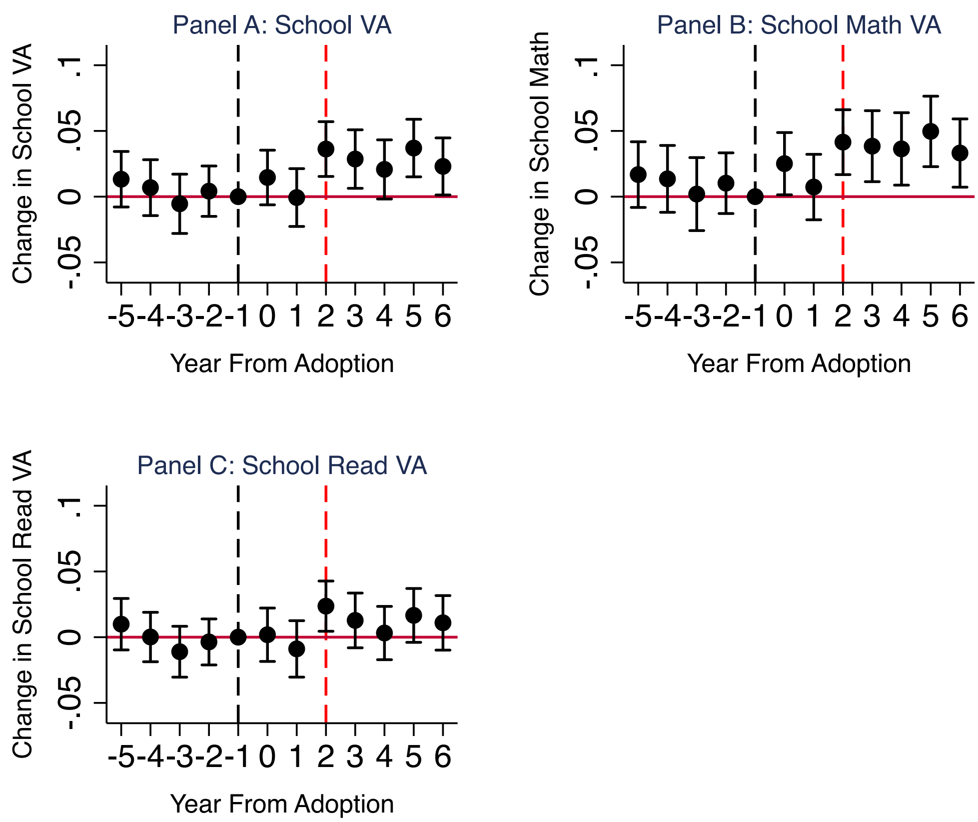
Notes: This figure depicts the kernel density plots of our school value added (VA) estimates for the public and choice schools in our sample. Panel A shows the kernel density plots of schools in the years before the voucher program was implemented. Panel B shows those same estimates in the years after the program was expanded. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported.

Figure 2: Kernel Density Plots of School VA



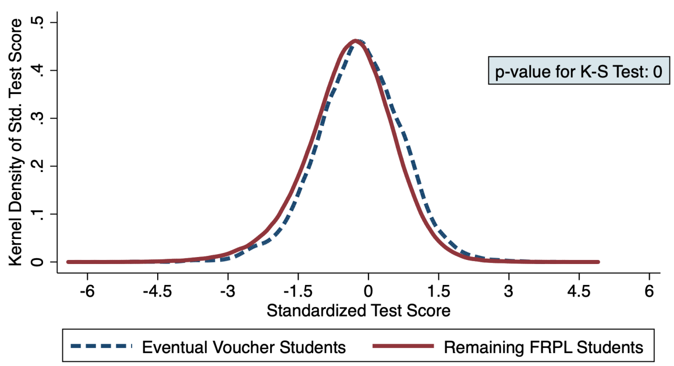
Notes: This figure depicts the kernel density plots of our school value added (VA) estimates for the public schools in our sample. Each panel plots school VA across two time periods: pre-2011 and post-2013 to align with the policy time horizons. Panel A shows the kernel density plots of schools facing high exposure to the policy. Panel B shows the kernel density plots for the control group. High exposure is defined as having a choice school within 5 miles of the school's location. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported.

Figure 3: Event-Study Results of Voucher Policy



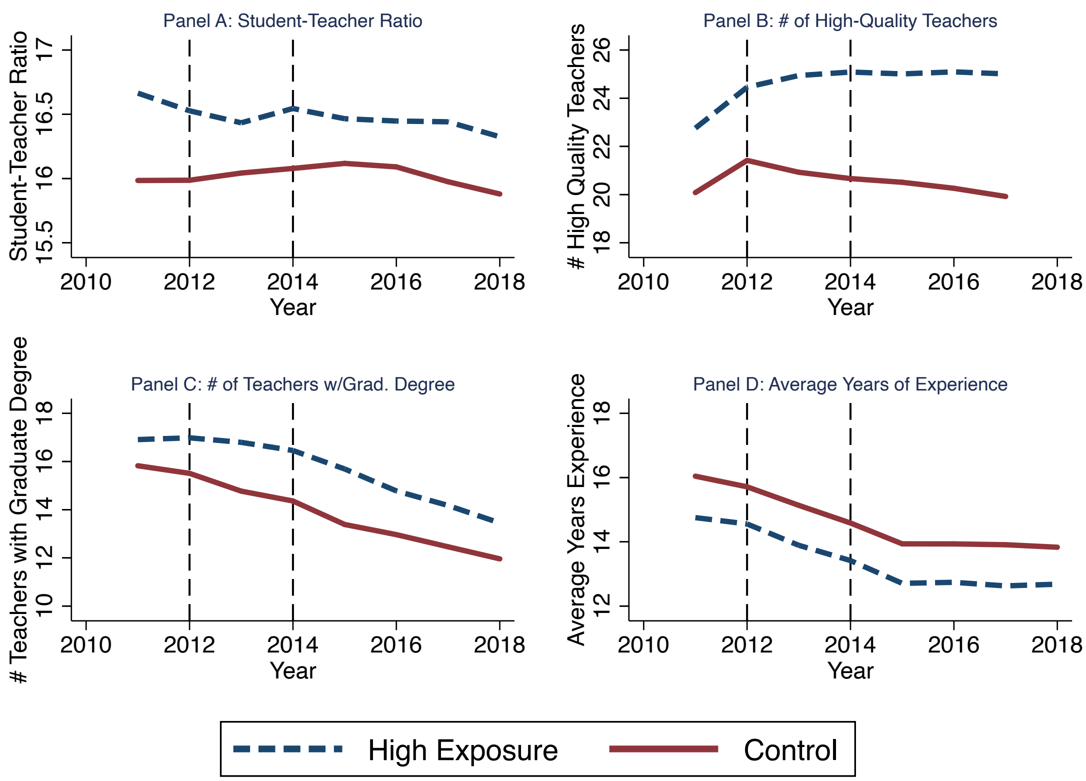
This figure presents the event-study estimates from Equation (3). Figure 3(a) plots the estimates for overall school value-added, Figure 3(b) plots the estimates for school math value-added and Figure 3(c) plots the estimates for school reading value-added. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Figure 4: Kernel Density Plots of Standardized Test Scores



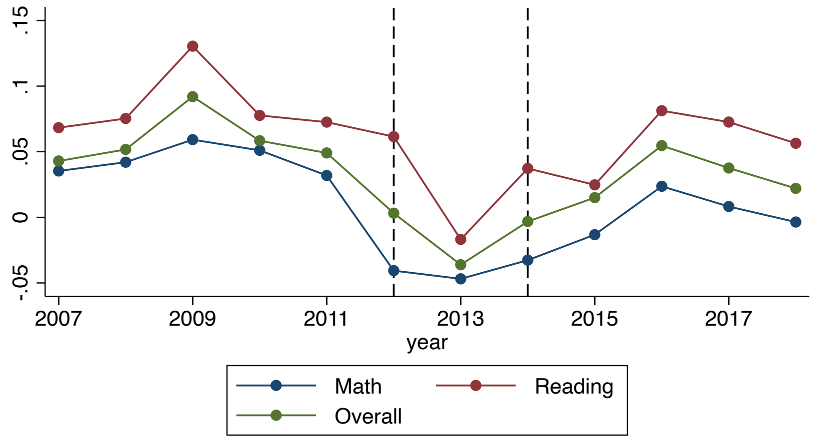
Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public schools in the years before the voucher program. This figure shows the kernel density plots for the eventual voucher students and students remaining in the public school despite qualifying for a 90% voucher. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported.

Figure 5: Public School Inputs



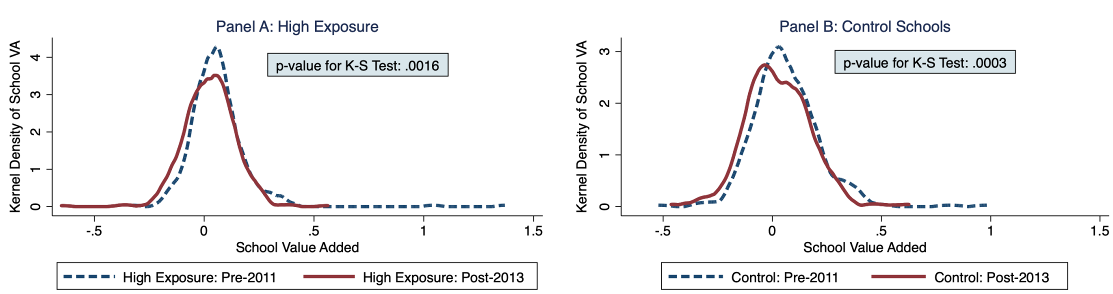
Notes: This figure presents the average student-teacher ratio (Panel A), number of high-quality teachers (Panel B), number of teachers with a graduate degree (Panel C) and average years of experience of teachers (Panel D) across public schools in the sample. High-exposure is defined as having a choice schools within five miles of the public school's location. Data on student-teacher ratios come from the Common Core of Data from the National Center of Education Statistics. Data on teacher characteristics come from the IDOE-Database.

Figure 6: Choice School Value-Added



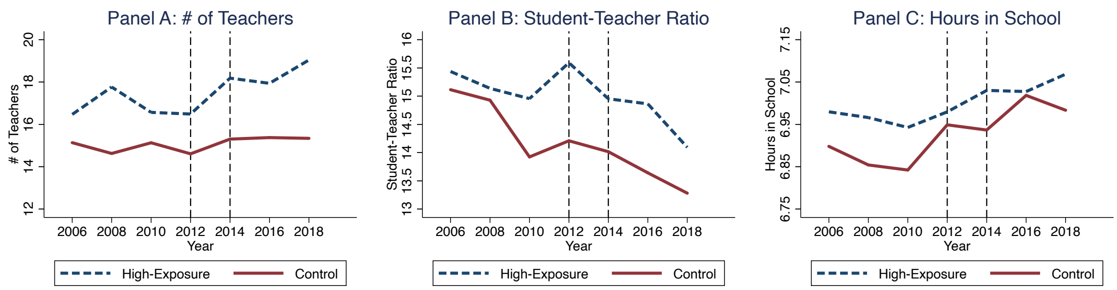
Notes: This figure depicts the average school value-added estimates across all choice schools in each year of the sample. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The dashed lines represents the years the voucher program was implemented and expanded.

Figure 7: Kernel Density Plots of School VA



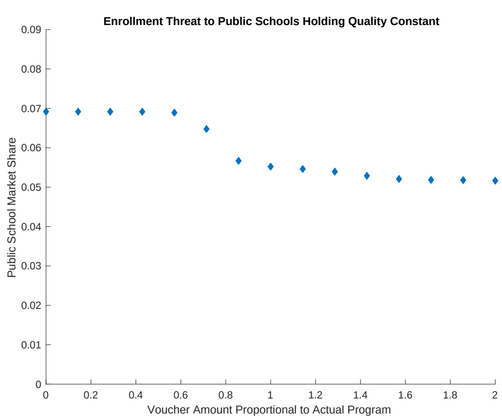
Notes: This figure depicts the kernel density plots of our school value added (VA) estimates for the public schools in our sample. Each panel plots school VA across two time periods: pre-2011 and post-2013 to align with the policy time horizons. Panel A shows the kernel density plots of schools facing high exposure to the policy. Panel B shows the kernel density plots for the control group. High exposure is defined as being in the top tercile of the distribution of number of public schools in 5-miles. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported.

Figure 8: Choice School Inputs

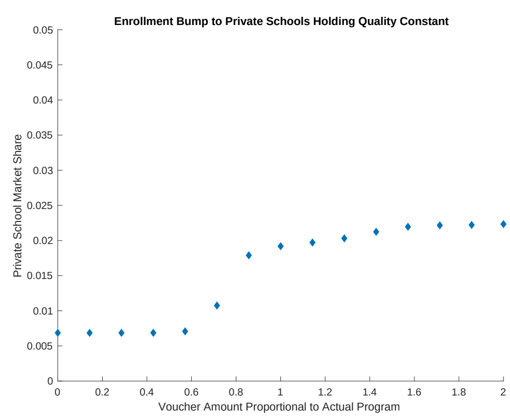


Notes: This figure depicts the average number of teachers (Panel A), the average student-teacher ratio (Panel B), and the avereage hours spent in school (Panel C) across high-exposure and control choice schools in the sample. High-exposure choice schools are those in the top tercile of the distribution of number of public schools within 5 miles of the choice school's location. Data on choice school inputs come from the Private School Universe Survey conducted by the National Center for Education Statistics. Data are only avaiable in every other year. The dashed lines represent the years the voucher program was implemented and expanded.

Figure 9: Changes in Enrollment Varying Voucher Amount



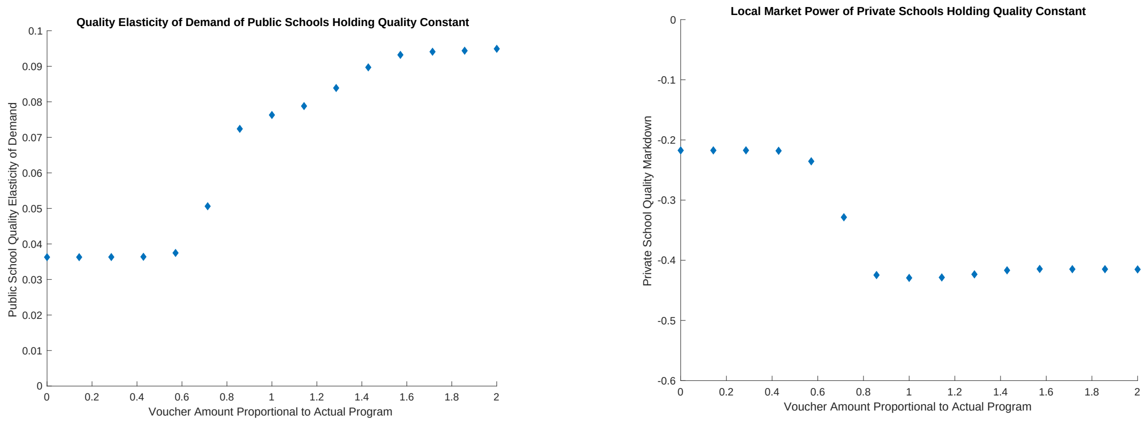
(a)



(b)

Notes: This figure presents the average market shares (enrollment) for public (Panel A) and private schools (Panel B) across our counterfactual voucher policy where we vary the voucher amount. The x-axis represents the multiplier we use to calculate the maximum voucher amount. A detailed description of our demand estimation used to created this figure can be found in Appendix Section E1.

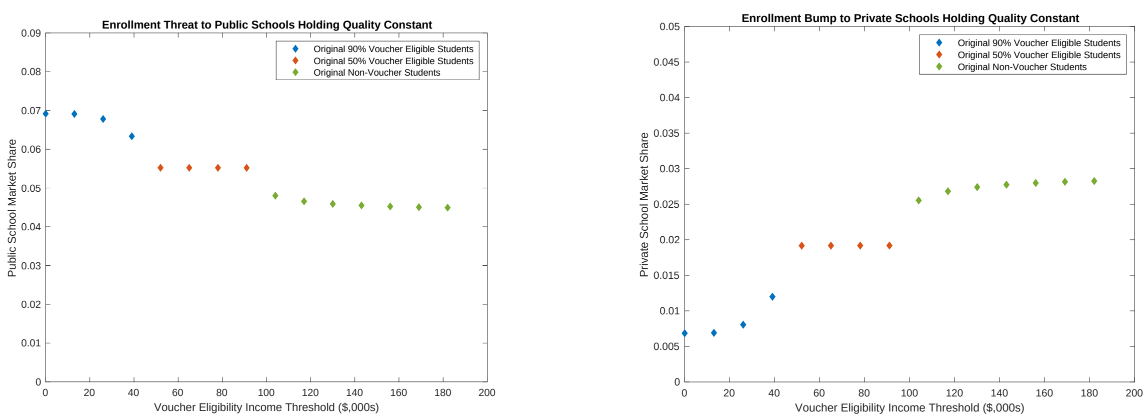
Figure 10: Changes in Incentive to Provide Quality Varying Voucher Amount



(a) (b)

Notes: This figure presents the quality elasticity of demand for public schools (Panel A) and the quality markdowns for private schools (Panel B) across our counterfactual voucher policy where we vary the voucher amount. The x-axis represents the multiplier we use to calculate the maximum voucher amount. A detailed description of our demand and supply estimation used to created this figure can be found in Appendix Section E1.

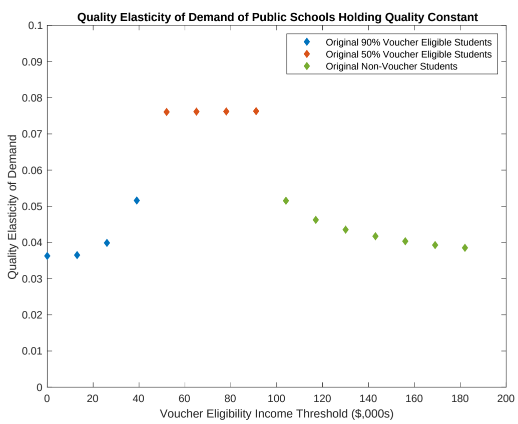
Figure 11: Changes in Enrollment Varying Voucher Eligibility



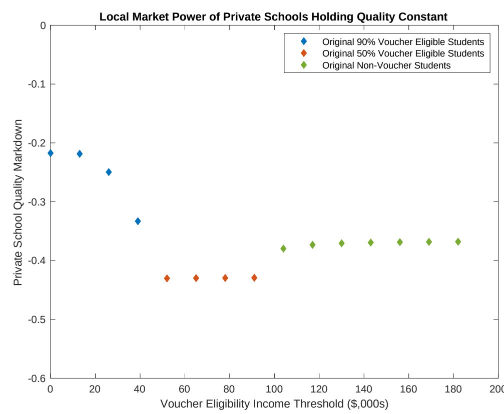
(a) (b)

Notes: This figure presents the average market shares (enrollment) for public (Panel A) and private schools (Panel B) across our counterfactual voucher policy where we vary the income threshold for voucher eligibility. The x-axis represents the maximum household income allowed to qualify for a voucher. A detailed description of our demand estimation used to created this figure can be found in Appendix Section E1.

Figure 12: Changes in Incentive to Provide Quality Varying Voucher Eligibility



(a)



(b)

Notes: This figure presents the quality elasticity of demand for public schools (Panel A) and the quality markdowns for private schools (Panel B) across our counterfactual voucher policy where we vary the income threshold for voucher eligibility. The x-axis represents the maximum household income allowed to qualify for a voucher. A detailed description of our demand estimation used to created this figure can be found in Appendix Section E1.

Tables

Table 1: Summary Statistics of High Exposure vs. Control Schools - Public

	(1) High Exposure	(2) Control	(3) Difference
# of Students Taking ISTEP+ Exam	262	217	45***
	(241)	(176)	
School VA	0.021	0.018	.003
	(0.146)	(0.153)	
School Math VA	0.025	0.026	.001
	(0.150)	(0.143)	
School Reading VA	0.009	-0.002	.011
	(0.135)	(0.133)	
% White	0.648	0.914	-0.265***
	(0.271)	(0.121)	
% Black	0.156	0.017	0.139***
	(0.200)	(0.097)	
% FRPL	0.550	0.415	0.135***
	(0.257)	(0.155)	
N	727	552	

This table presents summary statistics for the set of schools identified as high exposure and the control group in the year before the voucher policy was implemented. High exposure is defined as having at least one nearby choice school. Column (3) denotes the difference in the means between schools in the control group and those highly exposed to the program. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 2: Summary Statistics of High Exposure vs. Control Schools - Choice

	(1) High Exposure	(2) Control	(3) Difference
# of Students Taking ISTEP+ Exam	145	107	38**
	(89)	(73)	
School VA	0.056	0.059	.003
	(0.197)	(0.123)	
School Math VA	0.048	0.052	.004
	(0.254)	(0.170)	
School Reading VA	0.082	0.076	-.006
	(0.153)	(0.105)	
% White	0.749	0.904	0.154***
	(0.271)	(0.102)	
% Black	0.077	0.013	-0.065***
	(0.158)	(0.389)	
% FRPL	0.264	0.101	-0.163***
	(0.287)	(0.103)	
N	54	124	

This table presents summary statistics for the set of schools identified as high exposure and the control group in the year before the voucher policy was implemented. High exposure is defined as having at least one nearby choice school. Column (3) denotes the difference in the means between schools in the control group and those highly exposed to the program. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 3: DiD Results on the Effects of High Exposure on School VA

	(1) School Value-Added	(2) School Value-Added	(3) School Math Value-Added	(4) School Math Value-Added	(5) School Reading Value-Added	(6) School Reading Value-Added
$Post_t \cdot HighExp_s$	0.023***	0.009	0.030***	0.015*	0.013***	-0.000
	(0.006)	(0.006)	(0.007)	(0.008)	(0.005)	(0.006)
Interaction with High Share of FRPL in 2010		0.030***		0.032***		0.028***
		(0.008)		(0.010)		(0.007)
Observations	15,348	15,348	15,348	15,348	15,348	15,348
R-squared	0.448	0.449	0.433	0.434	0.455	0.456
Baseline Mean	0.0197	0.0197	0.0255	0.0255	0.00390	0.00390

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Columns (2), (4), and (6) include the interaction of high exposure and an above median share of FRPL students in the year before the voucher policy was implemented. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 4: DiD Results on Public School Inputs

	(1) Student-Teacher Ratio	(2) # of Teachers w/Grad. Degree	(3) # of HQ Certified Teachers	(4) Avg. Years of Experience
$Post_t \cdot HighExp_s$	0.11	0.68***	1.48**	-0.090
	(0.15)	(0.24)	(0.31)	(0.15)
Observations	9,963	10,179	10,179	10,179
Baseline Mean	18.34	16.91	22.88	14.75

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Observations are lower compared to other tables because of limited availability of data. Data on teacher characteristics come from the IDOE-CREO database and student-teacher ratios are calculated from the Common Core of Data on Public Schools.

Table 5: DiD Results on Attendance and Suspension Measures

VARIABLES	(1) Percent Days Attend	(2) Total Days Attend	(3) Percent Expelled or Suspended
$Post_t \cdot HighExp_s$	0.327***	0.517**	-0.056
	(0.116)	(0.215)	(0.156)
Observations	15,336	15,336	15,336
R-squared	0.866	0.860	0.769
Baseline Mean	88.62	159.5	5.594

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database. One school is missing data for all years on attendance, so the number of observations is slightly less than other tables.

Table 6: DiD Results on Demographics of Students Enrolled

VARIABLES	(1) Share Female	(2) Share White	(3) Share Black	(4) Share Hispanic	(5) Share FRPL
$Post_t \cdot HighExp_s$	-0.188	-2.719***	0.267*	2.268***	0.383
	(0.211)	(0.250)	(0.151)	(0.204)	(0.354)
Observations	15,348	15,348	15,348	15,348	15,348
Baseline Mean	49.58	76.30	9.607	8.082	49.20

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as being in the top tercile of the distribution of number of nearby public schools. Data on enrollment come from the IDOE-CREO database.

Table 7: DiD Results on Predicted School Value-Added

VARIABLES	(1) Predicted School VA	(2) Predicted School Math VA	(3) Predicted School Reading VA
$Post_t \cdot HighExp_s$	-0.010**	0.003	-0.023***
	(0.004)	(0.006)	(0.003)
Observations	15,348	15,348	15,348
R-squared	0.354	0.274	0.427

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. Regressions include school and year fixed effects. High exposure is defined as having at least one nearby choice school. Data on test scores come from the IDOE-CREO database. Predicted School Value-Added are estimated by regressing value-added in 2007 on school characteristics and using the regression coefficients to predict school-value added for all years in the sample.

Table 8: Heterogenous DiD Results of Voucher Program

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
Panel A: Large Baseline Enrollment			
$Post_t \cdot HighExp_s$	0.029***	0.036***	0.019***
	(0.008)	(0.010)	(0.007)
Interaction with Above Median Baseline Enrollment	-0.011	-0.013	-0.010
	(0.007)	(0.009)	(0.006)
Observations	15,348	15,348	15,348
R-squared	0.448	0.434	0.455
Baseline Mean	0.0197	0.0255	0.00390
Panel B: High Baseline School Value-Added			
$Post_t \cdot HighExp_s$	0.045***	0.059***	0.029***
	(0.007)	(0.008)	(0.006)
Interaction with Above Median Baseline School VA	-0.041***	-0.053***	-0.030***
	(0.007)	(0.009)	(0.006)
Observations	15,348	15,348	15,348
R-squared	0.450	0.436	0.456
Baseline Mean	0.0197	0.0255	0.00390
Panel C: Above Median Neighborhood Income			
$Post_t \cdot HighExp_s$	0.027***	0.032***	0.019***
	(0.007)	(0.009)	(0.006)
Interaction with Above Median Neighborhood Income	-0.007	-0.005	-0.011
	(0.008)	(0.009)	(0.007)
Observations	15,348	15,348	15,348
R-squared	0.448	0.433	0.455
Baseline Mean	0.0197	0.0255	0.00390

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. A small (big) school is defined as one that falls below (above) the median in total enrollment in the 2006-2007 AY. A low (high) baseline VA school is defined as one that falls below (above) the median in VA in the 2006-2007 AY. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 9: DiD Results Using Choice Schools

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	-0.010	-0.012	-0.014
	(0.018)	(0.022)	(0.014)
Observations	2,136	2,136	2,136
R-squared	0.295	0.312	0.300
Baseline Mean	0.0490	0.0319	0.0725

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as being in the top tercile of the distribution of the number of nearby public schools. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 10: DiD Results on Choice School Inputs

	(1) Full-Time Teachers	(2) Student/Teacher Ratio	(3) Hours in School Day
$Post_t \cdot HighExp_s$	-0.342	0.930**	-0.080
	(0.893)	(0.418)	(0.051)
Observations	977	977	977
Baseline Mean	15.57	14.24	6.873

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as being in the top tercile of the distribution of the number of public schools within five miles. Data on choice school inputs comes from the Private School Universe Survey which is conducted biannually. There are fewer observations in this analysis because of the survey design.

Appendix

Table A1: DiD Results With Shrunken Value-Added Estimates

VARIABLES	(1) Shrunk School VA	(2) Shrunk Math VA	(3) Shrunk Reading VA
$Post_t \cdot HighExp_s$	0.021***	0.026***	0.014***
	(0.005)	(0.007)	(0.005)
Observations	15,348	15,348	15,348
R-squared	0.450	0.438	0.442
Baseline Mean	0.0163	0.0274	0.01

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1) and shrunk according to a empirical bayes approach (Kane and Staiger, 2008).

Table A2: DiD Results Varying School VA Estimation

	(1)	(2)	(3)	(4)
VARIABLES	Baseline	School-Year FE Only	Including Demographics	Including Previous Test Score
$Post_t \cdot HighExp_s$	0.023***	0.024**	0.028***	0.023***
	(0.006)	(0.010)	(0.010)	(0.006)
Observations	15,348	15,348	15,348	15,348

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as being in the top tercile of the distribution of number of nearby public schools. Data on enrollment come from the IDOE-CREO database.

Table A3: DiD Results With Various Definitions of Nearby Choice School

	(1)	(2)	(3)	(4)	(5)
	Within 3 miles	Within 5 miles	Within 8 miles	Within 10 miles	Within 15 miles
Panel A: School Value-Added					
$Post_t \cdot HighExp_s$	0.025***	0.023***	0.013**	0.009	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Observations	15,348	15,348	15,348	15,348	15,348
R-squared	0.448	0.448	0.447	0.447	0.447
Baseline Mean	0.0197	0.0197	0.0197	0.0197	0.0197
Panel B: School Math Value-Added					
$Post_t \cdot HighExp_s$	0.033***	0.030***	0.016**	0.011	0.004
	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
Observations	15,348	15,348	15,348	15,348	15,348
R-squared	0.434	0.433	0.432	0.432	0.432
Baseline Mean	0.0255	0.0255	0.0255	0.0255	0.0255
Panel C: School Reading Value-Added					
$Post_t \cdot HighExp_s$	0.016***	0.013***	0.008	0.004	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	15,348	15,348	15,348	15,348	15,348
R-squared	0.455	0.455	0.455	0.454	0.454
Baseline Mean	0.00390	0.00390	0.00390	0.00390	0.00390

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school within a certain number of miles as indicated in each of the columns. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A4: DiD Results on School VA with and without Baseline Covariates

	(1) School Value-Added	(2) School Value-Added	(3) School Math Value-Added	(4) School Math Value-Added	(5) School Reading Value-Added	(6) School Reading Value-Added
$Post_t \cdot HighExp_s$	0.010**	0.0229***	0.016**	0.0302***	-0.002	0.0127***
	(0.005)	(0.0056)	(0.006)	(0.0071)	(0.004)	(0.0048)
Observations	15,348	15,348	15,348	15,348	15,348	15,348
R-squared	0.444	0.4479	0.429	0.4333	0.450	0.4547
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Covariates	No	Yes	No	Yes	No	Yes
Baseline Mean	0.0197	0.0197	0.0255	0.0255	0.00390	0.00390

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Odd columns show results when baseline covariates are excluded from the regression. Even columns show the baseline results with the inclusion of baseline covariates. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A5: DiD Results on School VA Using a Continuous Measure

	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot NumClose_s$	0.003***	0.004***	0.002***
	(0.001)	(0.001)	(0.001)
Observations	15,348	15,348	15,348
R-squared	0.448	0.433	0.455
Baseline Mean	0.0197	0.0255	0.00390
Avg. Num. Close Schools	4	4	4

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. $NumClose_s$ is a continuous measure of the number of choice schools within 5 miles. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A6: Student-Level DiD Results of Voucher Program

VARIABLES	(1) Standardized Test Score	(2) Standardized Math Score	(3) Standardized Reading Score
Panel A: High Exposure Public Students vs. Control			
$Post_t \cdot HighExp_s$	0.011**	0.017***	0.003
	(0.004)	(0.006)	(0.004)
Observations	3,753,591	3,753,591	3,753,591
R-squared	0.774	0.705	0.670

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A7: DiD Results by Title I Status

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
Panel A: Had Title I Program			
$Post_t \cdot HighExp_s$	0.018***	0.029***	0.005
	(0.007)	(0.008)	(0.006)
Interaction with Had Title I Program in 2010	0.010	0.002	0.017**
	(0.008)	(0.009)	(0.007)
Observations	15,348	15,348	15,348
R-squared	0.449	0.434	0.457
Baseline Mean	0.0201	0.0258	0.00441
Panel B: Close to Title I Eligibility Threshold			
$Post_t \cdot HighExp_s$	0.023***	0.031***	0.012**
	(0.006)	(0.008)	(0.005)
Interaction with Close to Title I Eligibility Threshold	-0.001	-0.007	0.005
	(0.008)	(0.009)	(0.006)
Observations	15,348	15,348	15,348
R-squared	0.449	0.434	0.457
Baseline Mean	0.0201	0.0258	0.00441

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Panel A includes an interaction term that indicates whether a high-exposure public school had a Title I program in 2010. Panel B includes an interaction term that indicates whether a high-exposure public school was within 5 p.p. of the cutoff for Title I eligibility. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A8: DiD Results on the Set of Control Schools

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	-0.000	-0.002	0.002
	(0.009)	(0.011)	(0.008)
Observations	6,636	6,636	6,636
R-squared	0.464	0.453	0.450
Baseline Mean	0.0176	0.0261	-0.00236

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one choice school within 8 miles. Data on enrollment come from the IDOE-CREO database.

Table A9: DiD Results Removing Public Schools With Choice School Within 3-8 Miles

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	0.024***	0.031***	0.015***
	(0.007)	(0.008)	(0.006)
Observations	11,844	11,844	11,844
R-squared	0.441	0.427	0.441
Baseline Mean	0.0143	0.0196	-0.00245

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one choice school within 3 miles. Control public schools are those that do not have a choice school within 8 miles. Data on enrollment come from the IDOE-CREO database. One school is missing data for all years on attendance, so the number of observations is slightly less than other tables.

Table A10: DiD Results on School VA Dropping Marion County

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	0.019***	0.026***	0.010**
	(0.006)	(0.007)	(0.005)
Observations	13,680	13,680	13,680
R-squared	0.450	0.436	0.455
Baseline Mean	0.0225	0.0272	0.00665

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A11: DiD Results Dropping Each County in Indiana

County Dropped	Estimate	Standard Dev.	Observations	County Dropped	Estimate	Standard Dev.	Observations
Adams	0.019***	(0.006)	13,644	Lawrence	0.020***	(0.006)	13,536
Allen	0.020***	(0.006)	12,924	Madison	0.019***	(0.006)	13,572
Bartholomew	0.019***	(0.006)	13,548	Marion	0.019***	(0.006)	13,680
Benton	0.019***	(0.006)	13,668	Marshall	0.020***	(0.006)	13,572
Blackford	0.019***	(0.006)	13,668	Martin	0.019***	(0.006)	13,680
Boone	0.018***	(0.006)	13,536	Miami	0.019***	(0.006)	13,632
Brown	0.019***	(0.006)	13,656	Monroe	0.020***	(0.006)	13,476
Carroll	0.019***	(0.006)	13,668	Montgomery	0.019***	(0.006)	13,584
Cass	0.018***	(0.006)	13,608	Morgan	0.019***	(0.006)	13,500
Clark	0.019***	(0.006)	13,464	Newton	0.018***	(0.006)	13,644
Clay	0.019***	(0.006)	13,596	Noble	0.019***	(0.006)	13,584
Clinton	0.019***	(0.006)	13,620	Ohio	0.019***	(0.006)	13,692
Crawford	0.019***	(0.006)	13,656	Orange	0.019***	(0.006)	13,644
Daviess	0.020***	(0.006)	13,596	Owen	0.019***	(0.006)	13,644
Dearborn	0.019***	(0.006)	13,584	Parke	0.018***	(0.006)	13,620
Decatur	0.019***	(0.006)	13,632	Perry	0.019***	(0.006)	13,656
Dekalb	0.019***	(0.006)	13,584	Pike	0.019***	(0.006)	13,668
Delaware	0.020***	(0.006)	13,452	Porter	0.020***	(0.006)	13,224
Dubois	0.019***	(0.006)	13,548	Posey	0.017***	(0.006)	13,620
Elkhart	0.018***	(0.006)	13,212	Pulaski	0.019***	(0.006)	13,656
Fayette	0.019***	(0.006)	13,620	Putnam	0.019***	(0.006)	13,596
Floyd	0.019***	(0.006)	13,560	Randolph	0.019***	(0.006)	13,584
Fountain	0.019***	(0.006)	13,656	Ripley	0.019***	(0.006)	13,584
Franklin	0.019***	(0.006)	13,656	Rush	0.019***	(0.006)	13,668
Fulton	0.018***	(0.006)	13,644	Scott	0.019***	(0.006)	13,620
Gibson	0.019***	(0.006)	13,596	Shelby	0.019***	(0.006)	13,572
Grant	0.019***	(0.006)	13,548	Spencer	0.019***	(0.006)	13,620
Greene	0.020***	(0.006)	13,596	St. Joseph	0.022***	(0.006)	13,404
Hamilton	0.018***	(0.006)	13,140	Starke	0.019***	(0.006)	13,644
Hancock	0.020***	(0.006)	13,560	Steuben	0.019***	(0.006)	13,608
Harrison	0.019***	(0.006)	13,572	Sullivan	0.019***	(0.006)	13,632
Hendricks	0.018***	(0.006)	13,416	Switzerland	0.019***	(0.006)	13,668
Henry	0.019***	(0.006)	13,548	Tippecanoe	0.018***	(0.006)	13,392
Howard	0.018***	(0.006)	13,524	Tipton	0.019***	(0.006)	13,656
Huntington	0.019***	(0.006)	13,608	Union	0.019***	(0.006)	13,668
Jackson	0.019***	(0.006)	13,560	Vanderburgh	0.017***	(0.006)	13,392
Jasper	0.019***	(0.006)	13,644	Vermillion	0.020***	(0.006)	13,644
Jay	0.020***	(0.006)	13,596	Vigo	0.020***	(0.006)	13,416
Jefferson	0.020***	(0.006)	13,632	Wabash	0.021***	(0.006)	13,608
Jennings	0.019***	(0.006)	13,620	Warren	0.019***	(0.006)	13,656
Johnson	0.017***	(0.006)	13,380	Warrick	0.019***	(0.006)	13,560
Knox	0.017***	(0.006)	13,608	Washington	0.019***	(0.006)	13,644
Kosciusko	0.018***	(0.006)	13,512	Wayne	0.019***	(0.006)	13,536
LaGrange	0.020***	(0.006)	13,680	Wells	0.019***	(0.006)	13,620
LaPorte	0.021***	(0.006)	13,380	White	0.019***	(0.006)	13,608
Lake	0.016***	(0.006)	12,684	Whitley	0.019***	(0.006)	13,608

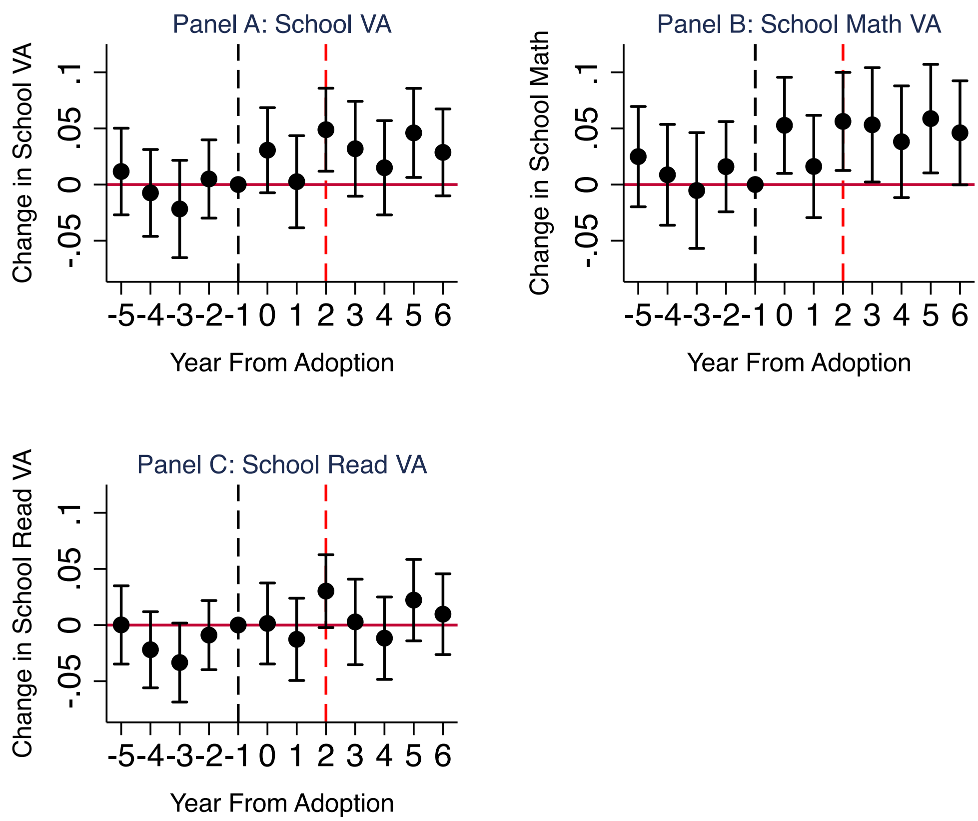
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table A12: DiD Results Using Choice Schools - Varying Definition of High Exposure

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	-0.009	-0.019	-0.002
	(0.016)	(0.020)	(0.013)
Observations	2,136	2,136	2,136
Baseline Mean	0.0490	0.0319	0.0725

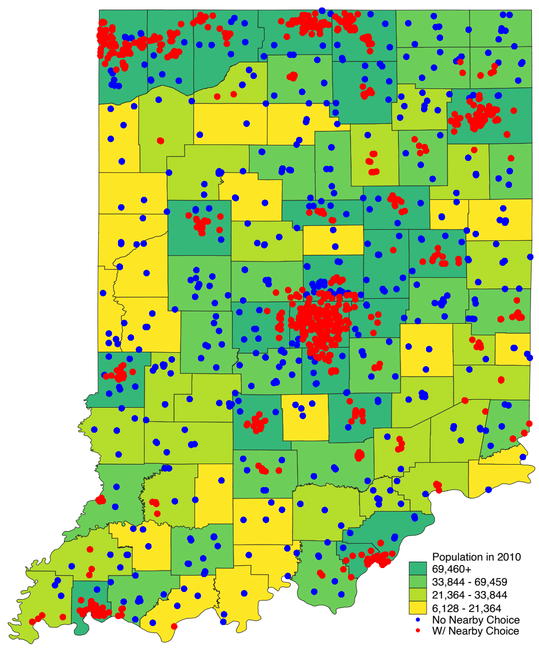
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the school level. Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having an above median share of public school students within five miles qualifying for the voucher in 2010. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Figure A1: Event-Study Results of Voucher Policy - High Share of FRPL



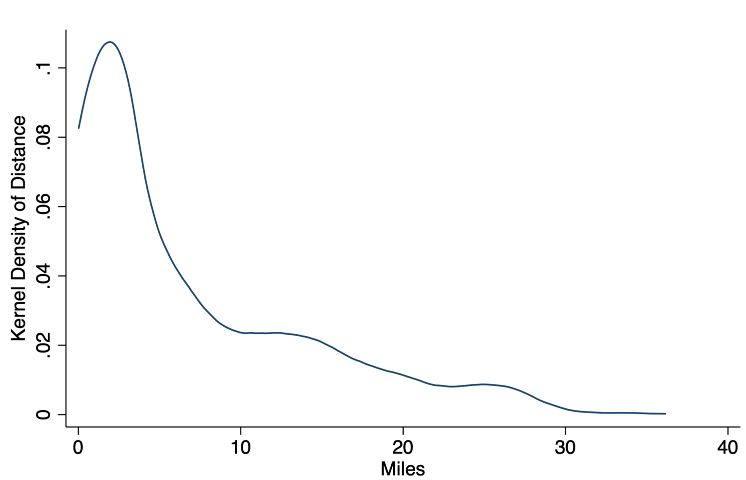
This figure presents the event-study estimates from Equation (3). Figure A1(a) plots the estimates for overall school value-added, Figure A1(b) plots the estimates for school math value-added and Figure A1(c) plots the estimates for school reading value-added. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Figure A2: Locations of High Exposure and Control Public Schools



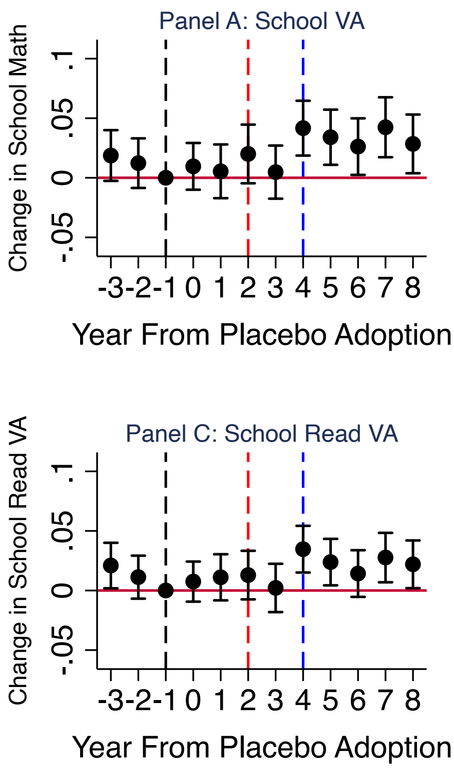
This figure plots the location of each public school in our sample across Indiana. The red dots indicate the public schools that have a choice school within 5 miles of its location. The blue dots represent the public schools in our control group. The map also shows the population counts for each county in the state in the year 2010. Yellow counties are the least populous, while dark green counties are the most populous. Data on the locations of schools comes from IDOE-CREO database and information on population comes from the U.S. Census Bureau, 2010 Census.

Figure A3: Kernel Density Plot of Distance to Nearest Choice School



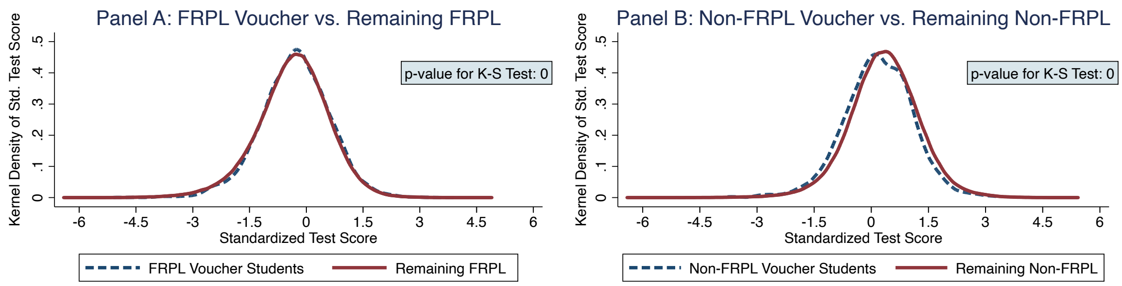
Note: This figure depicts the kernel density plot of the distances between every public school in our sample and the nearest choice school. Distance is caluclated using radial distances between physical addresses. Data on addresses of schools comes from the IDOE-CREO database.

Figure A4: Placebo Event-Study Results of Voucher Policy



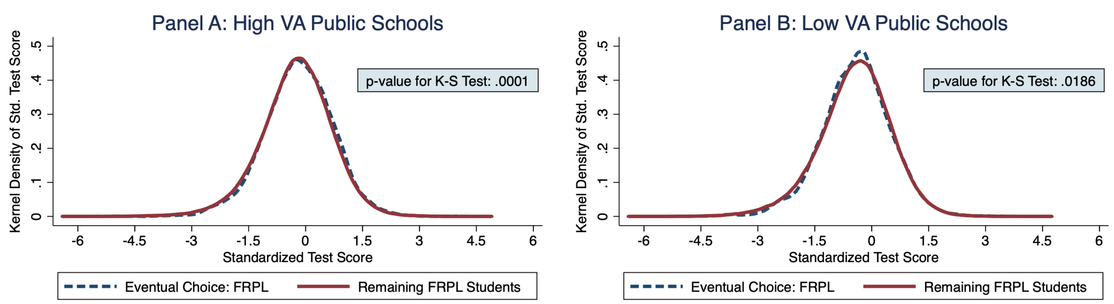
This figure presents the event-study estimates using placebo treatment years. Figure A4(a) plots the estimates for overall school value-added, Figure A4(b) plots the estimates for school math value-added and Figure A4(c) plots the estimates for school reading value-added. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Figure A5: Student Sorting Across FRPL Status



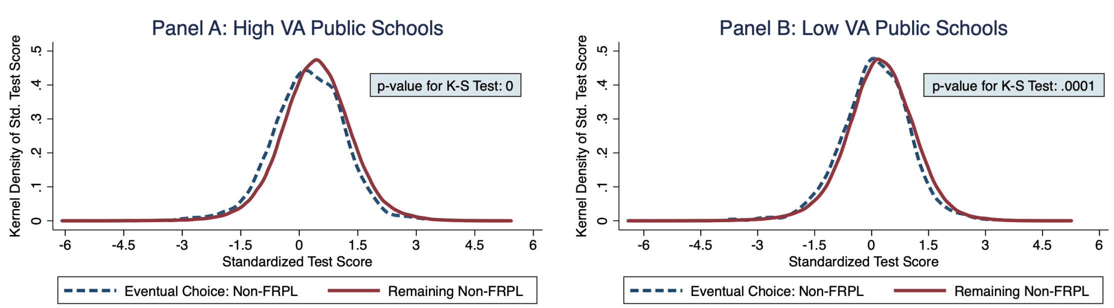
Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public schools in the years before the voucher program. Each panel plots test scores for students who eventually use a voucher and those that remain in public school. Panel A shows the kernel density plots for FRPL eventual voucher students and all FRPL remaining public school students. Panel B shows the kernel density plots for non-FRPL eventual voucher students and non-FRPL students remaining in the public school. Test scores are standardized by year and grade. Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported for each panel.

Figure A6: Kernel Density Plots of Students in Initially High vs Low VA Public Schools: FRPL



Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public schools in the years before the voucher program. Each panel plots the test scores for FRPL students who use a voucher and those that remain in public school despite qualifying for a 90% voucher. Panel A shows the kernel density plots for students in initially high value-added public schools. Panel B shows the kernel density plots for students in intiatially low value-added public schools. Test scores are standardized by year and grade. Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported for each panel.

Figure A7: Kernel Density Plots of Students in Initially High vs Low VA Public Schools: Non-FRPL



Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public schools in the years before the voucher program. Each panel plots the test scores for students who eventually use a voucher and those that remain in public school. Neither group ever qualifies for FRPL. Panel A shows the kernel density plots for students in initially high VA public schools. Panel B shows the kernel density plots for students in intiatially low VA public schools. Test scores are standardized by year and grade. Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported for each panel.

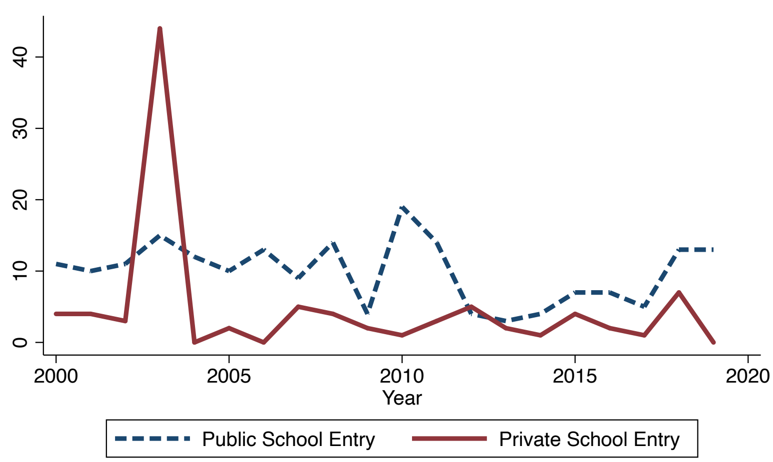
B1 Appendix - Entry and Exit of Schools

Entry and exit into the market is an important supply-side response to consider when evaluating a voucher program. The goal of this appendix section is to illustrate that the Indiana Choice Scholarship Program did not induce significant entry or exit for either public or private schools.

Entry of Public and Private Schools

To understand how entry of schools has changed over the last twenty years, Figure B1 plots the number of new public and private schools opening in each year from 2000-2019. We only include those schools serving grades third through eighth to match our analysis sample. On average 10 new public schools and 5 new choice schools are opened each year across the state. There is a large spike in the number of private schools in the year 2003. However, there does not seem to significant change in this trend following the adoption of the Indiana Choice Scholarship. We, therefore, conclude that the voucher program did not induce meaningful changes in school entry.

Figure B1: Number of Entering Schools by Year



Notes: This figure depicts the number of public and private schools that opened in the state of Indiana in each year from 2000-2019. Data is only shown for schools th cater to grades 3-8. Data on school openings comes from the historical school directories from the IDOE-CREO database.

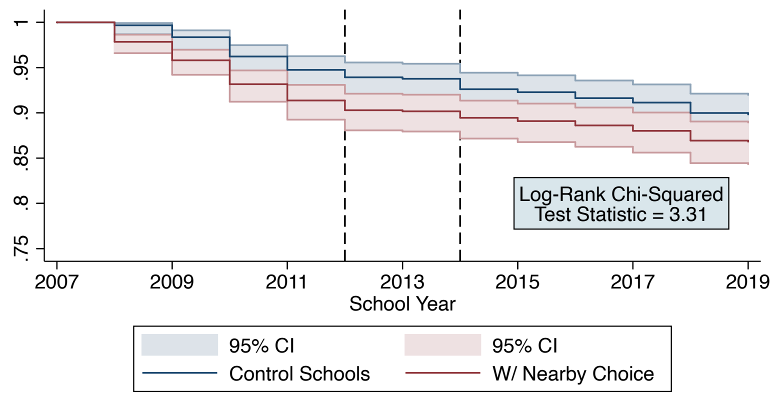
Closures of Traditional Public Schools

We pay particular attention to the closure of traditional public schools because as discussed in Chen and Harris (2021) these events could induce student sorting that biases our results[49]. We investigate

[49] Chen and Harris (2021) explore this idea in the context of charter school penetration.

this potential mechanism by first examining if public schools located near a choice school saw a differential increase in their likelihood to close. This is done by estimating a Kaplan-Meier survivor function as shown in Figure B2. It is visually apparent that being located within five miles of an eventual choice school did not increase the likelihood that the public school would close.

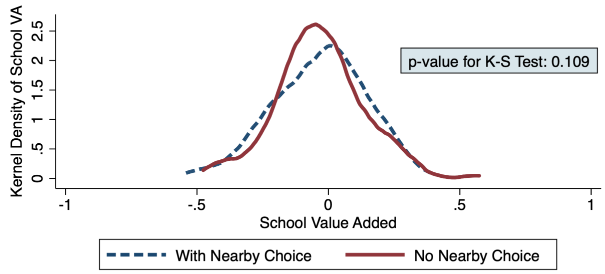
Figure B2: Survivor Model for Public School Closures



Notes: This figure depicts the Kaplan-Meier survivor function for the set of public schools that existed in Indiana at the start of the sample period. The graphs are separated by whether or not the observation has a near-by choice shcool. A choice school must be within five miles of a public school to be considered near-by. The chi-squared test statistic for the log-rank test of equality is reported. The dashed lines represent the years the voucher program was implemented and expanded.

We next examine whether the quality of the closed public schools differed between those with a nearby choice school and those without one. If the high exposure public schools in our sample receive students from higher quality, closed public schools (when compared to the control group), our estimates may be biased upward. We, therefore, examine whether the distributions of school value-added are equal between these two groups of closed public schools in the years before they close. Figure B3 plots the kernel density functions for the set of public schools that close throughout our sample period. The kernel density functions are estimated separately for the (eventually closed) public schools within five miles of a choice school and those without a nearby choice competitor. It may seem that the school value-added is higher for the set of (eventually closed) public schools within five miles of a choice school, but we fail to reject the null hypothesis of the Kolmogorov-Smirnov equality of distributions test at the 10% level. We take this as suggestive evidence that the quality of the closed public schools did not differ based on the distance to the nearest choice school.

Figure B3: Kernel Density Plot of School VA- Closed Schools

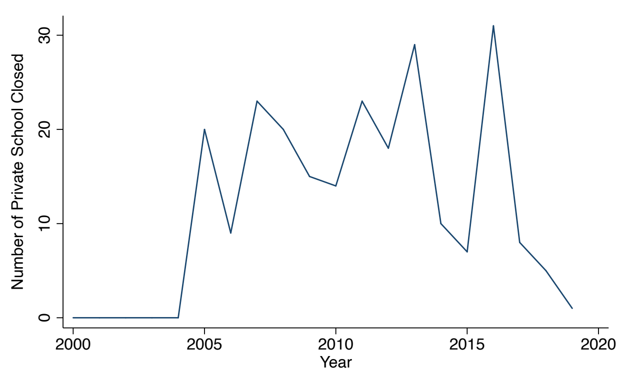


Notes: This figure depicts the kernel density plots of our school VA estimates for the set of public schools that closed during the sample period. The blue line shows the kernel density plot for the schools that have a nearby choice competitor and the red line shows the same for the schools that do not. A near by competitor is defined as having a choice school within 5 miles of the school's location. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The p-value for the Kolmogorov-Smirnov equality-of-distributions test is reported.

Closures of Private Schools

We also examine the closures of private schools across the state. Figure B4 plots the number of private schools that close in each year from 2000-2019. We only include schools serving grades third through eighth to match our analysis sample. On average 15 private schools close each year across the state (not including the years that zero schools close). There does not seem to be a systematic change in the trend of private school closures as the program is adopted. We, therefore, conclude that we do not have strong evidence to suggest that the voucher policy induced meaningful changes in the likelihood that a private school would close.

Figure B4: Number of Private School Closures by Year



Notes: THis figure depicts the number of private schools that closed in the state of Indiana between 2000 and 2019. Data is only schown for schools that cater grades third through eight. Data on school closures comes from the historical school directories from the IDOE-CREO database.

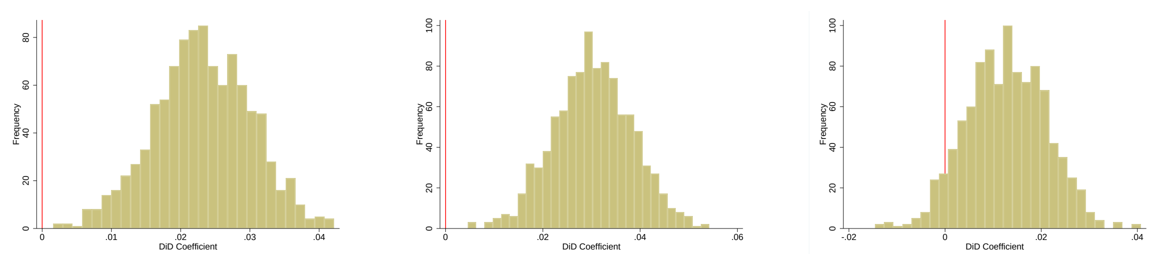
C1 Appendix - Bootstrapping

Deeb (2021) shows that when value-added is the outcome variable of interest in a regression, the regression's robust standard errors used to draw inference are invalid. We, therefore, propose a bootstrapping procedure to correct this issue for our public school analysis.

In each of 1000 iterations, we sample 100 students (with replacement) within each school to be included in the value-added regression described in Equation (1). Therefore, each iteration returns a unique set of school value-added measures that we can use in our difference-in-differences specification. Given this set up, we run Equation (2) on each set of unique school value-added estimates and plot the results on a histogram. We construct new confidence intervals using the standard error of this distribution of difference-in-differences results. We perform this exercise separately for overall school value-added, math school value-added and reading school value-added.

Figure C1 depicts the results of this exercise. Each panel shows the histogram of difference-in-differences results for each of our outcome variables of interest. We highlight where in the distribution the coefficient equals zero to give a sense of the number of iterations that resulted in the voucher program having zero effect on high-exposure public schools. Across all of the panels, it is evident that a majority (if not all) iterations resulted in positive effect of the program on high-exposure public schools. Table C1 displays our standard difference-in-differences coefficients along with our newly constructed confidence intervals.

Figure C1: Bootstrapped DiD Results



(a) School Overall VA (b) School Math VA (c) School Reading VA

Table C1: DiD Results with Bootstrapped Confidence Intervals

VARIABLES	(1) School Overall Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
Standard DiD Estimate	0.023	0.030	0.013
Bootstrapped 95% CI	[0.0227, 0.0236]	[0.0298, 0.0308]	[0.0122, 0.0132]

Each coefficient is the result of a separate estimation. All regressions include school and year fixed effects. Baseline covariates include the share of students that are female, white, black, section 504, special education, and receive testing accommodations in the 2006-2007 academic year. High exposure is defined as having at least one nearby choice school. Confidence intervals are calculated according to the procedure describe in C1.

D1 Appendix - Structural Model Specification

D1.A Demand

We will follow the literature on school choice and model a student's schooling decision as a standard discrete choice logit model. All students decide to attend a school j from $j \in \{1, ..., j, ..., S\}$, where S is the number of schools in the market. Students also have restricted choice sets. More specifically, they can only decide between attending a private school or their zoned public school.

Specifically, we model a student i's random utility for attending school j by the following:

$$U_{ij} = \alpha_i P_{ij} + \lambda_i d_{ij} + \gamma_i q_j + \xi_j + \epsilon_{ij} \tag{6}$$

where $P_{ij} = P_j - e_i \cdot Voucher_{ij}(I_i, P_j)$ and $e_i = \mathbb{1}\{\text{student } i \text{ is eligible for voucher}\}$, d_{ij} is the distance from student i's home to school j, q_j is the quality of school j (measured as school value-added), ξ_j are unobserved characteristics for school j and ϵ_{ij} is a random preference shock that is distributed as extreme value type 1.

Furthermore, we define mutually exclusive observable groups based on voucher eligibility (income) by which we allow taste parameters to vary.

$$\text{S90} :- \{\text{Set of students eligible for a 90\% voucher}\}$$
$$\text{S50} :- \{\text{Set of students eligible for a 50\% voucher}\}$$

Then,

$$\alpha_i = \alpha + \alpha_1 \mathbb{1}\{i \in S90 \cup S50\}$$
$$\lambda_i = \lambda_1 \mathbb{1}\{i \in S90\} + \lambda_2 \mathbb{1}\{i \notin S90\}$$
$$\gamma_i = \gamma + \gamma_1 \mathbb{1}\{i \in S90\} + \gamma_2 \mathbb{1}\{i \in S50\} + \rho \nu_i$$

where ν_i is distributed as lognormal(0,1). We use a lognormal as our random coefficient distribution mainly based on findings in previous literature that all households value quality positively. This specification allows preferences for price, quality and distance to differ between high and low-income households. Our random coefficient on quality allows us to fit more flexible substitution patterns for households.

Define $V_{ij} = \alpha_i P_{ij} + \lambda_i d_{ij} + \gamma_i q_j + \xi_j$, then by integrating out the ϵ_{ij} from U_{ij} we get the standard logit CCPs for student i choosing school j given by the following:

$$S_{ij} = \frac{e^{V_{ij}}}{\sum\limits_{k \in C_i} e^{V_{ik}}} \tag{7}$$

where C_i is student i's choice set as discussed previously. Note that V_{ij} has to be normalized to 0 for a school in the choice set since there is no explicit outside option.

We obtain market shares by aggregating individual choice probabilities for each school j

$$S_j = \frac{1}{n} \sum_i S_{ij} \tag{8}$$

There are two implicit assumptions we make with this choice of modeling. First, all households perfectly observe and make decisions with respect to school quality (school value-added). While it is debated whether this is true for school value-added, we are following the literature by using this measure. Second, we assume capacity constraints for schools are not binding. Although we do not have data on school capacity, conversations with those engaged with schools in Indiana suggest that capacity for choice schools was not an issue during this time.

D1.B Supply Side

Objective Function of Choice Schools

We assume that private schools are profit maximizers when we model supply. However, there are reasons to believe schools might set tuition and quality under a different objective function. We justify our modeling choice by highlighting the instances in which schools have behaved like profit maximizers and that a large proportion of choice schools' revenue comes from the tuition charged when enrolling students. Below is a quote from an NPR article suggesting that private schools in Indiana were concerned with profits when making decisions for the school (Turner et al., 2017).

> *"We've been seeing some financial troubles here at St. Jude Parish," Runyon said in a formal presentation that was recorded in 2014 and posted on the church's website. The parish was in its third straight year of financial losses. One big reason for the losses: The church was pouring money from its offertory into the school and neglecting repairs to its steeple and cooling system...*
>
> *Not long after, the program was expanded dramatically to include children who had never attended a public school. Suddenly, many St. Jude students qualified. All they had to do was apply. "The effect on that this year," Runyon told parishioners in 2014, "it would have been $118,000 of money we just left there, that the state of Indiana wanted to give me, and we weren't able to take advantage of it."*

Runyon's presentation — since taken down from the church's website — was a pitch for a new way of distributing financial aid to St. Jude students, one that would maximize the money coming in through vouchers and allow the parish to use more of its offertory elsewhere.

Optimal Tuition and Quality

Following our model assumptions we show private schools' objective function below:

$$\max_{p_j \geq 0, q_j} (p_j - c(q_j))S_j(\mathbf{p}, \mathbf{q}) \tag{9}$$

where p_j is the tuition charged by school s, $c(q_j)$ represents marginal cost which is a function of q_j, the quality of school j, and S_j denote the market share which is dependent on the vector of prices and qualities. It is important to note that schools receive the entire tuition amount even if the household receives a voucher. The difference comes from who pays, not how much is paid.

Differentiating the above equation w.r.t p_j and rearranging gives the following first-order condition:

$$p_j^* = c(q_j) - \frac{S_j(.)}{\frac{\partial S_j(.)}{\partial p_j}} \tag{10}$$

The pricing F.O.C. resembles that of the traditional Nash-Bertrand model with differentiated products. Here p_j^* is equal to the marginal costs of an additional student and a markup term. As mentioned previously, the marginal cost of an additional student is an increasing function of quality. The markup term is the standard markup term in Nash-Bertrand pricing models which results from schools having local market power.

If we parameterize $c(q_j) = \theta_0 + \theta_1 q_j$, where $\theta_0, \theta_1 > 0$, then by differentiating variable profits w.r.t q_j and rearranging, we get the following first-order condition for optimal quality:[50]

$$q_j^* = \frac{p_j - \theta_0}{\theta_1} - \frac{S_j(.)}{\frac{\partial S_j(.)}{\partial q_j}} \tag{11}$$

The F.O.C. for optimal quality is similar to that for optimal tuition. The first term can be thought of as the level of quality chosen if schools had no market power (quality level that achieves zero profits). The second term is the markdown on quality for schools with local market power. This markdown on quality is similar to that in (Neilson, 2021). It is a measure of local market power for each private school. Quality markdowns are a function of the types of students nearby, how they tradeoff quality, OOP expenses, and distance as well as the vector of school attributes including tuition, quality, and locations.

[50]Note that we will not need to assume a functional form for marginal costs for our counterfactuals. We do it for notational simplicity.

Since we opt to not model public schools directly, we do not have a direct counterpart to a quality markdown for them. However, it can be shown that the quality markdown (for private schools) is inversely proportional to its quality elasticity of demand. We use this relationship as motivation for estimating quality elasticities of demand to understand public school incentives to improve quality under various voucher programs.

We use this model framework to estimate changes in enrollment, quality elasticities (for public schools), and quality markdowns (for private schools) as we vary the voucher program holding all other variables constant (i.e tuition, quality, school locations).

E1 Appendix - Structural Model Estimation

E1.A Estimation Overview

We estimate parameters from the demand model outlined in appendix D1 using two steps which we detail below. Recall that we defined the indirect utility that student i receives from attending school j is given by $V_{ij} = \alpha_i P_{ij} + \lambda_i d_{ij} + \gamma_i q_j + \xi_j$. We can rewrite this as $V_{ij} = \delta_j + \mu_{ij}$.

Here μ_{ij} captures all features of utility that vary by individual and school, namely, the non-linear individual heterogeneity terms. The δ_j, mean utility for school j, captures everything that doesn't vary within the school (i.e. mean valuation of quality, tuition, as well as unobserved school characteristics).

In the first step of estimation, we use individual-level choice and demographic data to estimate the non-linear parameters and mean utilities using simulated maximum likelihood.

In our second step, we want to estimate the mean valuation of quality and tuition. To do so, we first need to decompose δ_j into observed and unobserved components.

$$\delta_j = \alpha tuit_j + \gamma qual_j + \xi_j \tag{12}$$

where $tuit_j$ and $qual_j$ is school j's tuition and quality, respectively. ξ_j are unobserved (to the econometrician) characteristics. Generally, we are worried about the possible correlation of ξ_j with both tuition and quality. To account for this possibility we need to instrument for both tuition and quality as they are endogenous if schools observe the ξ_j prior to choosing tuition and quality.

We focus on third-grade students in South Bend, Indiana from 2011-2018 to estimate the model. In total, this amounts to analyzing school choices by 21,545 students across the 7 school years ($\sim$ 3100 students per year).

E1.B Simulation Overview

In addition to simulating draws for our random coefficient in the model, we also have some data limitations. Most importantly, we do not observe latitude/longitude coordinates for students who do not use a voucher.[51] To deal with this complication, we simulate locations for those students using moments from the American Community Survey (ACS) data.

We first find the total number of households who are in each public school's catchment region from the ACS data. We next assign 1) public school students to a catchment region due to zoning of schools, and 2) voucher students can be assigned to catchment regions since we know their locations.

[51] Note that this means that for eligible students who do not actually use a voucher we would not observe information on locations.

Next, we assume that all households in a catchment region are either a public school, voucher, or non-voucher household. With this assumption, we can calculate the number/proportion of households in each catchment region that do not use vouchers.

Now, for every student for whom we do not observe latitude/longitude coordinates, we can assign them a random point within their probabilistically assigned catchment region. We repeat this process 50 times per individual and compute the likelihood function as normal.

E1.C Identification

We use cross-sectional variation by school year in out-of-pocket expenses students would pay to attend private schools to identify the marginal utility of out-of-pocket expenses. Out-of-pocket expenses can vary with voucher eligibility status which varies within cohorts of students as well as tuition that a school charges which varies each school year. We are not able to separately identify heterogeneity terms for individuals who receive a 50% voucher vs a 90% voucher due to no variation in out-of-pocket expenses for 90% voucher students. This would lead to collinearity with mean utilities hindering our identification.

We identify distance parameters using the cross-sectional variation of observed student locations across cohorts. We do not separately identify distance parameters for 50% voucher-eligible students and students who are not eligible. We opt for this parameterization because of data limitations which were discussed above. Lastly, we identify parameters on quality using variation in value-added both within and across school years.

To identify the mean valuations of quality and tuition we employ 2SLS on equation 12 above. The first set of instruments we use are the average distance a student in the market must travel to attend their own school and the average distance to a competitor's school. Since we take locations as exogenous in our model distance traveled should be orthogonal to unobserved product characteristics. The second set of instruments we use is the proportion of 90% eligible students who live within 3 miles of their own school and the average proportion of 90% eligible students who live within 3 miles of their competitors' schools.

E1.D Results

In Table E1 below, we show the results from our estimated demand model. We find heterogeneity patterns that are very much in line with previous literature that estimates household demand for schools. First, we find that lower-income households are more sensitive to out-of-pocket expenses than their higher-income counterparts. Similarly, we find that lower-income households are more sensitive to traveling further distances than higher-income households. Finally and perhaps most importantly, we find that lower-income households are less sensitive to quality than higher-income households. In fact, lower-income households are quite inelastic with respect to quality.

This rich preference heterogeneity leads to variation in local market power when it comes to a school's incentive to provide quality. Our results suggest that schools near many lower-income students will likely have much weaker incentives to invest in quality than schools in higher-income neighborhoods.

Table E1: Demand Estimation Results

	α_1	λ_1	λ_2	γ_1	γ_2	ρ	γ	α
Estimate	-4.31	-0.91	-0.56	-3.48	-1.65	0.74	3.73	-0.14
S.E.	0.04	0.01	0.02	0.23	0.31	0.11	7.32	0.28