Evaluating the Impact of Voucher Programs on Incumbent Students in Participating Private Schools

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Abstract

The rapid expansion of private school choice programs has renewed policy interest in their broader effects, yet existing evidence focuses primarily on participating students or those remaining in public schools. We examine effects on incumbent students in voucher-participating private schools using a mixed methods approach. Leveraging student-level longitudinal data in Indiana, which has one of the nation's largest voucher programs, we estimate the impact of variation in exposure to voucher peers on the achievement of incumbent students. We find that higher shares of voucher students are associated with declines in test scores, particularly in the early years of the program and among incumbent students at the tails of the achievement distribution. Contemporaneous teacher and student interview evidence suggests these effects reflect a combination of increased student academic heterogeneity, instructional adaptation, and changes in peer dynamics. These findings suggest that early stages of voucher program implementation can generate short-run disruptions to private school classroom environments, with implications for how new and expanding programs are designed and evaluated.

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I Introduction

Private school voucher programs are becoming an increasingly popular policy to increase access to schooling options in the United States. Since their inception in 1991, there has been a large increase in the number of state funded programs and a growth in the share of students eligible to participate. As a result, a large literature examines the impacts of these programs on students using vouchers (Rouse, 1998; Mayer et al., 2002; Howell et al., 2002; Wolf et al., 2010; Witte et al., 2014; Chingos and Peterson, 2015; Abdulkadiroğlu et al., 2018; Waddington and Berends, 2018) and the competitive effects on students remaining in the public school system (Hoxby, 2003; Figlio and Rouse, 2006; Chakrabarti, 2008; Chiang, 2009; Greene and Marsh, 2009; Chakrabarti, 2013; Rouse et al., 2013; Figlio and Hart, 2014; Chingos et al., 2025). However, much less attention has been focused on how voucher programs impact participating private schools and their existing students.[1]

One way voucher programs could directly impact the educational outcomes of incumbent private school students is through the introduction of new peer groups (Epple and Romano, 1998). Students using a voucher to transition from a public to private school (from hereon referred to as voucher students) may face disruption effects, which could divert school resources away from incumbent students. Alternatively, if voucher students are academically motivated or come from families who are highly engaged, there may be a positive effect on incumbent students. Understanding whether and to what extent the introduction of voucher peers impacts incumbent private school students is essential for understanding the total impact of private school voucher programs.

The challenge with empirically examining the impact of voucher peers on the educational performance of incumbent private school students is both the lack of data on private school students and methodological limitations due to non-random sorting. Our study seeks to overcome these issues by leveraging a rich set of student-level longitudinal data, which includes demographic, achievement, and attendance information for all students in voucher-participating private schools. To deal with the selection issue, we make use of the cross-grade, within-school variation in the share of students using a voucher and show that this variation in peers is uncorrelated with observed student characteristics including predicted test scores.

Our context centers around the Indiana Choice Scholarship Program (ICSP), one of the largest private school voucher programs in the United States, which was adopted in the 2011 and expanded in 2013. We begin with student-level testing data that covers all students in the state between the 2011-2012 and 2017-2018 academic years (AY). We use these data to construct individual-level measures of standardized test scores for non-voucher private school students and school-grade-cohort

[1] Notable exceptions include studies that examine the effects of these policies on private school enrollment, finances, school inputs, and quality (Rinz, 2015; Hungerman and Rinz, 2016; Adger and Felegi, 2025).

measures of the share of voucher students.[2] We define a voucher student as one that uses a voucher to attend the participating private school after having attended a public school for at least one academic year. Using this dataset, we estimate the effects of voucher peers on incumbent students, using a fixed effects model that relies on the plausibly exogenous variation in the proportion of voucher students in a school by grade cohort.

We complement the administrative data analysis with an analysis of two waves of interviews conducted in nine voucher-participating private schools during the same policy period (first wave, 2012-13; second wave 2017-18 and 2018-19). We use these data to provide direct evidence on classroom conditions and instructional processes that are difficult to observe in administrative records. Specifically, we analyze teacher and incumbent non-voucher student interview transcripts using a structured deductive coding framework to document how educators and students describe classroom composition, instructional practices, and peer dynamics as voucher enrollment increases. This qualitative evidence provides contemporaneous insight into classroom processes and experiences that may plausibly explain any observed peer effects on incumbent student achievement. In doing so, it helps contextualize how schools and classrooms adjust to expanding voucher participation and the ways in which these adjustments may affect students who were already enrolled in private schools.

From our fixed effects approach, we find that, on average, moving from a cohort with no voucher students to a cohort entirely composed of voucher students reduces overall test scores between 16 to 23 percent of a standard deviation, math test scores between 17 and 23 percent of a standard deviation and ELA test scores between 12 and 19 percent of a standard deviation. The marginal effect of adding an additional voucher student to a cohort of 40 hovers around half a percent of a standard deviation across each of these outcomes of interest. However, the impact of voucher peers on test scores varied significantly across student groups. While we find no evidence of heterogeneous results across gender, there are significant differences in outcomes based on whether incumbent private school students identify as non-White and their prior test scores. We might expect the academic outcomes of non-White incumbent students to be particularly sensitive to the exposure of vouchers students due to the changes in cohort composition. Furthermore, students on both tails of the achievement distribution may be differentially impacted by exposure to voucher peers if there are shifts in school resources away from incumbent to voucher students. In line with these mechanisms, we find that the declines in academic achievement are driven by non-White incumbent students and those at the top and bottom of the prior achievement distribution.

We further examine the impact of voucher peers on incumbent private school students by exploring heterogeneity by program timing. We may expect heterogeneous results across the policy

[2]We focus our analysis on incumbent private school students in order to avoid issues of reflection Manski (1993).

time horizon if schools mature in how they work with, integrate, and support new students, or if the composition of voucher students changes as the program expands. When we split the sample by years before and after ICSP expansion, we find that the association between the share of voucher peers and academic achievement of non-voucher students is concentrated in the first two years of the program. These results suggest that participating private schools are making adjustments as the program matures. We find some evidence that voucher students entering private schools after the expansion of ICSP differed from those entering in the first two years in both prior academic achievement and demographic characteristics, suggesting that schools may be doing a better job of screening out potentially disruptive students. Furthermore, across the two waves of interviews, there was a reduction in reports of the need to adjust instructional practices.

To understand why an increased share of voucher peers has negative impacts on incumbent private school students, document changes in school-grade characteristics. We find that higher shares of voucher peers is associated with lower shares of White students, higher shares of Black students and higher shares of students that qualify for subsidized lunch, aligning with the characteristics of voucher students themselves. Importantly, we also find that higher shares of voucher peers is associated with increases in the total number of students in a school-grade cohort, suggesting that at least part of our results are explained by increasing class sizes.

Prior work shows that the attendance and discipline of classmates can have a direct impact on student's own academic achievement (Gottfried, 2009; Lavy and Schlosser, 2011). We, therefore, also examine attendance and discipline rates of voucher students as a potential mechanism for our findings. We find that voucher students miss more days of school than existing students. Specifically, the median voucher student misses an additional two days of school compared to their non-voucher counterparts. Furthermore, using data from the public schools, we also find that as ICSP matured, students that left the public school system to use a voucher were increasingly likely to have faced disciplinary action before participating in the program. By 2017, around 16 percent of eventual voucher students had ever been suspended or expelled. Together, these results suggest that increased disruption in the classroom are a potential mechanism behind the declines in achievement.

The findings from the teacher and student interviews provide complementary evidence on the mechanisms underlying these patterns. Across interviews, teachers most consistently described changes in classroom academic composition, particularly increased heterogeneity in student preparedness, which led to greater differentiation, reteaching, and adjustments to instructional pacing. These instructional demands were more prominent than behavioral challenges, which were mentioned but less consistently emphasized. In contrast, students primarily described changes in peer relationships and classroom climate associated with the influx of new students, with limited evidence

of perceived changes in instruction or access to teacher attention. Across both teachers and students, reports of these changes were more prevalent in the early years of the program and less frequently framed as disruptions in later periods, consistent with a pattern of initial adjustment followed by adaptation.

Our results are robust to model specification choices and sample restrictions that threaten the validity of our findings. First, we assess the credibility of our main identifying assumption by showing that greater exposure to voucher peers is uncorrelated to changes in incumbent students' own predicted test score, demographic characteristics, or the likelihood of leaving a participating private school. Second, we assuage concerns of insufficient variation in our main independent variable of interest by plotting the across grade variation in voucher peer share over our entire sample, as well as by year and grade-level. Third, we address the concern that our results are driven by an additional influx of new students rather than a specific effect from greater exposure to voucher students by running a falsification test where we replace the share of voucher students with the share of non-voucher new students in a school-grade-cohort. We find limited impacts of new students on incumbent student's academic achievement, bolstering the claim that the impacts of voucher students is not solely driven by the fact that they are new to the participating private school. Lastly, we show are results are quantitatively similar when we restrict our outcome sample to only include incumbent private school students that never use a private school voucher themselves.

Our paper contributes to the growing economics literature on private school choice programs. Many papers examining private school vouchers in the United States 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), while Chingos and Peterson (2015) and Chingos et al. (2025) focus on longer-term educational impacts including high school graduation, college enrollment and college graduation. Our paper complements this prior work by demonstrating that the effects of voucher policies impact the educational outcomes of students not participating in the program. Specifically, we show that as ICSP is implemented, incumbent private school students experience declines in academic achievement. By establishing these effects of ICSP on private choice school students, we can better understand the total effect of voucher policies as they are adopted and expanded.

A large body of work 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; Figlio and Karbownik, 2016). Overall, these studies investigate the introduction of school voucher programs and find mod-

est positive effects on public school performance. Our results show that while students remaining in public schools benefit from the competitive pressure vouchers create, incumbent private school students experience comparable declines in academic achievement. This symmetry suggests the welfare effects of voucher programs must consider that the benefits to public school students may be partially offset by the costs borne by incumbent private school students.

The most similar work to ours examines the responses of private schools to voucher programs. This area of research is understudied in the United States because of the difficulty in obtaining consistent data. Private schools, in most states, receive minimal oversight and are not subjected to the same data reporting requirements as public schools. Therefore, much of the research in this space focuses on examining responses using data on potentially selected subsets of private schools or on publicly available datasets with limited information on academic performance.

Rinz (2015) and Hungerman and Rinz (2016) use data from the National Center for Charitable Statistics (NCCS) and the Private School Survey (PSS) to show the impact of early voucher programs on educational inputs, enrollment and fiscal outcomes at private schools. More recently, Harris and Olivier (2026), combine the NCCS and PSS with data from the Private School Review to examine the effect of voucher programs on enrollment and tuition following the expansion of universal school voucher programs since 2020. Overall, these studies find that voucher programs increase private school revenue, through the influx of public funds and higher tuition, and increase enrollment and student-teacher ratios. The finding that voucher programs are associated with higher student-teacher ratios and higher per-pupil expenditures leads to theoretically ambiguous impacts on student achievement. Our study, therefore, directly builds on the results of these papers by examining the impact of a large-scale voucher program on the academic achievement of private school students.

Adger and Felegi (2025) also study the impact of the Indiana Choice Scholarship Program (ICSP) on private school responses by evaluating the impact of the program on school quality. The authors find suggestive evidence that average private school quality fell following the adoption of ICSP, with private schools surrounded by relatively more public schools witnessing slightly larger declines. The main contribution of our paper over this prior work is the ability to examine the mechanisms that explain why private schools saw decreases in quality following the adoption of ICSP. Our findings show that ICSP had meaningful negative impacts on the academic achievement of incumbent private school students, suggesting that peer disruption and the reallocation of instructional resources are important mechanisms behind the aggregate quality declines observed at the school level.[3]

[3]Furthermore, Waddington and Berends (2018) show that students participating in the Indiana Choice Scholarship Program saw declines in their test scores following the switch from the public to private sector compared to matched remaining public school students. Our results show that the declines in average private school quality reported in Adger and Felegi (2025) were not solely driven by these voucher participants.

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. Section IV describes the empirical strategy, lays out the regression specifications, and provides a description of the qualitative analysis. Section V contains the main results, which include our heterogeneity analysis. Section VI offers conclusions from this research.

II The Indiana Choice Scholarship Program

The Indiana Choice Scholarship Program (ICSP) is currently the one of the most expansive private school choice programs in the United States in terms of student participation (70,095 participants as of the 2023-24 school year), private school participation (357 schools statewide), and eligibility (about 97% of families with children are eligible)[4] (Indiana Department of Education, 2024). Enacted prior to the start of the 2011-12 school year, the ICSP began with a modest scope and student participation. For the first two years of the program, students needed to have been previously enrolled in a public school for at least one year prior to receiving a voucher and attending a private school. As such, nearly all early participants in the ICSP switched from a public to a private school at some point in their educational history. There were also caps on the number of total participants in the first two years, though these thresholds were not met as 3,911 students participated in the first year (2011-12) and 9,139 in the second year (2012-13) (Indiana Department of Education, 2016).

The first expansion of the ICSP prior to the 2013-14 school year eliminated participation caps and expanded eligibility pathways. These pathways include students whose siblings previously participated in the program, kindergarten students, students with special learning needs, and students living in failing school districts, among others (Indiana Department of Education, 2016). As a result, more students who were either previously enrolled in private schools or who enrolled in a private school at the start of their educational history (i.e. kindergarten) began participating in the program. This expansion greatly increased overall participation, with 36,290 students using a voucher statewide by the 2018-19 school year, of whom about 60% had never been previously enrolled in a public school (Indiana Department of Education, 2019).

In addition to students needing to qualify through one of the eligibility pathways, the ICSP

[4]There are three main types of private school choice programs including tax credit scholarships, education savings accounts, and voucher programs. The Indiana Choice Scholarship Program is the largest single statewide voucher program by enrollment (the collection of voucher programs across Ohio have higher total enrollment) and Indiana has the fifth highest total enrollment of students in private school choice programs across all states. Indiana is also sixth in terms of percentage of educational expenditures spent on private school programs (need citation–see KYPolicy brief for example).

is also a means-tested program based on household income. The income eligibility threshold 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. For the first ten years of the program, students at or below the threshold for FRPL were eligible for a voucher with a value up to 90% of per-pupil state funding attached to students in the school district of their residence, while students at or below 150% of the threshold for FRPL were eligible for a voucher up to 50% of per-pupil state funding for their district of residence (Indiana Department of Education, 2019). For example, in the 2018-19 school year, a family of four with household income up to $46,435 could be eligible for a 90% voucher and a family of four with household income up to $69,653 could be eligible for a 50% voucher.

The actual voucher amount distributed to the private school in which the student enrolled equals the minimum of school tuition and fees or the qualified voucher amount. During the 2018-19 school year, the average voucher amount for students in grades 1-8 was $4,965 for students qualifying for the 90% voucher and $2,923 for those receiving the 50% voucher with about 40 percent of all students receiving a voucher that fully covered tuition and fees at their participating private school (Indiana Department of Education, 2019). At the time, the more expansive inclusion of both low- and modest-income families made the ICSP unique in comparison to other private school choice programs across the country, which typically had lower income thresholds. Given these considerations, Indiana may be a context a context where we might expect to see larger impacts on existing students in private schools in the early years of a voucher program.

Following the COVID-19 pandemic, Indiana joined a recent and growing trend of states enacting or expanding their private school choice program to have universal or near-universal eligibility by eliminating pathways and/or raising household income thresholds. Currently, 18 states have at least one such expansive program in operation or legislation recently passed (Lieberman et al., 2024). In Indiana, several changes have taken place to expand the program. First, the 50% voucher threshold was eliminated, making all qualifying families eligible for a 90% voucher. Second, the income threshold was first raised to 300% of the FRPL threshold in the 2021-22 school year and further to 400% of the FRPL threshold in the 2022-23 school year to present. Third, the eligibility pathways were eliminated prior to the 2023-24 school year such that eligible students must simply (1) have legal residence in Indiana and (2) be between ages 5 and 22 as of October 1 of the school year. At present, this means that any school age child living in Indiana whose family earns below 400% of the FRPL threshold ($222,000 for a family of four in the 2023-24 school year) is eligible for a voucher to cover the cost of tuition at a private school of up to 90% of the state per pupil allocation in the district of their residence (an average voucher award of $6,264 as of the 2023-24 school year) (Indiana Department of Education, 2024).

Our inquiry focuses on the first seven years of the ICSP (the 2011-12 through the 2017-18 school years–inclusive of the initial enactment and first expansion) for multiple reasons. First, in the early years of the program, many participating students transitioned from public to private schools, creating greater potential for disruption to existing private school environments. In later years–especially after the most recent expansions–the vast majority of new voucher recipients were already enrolled in private schools (Indiana Department of Education, 2024). Second, Indiana changed its annual assessments starting in the 2018-19 school year and did not conduct any statewide assessments in the 2019-20 school year due to the COVID-19 pandemic. Third, we have complementary qualitative data from a sample of schools at two time points early in the program (2012-13 and 2017-18 school years) that can help to contextualize these findings. Even though the scope of our focus is on the voucher program with more constrained eligibility, we believe these findings will be relevant to any state implementing any new private school choice program–regardless of expansiveness–as private schools will likely face an influx of new students in the early years of the program that will include lower-income families who may not have been able to afford private school tuition in the absence of a voucher or other form of subsidy such as a publicly-funded Education Savings Account. We will further discuss the extent to which we can generalize our findings in our conclusions.

III Data

III.A Indiana Department of Education Data

The quantitative data for this project come from the Indiana Department of Education's (IDOE) student longitudinal data system. We accessed these data through a longstanding data sharing agreement between the IDOE and the Center for Research on Educational Opportunity (CREO) at the University of Notre Dame. The IDOE database contains student-level data with information on the test scores, voucher take-up, and demographics of all students enrolled in a public, private, or charter schools in Indiana. The data span from the 2005-06 through present. However, our study sample only covers the 2011-12 through 2017-18 years to coincide with ICSP's initial implementation and first expansion but prior to the statewide test changing in 2018-19.

We focus on students in voucher-participating private schools that serve any students in grades 3-8. Standardized testing takes place annually in these grades in the subjects of mathematics and English/language arts. In our analyses, we standardized test scores are within year and grade, so that all estimates can be interpreted as standard deviations. Participation in annual testing and data reporting is required by voucher-participating private schools for not only voucher students, but also all non-voucher students in the school as well. These requirements are a contingent part

of accepting vouchers and also required in order to remain accredited by the state (Indiana Code §20-32-5-17). Not only are the IDOE data unique because of the robust, comparable data in both public and private schools, but many private schools participated in annual statewide testing prior to the enactment of the voucher program (including most of the state's Catholic schools) for the purposes of accreditation, receiving a state accountability letter grade for marketing purposes, and participation in statewide athletics competitions. As such, for many private schools in our sample, statewide testing was not a concurrent shock as part of participating in the voucher program.

Demographic information in the IDOE data varies depending on whether a student in a private school uses a voucher. 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 non-voucher private school students, we have information on whether they would qualify for a 90% voucher as it is the same cutoff for free or reduced-price lunch eligibility, which is recorded for each student. For students using a voucher, we also have information on their home address, the tuition they are charged, their pathway for qualifying, and the amount they receive. Our final dataset for the quantitative analysis consists of individual-level information on test scores and demographics for students in the third through eighth grades that were previously enrolled in a private school and had non-missing test score and demographic data.

Table 1 reports the summary statistics of incumbent private school students and voucher students. Only non-voucher private school students are in the regression sample. Compared to students that use a voucher, non-voucher private school students are predominantly White (88% versus 66%), are much less likely to qualify for subsidized lunch (2.5% versus 47.4%), and use testing accommodations at lower rates (5% versus 11.4%). Furthermore, incumbent private school students outperform voucher students on standardized tests. Incumbent private school students, on average, score half a standard deviation above all students in the state, while voucher students perform around 5 percent below the average in Math and 1 percent above the average in English Language Arts. The differences between the two groups of students supports the idea that the ICSP introduced new groups of students into participating schools, suggesting a potential role for peer effects.

III.B Interview Sample and Data

We also used interview data from two waves of semi-structured interviews with principals, teachers, parents, and students conducted in ICSP-participating private schools in Indiana. These data were collected by a larger project team at the University of Notre Dame initially during the 2012-13 school year (first wave) with a follow up spanning the 2017-18 and 2018-19 school years (second wave). The 2012-13 year corresponds to the second year after the enactment of the voucher program, and schools

were adjusting to increasing enrollments of new students. The 2017-18 and 2018-19 years were well after the program's first expansion and when voucher enrollments had reached a steady state of between 35,000 and 40,000 students statewide receiving vouchers annually.

Because our primary inquiry concerns peer effects on non-voucher students in grades 3-8, the most relevant interview subjects are teachers and non-voucher students in the elementary and middle testing grades. To select teachers for interviews, the project team stratified by grade level, ensuring that at least one teacher was selected from grades 2-3, 4-5, and 6-8. All teachers were approved by the principal before the project team contacted them directly to volunteer to participate. If a teacher initially declined to participate, a different teacher was randomly chosen within the same stratum. To select students for participation, the project team relied on volunteers, sending a flier home with students describing the study and seeking the approval of parents to participate. Interested parents and students then directly contacted the project team to schedule an interview. We provide teachers and families with a nominal gift card as a remuneration for their participation.

We completed interviews in nine schools serving students in grades 3-8. Eight of the nine schools were Catholic schools and the other school was Christian-affiliated, of which the vast majority of voucher-participating schools in the state are religiously affiliated. The schools were spread across geographic regions, including Fort Wayne, Indianapolis, and South Bend[5]. In these nine schools, we conducted interviews with 34 teachers in the first wave. In five of these schools, we also conducted interviews with 10 non-voucher students in the first wave. In the second wave of data, the same eight Catholic schools participated in teacher interviews (25 teachers total, 10 of whom were the same as in the first wave). In six of these schools, we also conducted interviews with 13 non-voucher students. The Christian-affiliated school declined to participate in the second wave.

The content of the semi-structured interviews focused on the implementation, early impacts, and later steady-state of the ICSP after adding additional eligibility pathways beyond prior public school enrollment. Most relevant to this study, the interviewers asked teachers in both waves open-ended questions about instructional and curricular changes, changes in classroom dynamics, and impacts on existing students. The interviewers asked non-voucher students open-ended questions about peer interactions, integration of new students, and changes in classroom dynamics.

[5]School participation was voluntary and the interview sample was not intended to be statistically representative. However, the interview schools are broadly similar to the population of religious private schools in Indiana along observable demographic characteristics, though Catholic schools were more likely to respond and agree to participate. The project team also considered the level and share of voucher enrollment when recruiting schools.

IV Research Design and Analysis

IV.A Empirical Strategy

To estimate the effects of voucher students on their peers we use a fixed effects model that relies on plausibly exogenous variation in the proportion of students participating in ICSP in a school-grade-cohort. Measuring such effects has proven difficult because of both the reflection and selection problems. The reflection problem exists because student and peer outcomes occur simultaneously, making it difficult to distinguish between the effect of a group on an individual and the effect of an individual on a group (Manski, 1993). To overcome this problem, we define peer quality as the proportion of one's peers who use a voucher to attend the private school. Thus, we assume that a child's peers do not cause that child to use a voucher. This assumption is met as voucher eligibility is set by a child's past education and their family's income which cannot be changed by the presence of students that use a voucher.

The selection problem arises because students self-select into schools and peer groups that are similar to them (Hoxby, 2000). In the absence of being able to randomize students into peer groups, the main approach to overcome selection has been to exploit the natural variation in cohort composition across time within a given school (Bifulco et al., 2011; Carrell et al., 2018; Balestra et al., 2022; Hill et al., 2023; Figlio et al., 2024). We follow this approach and argue that while there is selection into schools, there is year-to-year variation in the proportion of peers using a voucher across cohorts within the same school because of legislated changes in the requirements for the Indiana Choice Scholarship Program and natural variation in cohort composition. It is precisely this variation that we exploit in order to identify the impact of voucher peers.

Specifically, we estimate the following model:

$$Y_{igst} = \alpha + \beta_1 \frac{\sum_{k \neq i}^{K} Voucher_{kgst}}{n_{gst}} + X_{igst}^{'}\gamma + \lambda_{gs} + \sigma_{gt} + \epsilon_{isgt} \tag{1}$$

where i, g, s and t represent the individual, grade, school and academic year, respectively. In our preferred sample, individuals included in the regression (denoted by i) are students that never attended a public school. This sample includes both never voucher students and those that received a voucher only after they had already attended the school.[6] Y_{igst} represents the outcome variables of interest test overall, Math, and English Language Arts standardized test scores. Overall test scores are calculated by taking the average of the standardized Math and English Language Arts (ELA)

[6]The latter group of students are those that only qualified following the expansion of the program. Our results are not sensitive to this sample construction. In Appendix Table A2 we present the results including only never-voucher students.

for each student in each grade. $\frac{\sum_k^K Voucher_{kgst}}{n_{gst}}$ is the proportion of peers in a school-grade-cohort that use a voucher to attend the private school.[7]

We measure exposure to voucher peers as the share of students using a voucher that qualified because they had previously attended a public school. We denote these students as k in the regression. We focus on this group since voucher students that qualified via another track were likely already at the private school and it is unlikely that receiving a voucher (i.e., having the state pay for schooling) would impact non-voucher peers through any discernible mechanism. X_{igst} is a vector of individual i's characteristics, including race, gender, subsidized lunch, and accommodation status. λ_{gs} and σ_{gt} are school-grade and grade-year fixed effects, respectively. In our main specification, we account for unobserved time-variant school characteristics by adding school-specific linear time trends. This approach has the advantage of controlling for unobserved factors correlated with time trends in school composition that may confound peer effects in schools. All standard errors allow for arbitrary correlation in errors at the school level.[8]

IV.B Validity Checks

The key identifying assumption behind our empirical strategy is that there is no omitted variable that is time-varying and school specific, not captured by the school-specific time trends, is correlated with both the share of voucher peers and standardized test scores, and not included in the individual-level controls. To assess the credibility that such factors do not exist, we conduct several balance checks to test whether the share of voucher peers in a school cohort is systematically related to student background characteristics observable in the IDOE data.

We begin by testing whether greater exposure to voucher peers is correlated to changes in predicted test scores. Specifically, we regress test scores on the full set of fixed effects and controls, excluding voucher peer share, and use the estimated coefficients to predict a test score for each individual in the sample. This measure captures a linear combination of individual characteristics, where the weights are chosen to best predict test scores. Column (1) in Table 2 presents the results of this exercise and shows that predicted overall test score is uncorrelated with the share of voucher peers, suggesting our identifying assumption holds in our context. Nevertheless, Columns (2)-(7) of Table 2 reports the results of a series of placebo tests checking whether the within-school-grade variation in the share of voucher peers is associated with changes in students' gender, race, subsidized lunch status, and accommodations status. We choose these outcomes because these characteristics

[7] We measure peer voucher use at the cohort level as opposed to the classroom level due to potential nonrandom selection of students into classroom within a school and grade.

[8] Appendix Table A1 shows the robustness of our results to the inclusion of controls and weights.

cannot be causally affected by peers but are likely correlated with the unobserved characteristics of other students selecting into the school. We find that exposure to voucher peers is mostly unrelated to these outcomes, with exceptions for the female and any accommodations variables. To ensure that these variables do not invalidate our estimates, we control for these student characteristics in each of our specifications.

Columns (8)-(10) in Table 2 report the results of additional placebo tests that examine whether the share of voucher peers is associated with incumbent students being dropped from the sample (because of missing student characteristics), incumbent students switching schools, or missing test scores. Across the board, we find no evidence that of an association between voucher peer share and these outcomes. These results are significant because they show that incumbent private school students were not more likely to leave schools that had higher shares of voucher peers.

Another important challenge in our empirical approach is the possibility that there is insufficient variation in voucher peer share across grades within school, which would attenuate our results. Panel A of Figure 1 displays the within school, across grade variation in the voucher peer share across all observations in our sample and shows 33% of school-grade-year observations report at least 5 percent of students using a voucher and 15% report at least 10 percent. This pattern does not change if we examine observations separately by year (Panel B) or grade-level (Panel C). Panel D of Figure 1 shows the proportion of voucher peers at the school-grade level after residualizing on cohort and school fixed effects, as well as school-specific linear time trends. This figure helps bolster the claim that there is sufficient variation in voucher peer share to identify the effects we estimate.[9]

IV.C Qualitative Analysis

The quantitative analysis described above estimates peer effects by relating achievement outcomes of non-voucher private school students to within-grade variation in voucher student enrollment. The interview data used in the qualitative portion of this study are not used for identification and do not affect estimation; instead, they provide contemporaneous evidence on classroom processes that may plausibly mediate the estimated effects. In particular, we use the interviews to assess whether educators and students describe changes in classroom conditions–such as behavior and classroom management challenges, increased differentiation demands, and shifts in instructional time–that are consistent with the mechanisms implied by the quantitative results. Although the interviews were conducted prior to the development of the quantitative analysis for this paper, they occurred during

[9] Alternatively, in Appendix Figure A1 we plot the cross-grade, within-school range in voucher peer share in each of the schools in our sample. We continue to find evidence that there is sufficient variation in our independent variable of interest.

the same years as our main analytical window. Therefore, interviews provide contemporaneous evidence on how teachers and students experienced the introduction and expansion of Indiana's voucher program.

Following established approaches for integrating qualitative evidence within mixed methods research (Creswell and Plano Clark, 2018), we applied a deductive coding framework focused on identifying references to potential mechanisms underlying the observed achievement effects. The qualitative coding was led by one author, who developed and applied the coding scheme through an iterative process, with ongoing discussion and feedback from the other author.

To begin, we used a set of pre-specified mechanism codes drawn from prior qualitative and mixed-methods research on Indiana's voucher program (Austin, 2018; Berends et al., 2023) to code a pilot sample of teacher and student interviews. We then assessed the validity of these initial codes in two ways: first through systematic comparison to earlier coding of the same interviews conducted under the original broader inductive framework; second through the use of a generative AI tool (ChatGPT). Once pilot coding was verified, we further used the generative AI tool to assist in refining code definitions and applying coding rules to the full set of interview transcripts, with separate analyses of the teacher and student interviews. We conducted coding through an iterative and researcher-guided process in which we specified inclusion and exclusion criteria and reviewed the coded outputs. We retained full responsibility for all coding decisions, validation, and interpretation.

For teachers, the coding domains include instructional attention (e.g., differentiation, allocation of time), classroom composition (e.g., student heterogeneity, new students), behavioral management, instructional practice and pacing, and teacher workload or strain. For non-voucher students, the coding domains include academic experience (e.g., learning conditions, rigor), attention from the teacher, classroom composition and climate, social integration (integration difficulty, peer relationships), and voucher awareness.

For both teacher and student interviews, we used a dual-layer deductive coding framework. The first layer of coding captures statements reflecting change over time (before vs. after voucher implementation), comparisons across students (voucher vs. non-voucher or new vs. incumbent students), or processes linking changes in classroom conditions to instruction or learning within these domains. The second layer of coding captures the presence of domains regardless of explicit change, allowing for comparisons in prevalence across waves. The first layer contains an intentionally sparse application of codes to prioritize plausible pathways related to our quantitative findings, while the general layer captures broader classroom conditions.

We summarize findings descriptively by reporting the share of interviews in which each coding domain appears. We interpret code frequencies as descriptive indicators of the prevalence of reported

mechanisms rather than statistical tests, and we include representative quotations to illustrate how interview content supports—or in some cases contrasts with—the patterns observed in the quantitative estimates. Although our summary of the interviews is not generalizable in a statistical sense, the qualitative findings offer structured, first-hand perspectives that contextualize and help explain the potential mechanisms behind the peer effects observed in the quantitative results.

V Results on Incumbent Private School Students

We begin by describing the estimated effects of the Indiana Choice Scholarship Program on existing private school students. The results of our main analysis are reported in Table 3. Each cell of the table represents the coefficient on the proportion of voucher peers for separate regressions. Each column shows the results for an individual outcome of interest. Columns (1) and (2) present the results for overall standardized test scores, Columns (3) and (4) present the results for standardized Math scores, and Columns (4) and (6) present the results for standardized English Language Arts scores. Even columns report our specifications that include school-specific linear time trends.

We find that higher shares of voucher peers in a school-grade cohort is negatively associated with academic performance of existing private school students. Moving from a cohort with no voucher students to a cohort entirely composed of voucher students reduces overall test scores between 16 to 23 percent of a standard deviation, math test scores between 17 and 23 percent of a standard deviation and ELA test scores between 12 and 19 percent of a standard deviation. The marginal effect of adding an additional voucher student to a cohort of 40 hovers around half a percent of a standard deviation across each of these outcomes of interest. In our sample, the average number of voucher students in a school-grade-cohort is just over 1 and the average size of the cohort is just under 40, aligning these marginal estimates with the rollout of the program in our context.

We can begin to benchmark the magnitude of our results by comparing the size of our estimates to the peer effects that are obtained in studies that examine the influence of potentially disruptive children. Balestra et al. (2022) study the effect of disruptive peers on educational achievement in Switzerland, using variation in the share of children requiring special needs in school-grade-class cohorts. The authors conclude that adding one special needs student to a classroom of twenty causes the achievement of non-special needs students to fall by 2.5% of a standard deviation. They estimate the peer effect parameter at the school-grade-classroom level, which complicates a direct comparison with our main estimate. However, when the authors re-estimate the their main analysis using the same aggregation level, they find an effect of just under 1% of a standard deviation, when adding one special needs student to a class of 20. This effect is similar in magnitude to those reported in Table 3.

Our results are also similar in magnitude to those reported in Carrell et al. (2018), which studies the effect of disruptive peers on test scores (and long-run education and labor market outcomes), using variation in the share of children exposed to peers that faced domestic violence. They estimate that adding one disruptive student to a school-grade cohort of 25 reduces achievement by 0.014 standard deviations for students in grades 3 to 5.

A potential concern is that our estimates reflect general adjustment costs associated with an influx of new students into a private school, rather than the specific effect of greater exposure to voucher students. To assess this possibility, we conduct a falsification test in which we re-estimate Equation 1 replacing the voucher exposure measure with the share of students who are new to the school within a given school–grade cohort. If our main results were driven by general entry effects rather than an impact from voucher students, we would expect to observe similar patterns in this falsification exercise. Instead, Appendix Table A3 shows that we find no statistically significant effect of the share of new students in a school-grade cohort on our outcome variables of interest. These results provide support for the claim that our results are not solely driven by the fact that voucher students are new to the private school.

V.A Mechanisms

Given the declines we find in test scores, we next examine changes in cohort characteristics and class sizes. We begin by documenting the link between higher shares of voucher peers and the demographic composition of school-grade-cohorts. Table 4 reports the results of Equation 1 where the outcomes of interest of school-grade-year measures of demographic variables (Percent Female, Percent White, Percent Hispanic, etc.).[10] Table 4 shows that higher shares of voucher peers is associated with a statistically significant decrease in the percent of White students, and statistically significant increases in the percent of Black, Hispanic and students qualifying for reduced price lunch. These results are in-line with the characteristics of voucher students as shown in Table 1.

Column (11) of Table 4 also shows that higher shares of voucher peers is associated with larger cohort sizes, suggesting that the declines in the academic achievement of existing private school students may be driven in part by increasing class sizes. Krueger (1999) finds that a reduction of 7 students (from a class size of 22 - a 32% decrease) increases test scores by 20 percent of a standard deviation. Using this estimate, we can perform a back-of-the-envelope calculation to understand how much of our findings can be explained by increased class sizes alone. We find an increase of 8% in class sizes (3 off a base mean of 38). The Krueger (1999) estimates suggests an increase of

[10] Importantly, these variables are calculated including both incumbent private school students and the voucher students themselves.

8% in class sizes would reduced test score by 5% of a standard deviation; about one-third of the impact we estimate in our preferred specification in Table 3. While significant, these results suggest that the impact of voucher peers on incumbent private school students are not only the result of the downstream effects of increased class sizes. Moreover, the findings on class sizes contrasts the interview data, in that no teachers reported any concerns or issues with class sizes having increased to the degree that it is impacting their instruction.

Prior work has shown that both attendance and discipline influence the academic outcomes of peers (Gottfried, 2011; Lavy and Schlosser, 2011). If voucher students have higher rates of absenteeism than their incumbent private school peers, teachers may be forced to split their time between students who have and have not missed the previous lessons, reducing the academic achievement of incumbent private school students. Similarly, if voucher students have high discipline rates, this could lead to increased disruptions in class that reduces achievement. We, therefore, use attendance and discipline data in the IDOE-CREO database to evaluate differences in attendance and suspensions/expulsions by voucher status. Unfortunately, information on suspensions and expulsions is only available for public school students. Therefore, the comparisons we make involving discipline are across public school students in third through eighth grade that either go on to use a voucher or never use a voucher.

Panel A of Figure 2 plots the distribution of school days attended by voucher status. We find that voucher students miss more days of school than incumbent private school students, suggesting that our results could be driven by the differences in attendance. Panel B of Figure 2 plots the share of students between third through eighth grade that are suspended or expelled in a given year by eventual voucher status. Specifically, in 2007 only 5 percent of students that eventually left a public school to use a voucher were suspended or expelled. That number increased to 16 percent in 2017. We see an opposite pattern for remaining students. In 2007, 12 percent of remaining public school students in the third through eighth grade were suspended or expelled and that number decreased to around nine percent in 2017. The increasing rate of suspensions/expulsions of eventual voucher students suggests that they may serve as increasingly disruptive peers, potentially explaining the declines in test scores of non-voucher private school students.

V.B Heterogeneity by Student Characteristics

We have found consistent evidence that greater exposure to voucher peers is associated with declines in test scores. However, these average estimates across all students could differ across various subgroups. Therefore, we disaggregated the results by the following student characteristics: gender, race and prior test performance. We calculated these estimates by introducing interactions of the

student characteristics with the $\frac{\sum_k^K Voucher_{kgst}}{n_{gst}}$ variable in Equation 1. Table 5 displays the results of our heterogeneity analysis by student subgroup for our three outcome measures of interest: overall test score, math test score, and ELA test score, respectively.

Panel A reports the differences in outcomes for male and female students. The interaction term in Panel A isolates the differential impact of greater exposure to voucher peers for female students compared to their male counterparts. We may expect differences in the impact of voucher peers across gender because prior work shows that boys' academic outcomes can be more sensitive to disruptive classroom peers than girls (Lavy et al., 2012; Carrell et al., 2018; Bietenbeck, 2020), which could generate differential effects if voucher students are more likely to exhibit disruptive behavior. Despite this theoretical reason to expect heterogeneous effects, the estimate on the interaction term with identifies as Female is statistically insignificant. This result implies that Female students see similar reductions in test score performance as their male counterparts.

In Panel B, we examine whether the impact of voucher peers varies by race. The interaction term in Panel B isolates the differential impact of greater exposure to voucher peers for non-White students compared to their White counterparts. Non-White incumbent students may be more adversely affected by the introduction of voucher peers since voucher students are disproportionately non-White (see Table 1), so their arrival changes the racial composition of classrooms (as shown in Table 4), and prior work suggests that students tend to be more sensitive to peers of the same race (Hoxby, 2000). Consistent with this hypothesis, we find that the estimate on the interaction term with identifying as non-White is negative and statistically significant. These findings suggest that the decline in academic performance is larger (more negative) for non-White students.

Panel C reports the effect of voucher peers on test scores by previous academic performance.[11] Specifically, we include two interaction terms. The first identifies the interaction between exposure to voucher peers and whether a student's prior year test score was in the bottom distribution. The second is an interaction term between voucher exposure and whether a student was in the top tercile of prior year academic performance. There are reasons to expect effects at both ends of the distribution. Low-achieving incumbent students may be particularly vulnerable if voucher students draw school resources that would have otherwise been directed toward them. On the other hand, high-achieving students may also be adversely affected if the introduction of voucher peers slows instructional pace or reduces the complexity of classroom material as teachers adjust to a wider range of student preparation levels. Indeed, we find that incumbent students at both the top and bottom of the prior achievement distribution experience larger declines in math test scores relative to

[11] Because we only have previous test performance for those in the fourth grade and above, this analysis necessarily drops students in the third grade.

those in the middle. For ELA, the negative effects are concentrated among low-achieving incumbent students. The differential results across subjects may stem from the greater school-dependence of math relative to ELA, as high-achieving students may be better positioned to supplement ELA instruction through independent reading and home learning.

We also examine whether the impacts of voucher peers differs across baseline school characteristics. Schools with a below median, baseline non-White or free/reduced-price lunch share of students may experience larger disruptions from the demographic changes brought by voucher students. On the other hand, schools with an above median, baseline enrollment or school value-added may be better equipped to integrate voucher students through more experienced teachers or stronger administrative capacity. To answer this question, we disaggregated our baseline results by the following pre-policy school characteristics: enrollment of non-White students, FRPL students, total enrollment and school value-added. We calculated these estimates by splitting the sample by whether a school fell above/below the median value in the 2010-2011 academic year. Figure 4 displays the results of this heterogeneity analysis. Each panel of the figure, for a given pre-policy characteristic, displays the coefficient on the share of voucher students separately for schools above and below the median baseline value. Contrary to our hypotheses, we fail to find any significant differences based on initial school characteristics, suggesting that no observable school characteristic predicts a school's ability to buffer its incumbent students from these disruptions.

V.C Heterogeneity by Program Timing

The Indiana Choice Scholarship Program was adopted and expanded in two separate academic years (2011-2012 and 2013-2014, respectively) during our sample period. One might then wonder if the different program structures had differential effects on existing private school students. We might expect differential results by program timing if schools adjust as the program matures - either by changing school resources or the set of voucher students they admit into the school.

We answer this question by splitting the sample by years before and after the expansion. Table 6 reports the results by program timing for each outcome variable of interest. Odd columns report the coefficients when estimated using data from 2012 and 2013, while even columns report the coefficients when estimated using data from 2014 onward. We find that the association between the share of voucher peers and academic performance of existing private school students are concentrated in the first two years of ICSP. All coefficients in the years post-expansion are statistically indistinguishable from zero. Furthermore, even if we took the estimated coefficients seriously, they are nearly half the size of estimates in the pre-expansion time period.

To understand why we see dissipating effects of voucher peers as the program expands, we begin

by examining differences in voucher student characteristics across the two time periods. Figure 3 plots the test score distribution of voucher students that entered participating private schools before and after the expansion of the Indiana Choice Scholarship Program. The distribution of test scores for voucher students entering in participating private schools following the expansion of ICSP is to the right of the same distribution for voucher students entering in the first two years of the program. These results suggest that participating private schools may be selecting more academically prepared students as the program matures.

Table 7 reports the summary statistics on voucher students that moved from the public to private sector in the first two years of ICSP (Columns (1) and (2)) versus after the expansion (Columns (3) and (4)). Compared to students that used the voucher between 2012 and 2013, newer cohorts were less likely to identify as White (48.6% versus 51.4%) , were more likely to identify as Hispanic (26.3% versus 21.8%) , and use testing accommodations (14.1% versus 10.2% for Math and 14.0% versus 9.9% for ELA). Interestingly, these demographic characteristics are more dissimilar to the average statistics of non-voucher students shown in Table 1. We also continue to find that newer voucher students performed better on standardized tests prior to their move to the participating private schools. Voucher students entering after the expansion of ICSP performed, on average, around 7 percent of a standard deviation above the 2012 and 2013 cohorts on both math and ELA. We take these differences in student characteristics over time as evidence that participating private schools adjusted the composition of voucher students after the program expanded, which may play a role in the change of their impact on incumbent students.

VI Results from Teacher and Student Interviews

VI.A Teachers

We next examine teacher interviews to assess potential mechanisms through which increased voucher enrollment may affect incumbent students. Teachers generally did not frame their responses explicitly in terms of the voucher program itself, as teachers indicated varied knowledge of which students in their classroom were receiving vouchers. Instead, they nearly universally described classroom changes in terms of "new students," differences in instruction or preparation compared to prior years, or broader shifts in classroom composition. Across interviews, teachers most consistently emphasized instructional and organizational mechanisms, particularly those related to the variation in student preparedness and ability level along with the resulting demands on instruction.

Nearly all teachers in the first wave referenced changes in classroom composition, most commonly in terms of wider variation in academic readiness. Teachers frequently described incoming students as

arriving with gaps in foundational skills, particularly in math and reading. For example, one teacher noted that "*when they come in... they can be a little bit lower for math, especially basic skills*," highlighting how differences in prior preparation shape classroom instruction. Another teacher described a particularly stark case in which "*he's in eighth grade, but he's working at about a fifth grade level*," underscoring the magnitude of some of these gaps. These shifts in academic composition are closely tied to changes in instructional practice.

Teachers consistently linked greater student ability heterogeneity to increased differentiation, reteaching, and adjustments to pacing. As one teacher explains, "*some of the students... if they don't have the basics it's really hard to get them into pre-algebra or algebra*," illustrating how variation in preparedness can constrain the progression of the class as a whole. In response, teachers described adopting strategies such as ability grouping, small-group instruction, and targeted remediation, which had not previously been regular practice in many instances. At times, these adjustments reflect implicit tradeoffs in how instructional time is allocated across students.

In contrast, evidence on behavioral or disciplinary mechanisms is more limited in comparison to the new instructional demands, though not entirely absent. While some teachers described challenges associated with student transitions or differences in behavioral expectations, these accounts were less consistent than those related to academic preparedness and instructional adjustment. Similarly, while teachers occasionally referenced increased workload or strain, these experiences were typically framed as a byproduct of instructional adaptation rather than as an independent mechanism.

Comparing across waves, the prevalence of mechanism-relevant statements declined across all domains. Teachers in the first wave more frequently described changes in classroom composition, instructional demands, and the need to adjust pacing or grouping strategies. In the second wave, these same domains remained present but are less often described as changes. Instead, teachers more commonly describe these practices as part of their routine instructional approach. This pattern is consistent with an initial period of disruption followed by adaptation, in which schools and teachers adjust to a more heterogeneous student population.

At the same time, the broader domains identified in the general coding layer remain prevalent across both waves. Teachers in both periods frequently referenced differentiation and variation in student readiness, even when these are not framed as new or changing conditions. For instance, teachers in the later wave often described practices such as ability grouping or targeted support as standard features of instruction rather than responses to recent changes. In fact, all mentions of the term "ability grouping" appeared exclusively in wave 2 from teachers in five of the eight schools.

Overall, these findings suggest that teachers perceive the primary mechanisms associated with increased voucher enrollment to operate through changes in student composition and the resulting

demands on instruction, rather than through broader disruptions to classroom climate. While these mechanisms appear to diminish over time, the underlying instructional challenges associated with increased student heterogeneity remain. In later periods, these conditions appear to be experienced not as disruptions, but as normalized features of classroom practice, consistent with the reduction of mechanism-relevant language in wave 2.

VI.B Students

We next examine student interviews first to assess potential mechanisms through which new students in the school using vouchers may affect incumbent students. Notably, students demonstrated limited explicit awareness of the voucher program itself, often describing classroom changes in terms of "new students" or shifts in peer composition without directly linking these changes to vouchers or financial aid, similar to their teachers. Overall, student accounts emphasized social and relational mechanisms related to the influx of new students rather than academic disruption.

Nearly all students within the first wave referenced changes in classroom composition or climate. For example, one student explained that "*some students get left out now, because there's more students in our class*," directly linking increased enrollment to changes in peer interactions. Another student similarly described how "*people keep a tighter circle... they're not friends with everybody anymore*," suggesting that classroom changes are associated with more fragmented peer groups. These shifts are closely tied to social integration challenges. Several students described difficulty in forming or maintaining friendships in the presence of new students. As one student notes, "trying to find new friends... when you change schools is the hardest thing to do," highlighting the friction associated with peer re-sorting.

In contrast, direct evidence of changes in instruction is limited. While students may have described how teachers structure access to help—for example, noting that "you can just go up and ask the teacher what you don't understand"—any of these statements reflected general access to teacher support rather than diminished availability. Explicit evidence of increased competition for teacher attention, a potential mechanism resulting from increased class sizes, was rare. Similarly, very few students described changes in academic rigor or learning pace attributable to classroom composition.

Comparing across waves, the prevalence of mechanism-relevant statements decreased substantially. Mentions of classroom composition related to new students fell from 90% in wave 1 to 33% in wave 2, and references to integration difficulty fell from 60% to 33%. Similar to teacher reports, though through different mechanisms, this pattern is consistent with what appears to be an initial period of disruption followed by adaptation, in which the impacts of an increasing number of new

students in the classroom become less salient or are at least perceived as less disruptive over time.

At the same time, the broader domains highlighted in the first layer of mechanism coding remained present across both waves when applying a more general coding framework. Students in both periods continued to reference classroom composition, peer relationships, and teacher support at high rates, even when these experiences were not framed as changes potentially tied to the voucher program. For instance, students in wave 2 commonly described peer interactions in stable terms, such as noting that "*it's pretty easy [to make friends] because everybody's friendly*," suggesting an environment in which peer inclusion is routinized.

For students, these findings suggest that while the disruptive effects of changes in classroom composition diminish over time from the perspective of students, the underlying features of student experience—particularly peer dynamics and classroom structure—remain important. Effectively, students in later periods appear to experience these conditions as normalized rather than as deviations from prior conditions, which may explain the attenuation of mechanism-relevant language in wave 2 despite continued exposure to similar classroom environments.

VI.C Connection Between Peer Effects and Interview Findings

When taken together, the quantitative peer effects and interview findings align most clearly around mechanisms related to changes in classroom composition and the resulting instructional demands placed on teachers. The quantitative results show that higher shares of voucher peers are associated with declines in incumbent students' achievement, particularly in the early years of the program and among students most sensitive to instructional changes. The interview data provide direct evidence of how these mechanisms operate in practice. Teachers consistently described increased heterogeneity in student preparedness, which led to greater differentiation, reteaching, and adjustments to pacing. These instructional shifts map closely onto classroom processes that would plausibly generate the observed declines in test scores, especially for incumbent students who needed to adjust to these new instructional styles and techniques.

At the same time, the qualitative findings both refine and complicate the mechanisms suggested by the quantitative analysis. While the administrative data point to attendance and disciplinary differences as potential sources of disruption, teachers rarely centered these behavioral challenges in their accounts, instead emphasizing academic preparedness and the need for instructional adaptation. Student interviews point in a different direction still, as students reported few changes in instruction or access to teacher attention and instead focused on shifts in peer relationships and classroom climate, which could be more directly related to behavior. These differences suggest that no single mechanism fully explains the quantitative results. Rather, the negative achievement effects likely

reflect a combination of channels: some role for disruption, as implied by the administrative data, alongside more pervasive changes in instructional pacing and classroom dynamics that are more consistently reflected in the interview evidence.

Finally, both strands of evidence point to a common pattern of initial disruption followed by adaptation. Negative peer effects are concentrated in the early years of the voucher program and attenuate over time. Similarly, earlier interviews emphasize instructional disruptions associated with new student inflows, while later interviews frame these conditions as normalized features of the classroom. This convergence strengthens the interpretation that schools adjust to the introduction of voucher students, reducing the magnitude of peer effects over time while the underlying instructional and social dynamics remain in place.

VII Discussion and Conclusion

This paper examines how the initial implementation and expansion of the Indiana Choice Scholarship Program affected incumbent students in participating private schools. We find that exposure to greater shares of voucher peers within a school-grade cohort is associated with declines in incumbent students' academic performance and vary by student race/ethnicity and prior academic performance. Our estimates are modest in magnitude, but they fall in the range of other papers evaluating the spillover effects of potentially disruptive peers (Carrell et al., 2018; Balestra et al., 2022). However, the declines are concentrated in the initial two years of the programs suggesting that there are not persistent declines in performance over time associate with the introduction of voucher students.

We provide complementary qualitative evidence to help contextualize these patterns. This evidence points to increased heterogeneity in student academic preparedness, shifts in instructional pacing and differentiation, and changes in classroom dynamics as key mechanisms. Taken together, the quantitative and qualitative findings suggest that the immediate influx of voucher students upon implementation of a program generates short-run disruptions to classroom environments that have measurable consequences for private schools and their existing students and teachers.

These results contribute to a growing literature on private school choice by shifting attention to outcomes within participating private schools. Prior work has largely focused on the effects of vouchers on participating students or on students remaining in public schools, often highlighting mixed effects for the former and modest competitive effects for the latter. Our findings extend this literature by showing that voucher programs also generate meaningful, negative, and short-term spillover impacts on non-participating students within private schools. In this sense, our results suggest that the total effects of voucher policies must not account only for the gains or losses among

participants or competitive responses in public schools, but also for the adjustment costs borne by private schools during policy implementation.

We believe our results are particularly relevant in the current policy environment, where policymakers across states have either recently enacted or expanded private school choice programs (vouchers, tax credit scholarships, and educational savings accounts) or are considering doing so. The passage of the federal school choice tax credit through the One Big Beautiful Bill Act also has implications beyond these individual states as there may be motivation for states to opt into a federal tax credit program that might otherwise not enact a statewide choice program. Although our analysis focuses on Indiana's program during the time in which it was means-tested (it is now near-universal), the core mechanisms we identify are likely to apply broadly to new and expanded programs in other states, even with a (near-)universal eligibility provision. In these contexts, participating schools will still likely face an influx of new students irrespective of policy design, and thus may be susceptible to similar short-run disruptions as we described here.

In particular, states with new or expanded private school choice programs should give close attention to integrating new students in private schools who have varied academic backgrounds and prior schooling experiences. Our results also caution policymakers and school leaders against interpreting early disruptions as persistent failures or responding by restricting private school access to students perceived as more challenging to serve. Instead, our evidence suggests that schools can and do respond to these initial challenges and changes over time as new practices become embedded in routine instruction and school culture.

We also note some limitations with our study. Our estimates capture average effects on incumbent students within one state and program context and may not generalize to all private school settings or choice policy designs. While we assuage identification concerns through our empirical strategy and falsification tests, we cannot fully rule out all sources of unobserved variation. And although the qualitative evidence provides valuable insight into potential mechanisms, it is not designed to establish causal pathways or represent all participating schools. Despite these limitations, this study provides some of the first systematic evidence on how voucher programs impact incumbent students within private schools and highlights the importance of considering implementation dynamics when evaluating the broader impacts of school choice policies.

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VIII Tables

Table 1: Descriptive Statistics

	Non-Voucher Students		Public School Voucher Students		
	Mean	SD	Mean	SD	**Difference**
	(1)	(2)	(3)	(4)	(5)
Panel A: Demographic Characteristics					
White	0.842	(0.365)	0.490	(0.500)	0.352***
Black	0.027	(0.163)	0.162	(0.368)	-0.134***
Hispanic	0.073	(0.259)	0.257	(0.437)	-0.184***
Female	0.501	(0.500)	0.515	(0.500)	-0.014***
FRPL	0.142	(0.349)	0.639	(0.480)	-0.498***
Has Math Accommodation	0.067	(0.250)	0.135	(0.342)	-0.068***
Has ELA Accommodation	0.067	(0.250)	0.134	(0.341)	-0.067***
Panel B: Education Outcomes					
Standardized Test Score	0.382	(0.836)	-0.243	(0.834)	0.626***
Standardized Math Score	0.302	(0.844)	-0.303	(0.841)	0.605***
Standardized ELA Score	0.422	(0.863)	-0.142	(0.858)	0.563***

Notes: This table shows descriptive statistics about non-voucher and voucher students at participating private schools throughout the sample time frame. Data on enrollment and test scores come from the IDOE-CREO database. Non-voucher students are defined as incumbent private school students that never attended a public school, while public school voucher students are those that used the voucher to move from the public to private sector. Column (5) reports the difference in the means between public school voucher students and non-voucher students.

Table 2: Results on Exogenous Student Characteristics and Attrition

VARIABLES	Predicted Test Score	Female	White	Black	Hispanic	FRPL	Any Accomm.	Drop Sample	Change Schools	Missing Scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure to Voucher Peers										
Fraction Peers using Voucher	0.014	0.102***	-0.002	-0.018	0.028	0.036	0.060*	0.015	0.009	-0.015
	(0.074)	(0.039)	(0.032)	(0.022)	(0.022)	(0.041)	(0.036)	(0.034)	(0.008)	(0.015)
	[< 0.001]	[0.003]	[< -0.001]	[< -0.001]	[< -0.001]	[0.001]	[0.002]	[< 0.001]	[< 0.001]	[< 0.001]
Observations	126,756	159,016	159,016	159,016	159,016	159,016	159,016	166,235	159,016	168,451

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 grade-year, school-grade fixed effects, as well as a school-specific linear trend. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database. The number of observations in Column (1) are significantly lower because we cannot include prior test score for third graders.

Table 3: Results on Student Achievement

VARIABLES	Std. Test Score	Std. Test Score	Std. Math Score	Std. Math Score	Std. ELA Score	Std. ELA Score
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Voucher Peers						
Fraction Peers using Voucher	-0.231**	-0.161**	-0.226**	-0.173*	-0.187**	-0.129*
	(0.093)	(0.072)	(0.108)	(0.088)	(0.088)	(0.077)
	[-0.008]	[-0.005]	[-0.008]	[-0.006]	[-0.006]	[-0.004]
Observations	159,016	159,016	159,016	159,016	159,016	159,016
R-squared	0.215	0.222	0.202	0.212	0.198	0.204
School-Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School Specific Linear Time Trend	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 grade-year and school-grade fixed effects. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.

Table 4: Results on Cohort Characteristics

	Percent Female	Percent White	Percent Black	Percent Hispanic	Percent FRPL	Percent ELL	Percent Special Educ.	Percent Section 504	Percent Math Acc.	Percent ELA Acc.	Total Number Students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Exposure to Public School Voucher Peers											
Fraction Peers using Voucher	0.051	-0.220***	0.058***	0.083***	0.269***	0.025	-0.022	-0.005	0.043	0.038	3.679**
	(0.032)	(0.029)	(0.017)	(0.019)	(0.037)	(0.031)	(0.018)	(0.008)	(0.038)	(0.039)	(1.787)
Observations	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016
Baseline Mean	0.484	0.825	0.031	0.062	0.130	0.033	0.05347	0.017	0.059	0.057	38.290
School-Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	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 grade-year, school-grade fixed effects, as well as a school-specific linear trend. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.

Table 5: Heterogeneity Effects of Voucher Peers

VARIABLES	(1) Overall Test Score	(2) Math Test Score	(3) ELA Test Score
Panel A: Identifies as Female			
Fraction Peers Using Public School Voucher	-0.121	-0.161	-0.097
	(0.092)	(0.105)	(0.093)
	[-0.003]	[-0.004]	[-0.002]
Interaction with Identifies as Female	-0.071	-0.016	-0.055
	(0.105)	(0.098)	(0.119)
	[-0.002]	[<-0.001]	[-0.001]
Observations	159,016	159,016	159,016
Panel B: Identifies as non-White			
Fraction Peers using Public School Voucher	-0.031	-0.053	-0.004
	(0.083)	(0.098)	(0.087)
	[-0.001]	[-0.001]	[<-0.001]
Interaction with Identifies as non-White	-0.443***	-0.410***	-0.426***
	(0.141)	(0.154)	(0.144)
	[-0.011]	[-0.010]	[-0.011]
Observations	159,016	159,016	159,016
Panel C: Prior Test Performance			
Fraction Peers using Public School Voucher	0.021	0.022	-0.022
	(0.083)	(0.094)	(0.090)
	[0.001]	[0.001]	[-0.001]
Interaction with Prior Year's Score in Bottom Tercile	-0.397***	-0.399***	-0.260***
	(0.098)	(0.122)	(0.091)
	[-0.010]	[-0.010]	[-0.007]
Interaction with Prior Year's Score in Top Tercile	-0.267**	-0.455***	0.060
	(0.130)	(0.132)	(0.142)
	[-0.007]	[-0.011]	[-0.002]
Observations	126,762	126,762	126,762

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 grade-year, school-grade fixed effects, as well as a school-specific linear trend. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.

Table 6: Heterogeneity by Program Expansion

VARIABLES	Test Score Pre-Expansion	Test Score Post-Expansion	Math Score Pre-Expansion	Math Score Post-Expansion	ELA Score Pre-Expansion	ELA Score Post-Expansion
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Voucher Peers						
Fraction Peers Using Voucher	-0.272*	-0.090	-0.319**	-0.075	-0.198	-0.081
	(0.140)	(0.111)	(0.153)	(0.120)	(0.153)	(0.107)
	[-0.007]	[-0.002]	[-0.008]	[-0.002]	[-0.005]	[-0.002]
Observations	46,919	112,090	46,919	112,090	46,919	112,090

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 grade-year and school-grade fixed effects. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.

Table 7: Descriptive Statistics

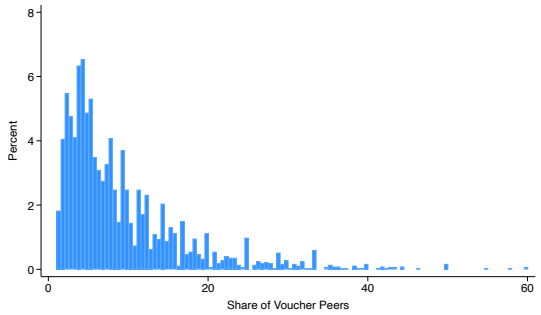
	Voucher Students 2012-2013		Voucher Students 2014 and After		
	Mean	SD	Mean	SD	**Difference**
	(1)	(2)	(3)	(4)	(5)
Panel A: Demographic Characteristics					
White	0.514	(0.500)	0.486	(0.500)	0.027***
Black	0.176	(0.381)	0.159	(0.366)	0.017**
Hispanic	0.218	(0.413)	0.263	(0.441)	-0.046***
Female	0.511	(0.500)	0.516	(0.500)	-0.005
FRPL	0.625	(0.484)	0.642	(0.479)	-0.017*
Has Math Accommodation	0.102	(0.303)	0.141	(0.348)	-0.039***
Has ELA Accommodation	0.099	(0.299)	0.140	(0.347)	-0.041***
Panel B: Education Outcomes					
Standardized Test Score	-0.319	(0.845)	-0.231	(0.832)	-0.088***
Standardized Math Score	-0.366	(0.863)	-0.293	(0.836)	-0.073***
Standardized ELA Score	-0.201	(0.856)	-0.132	(0.858)	-0.069***

Notes: This table shows descriptive statistics on voucher students that entered participating private school before and after the expansion of the Indiana Choice Scholarship Program. Data on enrollment and test scores come from the IDOE-CREO database. Public school voucher students are those that used the voucher to move from the public to private sector. Column (5) reports the difference in the means between voucher students that entered after 2014 versus in the first two years of the program.

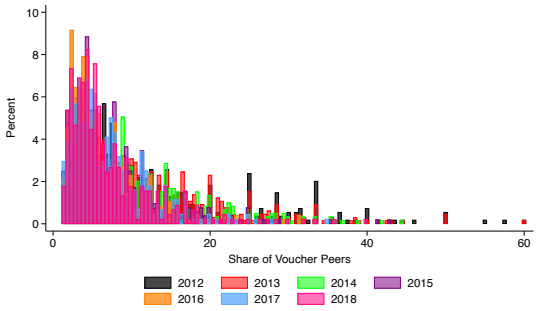
IX Figures

Figure 1: Variation in Share of Voucher Peers

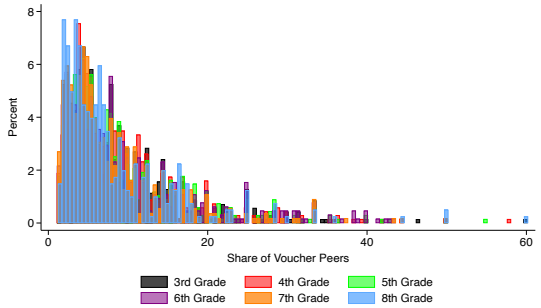
(a) Overall Variation by School-Grade Cohort (>0)



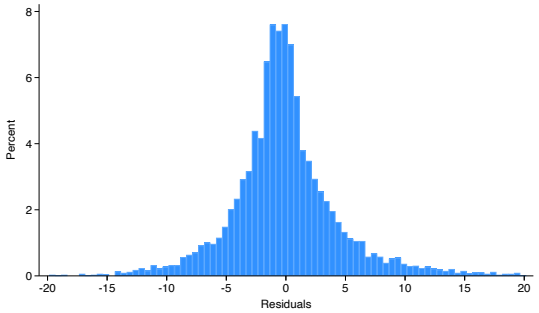
(b) Variation Across Years



(c) Variation Across Grades



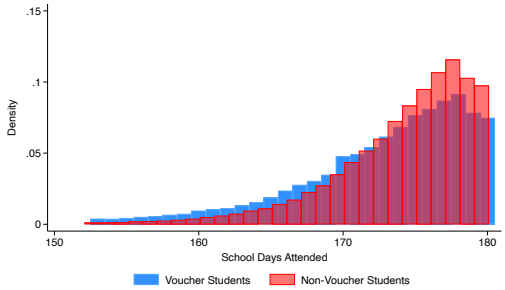
(d) Residualized Variation



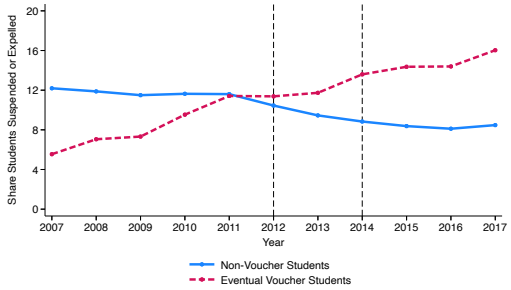
Notes: This figure presents histograms of the variation in voucher peers across school-grade cohorts. Panel A presents the variation across all schools and years in the sample period, Panel B plots separate histograms by year, Panel C plots separate histograms by grade, and Panel D residual variation once accounting for the school-grade, grade-year and school-specific linear time trends. Data on enrollment and test scores come from the IDOE-CREO database.

Figure 2: Attendance and Discipline by Voucher Status

(a) Attendance

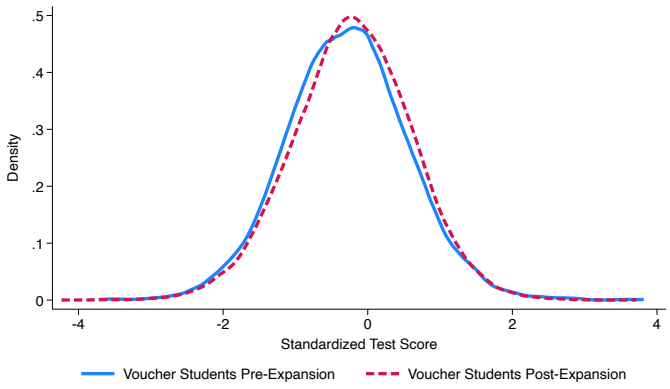


(b) Discipline



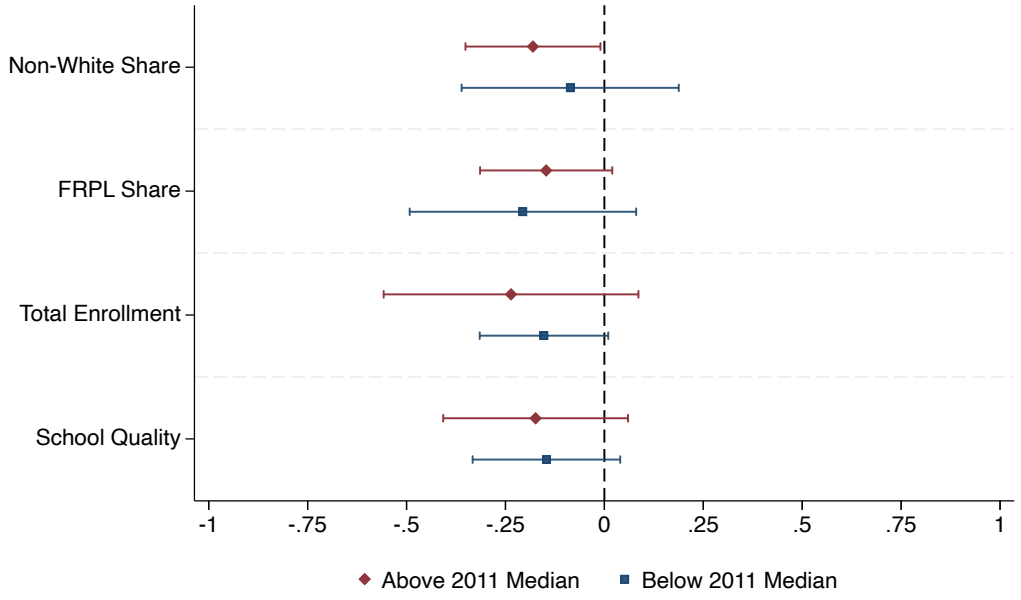
Notes: This figure presents the attendance and discipline rates by voucher status. Panel A presents histograms of the number of school days attended for public school voucher students (blue) and their non-voucher counterparts (red). Public school voucher students are those that used the voucher to move from the public to private sector. Panel B presents average discipline rates by year across public school students that either eventually use a voucher to attend a private school (red) or remain in the public school system (blue). Data on enrollment, attendance and discipline come from the IDOE-CREO database.

Figure 3: Test Score Distribution of Voucher Students by Program Timing



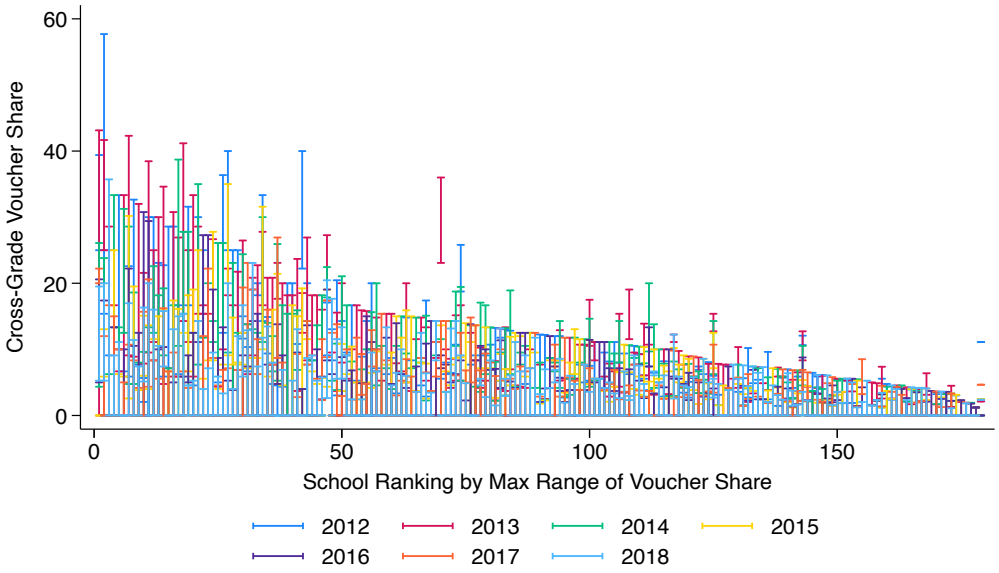
Notes: This figure presents the kernel density plots of standardized test scores for voucher students that attended a participating private school before and after the expansion of the Indiana Choice Scholarship Program. Data on enrollment and test scores come from the IDOE-CREO database.

Figure 4: Heterogeneity by Pre-Policy School Characteristics



Notes: This figure presents the estimates of Equation 1 when we split the sample by pre-policy school characteristics. Each point is the result of a separate estimation. Red diamonds always represent the results for above median baseline value and Blue squares always represent the results for below median baseline value. 95% confidence intervals are reported. All estimations include school-grade, grade-year fixed effects, as well as school specific linear time trends. Data on enrollment and test scores come from the IDOE-CREO database.

Figure A1: Cross-Grade Variation in Voucher Peer Share



Notes: This figure presents the cross-grade variation in the share of voucher peers for each school and year in the sample. The bars represent the range of the share of voucher students in school-grade cohort in a given school-year combination. The x-axis orders the schools by the max range of voucher share across the entire sample period. Data on enrollment come from the IDOE-CREO database.

Table A1: Stability of Results on Student Achievement

	Overall				Math				ELA			
	No Controls No Weights	Weights Only	Controls Only	Preferred	No Controls No Weights	Weights Only	Controls Only	Preferred	No Controls No Weights	Weights Only	Controls Only	Preferred
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to Public School Voucher Peers												
Fraction Peers using Voucher	-0.121*	-0.206***	-0.102	-0.161**	-0.120	-0.229**	-0.094	-0.173*	-0.109	-0.156*	-0.099	-0.129*
	(0.071)	(0.079)	(0.068)	(0.072)	(0.082)	(0.092)	(0.081)	(0.088)	(0.075)	(0.083)	(0.070)	(0.077)
Observations	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016	159,016
School-Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Specific Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Weights	No	Yes	No	Yes	No	Yes	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. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.

Table A2: Results on Student Achievement with Restricted Sample

VARIABLES	Std. Test Score	Std. Test Score	Std. Math Score	Std. Math Score	Std. ELA Score	Std. ELA Score
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Voucher Peers						
Fraction Peers using Voucher	-0.251**	-0.199*	-0.254*	-0.221*	-0.200*	-0.148
	(0.120)	(0.104)	(0.137)	(0.118)	(0.118)	(0.113)
	[-0.006]	[-0.005]	[-0.006]	[-0.006]	[-0.005]	[-0.004]
Observations	108,453	108,453	108,453	108,453	108,453	108,453
R-squared	0.187	0.195	0.180	0.190	0.175	0.181
School-Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School Specific Linear Time Trend	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. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. The included sample only includes students that never used a voucher. Data on enrollment and test scores come from the IDOE-CREO database.

Table A3: Falsification Test Using Non-Voucher New Students

VARIABLES	Std. Test Score	Std. Math Score	Std. ELA Score
	(1)	(2)	(3)
Fraction Non-Voucher New Students	-0.095	-0.104	-0.090
	(0.099)	(0.109)	(0.100)
	[-0.002]	[-0.003]	[-0.002]
Observations	124,039	124,039	124,039

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 grade-year, school-grade fixed effects, as well as a school-specific linear trend. Individual controls include gender, race, FRPL status, and accommodation status. All regressions are weighted by the inverse number of times a student is observed in the sample. Public school voucher students are those that used the voucher to move from the public to private sector. The marginal effect of adding one voucher peer to a class of 40 is shown in brackets, and is defined as the coefficient divided by 40. Data on enrollment and test scores come from the IDOE-CREO database.