Supply-Side Responses in School Choice

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Abstract

Despite the growth of private-school voucher programs, our understanding of their effectiveness relies on results from small-scale randomized control trials. We show that those results may not translate to programs at scale by examining changes in school quality following the implementation of the Indiana Choice Scholarship Program. We find that public schools facing high exposure to the policy increased quality while participating private schools decreased quality. Initially poor performing public schools drive our results, suggesting that the public school quality gap shrunk because of the program. Policymakers should consider these indirect effects to understand vouchers' total 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 first U.S. voucher program, Milwaukee Parental Choice, featured an enrollment limit of 1% of the public school population when it launched in 1991. Today, the average voucher program has no enrollment cap, and around 26% of families qualify to participate (EdChoice, 2021). 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; Wolf et al., 2010; Witte et al., 2014; Abdulkadiroğlu et al., 2018).[1] While these RCTs provide useful estimates of the average effect of being offered a voucher, their results may not capture the overall impact of voucher programs when 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). Examining school responses to voucher programs is essential to understanding how such programs impact educational outcomes for students not directly participating in the program.

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. 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 these 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 that face increased

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

competition because they are located within five miles of a private school that eventually accepts voucher students. Private schools that accept voucher 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.[2] The resulting data sets track school quality for those in our high exposure and control groups, both before and after the implementation of the voucher program.[3]

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 ICSP for 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 of a standard deviation (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 free or reduced-price lunch witnessed the largest improvements in school quality. We might expect these schools to have a greater response since a larger share of their students automatically qualify for a voucher. Specifically, these schools saw increases of 0.04 s.d. in overall school VA, 0.05 s.d. in math VA, and 0.03 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. Both smaller public schools and schools in lower-income neighborhoods may be more sensitive to changes in enrollment and might increase quality to avoid risking closure. Sim-

[2]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. The use of a radial distance to distinguish between treated and control groups has been used in several contexts including understanding the role of traffic conditions on infant health outcomes (Currie and Walker, 2011), the impacts of the introduction of charter schools on traditional public schools (Cordes, 2018), and the effects of fracking on infant health through drinking water quality (Hill and Ma, 2022). In Section III.B, we discuss alternative methods for distinguishing high exposure schools.

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

ilarly, public schools that were initially poor performing may face additional pressure as the voucher program allows parents to exercise an additional form of choice. 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, our results lead us to conclude that the gap in public-school quality shrunk following the implementation of ICSP.

We also employ 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 among high exposure public schools are seen only after the program's expansion. This result indicates that despite facing potential enrollment losses when the program was adopted, public schools only responded once there was 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 increase quality, as measured by VA, we combine a school-level dataset on available teachers between the 2010-2011 and 2017-2018 academic years with the National Center for Education Statistics's Common Core of Data on Indiana public schools. We do not find strong evidence that following the implementation of ICSP, high exposure public schools saw changes in their student-teacher ratios. However, high exposure public schools saw an increase of 0.7 teachers with a graduate degree and 1.75 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 no changes in the percent of students ever suspended or expelled. These findings suggest that in response to ICSP, schools increased quality in ways that improved outcomes beyond test scores.

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 the extent to which student sorting occurs after the implementation of ICSP. We find that high exposure public schools see a decline of 2.7 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, but remain in the public school system.[4] 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 take these results as evidence that the improvements in school quality are not due to student sorting.[5] Additionally, we use information on student-class links to create a sample of students in the public schools that did not have classes with ever voucher students. We re-estimate school value-added with this sample under the assumption that school quality calculated with this group of students would be less impacted by potential peer effects. Our difference-in-differences results using this sample continue to show that high exposure public schools saw meaningful increases in school quality following the implementation of ICSP, further bolstering our claim that composition of students does not drive our results.

Our public school results 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 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 and find that changes in school quality occurred only after the expansion of the voucher policy, further bolstering our conclusions that pre-trends do not drive our results. As an additional validity check, we employ the event-study sensitivity analysis proposed in Rambachan and Roth (2023). We find that our treatment effects are robust to allowing for violations of parallel trends up

[4] 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.

[5] 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 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.

to the max violation in the pre-trend period. 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. The analysis produces 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 and influenced 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. How might the response of choice schools differ from public schools? Rather than face the potential loss of students, choice schools are now competing to receive additional students. How schools might change quality because of these potential new students is unclear. If competition is centered around the choice of public versus private schooling, choice schools may have the incentive to reduce quality since providing quality is costly and lower-income families tend to be less sensitive to these types of changes (Hastings et al., 2005; Neilson, 2021). However, if competition is instead focused between private schools, choice schools have the incentive to improve quality since competition along the price dimension is essentially eliminated with the voucher program.

We find that choice schools see declines in average quality on all dimensions during the first year of the voucher program. 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 use the Private School Universe Survey to understand to what extent choice schools alter their school inputs durng our sample period. Following the adoption of ICSP, high exposure choice schools see a statistically significant increase of 0.83 (off a base mean of 14.22) 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.

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 test score gains (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.[6]

A large body of work evaluates the supply-side responses to school choice programs. Many of these papers focus on the public school response to the introduction of charter schools (Figlio et al., 2024; Cohodes and Parham, 2021; Imberman, 2011b; Gilraine et al., 2021).[7] We give 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.[8] 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).[9] 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 avoid some identification issues present within the literature.[10]

[6]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).

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

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

[9]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.

[10]For example, Figlio and Hart (2014) mentions that 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, and the researchers cannot disentangle the effects of school vouchers from the

Figlio et al. (2020) also studies the effects of voucher program expansion by leveraging the Florida Tax Credit Scholarship's growth from 2003 to 2018. The authors use variation in the growth of the program and pre-policy levels of local competition to estimate the intensive marginal 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 estimated off legislated changes in eligibility. Understanding the effects based on this dimension may be of particular interest as policymakers can set the limits for eligibility and voucher amount and cannot directly control the number of students participating.[12] We also examine changes in the quality of participating private schools, which is critical for examining voucher programs' total impact. 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 remainder 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, 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 offers conclusions from this research.

performance effects of accountability pressure.

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

[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 quality is still an open question (Hungerman and Rinz, 2016; Hungerman et al., 2019; Rinz, 2015).

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 participants) and eligibility (over 79% of families with children are eligible)[14]. Initially, the program capped participation at 5,000 and 7,500 students for the 2011-2012 and 2012-2013 AYs, 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 meet the income requirements and qualify under one of eight eligibility tracks.[15]

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 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. During the 2020-2021 school year, the average voucher amount for students in grades 1-8 was \$5,311 for students qualifying for the 90% voucher and \$3,094 for those 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.[16] 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

[14]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.

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

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

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 where we might expect to see larger impacts on school quality.

III Data

The data for this project come 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.[17] The database covers the 2005-2006 through 2017-2018 AY. We focus on students in schools that serve anyone in grades 3-8. Standardized testing is consistent between these grades and is required in both public and private schools in order to remain accredited (Indiana Code §20-32-5-17), which allows for a consistent sample across the sample years. Our dataset is advantageous because it includes information on private schools before ICSP was adopted. Many voucher programs require participating private schools to administer state exams once they accept voucher students, but this means testing data only exists in post-adoption. Indiana private schools had the incentive to be accredited before ICSP because it was required if a school wanted to participate in the Indiana Athletic Association (Association, 2021).[18]

Demographic information in the IDOE-CREO database varies depending on whether a student attends a public, private non-choice, or private choice school. (hereafter referred to

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

[18] Another advantage of our dataset is that we can observe students not born in the state of Indiana. Figlio et al. (2020) is restricted to conduct the analysis on students born in the state of Florida.

as public, private, and choice schools, respectively). 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 have information on whether a student would qualify for a 90% voucher as it is the same cutoff for free/reduced-price lunch. We have additional 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%), and the 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, 1,280 public elementary and middle schools and 178 choice schools remain.[19]

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 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 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.

[19] This restriction necessarily means the set of schools in our sample is positively selected. We discuss entry into and exit from the educational market in Appendix Section B1. We can make a few comparisons across private schools in and out of the sample using the Private School Universe Survey. Appendix Table A13 shows that private schools in the sample tend to be larger and have higher student-teacher ratios than private schools not included in the sample.

To calculate school VA, we run the following OLS regression:[20]

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

where $testscore_{ist}$ is the test score for a student i, at school s in 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 manner 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, the distribution of quality for choice schools is the right of public schools. The p-value from the Kolmogorov-Smirnov test confirms that the two distributions are not identical. 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.

[20] 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 school's exposure to the voucher policy relies on the radial distance between the physical address of each of the public schools in the sample and all of the eventual choice schools in Indiana. A public school is considered to face high exposure to the voucher policy if the nearest eventual choice school is within five miles of its location.[21] We find that around half of the public schools in the sample have at least one nearby choice school.[22] Public schools whose nearest choice competitor is outside the five-mile radius comprise our control group. Nearly all choice schools (over 98%) are located within five miles of a public school; therefore, we distinguish between high exposure and control choice schools by where they fall 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 218 in control schools. They also had a smaller share of their students identified as White, 65% versus 91%; had a larger share of students identified as Black; 16% versus 2%; and had a larger share of students qualify for subsidized lunches, 55% versus 42%.[23]

[21]The results are robust to this definition of having a competitor. Appendix Table A3 presents our results using 3, 5, 8, 10 and 15 miles as the required distance. Our results are relatively stable and remain statistically significant whether the definition for high exposure is set to 3, 5 or 8 miles. Beyond those values, our estimates lose significance. This result makes sense because, as shown in Figure 2, schools more than five miles away from a private school competitor see no changes in school value-added, so as they are added to the treatment group the average increase in VA falls.

[22]Appendix Figure A1 shows the distribution of the distance between each public school in our sample and their nearest choice school.

[23]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.

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 VA estimates, respectively. We find a similar pattern in the comparison between high exposure and control choice schools, presented in Table 2. Importantly, our empirical strategy does not rely on the equality 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 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 reflect what the schools facing high exposure would have experienced had the voucher policy not been implemented. While this assumption is ultimately untestable, we address this concern by reporting the results of an event-study specification that allows the effect of the voucher program to vary by years since implementation.

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

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

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.[24] 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.[25]

We visually test the validity of the common trends assumption by presenting a set of event-study 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., 2024); and (3) we do not use 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 models would bias our results.

[24]In Appendix Table A4 and 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.

[25]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 because clustering at the school level significantly increases our standard errors. We, therefore, continue with our preferred specification.

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. For schools facing high exposure, the distribution of school value-added after voucher adoption is clearly to the right of the distribution before the policy was implemented. For schools in the control group, the distributions are statistically indistinguishable.[26] The p-values for the Kolmogorov-Smirnov equality-of-distributions confirm the two distributions are not identical. While not a formal difference-in-difference design, Figure 2 provides a visual 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 row, we include an interaction term to indicate whether a school with a nearby choice competitor also had an above-median share of its students who qualified for subsidized lunches in the year before the voucher program was introduced.[27] Each column shows the results for an individual outcome of interest. Columns (1) and (2) present the results on overall school VA; 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 who qualified 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

[26] 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 A6.

[27] 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 automatically qualify for the voucher.

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. When we include the interaction terms in columns (4) and (6), the results show 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.[28]

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

ICSP was adopted and expanded in two separate academic years (2011-2012 and 2013-2014, respectively). One might then wonder if the two events 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. This figure shows a small and statistically significant jump in school math VA in the year of adoption of ICSP; however,

[28] 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 A7).

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, public schools do not seem to 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 extant 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 A8 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 could differ 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).[29]

Table 4 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 with above- and below-median enrollment for

[29] 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.

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 regardless of whether they have relatively small or large baseline enrollment.

In Panel B, we examine the differences in outcomes for public schools with above- or 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 imply that the changes we see in school quality are driven by the schools that 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.[30]

Panel C reports the effects on quality by the mean 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 versus 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 outcomes of interest. These findings imply that schools see similar improvements in quality when facing the potential threat of competition regardless of whether they are located in a relatively poor or rich neighborhoods.

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, explore whether high exposure public schools with Title I funding drive our results. Panel A of Appendix Table A9 reports the differences in outcomes

[30] 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 (A6) or non-FRPL students (A7)

for public schools with and without a Title I program in year before ICSP was adopted. Panel B of Appendix Table A9 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.[31] 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 we find 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,[32] 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 4 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

[31] 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.

[32] High-Quality certification is determined by standards set by No Child Left Behind. States can add their own requirements. In Indiana, HQ certification requires passing an additional exam to indicate proficiency in a certain subject.

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 specification. In Table 5, 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 1.75 teachers with a HQ certification when compared to the control group.[33] 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 6 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, 2011a). After the implementation of ICSP, public schools facing the threat of choice school competition saw increases in attendance with no changes in suspensions/expulsions. Specifically, high exposure public schools saw increases in attendance of 0.3 percentage points (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 saw no change 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.[34] Overall, we take these results as evidence that in response ICSP, schools are increasing quality such that we see improvements beyond changes in test scores.

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

[34] 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.

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 funding remain unchanged.[35] If the latter is the case in Indiana, increases in available school funds could contribute to our results.[36]

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.

In Appendix D1, we compare expenditures and revenues across school districts with and without at least one high exposure public school. While the aggregation to the district-level hides potentially important differences across schools, we do not find any evidence that districts with at least one high exposure public school saw any significant changes in their finances relative to districts without any high exposure public schools. We take these results as suggestive evidence that changes in public school finances do not explain our results. Future work will explore this question at a greater length.

[35] 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.

[36] 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.

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 made by schools or are driven by the composition of students that remain in the public schools. In this section, we present evidence suggesting 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 7 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.

We next address the concern of student sorting on ability. Figure 5 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.[37]

[37] We do not have information on whether students remaining in the public school qualify for a 50% voucher; hence the comparison made in Figure 5 also compares 50% voucher students to FRPL students. Appendix Figure A4 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

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:

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

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 8 reports the results of this exercise. We find no evidence that high exposure public schools were predicted to improve their quality based on 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 result as strong evidence that it is changes made by schools that drive the improvements in quality we see. We also recognize that this exercise can only speak to how changes in observable school characteristics may have affected our school-quality results. The concern remains that non-random student sorting on unobservable characteristics is driving 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 that 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.

that do not qualify for a voucher.

The concern over changes in student composition stems from the idea that these changes impact our estimates of school value-added through peer effects. In an attempt to shut down the peer effects channel, we use student-class links available in the IDOE-CREO database to create a sample of students in public schools that did not attend classes with any ever voucher students. The school value-added estimates stemming from this group of students would ameliorate this concern if we assume that for a given student, peer effects are driven solely by the other students in their classes. A caveat to this exercise is that information on student-class links are only available from the 2010-2011 through 2017-2018 academic years, which limits the sample to only one year of pre-policy data. Nevertheless, Appendix Table A10 reports the results of our difference-in-difference strategy using school-value added estimates from this sample. We continue to find that following the adoption of ICSP, high exposure public schools saw improvements in quality on all fronts, suggesting that changes in peer effects are unlikely to be driving our results.

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 high exposure and control groups before ICSP, (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 analysis 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.[38] 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.[39] Furthermore, Appendix Figure A3 reports the results of the event-study sensitivity analysis as proposed by Rambachan and Roth (2023). Our breakdown value for a significant effect on overall school value-added in the year the ICSP program expanded (2013-2014 academic year) is equal to one, meaning our result is robust to allowing for violations of parallel trends up to the max violation in the pre-treatment period. The breakdown value for our results on math value-added is greater than the value for our results on reading value-added, 1.5 and 0.5 respectively, suggesting our results on changes in math quality are more robust to violations in the parallel trends assumption.

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, the largest county in the state and the home of Indianapolis. We find consistent evidence that, regardless of dropping Marion County, the signs and general significance levels of the interaction term of interest hold as shown in Appendix Table A11. Appendix Table A12 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.

Another concern is 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 affected school quality. How-

[38] We further show the robustness of our results using placebo treatment years. Appendix Figure A5 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).

[39] Appendix Figure A5 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.

ever, 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 whether 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, 2014b,a).

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.[40] 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, 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. Instead, we compare choice schools that pull students from a large pool of public schools

[40]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.

to those with fewer public schools in the area. We can then assess whether choice schools responded differently to the voucher program based on the potential number of students they could receive.[41] 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)). Columns (1), (2), and (3) present the results on overall school value-added, school math value-added, and school reading value-added, respectively. 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 the 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 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-

[41] We also show results in Appendix Table A14 that alter the definition of high exposure for choice schools. Rather than distinguishing treatment and control based on the distribution of the number of public schools within five miles, (1) 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 and (2) Define high exposure by whether the choice school is within five miles of an initially low value-added public school. We find similar results under these specifications.

in-differences specification shown in Table 10. We find evidence that following the adoption of ICSP, high exposure choice schools saw a statistically significant increase of 0.83 in their student-teacher ratio (off a base mean of 14.22) compared to the control group[42] 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 impact 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.

Similar to the public school results, we may be concerned that changes in student composition drive the declines in school quality we find for participating private schools. Indeed, Appendix Figure A8 shows that students who who eventually use the voucher performed worse on standardized tests compared to students already attending the choice schools. However, for student composition to be the driving factor behind the declines in overall school quality, it must be the case that in years of improving quality (2014-2016) we would see large exits of voucher students from choice schools back to the public school system. Appendix Figure A9 shows that this is not the case. Specifically, we find that the years of improving choice school quality correspond to the years of lowest rates of returning to the public school system, suggesting student composition is not the main mechanism behind our results.

[42]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.

VII 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).[43] 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.

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

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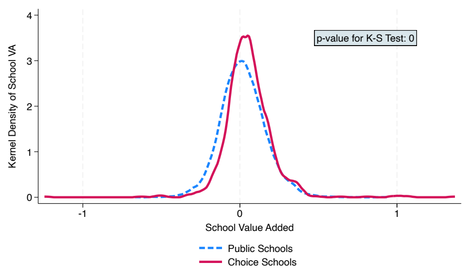
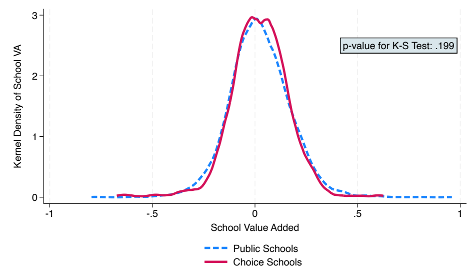
Figures

Figure 1: Kernel Density Plots - Public and Choice

(a) Pre-Policy



(b) Post-Expansion

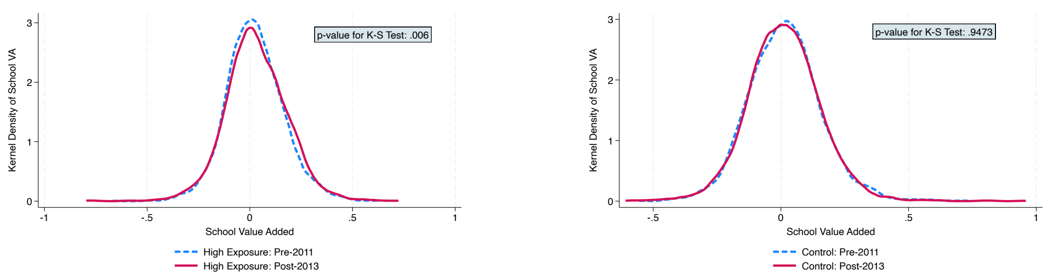


Notes: This figure depicts the kernel density plots of our school value-added (VA) estimates for the public and choice schools in our sample. Panel A shows the kernel density plots of schools in the years before the voucher program was implemented. Panel B shows those same estimates in the years after 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: School Value-Added Pre- and Post-Policy: Public

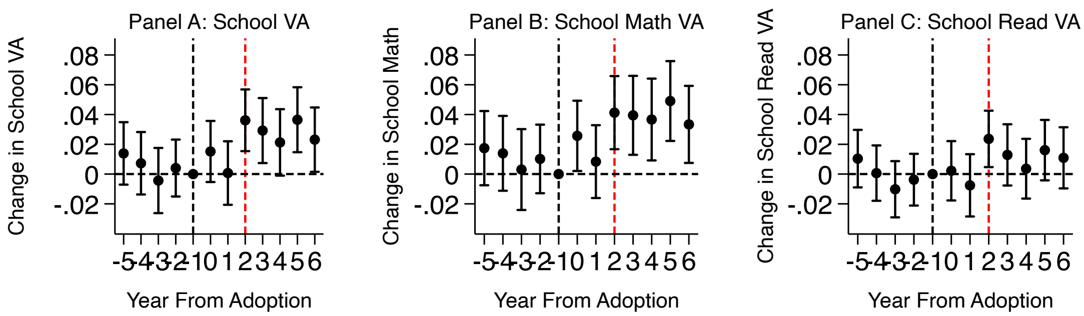
(a) High-Exposure Schools

(b) Control Schools



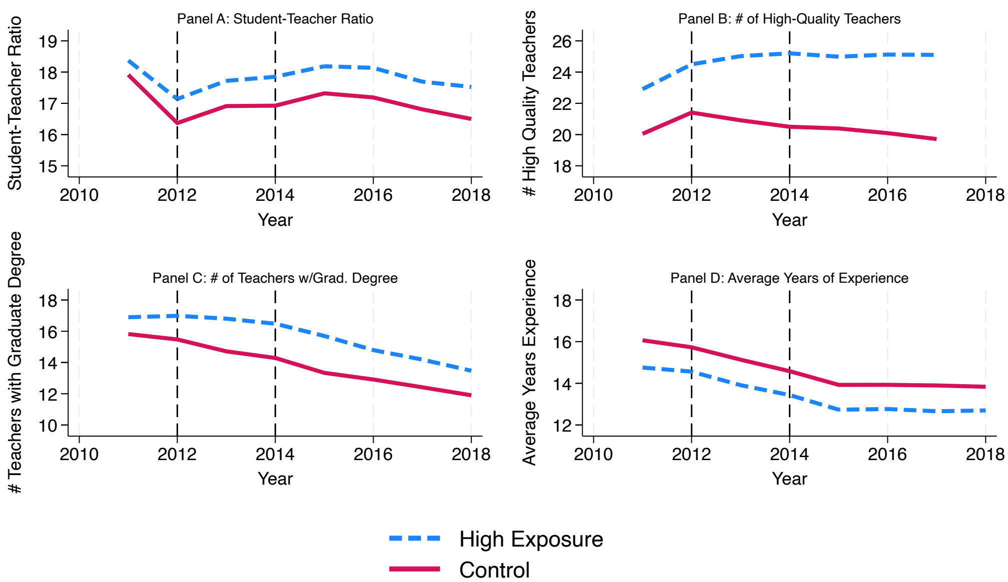
Notes: This figure depicts the kernel density plots of our school value-added (VA) estimates for the public schools in our sample. Each panel plots school VA across two time periods: pre-2011 and post-2013 to align with the policy time horizons. Panel A shows the kernel density plots of schools facing high-exposure to the policy. Panel B shows the kernel density plots for the control group. High-exposure is defined as having an eventual choice school within 5-miles of the school's location. School VA estimates are calculated using the OLS regression described in Equation (1). Data on test score 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



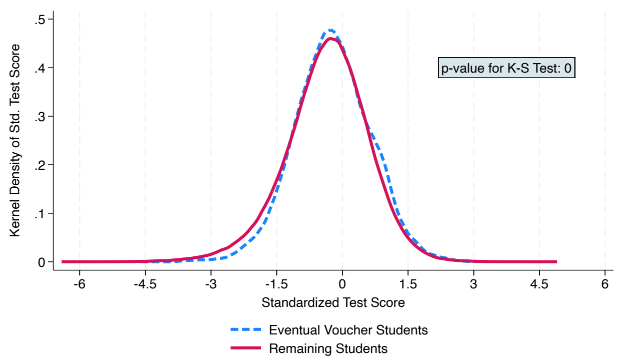
Notes: This figure presents the event-study estimates from Equation (3). Figure 3(a) plots the estimates for overall school value-added, Figure 3(b) plots the estimates for school math value-added and Figure 3(c) plots the estimates for school reading value-added. Each figure is the result of a separate 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 eventual choice school. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Figure 4: Public School Inputs



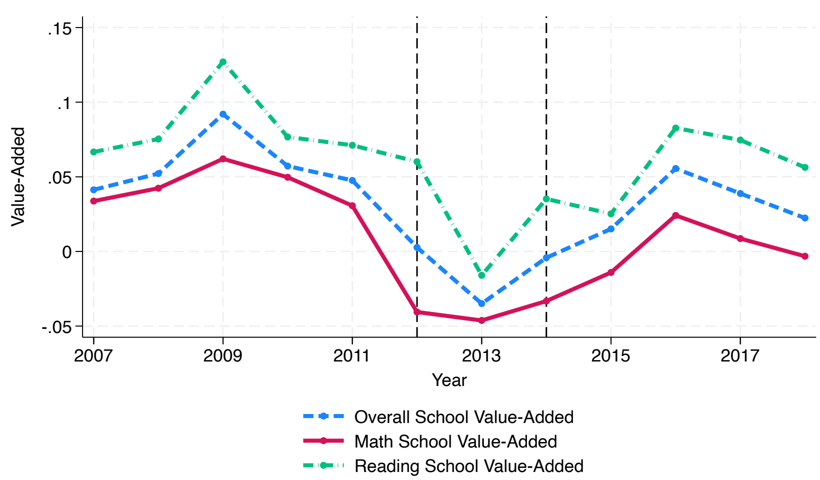
Notes: This figure presents the average student-teacher ratio (Panel A), number of high-quality teachers (Panel B), number of teachers with a graduate degree (Panel C) and average years of experience of teachers (Panel D) across public schools in the sample. High-exposure is defined as having an eventual choice school 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 5: 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 was adopted. 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.

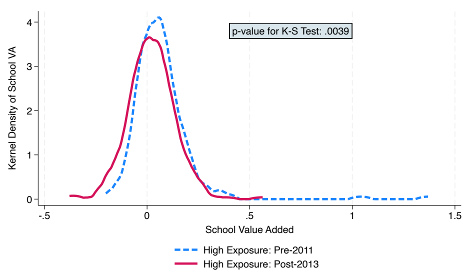
Figure 6: Choice School Value-Added



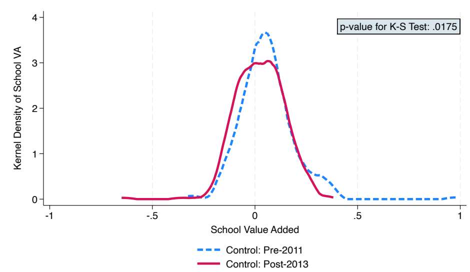
Notes: This figure depicts the average school value-added (VA) estimates across all choice schools in each year of the sample. School VA estimates are calculated using the OLS regression described by Equation (1). Data on test scores and enrollment come from the IDOE-CREO database. The black, dashed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

Figure 7: School Value-Added Pre- and Post-Policy: Choice

(a) High-Exposure Schools

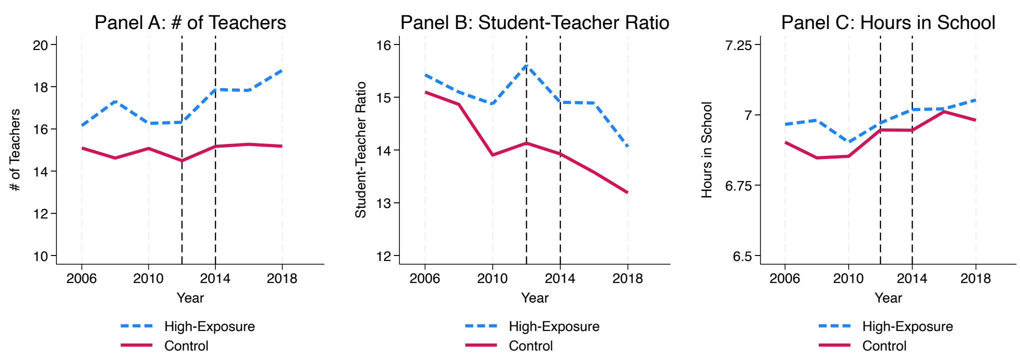


(b) Control Schools



Notes: This figure depicts the kernel density plots of our school value-added (VA) estimates for the choice 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 the number of public schools within 5 miles. School VA estimates are calculated using the OLS regression described in 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 average 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 available in every other year. The black, dashed lines represent the years the voucher program was implemented and expanded.

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	218	44***
	(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	553	

Notes: This table presents summary statistics for the set of schools identified as either high exposure or control 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	

Notes: This table presents summary statistics for the set of schools identified as either high exposure or control in the year before the voucher policy was implemented. High exposure is defined as being in the top tercile of the distribution of number of pubilc schools within five miles. 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	15,360	15,360	15,360	15,360	15,360	15,360
R-squared	0.448	0.449	0.433	0.434	0.455	0.456

Notes: 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: 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	15,360	15,360	15,360
R-squared	0.448	0.434	0.455
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	15,360	15,360	15,360
R-squared	0.450	0.436	0.456
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	15,360	15,360	15,360
R-squared	0.448	0.433	0.455

Notes: 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. A school in a poor (rich) neighborhood is defined as one that is located in a census block group that falls below (above) the median for average income in 2010. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

Table 5: 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
$Post_t \cdot HighExp_s$	0.09	0.67***	1.75**	-0.167
	(0.14)	(0.24)	(0.31)	(0.15)
Observations	9,963	10,179	10,179	10,179
Baseline Mean	18.37	16.90	22.91	14.76

Notes: 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 6: DiD Results on Attendance and Suspension Measures

VARIABLES	(1) Percent Days Attend	(2) Total Days Attend	(3) Percent Expelled or Suspended
$Post_t \cdot HighExp_s$	0.335***	0.531**	-0.000
	(0.109)	(0.202)	(0.002)
Observations	15,348	15,348	15,348
Baseline Mean	88.81	159.9	5.62

Notes: 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 7: 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	15,360	15,360	15,360	15,360	15,360
Baseline Mean	49.58	76.30	9.607	8.082	49.20

Notes: 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 having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database.

Table 8: 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	15,360	15,360	15,360
R-squared	0.354	0.274	0.427

Notes: 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 9: DiD Results Using Choice Schools

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	-0.009	-0.011	-0.013
	(0.018)	(0.022)	(0.014)
Observations	2,136	2,136	2,136

Notes: 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
$Post_t \cdot HighExp_s$	-0.150	0.830**	-0.061
	(0.857)	(0.399)	(0.053)
Observations	988	988	988
Baseline Mean	15.46	14.22	6.869

Notes: 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.

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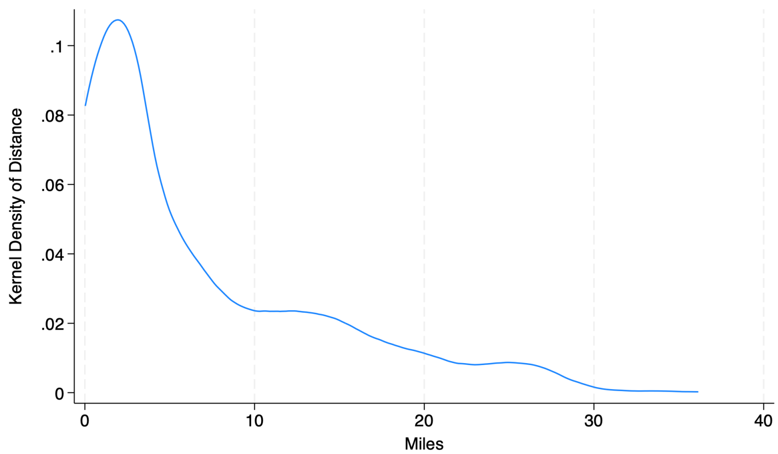
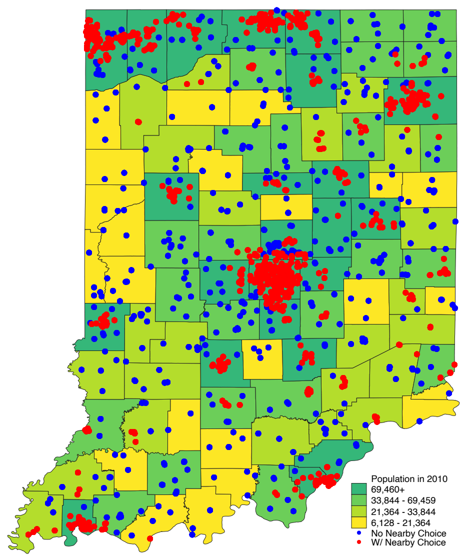
Figure A1: Kernel Density Plot of Distance to Nearest Choice School



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

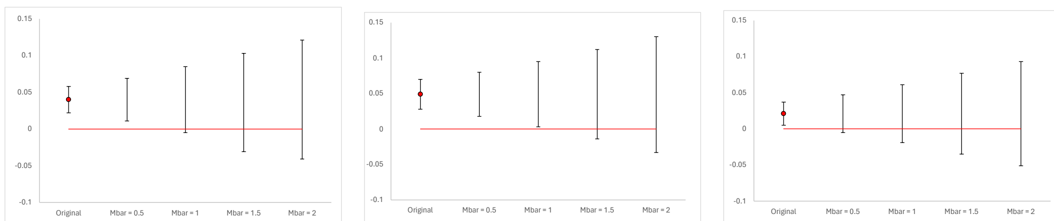
Figure A2: Locations of High Exposure and Control Public Schools



Notes: This figure plots the location of each public school in our sample across Indiana. The red dots indicate the public schools that have an eventual 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: Parallel Trends Sensitivity

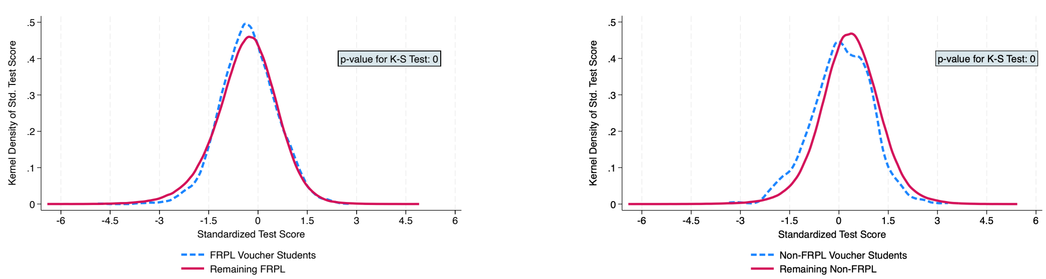
(a) Overall School Value-Added (b) Math Value-Added (c) Reading Value-Added



Notes: This figure depicts the results of the event-study sensitivity analysis proposed in Rambachan and Roth (2023). Each panel plots the robust 95% confidence sets for the treatment effect in the year the Indiana Choice Scholarship Program was expanded (2013-2014 academic year) using different values of the maximum post-treatment violation of parallel trends between consecutive periods (Mbar). Panel A shows these results for overall school value-added, Panel B shows these results for math school value-added and Panel C shows these results for reading school value-added. The breakdown value corresponds to the first value of Mbar that results in a confidence set that includes zero, suggesting a statistically insignificant treatment effect.

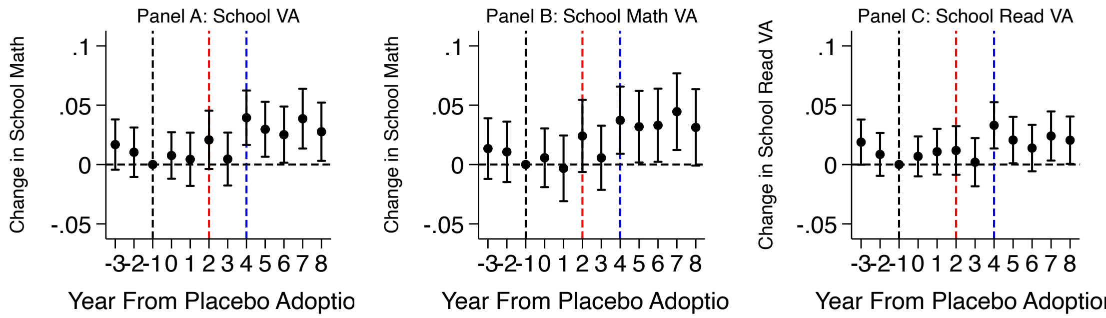
Figure A4: Student Sorting Across FRPL Status

(a) FRPL Voucher vs. FRPL Remain (b) Non-FRPL Voucher vs. Non-FRPL Remain



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.

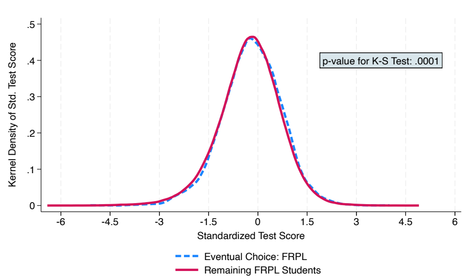
Figure A5: Placebo Event-Study Results of Voucher Policy



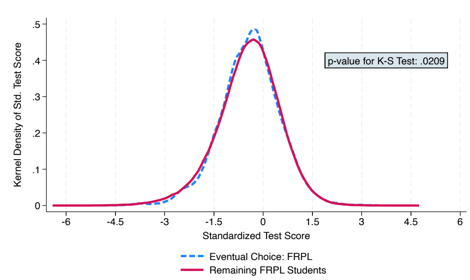
Notes: This figure presents the event-study estimates using placebo treatment years. Figure A5(a) plots the estimates for overall school value-added, Figure A5(b) plots the estimates for school math value-added and Figure A5(c) plots the estimates for school reading value-added. Each figure is the result 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 A6: Student Sorting High vs. Low VA Schools: FRPL

(a) Initially High VA Public School



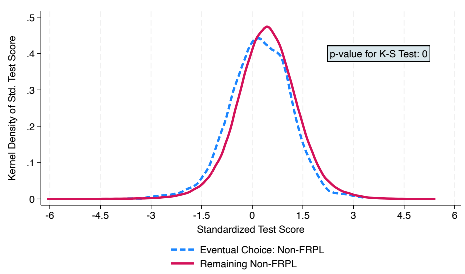
(b) Initially Low VA Public School



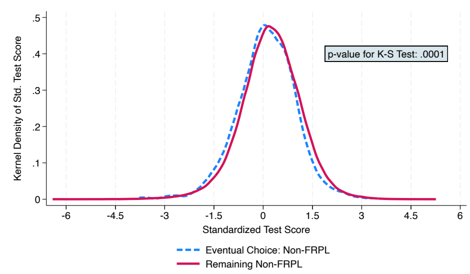
Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public school in the years before the voucher program. Each panel plots the test scores for FRPL students who use a voucher and those that remain in the public school despite qualifying for a 90% 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 initially 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: Student Sorting High vs. Low VA Schools: Non-FRPL

(a) Initially High VA Public School

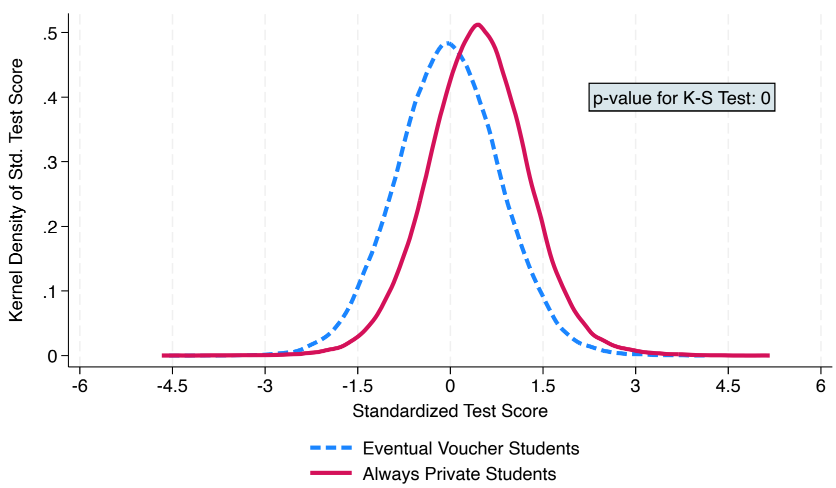


(b) Initially Low VA Public School



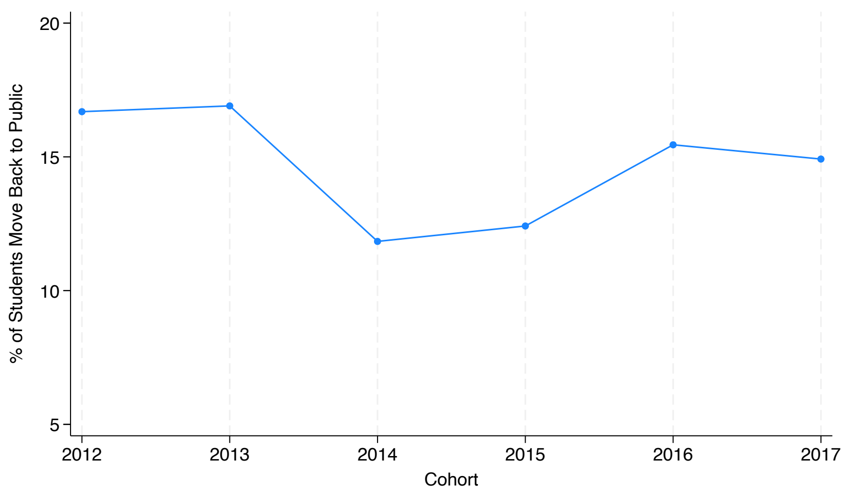
Notes: This figure depicts the kernel density plots of standardized test scores for the students attending public school in the years before the voucher program. Each panel plots the test scores for students who eventually use a voucher and those that remain in the public school. Neither group ever qualifies for FRPL. 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 initially 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 A8: Density Plots of Standardized Test Scores - Voucher vs. Always Private



Notes: This figure depicts the kernel density plots of standardized test scores for the students attending a choice school and had test scores in the years before the voucher program. The figure plots the test scores for students who eventually use a voucher and those that always attended the private 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 A9: Percent of Voucher Students Moving Back to Public Schools



Notes: This figure depicts the percent of voucher students that return back to the public school system in the following year. Data on enrollment come from the IDOE-CREO database.

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	15,360	15,360	15,360
R-squared	0.450	0.438	0.442

Notes: 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 (Kane and Staiger, 2008).

Table A2: DiD Results Varying School VA Estimation

VARIABLES	(1) Baseline	(2) School-Year FE Only	(3) Including Demographics	(4) 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	15,360	15,360	15,360	15,360

Notes: 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 having at least one nearby choice school. Data on enrollment come from the IDOE-CREO database.

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

	(1) Within 3 miles	(2) Within 5 miles	(3) Within 8 miles	(4) Within 10 miles	(5) 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	15,360	15,360	15,360	15,360	15,360
R-squared	0.448	0.448	0.447	0.447	0.447
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	15,360	15,360	15,360	15,360	15,360
R-squared	0.434	0.433	0.432	0.432	0.432
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	15,360	15,360	15,360	15,360	15,360
R-squared	0.455	0.455	0.455	0.454	0.454

Notes: 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*	0.0227***	0.016**	0.0295***	-0.002	0.0131***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.004)	(0.005)
Observations	15,360	15,360	15,360	15,360	15,360	15,360
R-squared	0.444	0.448	0.429	0.4333	0.451	0.455
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Covariates	No	Yes	No	Yes	No	Yes

Notes: 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	15,360	15,360	15,360
R-squared	0.448	0.433	0.455
Avg. Num. Close Schools	4	4	4

Notes: 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 eventual 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: 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

Notes: 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 A7: 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

Notes: 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 eventual choice school within 3 miles. Control public schools are those that do not have an eventual choice school within 8 miles. Data on enrollment come from the IDOE-CREO database.

Table A8: 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	3,754,754	3,754,754	3,754,754
R-squared	0.773	0.705	0.670

Notes: 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 A9: 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	15,360	15,360	15,360
R-squared	0.449	0.434	0.457
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	15,360	15,360	15,360
R-squared	0.449	0.434	0.457

Notes: 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 A10: DiD Results Estimating School VA Off Students Without Voucher Classmates

VARIABLES	(1) School Value-Added	(2) School Math Value-Added	(3) School Reading Value-Added
$Post_t \cdot HighExp_s$	0.035***	0.042***	0.027***
	(0.008)	(0.010)	(0.008)
Observations	10,120	10,120	10,120
R-squared	0.481	0.467	0.457

Notes: 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 2010-2011 academic year. High exposure is defined as having at least one eventual choice school within 5 miles. School value-added is calculated using Equation (1) on the set of students that did not ever have a class with an eventual voucher student. Data on enrollment, student-class links and test scores come from the IDOE-CREO database.

Table A11: 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

Notes: 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 VA is calculated using Equation (1).

Table A12: DiD Results Dropping Each County in Indiana

County Dropped	Estimate	Standard Dev.	Observations	County Dropped	Estimate	Standard Dev.	Observations
Adams	0.023***	(0.006)	15,300	Lawrence	0.024***	(0.006)	15,192
Allen	0.024***	(0.006)	14,580	Madison	0.023***	(0.006)	15,228
Bartholomew	0.022***	(0.006)	15,204	Marion	0.019***	(0.006)	13,692
Benton	0.023***	(0.006)	15,324	Marshall	0.024***	(0.006)	15,228
Blackford	0.023***	(0.006)	15,324	Martin	0.023***	(0.006)	15,336
Boone	0.022***	(0.006)	15,192	Miami	0.023***	(0.006)	15,288
Brown	0.023***	(0.006)	15,312	Monroe	0.023***	(0.006)	15,132
Carroll	0.023***	(0.006)	15,324	Montgomery	0.022***	(0.006)	15,240
Cass	0.022***	(0.006)	15,264	Morgan	0.023***	(0.006)	15,156
Clark	0.023***	(0.006)	15,120	Newton	0.022***	(0.006)	15,300
Clay	0.023***	(0.006)	15,252	Noble	0.023***	(0.006)	15,240
Clinton	0.022***	(0.006)	15,276	Ohio	0.023***	(0.006)	15,348
Crawford	0.023***	(0.006)	15,312	Orange	0.023***	(0.006)	15,300
Daviess	0.023***	(0.006)	15,252	Owen	0.023***	(0.006)	15,300
Dearborn	0.023***	(0.006)	15,240	Parke	0.022***	(0.006)	15,276
Decatur	0.022***	(0.006)	15,288	Perry	0.023***	(0.006)	15,312
Dekalb	0.023***	(0.006)	15,240	Pike	0.023***	(0.006)	15,324
Delaware	0.024***	(0.006)	15,096	Porter	0.024***	(0.006)	14,880
Dubois	0.023***	(0.006)	15,204	Posey	0.021***	(0.006)	15,276
Elkhart	0.022***	(0.006)	14,868	Pulaski	0.022***	(0.006)	15,312
Fayette	0.022***	(0.006)	15,276	Putnam	0.023***	(0.006)	15,252
Floyd	0.023***	(0.006)	15,216	Randolph	0.023***	(0.006)	15,240
Fountain	0.023***	(0.006)	15,312	Ripley	0.023***	(0.006)	15,240
Franklin	0.023***	(0.006)	15,312	Rush	0.023***	(0.006)	15,324
Fulton	0.022***	(0.006)	15,300	Scott	0.023***	(0.006)	15,276
Gibson	0.023***	(0.006)	15,252	Shelby	0.023***	(0.006)	15,228
Grant	0.023***	(0.006)	15,204	Spencer	0.023***	(0.006)	15,276
Greene	0.023***	(0.006)	15,252	St Joseph	0.025***	(0.006)	15,060
Hamilton	0.022***	(0.006)	14,796	St. Joseph	0.022***	(0.006)	15,036
Hancock	0.023***	(0.006)	15,216	Starke	0.022***	(0.006)	15,300
Harrison	0.023***	(0.006)	15,228	Steuben	0.022***	(0.006)	15,264
Hendricks	0.022***	(0.006)	15,072	Sullivan	0.023***	(0.006)	15,288
Henry	0.022***	(0.006)	15,204	Switzerland	0.023***	(0.006)	15,324
Howard	0.022***	(0.006)	15,180	Tippecanoe	0.022***	(0.006)	15,048
Huntington	0.023***	(0.006)	15,264	Tipton	0.023***	(0.006)	15,312
Jackson	0.022***	(0.006)	15,216	Union	0.023***	(0.006)	15,324
Jasper	0.023***	(0.006)	15,300	Vanderburgh	0.021***	(0.006)	15,048
Jay	0.024***	(0.006)	15,252	Vermillion	0.023***	(0.006)	15,300
Jefferson	0.023***	(0.006)	15,288	Vigo	0.024***	(0.006)	15,072
Jennings	0.023***	(0.006)	15,276	Wabash	0.025***	(0.006)	15,264
Johnson	0.021***	(0.006)	15,036	Warren	0.023***	(0.006)	15,312
Knox	0.021***	(0.006)	15,264	Warrick	0.023***	(0.006)	15,216
Kosciusko	0.022***	(0.006)	15,168	Washington	0.023***	(0.006)	15,300
LaGrange	0.023***	(0.006)	15,336	Wayne	0.023***	(0.006)	15,192
LaPorte	0.024***	(0.006)	15,036	Wells	0.023***	(0.006)	15,276
Lagrange	0.023***	(0.006)	15,252	White	0.023***	(0.006)	15,264
Lake	0.020***	(0.006)	14,340	Whitley	0.023***	(0.006)	15,264

Notes: 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 A13: Summary Statistics of Private Schools In and Out of the Sample

	(1)	(2)	(3)
# of Students Enrolled	253	84	-169***
	(155)	(131)	
# of Teachers	18	8	-10***
	(10)	(11)	
% White Students	84.11	86.16	2.06
	(21.16)	(23.89)	
% Black Students	3.47	7.27	3.80**
	(9.36)	(19.37)	
% Hispanic Students	7.25	3.35	-3.90**
	(16.07)	(9.01)	
Student-Teacher Ratio	14.23	12.85	-1.38***
	(3.15)	(6.76)	
% Full-Time Teachers	88.17	90.31	2.13*
	(9.78)	(14.33)	
N	175	437	612

Notes: This table presents summary statistics for the set of schools in and out of the private school sample in the year before the voucher policy was implemented. Data on private schools come the Private School Universe Survey. Three choice schools in the sample are not found in the Private School Universe Survey.

Table A14: 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
Panel A: Above Median FRPL Public School Student within 5 miles			
$Post_t \cdot HighExp_s$	-0.009	-0.019	-0.002
	(0.016)	(0.020)	(0.013)
Observations	2,136	2,136	2,136
Panel B: Within 5 Miles of Initially Low Value-Added Public School			
$Post_t \cdot HighExp_s$	0.009	0.006	0.007
	(0.014)	(0.017)	(0.012)
Observations	2,136	2,136	2,136

Notes: 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. Data on enrollment come from the IDOE-CREO database and school value-added is calculated using Equation (1).

B1 Appendix - Entry and Exit of Schools

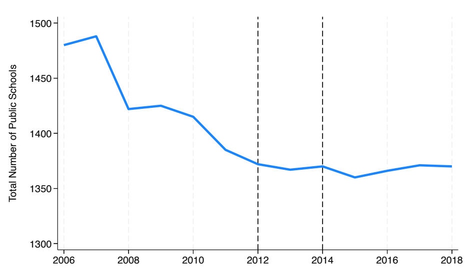
Entry and exit into the market is an important supply-side response to consider when evaluating a voucher program. The goal of this appendix section is to illustrate that the Indiana Choice Scholarship Program did not induce significant entry or exit for either public or private schools.

Number Public and Private Schools

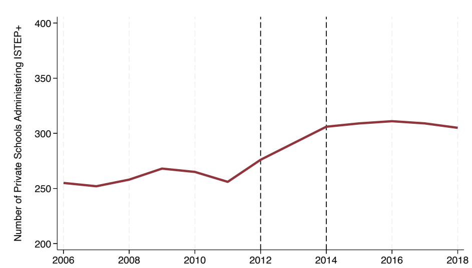
To understand how the number of schools has changed over the sample period, Figure B1 plots the number of public and private schools in each year from 2006-2019, respectively. Using the Common Core of Data, we find at the start of our sample period there are 1,480 public school serving grades 3-8 across the state. That number falls to 1,370 by the end sample. Importantly, there does not seem to be a significant change in the number public schools around policy adoption or expansion. Using the ISTEP+ data, we find at the start of our sample there are 255 private schools across the state serving grades 3-8. That number rises to 305 by the end of the sample period. We do find some evidence that following the adoption of ICSP there was a brief period of time where new private schools started testing their students with ISTEP+. However, we cannot distinguish whether these are new private schools or were existing private schools that started testing students in order to qualify for the voucher program.

Figure B1: Public and Private Schools Across the State

(a) Number of Public Schools



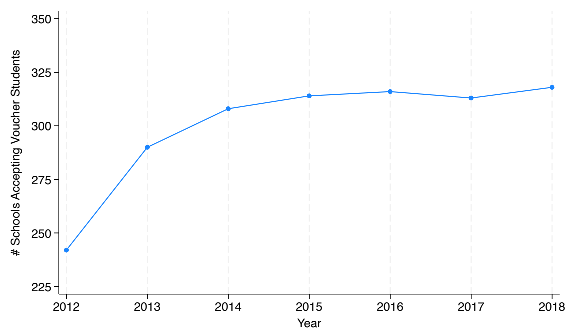
(b) Number of Private Schools



Notes: This figure plots the total number of public (Panel A) and private schools using the ISTEP+ exam (Panel B) in the state of Indiana in each year from 2006-2018. Data is shown only for schools that cater to grades 3-8. Data on the number of schools comes from the NCES Common Core of Data and the IDOE-CREO database.

Figure B2 examines the change in the total number of private schools accepting voucher students across our sample period including both primary, secondary and high schools. While there is a steady increase in the number of private schools accepting voucher students in the first couple of years of the program, since the expansion of the program the number of choice schools has remained steady.

Figure B2: Number of Private Schools Accepting Voucher Students



Notes: This figure plots the total number of private schools in the state of Indiana that accept voucher students for each year from 2012-2018. Data is shown for all schools catering to grades K-12. Data on the number of schools comes from the IDOE-CREO database.

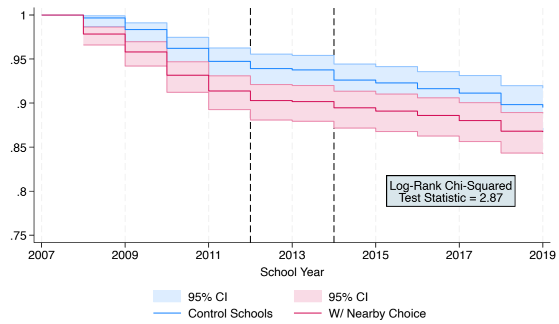
Closures of Traditional Public Schools

We pay particular attention to the closure of traditional public schools because as discussed in Chen and Harris (2022) these events could induce student sorting that biases our results.[44] We investigate this potential mechanism by first examining if public schools located near a choice school saw a differential increase in their likelihood to close. We estimate a Kaplan-Meier survivor function as shown in Figure B3. 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.

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

[44] Chen and Harris (2022) explore this idea in the context of charter school penetration.

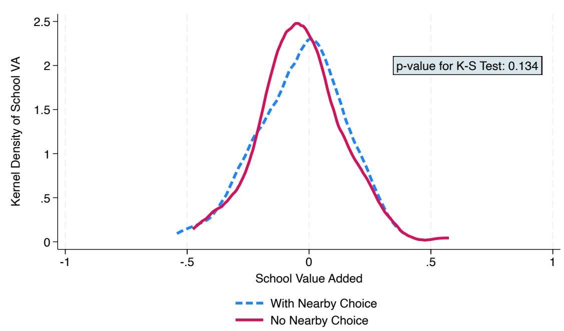
Figure B3: Survivor Model for Public School Closures



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 an eventual choice school with five miles of its location. The chi-squared test statistic for the log-rank test of equality is reported. The black, dashed lines represent the years the voucher program was implemented and expanded.

the distributions of school value-added are equal between these two groups of closed public schools in the years before they close. Figure B4 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 B4: Kernel Density Plot of School VA- Closed Schools



Notes: This figure depicts the kernel density plots of our school value-added (VA) estimates for the set of public schools that closed during the sample period. The blue, dashed line shows the kernel density plot for the public schools that were within five miles of an eventual choice school. The red, solid line shows the same estimate for public schools farther from an eventual choice school. School VA estimates are calculated using the OLS regression described in 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.

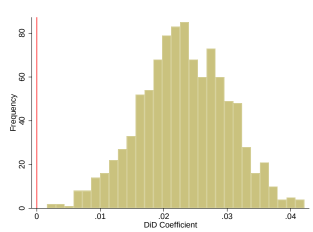
C1 Appendix - Bootstrapping

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.

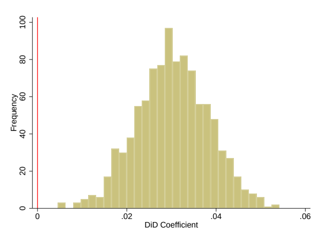
In each of 1000 iterations, we sample 100 students (with replacement) within each school to be included in the value-added regression described in Equation (1). Therefore, each iteration returns a unique set of school value-added measures that we can use in our difference-in-differences specification. 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.

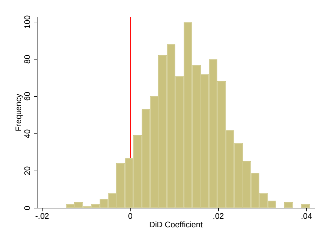
Figure C1: Bootstrapped DiD Results



(a) School Overall VA



(b) School Math VA



(c) School Reading VA

Notes: This figure depicts the histogram of results from our bootstrapping exercise. Panel A shows the results on Overall School Value-Added (VA), Panel B shows the results on School Math VA, and Panel C shows the results on School Reading VA. A description of the bootstapping exercise can be found in Appendix C1.

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]

Notes: 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 - Public School Finances

It is important to understand how private school voucher policies impact public school financing. Opponents of school choice policies often argue that the adoption of these programs would drain public school finances through direct cuts in state funding (Strauss, 2017). Unfortunately, school-level finance data is not available for a majority of our sample period. In this appendix, we will explore changes in public school funding at the district level.

Revenues

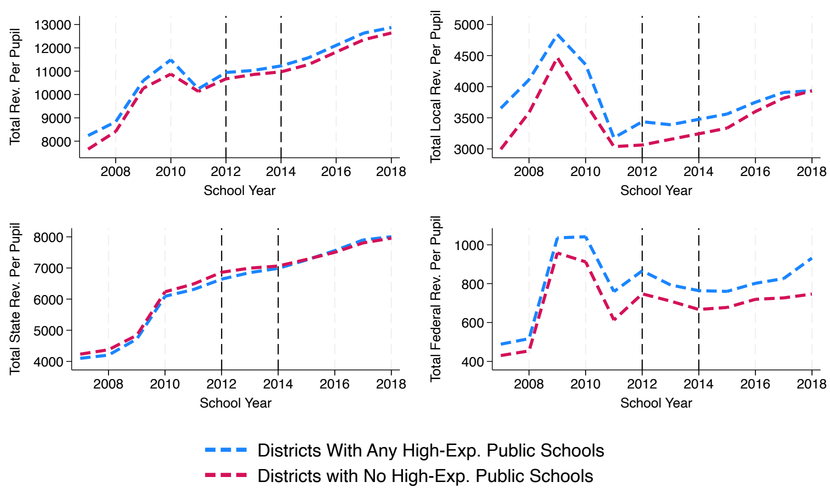
To understand how public school revenues changed during our sample, Figure D1 plots total revenues for school districts with and without a high exposure public school. There are 259 school districts across the state of Indiana and nearly 30% of them do not have a high exposure public school. Figure D1 plots the average total revenue per-pupil (Panel A) as well as average total revenues per-pupil by source: local (Panel B), state (Panel C) and federal (Panel D). Across each of the panels, we do not find strong evidence that public school finances differed significantly across public school districts with and without a high exposure public school. Table D1 formalizes this comparison using a difference-in-differences framework. Specifically, we run the following model:

$$\begin{aligned} Y_{dt} = \beta_1 Post_t \cdot HasHighExposure_d + \sum_{t=2007}^{2018} \eta_t (\mathbb{1}\{year = t\} * X_d^{2007}) \\ + \pi_d + \lambda_t + \mu_{dt} \end{aligned} \tag{6}$$

where Y_{dt} is the total revenue per-pupil in a district d at year t; $Post_t$ is an indicator that equals one in the years after the voucher policy was introduced; $HasHighExposure_d$ is an indicator that equals one if the public school district is identified as having at least one high exposure public school; π_d is a school district fixed effect that removes any time-invariant characteristics about the district that could otherwise bias our results; λ_t is a standard year fixed effect and μ_{dt} is our idiosyncratic error term. η_t captures the potentially time-varying effects of X_d^{2007}, a vector of initial district-level characteristics including number of public school students in the district, and demographic information about the students that attend public schools in the district.

After the implementation of ICSP, school districts with at least one high exposure public school did not see significant changes in public school revenues compared to the control districts. The only marginally statistically significant coefficient is on state revenues per-pupil and when considering the baseline mean of $6,409, the estimate is economically small ($< 2\%$). In fact, we can rule out any effect larger than effect than 3.5%.

Figure D1: Public School District Revenues



Notes: This figure presents the average total revenues per-pupil (Panel A), local revenues per-pupil (Panel B), state revenues per-pupil (Panel C) and federal revenues per-pupil (Panel D) across public school districts in the state. Having any high exposure public schools is defined as the school district containing at least one public school with an eventual choice school within five miles of the school's location. Data on public school district funding come from the Elementary/Secondary Information System (ElSi).

Table D1: DiD Results: School District Revenues

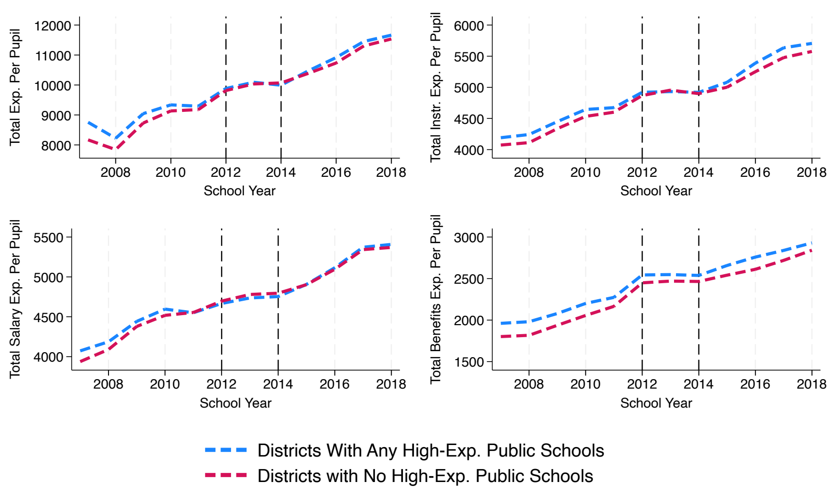
	(1)	(2)	(3)	(4)
VARIABLES	Total Rev. Per-Pupil	Local Rev. Per-Pupil	State Rev. Per-Pupil	Federal Rev. Per-Pupil
$Post_t \cdot HasHighExp_d$	44.30	-62.12	101.21*	5.15
	(113.22)	(84.67)	(60.15)	(33.14)
Observations	3,108	3,108	3,108	3,108
Baseline Mean	10,180	3,096	6,409	674.5

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the district level. Each coefficient is the result of a separate estimation. All regressions include district and year fixed effects. Baseline covariates include the share of students that are section 504, special education, and district public school enrollment in the 2006-2007 academic year. Having at least one high exposure public school is an indicator for whether the school district includes at least one public school within five miles of an eventual choice school. Data come from the Elementary/Secondary Information System (ElSi).

Expenditures

To understand how public school expenditures changed during our sample, Figure D2 plots average total expenditures per-pupil for school districts with and without a high exposure public school. Figure D2 plots the average total expenditure per-pupil (Panel A) as well as average total expenditures per-pupil by source: instruction (Panel B), teacher salaries (Panel C) and employee benefits (Panel D). Across each of the panels, we do not find strong evidence that public school expenditures differed significantly across public school districts with and without a high exposure public school. Table D2 confirms these patterns with the results from our difference-in-differences specifications. In table D2, we report the results of Equation 6 using our measures of school district expenditures. We do not find any statistically significant changes in school district expenditures across our two groups following the implementation of ICSP. We take these results as evidence that school financing does not drive the improvement we see in public schools.

Figure D2: Public School District Expenditures



Notes: This figure presents the average total expenditures per-pupil (Panel A), instructional expenditures per-pupil (Panel B), salary expenditures per-pupil (Panel C) and benefits expenditures per-pupil (Panel D) across public school districts in the state. Having any high exposure public schools is defined as the school district containing at least one public school with an eventual choice school within five miles of the school's location. Data on public school district funding come from the Elementary/Secondary Information System (ElSi).

Table D2: DiD Results: School District Expenditures

VARIABLES	(1) Total Exp. Per-Pupil	(2) Instruction Exp. Per-Pupil	(3) Salary Exp. Per-Pupil	(4) Benefit Exp. Per-pupil
$Post_t \cdot HasHighExp_d$	-31.10	19.41	-46.49	18.18
	(113.85)	(61.21)	(52.07)	(40.38)
Observations	3,108	3,108	3,108	3,108
Baseline Mean	9,228	4,632	4,552	2,211

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the district level. Each coefficient is the result of a separate estimation. All regressions include district and year fixed effects. Baseline covariates include the share of students that are section 504, special education, and district public school enrollment in the 2006-2007 academic year. Having at least one high exposure public school is an indicator for whether the school district includes at least one public school within five miles of an eventual choice school. Data come from the Elementary/Secondary Information System.