Impact of the Indiana Choice Scholarship Program: Achievement Effects for Students in Upper Elementary and Middle School*

R. Joseph Waddington
Mark Berends

Abstract

This paper examines the impact of the Indiana Choice Scholarship Program on student achievement for low-income students in upper elementary and middle school who used a voucher to transfer from public to private schools during the first four years of the program. We analyzed student-level longitudinal data from public and private schools taking the same statewide standardized assessment. Overall, voucher students experienced an average achievement loss of 0.15 SDs in mathematics during their first year of attending a private school compared with matched students who remained in a public school. This loss persisted regardless of the length of time spent in a private school. In English/Language Arts, we did not observe statistically meaningful effects. Although school vouchers aim to provide greater educational opportunities for students, the goal of improving the academic performance of low-income students who use a voucher to move to a private school has not yet been realized in Indiana. © *2018 by the Association for Public Policy Analysis and Management.*

INTRODUCTION

Educational reform efforts have often failed to deliver (Berends, Bodilly, & Kirby, 2002; Zimmer, Henry, & Kho, 2017). For instance, over the last decade, the federal government invested $7 billion in School Improvement Grants (SIG)—the largest federal investment to date—to improve performance in struggling schools (Kahlenberg, 2017). These grants were aimed at the lowest performing schools and gave them four options from which to choose: (1) transform the school by bringing in a new principal; (2) turn the school around by firing a majority of the teachers and the principal; (3) restart the school by turning over its management to charter schools; or (4) close the school and enroll students in higher achieving district schools. The result? Research has shown this investment did not have a significant impact on improving student outcomes: "Overall, across all grades, we found that implementing any SIG-funded model had no significant impacts on math or reading test scores, high school graduation, or college enrollment" (Dragoset et al., 2017, p. 3).

Current federal leadership has turned from the SIG approach to school choice policies and programs aimed at improving low-performing schools. These programs include charter schools, interdistrict choice programs, and private school vouchers

*Author order was determined randomly. Each contributed equally to this paper.

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or tax credit programs. School vouchers, or scholarships, are provided to qualifying families so they can send their children to the schools of their choice, whether public or private, religious or nonreligious. If voucher programs were succeeding, we would expect that participating students are experiencing new opportunities to learn, increasing their achievement growth, and closing achievement gaps (e.g., socioeconomic and racial/ethnic). In this paper, we assess those expectations by examining the impact of Indiana's Choice Scholarship Program (ICSP)—the largest single statewide voucher program in the United States—on low-income students' math and English/Language Arts (ELA) achievement. Despite the program's large size, little is known about its effects on Indiana schools and students.

This study aims to address this gap in a context where vouchers have not been randomly assigned to families in Indiana. Instead, our analysis uses a rich set of longitudinal, student-level records for public and private school students in grades 3 through 8. We used a variety of estimation strategies to examine the program's impact on low-income students receiving a voucher and switching from a public to a private school during the first four years of the voucher program (2011/2012 through 2014/2015). This group of voucher students aligns with the initial eligibility requirements and intent of the Indiana voucher program. Our research questions are as follows:

- What is the yearly impact of receiving a voucher and switching to a private school on low-income students' math and ELA achievement compared to peers remaining in public schools?
- What are the differences in yearly voucher impacts across various subgroups of students (i.e., by sex, race/ethnicity, English proficiency, or special education status) or private schools (i.e., Catholic vs. Other Religious, or urban locale)?

Overall, we found no consistent evidence that vouchers promote increased academic achievement among low-income recipients. In contrast, students who use a voucher to attend private schools experienced a modest average achievement loss in mathematics and no effects in ELA. The achievement loss in mathematics is greatest for students in the first two years after receiving a voucher and persists through four years. Our findings in both subjects are consistent across most student subgroups.

Our research contributes to the existing school voucher literature in three distinct ways. First, Indiana's statewide school voucher program is the nation's most expansive in terms of enrollment and in providing scholarships to both low- and modest-income families.[1] Second, our study is one of only a handful that finds modest and statistically significant negative effects of school vouchers on student math achievement. Third, we examined the effects of a voucher program that operates within a different context from other programs, whereby many private schools were participating in statewide testing prior to the implementation of the voucher program. Finally, unlike other statewide studies that cover a shorter time frame (Abdulkadiroglu, Pathak, & Walters, 2018; Figlio & Karbownik, 2016; Mills & Wolf, 2017), we examined the impact for students who have received a voucher to attend private schools up to four years.

In what follows, we provide a brief background on the Indiana voucher program and the evidence about the effects of vouchers on student achievement based on prior rigorous research. We go on to describe the data and our approach to analyzing the effects of the first four years of the statewide program. We conclude with the results and a discussion of their implications.

[1] The State of Ohio has more students enrolled in voucher programs across the state. However, these students are spread across five different programs, each with their own eligibility criteria and focus.

THE INDIANA CHOICE SCHOLARSHIP PROGRAM

Currently, 15 states have voucher programs, and 181,175 students are using them to attend private schools (EdChoice, 2018). In Indiana, which has the largest single program, 35,458 students received a voucher during the 2017/2018 school year and 318 private schools participated (76 percent of private sector schools statewide) (Indiana Department of Education, 2018). Authorized in 2011, the ICSP provides state payments to qualifying Indiana families to help offset tuition at participating schools. When the program began, students qualified for vouchers based on their prior enrollment in a public school and their family's total household income. Starting in 2013/2014, the state removed the cap on the number of eligible Indiana students who can receive a scholarship. It also expanded the criteria for eligibility to include kindergarten students, siblings of voucher students, special education students, and those located in the attendance zones of failing public schools.

Although researchers have studied the effects of various school voucher programs and policies over the past two decades (for reviews, see Austin & Berends, 2018; Epple, Romano, & Urquiola, 2017; Figlio, 2009; Shakeel, Anderson, & Wolf, 2016; Zimmer & Bettinger, 2015), the Indiana voucher program is unique.[2] Unlike other programs, the ICSP is aimed at both low- and modest-income families (Indiana Department of Education, 2018). Families with the lowest income may obtain vouchers for up to 90 percent of tuition (average 2017/2018 voucher value in grades 1 through 8 of about $4,800) at a participating private school if their annual income is equal to or less than 100 percent of the amount to qualify for reduced-price lunch, which is inclusive of free lunch eligibility (together referred to as "FRPL"), under the National School Lunch Program (Indiana Department of Education, 2018).[3] For a four-person household, that income threshold was $44,955. Moderate-income families may obtain 50 percent vouchers (average 2017/2018 voucher value in grades 1 through 8 of about $2,900) if their annual income is equal to or less than 150 percent of the amount to qualify for FRPL; for a four-person household, that income threshold was $67,433 (Indiana Department of Education, 2018).

VOUCHER RESEARCH ON STUDENT ACHIEVEMENT OUTCOMES

Voucher programs are typically aimed at low-income families to offer educational opportunities they may not otherwise access: schools that better meet their children's academic needs. Proponents claim that, as more schools compete for students, all schools will become more effective in encouraging positive student outcomes, especially for low-income students (Chubb & Moe, 1990). Friedman (1955, 1962) was one of the first to use this market theory, arguing that the cost of K-12 education should be covered by the government but parents should be able to choose the schools their children attend, whether public or private. Toward this end, Friedman promoted giving parents government vouchers as a way to accomplish a system of education that was publicly financed but delivered privately and publicly.

Critics, however, raise questions about the empirical validity of the market theory's key assumptions about parents as consumers (demand-side), schools (supply-side),

[2] With the new Trump administration and discussions about turning Title I money into vouchers and the possibility of bundling state and federal education funds, states like Indiana may be poised to significantly expand their voucher programs (Berends, 2018). Thus, understanding the effects of Indiana's student voucher use informs state and national education policy at a critical time.

[3] The amount of a voucher in Indiana is determined based on the lesser of the private school's tuition and fees (at which the student must first be accepted before receiving a voucher) and the state per-pupil subsidy granted to the public school district (corporation) in which the student resides. In 2017/2018, net voucher payments from the Indiana Department of Education to private schools totaled $154 million.

and the products that a market in education would generate (Austin & Berends, 2018; Finnegan, 2007; Henig, 1995; Hess, 2002; Levin, 1998). They emphasize that public schools support the "common school" model that promotes civic and democratic values among its students. In this light, critics argue, vouchers may increase already existing inequalities by skimming off the best students, decrease support of public schools due to falling enrollments in an era of fiscal challenges, and undermine our democracy.[4] Moreover, students who transfer with vouchers may experience achievement losses because student mobility is often associated with negative school outcomes, independent of the quality of the school (Grigg, 2012; Schwartz, Stiefel, & Cordes, 2017).

With the global expansion in the number of voucher programs, research addressing the effects of these programs has increased as well (Berends, 2018). Evidence can be drawn from both publicly and privately funded voucher programs in the United States and from international research (Epple, Romano, & Urquiola, 2017; Figlio, 2009; Shakeel, Anderson, & Wolf, 2016; Zimmer & Bettinger, 2015).

A number of voucher studies examining impacts on student achievement outcomes have focused on specific cities: Milwaukee (Greene, Peterson, & Du, 1998, 1999; Rouse, 1998; Witte, 2000; Witte et al., 2014), Charlotte (Cowen, 2008; Greene, 2001), Cleveland (Metcalf et al., 2002), Dayton (Howell & Peterson, 2006), New York City (Barnard et al., 2003; Jin, Barnard, & Rubin, 2010; Krueger & Zhu, 2004), and Washington, DC (Howell & Peterson, 2006; Wolf et al., 2010, 2011, 2013; Wolf & McShane, 2013). Generally, the experimental and quasi-experimental research in these cities shows either modest positive effects on student test scores for certain subgroups of students and for certain years of program participation, or no effects at all (Austin & Berends, 2018; Epple, Romano, & Urquiola, 2017; Figlio, 2009; Zimmer & Bettinger, 2015).[5]

More recent statewide studies on the impact of voucher programs in Louisiana and Ohio have shown negative effects on student achievement. Abdulkadiroglu, Pathak, and Walters (2018) examined the Louisiana Scholarship Program, analyzing data between 2008 (the first year of the program) and 2012. Following students who won and lost a lottery to receive a scholarship, the authors found significant and large negative effects for students who participated in the first year of the voucher program, with declines of 16 percentile points in math and 14 percentile points in reading. The effects were consistent across income groups, geographic areas, and private school characteristics (higher and lower proportion of white students, enrollment, achievement scores, and whether the private school was Catholic).

Mills and Wolf (2017) investigated the Louisiana program through its second year, reporting substantial negative effects in both math and reading in year one, but less negative effects in year two. Only the effects for mathematics were statistically significant after year two. In mathematics in year two, they found that students who won the voucher lottery and transferred to a public school scored 0.34 of a SD below those students who lost the voucher lottery. "The magnitude of these negative estimates," the researchers wrote, "is unprecedented in the literature of random-assignment evaluations of school voucher programs" (p. 2).

[4] During the first two years of the voucher program, public school districts (corporations) experienced a reduction in their state funding based on the number of students who left the district after receiving a voucher to attend a private school. Beginning in the 2013/2014 school year, the state began restoring these per-pupil subsidies back to public school districts to make up for state funding lost due to district students participating in the voucher program.

[5] The exception to these overall findings is a recent study in Washington, DC on the DC Opportunity Scholarship Program that found negative effects in mathematics after the first and second years of the program (Dynarski et al., 2017, 2018). The findings in DC are consistent with those of the statewide voucher programs.

These findings are consistent with what Figlio and Karbownik (2016) found in their evaluation of the Ohio EdChoice Scholarship Program. The researchers used propensity score-matching to estimate the program's effects because the program did not rely on a lottery to provide scholarships. Analyzing student-level data between 2007 and 2010, with several estimation specifications, they found significant negative effects on both reading and mathematics scores: about −0.40 to −0.20 SDs in reading and −0.60 to −0.45 SDs in mathematics.

In our study of the ICSP, we also found negative effects in mathematics for students who transfer from public to private schools with a voucher. However, our research differs from evaluations of the Louisiana and Ohio statewide voucher programs in a number of ways. Unlike Louisiana and Ohio, students in Indiana's public and private schools have all taken the same state tests for a number of years. Thus, our findings come from a state context where annual testing in grades 3 through 8 is common across the board, particularly in a broad sample of over 300 voucher-participating private schools. In the year prior to the voucher program (2010/2011), the average private school has achievement 0.1 to 0.2 SDs above the state mean in both math and ELA. While average private school achievement varies, many higher performing private schools participate in Indiana's voucher program than in other states.[6] In addition to broader income eligibility in Indiana, students from all public schools are eligible as opposed to just those enrolled in the lowest performing schools as in Ohio. While students in low- or modest-income families may be eligible to receive a voucher, we focused on the lowest-income students for estimation purposes and better comparisons of our findings with other contexts.

DATA AND MEASURES

Data Description

We used six years (2009/2010 school year through 2014/2015) of longitudinal, student-level demographic and test score records for this study, obtained through a data-sharing agreement with the Indiana Department of Education (IDOE). The records contain information about students attending public (traditional, charter, and magnet) and private schools (including voucher and non-voucher students) that participated in the Indiana Statewide Testing for Educational Progress Plus (ISTEP+) program. The ISTEP+ is aligned to the Indiana Academic Standards and serves as the main accountability-linked assessment for Indiana students in grades 3 through 8. Testing has taken place each spring since 2009 in mathematics and ELA (Indiana Department of Education, 2011).[7]

Indiana is unique because many private schools participate in the ISTEP+ program and other state reporting (304 schools statewide as of 2015). Participation in ISTEP+ testing is a requirement of all private schools participating in the voucher program.[8] However, nearly all K-8 Catholic schools and over 80 other religious and nonreligious K-8 private schools, participated in statewide testing as part of their accreditation process for several years prior to the start of the voucher program. Additional private schools began taking the ISTEP+ after starting participation in

[6] Most elite, nonsectarian private schools do not participate in the voucher program.
[7] The ISTEP+ is vertically equated across grades and consists of multiple-choice, constructed-response, and extended-response items scored using item response theory methods. Reliability coefficients range from 0.88 to 0.94 in ELA and 0.88 to 0.95 in math (Indiana Department of Education, 2011).
[8] The Indiana Department of Education holds voucher program-participating private schools accountable through their performance on the ISTEP+ assessment by restricting their ability to enroll students receiving vouchers should the school have two consecutive years of poor testing performance.

the voucher program. All students in private schools, including enrolling students with vouchers, take the test, regardless of whether an individual student received a voucher.

The robust annual participation in statewide testing and other reporting by private schools offers several advantages. First, we can make apples-to-apples achievement comparisons between voucher private and non-voucher public school students. Second, the number of participating schools and the testing of non-voucher private school students allow us to better describe the academic composition of the private school sector in Indiana. Third, because each student's testing records are longitudinally linked, we can observe changes in an individual student's achievement over time, regardless of the sector in which they are enrolled.

Measures

The primary outcomes of interest are students' annual ISTEP+ test scores in mathematics and ELA. These are the two subjects tested annually during grades 3 through 8. We standardized each of the scaled test scores relative to the mean and SD of students statewide within each subject, grade, and year of testing.[9] The standardized measures allow us to draw comparisons, in SD units relative to the state average of all test takers.

We used several student demographic and background characteristics reported in the IDOE data, including indicators of each student's gender, race/ethnicity,[10] FRPL status, English Language Learner status (ELL), special education status, and grade level. We created an indicator for grade retention from the previous year. We also observed whether a student received a voucher in each year. This allowed us to construct an annual indicator of voucher receipt and a measure of the total number of years a student received a voucher.

Along with voucher recipient status, we also observed the student's school of record within each year. The school records contain each school's National Center for Education Statistics (NCES) unique identification number. Using the NCES ID, we linked the schools to the Common Core of Data (CCD) and the Private School Universe Survey (PSS) to augment the available school-level data.[11] We used these data to create binary indicators of the school type (e.g., public, charter, magnet, Catholic, or other private) and private school locale (urban, suburban, or town/rural). We manually entered this information for schools with missing data.

ANALYTICAL SAMPLE AND ESTIMATION STRATEGY

Our main research question pertains to the academic achievement of voucher students who attend private schools. In an ideal experimental setting, voucher-eligible students attending public schools would be randomly assigned an offer to receive a voucher. We could then estimate unbiased intent-to-treat effects of being offered a

[9] Although the ISTEP+ is vertically equated, we did not use scaled scores for our outcome as the variation in scales differs between grade levels. This introduces additional measurement error; however, we adjusted for differences between years and across tests by controlling for grade and year fixed effects in all models.

[10] We re-coded the race/ethnicity variable into four categories: White, Black/African American, Latino/a, and Other Race/Ethnicity, due to the small number of voucher students identifying as either Asian/Pacific Islander, American Indian/Alaskan Native, or multiracial.

[11] The CCD contains annual demographic and background information for the universe of public schools. Similarly, the biennial PSS contains similar information for private schools. We applied CCD data to all public schools for each corresponding year. Similarly, we applied PSS data from the most recent prior year to all private schools.

voucher on student achievement by comparing the achievement gains of students offered and not offered a voucher. We would then also be able to use this assignment as an instrument in a two-stage least-squares approach for actual voucher use and attendance in a private school. Here, we could obtain the treatment-on-the-treated effects of private school attendance on student achievement. Many voucher programs (e.g., Milwaukee, New York City, Washington, DC, Louisiana) either randomly assigned vouchers or held voucher lotteries, enabling researchers to estimate causal effects.[12]

In Indiana, vouchers were not randomly assigned to students through the ICSP, making it challenging to assess the causal effects of receiving a voucher and attending a private school on student outcomes. Individual private schools participating in the voucher program are not required to hold lotteries to determine enrollment, except for oversubscribed schools. Most private schools had an excess supply of available seats over the period of our study, and we found no private schools that implemented enrollment lotteries.[13] We did observe any student who received a voucher and attended a private school in grades 3 through 8.[14]

Without a random-assignment of vouchers or a natural experiment such as a lottery, any assessment of the effects of Indiana's voucher program is subject to selection bias. Choosing to apply for and receiving a voucher depends on the active choices of parents and their children. These choices typically depend on student background, parental preferences, motivation, and available opportunities in public or other choice (e.g., charter, magnet) schools. For example, if students with high aptitude or motivation apply for and receive a voucher, then the performance of voucher students might appear better than non-voucher students because of potentially unobserved background differences between students. Thus, we cannot simply compare the achievement of voucher and non-voucher students.

Given the availability of longitudinal data and the eligibility criteria of the ICSP, we took several steps to mitigate selection bias. First, we will describe the process of creating a comparable sample of students who receive a voucher and attend a private school and students who do not receive a voucher and remained enrolled in a public school. Then, we will describe multiple strategies used to estimate the effects of the voucher program on student achievement. For both the sample construction and estimation strategies, we drew upon important lessons from recent literature that uses nonexperimental approaches to replicate the experimental estimates of school choice evaluations (Anderson & Wolf, 2017; Bifulco, 2012; Fortson et al., 2014) and the implementation of those lessons in the nonexperimental evaluation of charter schools (Angrist, Pathak, & Walters, 2013; Dobbie & Fryer, 2013, 2017).

We implemented several data restrictions prior to sample construction (see Appendix A).[15] These restrictions included requiring each student to have at least three years of test scores, including two years before receiving a voucher (a pre-baseline and baseline year). After these restrictions, we have a sample of 11,756 voucher students and 556,919 public school students.

[12] Recent evaluations of charter schools (e.g., Abdulkadiroglu et al., 2011; Angrist et al., 2012; Clark et al., 2015; Dobbie & Fryer, 2013) also used a similar approach in instances where charter schools hold enrollment lotteries.

[13] We contacted each of the five Catholic dioceses in the state, the Indiana Non-Public Education Association, and the Indiana Department of Education to confirm this in the first years of the voucher program.

[14] We observed very few voucher "decliners," or students who applied for a voucher but did not receive one. From principal and parent interviews, families only applied for vouchers if they knew they met the eligibility criteria.

[15] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Voucher Student Sample

A student must meet several eligibility criteria to qualify for a voucher in Indiana. One criterion from the initial implementation of the policy was that a student had to be enrolled in a public school (either traditional public, charter, or magnet) for at least one year immediately prior to receiving a voucher. In our cleaned data, 4,384 students moved from a public to a private school for the first time after receiving a voucher.[16] Of the students who were once enrolled in a private school without a voucher, 209 left for one year to attend a public school, and returned to a private school after receiving a voucher.[17] The other 7,163 students received a voucher for the first time while previously enrolled in a private school, largely a result of expanded voucher eligibility criteria beginning in the 2013/2014 school year.[18]

Our focus solely on the voucher students moving from a public to a private school for the first time yields several advantages for our analysis. First, this movement is typical of other voucher programs, and most evaluations compare voucher and public school students. With longitudinal records of public school students, we can draw comparisons of voucher students switching to private schools with students remaining in public schools and not receiving a voucher. Second, previous enrollment in a public school allowed us to establish a baseline level of student achievement before receiving a voucher and attending a private school for the first time observed in our data. We could have established a baseline prior to receiving a voucher for students previously enrolled in a private school; however, these students have prior private school experience that may have influenced the impacts of receiving a voucher and attending a private school in a different manner than first-time private school enrollees. We display comparisons of all voucher and non-voucher private school students in Appendix Table A1.[19] We found that voucher students initially enrolled in private schools are much higher achieving and less diverse along demographic and academic dimensions.

The second eligibility criterion for all voucher students is based on family income. The voucher income thresholds based on household size directly correspond to the thresholds for reduced-price lunch (RPL) eligibility, which is inclusive of free lunch eligibility (Indiana Department of Education, 2018). Students in families at or below the income threshold for RPL eligibility are eligible for a "90 percent" or "full" voucher for tuition at a private school. Students in families at or below 150 percent of the income threshold for RPL eligibility are eligible for a "50 percent" or "half" voucher.

Because of the direct correspondence with RPL eligibility, we focused on the 3,883 voucher students switching from public to private schools that either received a "full" voucher or received FRPL in the two years prior to receiving a voucher. We refer to this group of voucher students as "low-income."[20] We found non-low-income voucher students have much higher achievement before receiving a voucher and are less diverse along demographic and academic dimensions (see Appendix

[16] Some of these students eventually exit a private school after receiving a voucher and return to a public school. We included both students who remained in a private school and those who returned to a public school in our analysis.
[17] We believe these 209 students and their families made these decisions to become eligible for a voucher.
[18] Of these students, 649 later attended public school while 6,514 were always enrolled in a private school.
[19] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[20] As income fluctuates, we wanted to account for indications that a family is low-income in either the year before receiving a voucher (baseline year) or the year after. We used a similar procedure for public school students.

Table A1). Because we specifically focused on the lowest income voucher students who transitioned from a public to a private school, our analysis represents only a partial effect of the voucher program on student achievement. Thus, effects may differ for all other voucher students.

Public School Comparison Student Sample

As our voucher student sample consists of students leaving public schools to attend private schools, not all Indiana public schools are represented. One key takeaway from the quasi-experimental study design literature and the application of within-study comparisons in educational research (see Bifulco, 2012; Cook, Shadish, & Wong, 2008) is that treatment and comparison groups should be drawn from the same geographic location (i.e., the same school). So, we constrained our comparison sample to include only public school students of the same race/ethnicity and sex in the same grade, year, and public school as a student who receives a voucher and attends a private school the following year.[21] This process also establishes a baseline year from which we can draw comparisons between voucher and public school students over time. We refer to the exact match between voucher and public students by race-sex-year-grade-school as a "matching cell."

The exact matching approach on several observable dimensions helps to mitigate selection bias in terms of who does and does not receive a voucher. By matching students by grade, year, and school, we compare voucher and public school students in the same schooling context beginning at baseline. The exact matching based on a student's race and sex further accounts for variation in the selection process. For example, if students of a certain race/ethnicity were more or less likely to participate in the voucher program, we are now comparing them to their same-race peers who should share the same likelihood of selection into the program.

This is the same approach used by researchers in the nonexperimental evaluation of charter schools in Massachusetts, New York City, and Texas (see Angrist, Pathak, & Walters, 2013; Dobbie & Fryer, 2013, 2017). These researchers compared estimates using the nonexperimental approach with their own experimental estimates on the same sample of students and found similar results in terms of the effect of attending a charter school on student achievement or attainment outcomes. If differences existed, these researchers noted that the nonexperimental approach collectively tends to bias the effects slightly toward zero.

The exact matching approach shares important features with propensity score-matching (Rosenbaum & Rubin, 1983). Both approaches rely on the matching of students based on a limited set of observable characteristics of students that may be associated with the selection process. While the exact matching process is more precise than matching on propensity scores, the number of matching variables must be limited when using exact matching in order to construct a sufficiently powered sample of treatment and comparison students. We believe that race, sex, and sharing a baseline year, grade, and school are a reasonable set of criteria to mitigate selection bias, and there is an empirical basis for using these characteristics. Yet, as with propensity score-matching, we are adjusting for only observable differences between voucher and non-voucher students. Our estimates of the voucher program's effects remain subject to bias based on any unobservable characteristics that may drive selection into the program. We further detail these concerns when describing our approach to the estimation of voucher program effects.

[21] Some public school students have peers who leave to attend a private school with a voucher across several grades and years. To avoid replicating individual students in our sample, we randomly chose which of a given public school student's years serves as the baseline year.

After constructing the matching cells, we further constrained our comparison sample to include only public school students who may be eligible to receive a full voucher. As we previously described, "full" voucher eligibility corresponds directly to the income threshold for RPL eligibility. Therefore, we constrained the public-school comparison group to students who also received FRPL in the baseline or first post-baseline year. By constraining our sample, we can draw more relevant comparisons between low-income voucher students and non-voucher, low-income public school students who would also be considered "eligible" to receive a full voucher. In effect, this mitigates the unobserved influence that family income may have on selecting to attend a private school. We compared all non-low-income public school students to their voucher-eligible public school peers in Appendix Table A2.[22]

To finalize our analytical sample, we only included low-income voucher students who have a low-income public school peer of the same sex, race, and baseline school, grade, and year. Similarly, we only included public school students who have a voucher peer who shares the same characteristics as above at baseline. For each student, we have achievement data from at least three years in at least one subject: pre-baseline, baseline, and at least one year post-baseline.

Our analytical sample includes 34,587 low-income public school students who are in the same school, grade, and year at baseline as 3,363 students of the same sex and race/ethnicity who received a voucher and attended a private school in the subsequent year. Given data constraints, we matched 87 percent of all low-income voucher students who switch from a public to a private school to at least one public school peer at baseline. We compared the matched versus unmatched voucher students in Appendix Table A1 and public school students in Appendix Table A2.

Student Descriptive Characteristics

Before moving to our approach to estimation, we describe our analytical sample and display the results in Table 1. More than half of the voucher students in our sample are racial or ethnic minorities, with a slightly lower proportion of black students (0.238) and higher proportion of Latino/a students (0.226) relative to their low-income public school peers (0.252 proportion of black students and 0.168 of Latino/a students). The proportion of voucher students classified as ELLs is 0.123 and as special education students is 0.079 at baseline. In the voucher sample, ELL students are overrepresented and special education students underrepresented compared to the public school peer sample. Over half of the low-income voucher and public school students are attending urban public schools at baseline.

In terms of academic achievement, low-income voucher students are lower achieving compared to the state average by nearly one-fourth of an SD in both math and ELA. However, voucher students are higher achieving than their low-income public school peers by about a tenth of an SD in both subjects. The low average achievement and diversity of low-income voucher students previously attending public schools suggests that private schools are not "cream-skimming" the highest achieving students from public schools per se, but there is slight positive selection when comparing to voucher-eligible peers remaining in public schools.

In Table 1 we provided a comparison between low-income voucher and public school students. However, we can also compare low-income voucher students and all

[22] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 1. Descriptive comparison of matched analytical sample of low-income voucher and public school students in Indiana.

	Voucher	Public
Students	3,363	34,587
Schools	265	871
	Baseline measures	
Female	0.526	0.527
Black	0.238	0.252
Latino/a	0.226	0.168
Other race/ethnicity	0.076	0.027
English Language Learner	0.123	0.093
Special education	0.079	0.114
Retained	0.008	0.008
Attended charter	0.109	0.065
Attended magnet	0.027	0.020
Attended suburban school	0.230	0.243
Attended town/rural school	0.212	0.246
Mean math score	−0.244	−0.335
Mean ELA score	−0.220	−0.333
Mean math gain	−0.023	−0.055
Mean ELA gain	−0.010	−0.046
	First-Year post-baseline measures	
Attended Catholic school	0.538	–
Attended other private school	0.462	–
First-Year exit rate	0.163	–
Mean math score	−0.380	−0.330
Mean ELA score	−0.213	−0.313
Mean math gain	−0.135	0.005
Mean ELA gain	0.004	0.019

Notes: Table displays voucher and public school (traditional public, charter, or magnet) students with at least three years of test scores in either math or ELA (pre-baseline, baseline, first-year post-baseline) and matched within the same race-sex-year-grade-public school matching cell at baseline. Number of public schools reported at baseline and voucher private schools in first-year post-baseline. ISTEP+ Math and ELA scores measured in SD units, relative to the Indiana statewide mean and SD within each grade and year.
Source: Authors' calculations.

other private school students for added context (see Appendix Table A1).[23] Besides income differences, low-income voucher students switching from public to private schools are much more likely to be a racial or ethnic minority or ELL. This group of voucher students is also substantially lower achieving than their private school peers. Thus, low-income voucher students are moving into environments substantially behind their peers in terms of academic achievement (by up to more than a half SD), resulting in this group of voucher students experiencing a markedly different schooling context than previously.

Estimation Strategy

Although we have specifically matched low-income voucher and public school students who are more closely aligned than a broader sample of voucher and public

[23] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

school students, we still found a number of meaningful differences between the two groups at baseline. Voucher students are higher achieving than their matched public school peers at baseline. This observable difference, among others, suggests that the two groups of students may also differ on unobservable dimensions that influence selection into the voucher program as well as subsequent achievement outcomes. If we did not account for these baseline differences, our results would be subject to selection bias. Below, we describe our estimation approach that helps to enhance the internal validity of our estimates of the Indiana voucher program's impact on student achievement.

Our preferred model is an ordinary least squares (OLS) regression model with several covariates. We estimated this model for each individual year posttreatment, resulting in a total of four individual models to estimate the voucher program effects on student achievement in the first, second, third, and fourth year after receiving a voucher. We also estimated different effects for each subject (math and ELA) as the outcome in separate models, though the structure of the equation remained the same. We display the model for the first-year estimates in equation (1) below.

$$Y_{icgt} = \alpha + \beta Voucher_{icgt} + \pi Y_{icgt-1} + \omega Y_{icgt-2} + \delta \mathbf{X}_{icgt-1} + \theta_g + \tau_c + \upsilon_{icgt} \qquad (1)$$

Here, the achievement level (Y) for each student (i) in matching cell (c), grade (g), and post-baseline year (t) is a function of receiving a voucher and attending a private school ($Voucher_{icgt}$) as well as a host of other covariates.

In this model, we controlled for a vector of student baseline academic characteristics ($\mathbf{X}_{icgt-1}$) including baseline classification as an ELL or special education student. Grade fixed effects (θ_g) account for systematic differences in exams across grade levels. Matching cell fixed effects (τ_c) account for unobserved differences between the race-sex-year-grade-school matching cells at baseline. Effectively, these also account for systematic differences in exams across years as students within each cell take exams always within the same calendar year posttreatment. The term υ_{icgt} represents school cluster-robust standard errors to account for serial correlation among students within the same baseline public school cohort (same grade and year).

We also included in our preferred model two measures of a student's prior achievement in the same subject as the outcome, one at baseline (Y_{icgt-1}) and one pre-baseline (Y_{icgt-2}). Because lagged achievement scores are endogenous in the post-baseline years, these controls remain as the baseline and pre-baseline achievement measures for our estimates in the second, third, and fourth years post-baseline. In Appendix B, we detailed three alternative model specifications regarding the inclusion of a student's prior achievement in addition to a host of other robustness checks of our main results.[24] In the first, we controlled for only baseline achievement. In the second, we controlled for the first-, second-, and third-order polynomials of a student's baseline and pre-baseline achievement. In the third, we also controlled for the first-order baseline and pre-baseline measures of a student's prior achievement in the off-subject (e.g., we also control for prior ELA achievement when math is the outcome and vice versa).

After accounting for these pretreatment achievement differences between voucher and public school students, we describe the estimates of the voucher program impacts as the value-added achievement gains (or losses) from baseline. Thus, we define our main estimate (β) as the difference in the achievement gain (or loss) from baseline in a given post-baseline year between low-income voucher and public school students who share the same race/ethnicity and sex and are from the

[24] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

same baseline public school cohort. This estimate will be minimally biased if we have accounted for all covariates that could explain differences between the two groups.

By incorporating both baseline and pre-baseline achievement, we mitigate concerns regarding differing pretreatment trends between voucher and public students. This pretreatment phenomenon, known in the job-training literature as "Ashenfelter's Dip" (Ashenfelter, 1978), suggests that a substantial drop in student performance may be a signal to parents to have their child change schools. If this were the case, some students may be more likely to receive a voucher and attend a private school than others, yielding biased estimates. We believe that by accounting for pretreatment achievement in our models, we have effectively negated concerns with pretreatment trend differences between voucher students and their matched public school peers. However, because we estimated separate models for each post-baseline year, we could not incorporate a standard event study set of estimates derived from the same model to demonstrate parallel pretreatment trends between voucher and public students.

To further assuage these concerns, we estimated a set of two additional models, one with baseline achievement as the outcome and another with pre-baseline achievement as the outcome. These models contained the voucher indicator as well as the baseline student characteristics, matching cell fixed effects, grade fixed effects (for the pre-baseline model only, as these are collinear in the baseline model with the matching cells) and cluster-robust standard errors. The estimate on the voucher indicator shows the baseline and pre-baseline achievement level differences between voucher and public students within each matching cell. Although we found statistically significant differences in achievement levels between matched voucher and public students, the difference is a consistent 0.09 to 0.11 SD between the pre-baseline and baseline years (see Table 2 of our results). This suggests a parallel pretreatment achievement trend between the two groups. In addition to this investigation of pretreatment trends, we also conducted a series of other robustness checks for our preferred model estimates, all described in Appendix B and the results displayed in Appendix Tables B1 and B2.[25]

Regarding the external validity of our findings, because of how we defined our sample, all our estimates of the voucher program effect on achievement rely on low-income students who use a voucher to switch from public to private schools. This limits the generalizability of our findings because we did not estimate voucher effects for those who are always enrolled in private schools.[26] Voucher students switching from public to private schools are different in terms of baseline characteristics from voucher students always enrolled in private schools (see Appendix Table A1).[27] Our approach also requires at least three successive years of test score data, which excludes many students from the analysis.[28] Thus, most of our estimates of voucher program impacts are constrained to students in grades 5 to 8.

[25] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[26] We did not estimate voucher effects for students always enrolled in private schools because we did not want to make strong assumptions about prior achievement or geographic matching without a comparison school context.

[27] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[28] We could estimate models by only controlling for baseline achievement and not including pre-baseline achievement. This would require only a minimum of two years of observations for each student. As this is a nonexperimental evaluation, we valued enhanced internal validity over broader external validity. We had enough power to detect effects, particularly through three-years posttreatment. We also used the three years of observations data minimum to help identify low-income voucher students and omit voucher students making multiple switches.

Table 2. Annual effects of Indiana voucher program on student achievement.

A. Math achievement

	Pre-Baseline	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
All voucher	0.088***	0.106***	−0.146***	−0.172***	−0.168***	−0.173**
[Voucher students = 3,350]	(0.016)	(0.016)	(0.011)	(0.015)	(0.024)	(0.059)
Baseline covariates	Y	Y	Y	Y	Y	Y
Baseline and pre-baseline achievement	N	N	Y	Y	Y	Y
Grade fixed effects	Y	N	Y	Y	Y	Y
Matching cell fixed effects	Y	Y	Y	Y	Y	Y
N all students	37,601	37,601	37,601	21,354	9,156	1,757
Overall r^2	0.010	0.039	0.494	0.566	0.540	0.504
One total year with voucher	0.076***	0.069***	−0.135***			
[Voucher students = 1,497]	(0.023)	(0.022)	(0.017)			
Two total years with voucher	0.097***	0.126***	−0.158***	−0.156***		
[Voucher students = 1,076]	(0.029)	(0.029)	(0.019)	(0.019)		
Three total years with voucher	0.117***	0.139**	−0.145***	−0.181***	−0.147***	
[Voucher students = 602]	(0.036)	(0.035)	(0.025)	(0.025)	(0.026)	
Four total years with voucher	0.004	0.167***	−0.245***	−0.219***	−0.246***	−0.173**
[Voucher students = 175]	(0.073)	(0.081)	(0.046)	(0.048)	(0.057)	(0.059)

B. ELA achievement

	Pre-Baseline	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
All voucher	0.097***	0.111***	−0.002	−0.038**	0.013	0.062
[Voucher students = 3,348]	(0.015)	(0.016)	(0.011)	(0.015)	(0.023)	(0.053)
Baseline covariates	Y	Y	Y	Y	Y	Y
Baseline and pre-baseline achievement	N	N	Y	Y	Y	Y
Grade fixed effects	Y	N	Y	Y	Y	Y
Matching cell fixed effects	Y	Y	Y	Y	Y	Y
N all students	37,264	37,264	37,264	21,459	9,280	1,995
Overall r^2	0.021	0.086	0.498	0.537	0.552	0.490
One total year with voucher	0.069**	0.086***	0.009			
[Voucher students = 1,494]	(0.022)	(0.022)	(0.016)			
Two total years with voucher	0.117***	0.124***	−0.005	−0.018		
[Voucher students = 1,077]	(0.026)	(0.027)	(0.018)	(0.019)		
Three total years with voucher	0.131***	0.162***	−0.056*	−0.032*	0.021	
[Voucher students = 602]	(0.033)	(0.035)	(0.025)	(0.024)	(0.026)	
Four total years with voucher	0.022	0.077	−0.010	−0.153***	−0.009	0.061
[Voucher students = 175]	(0.073)	(0.078)	(0.042)	(0.046)	(0.045)	(0.052)

Notes: $^{*}P \leq 0.050$; $^{**}P \leq 0.010$; $^{***}P \leq 0.001$. ISTEP+ Math and ELA achievement measured in SD units, relative to the Indiana state mean and SD within each grade and year. Number of voucher students in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.
Source: Authors' calculations.

RESULTS

Main Effects of Receiving a Voucher and Attending a Private School

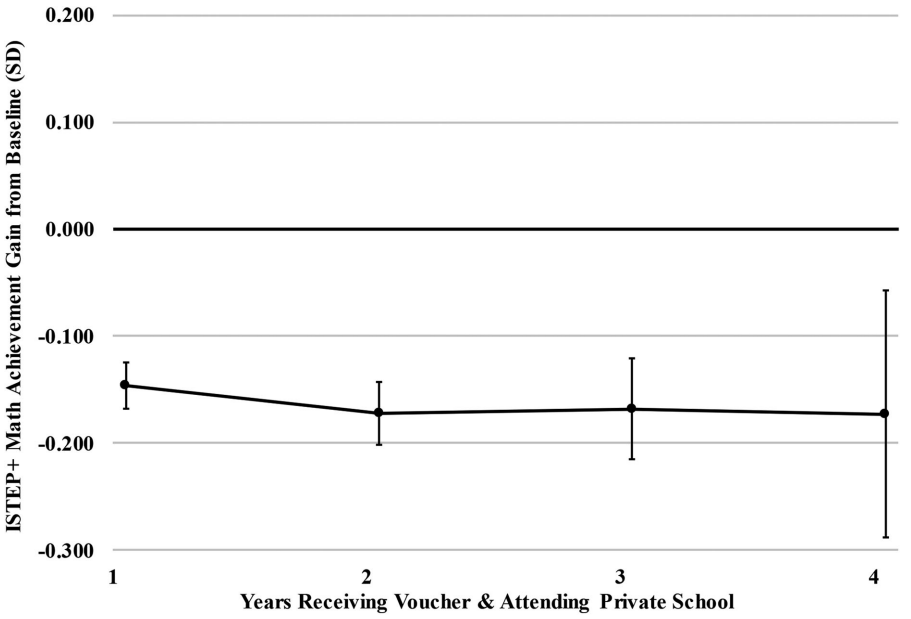
We begin by describing the estimated yearly effects in the first four years of the Indiana voucher program for low-income voucher students switching from a public to a private school. In Table 2, we display the results for mathematics and ELA. The first two columns provide the pre-baseline (i.e., year before baseline) and baseline test score estimates in columns 1 and 2, respectively. In mathematics, students who switch from public to private schools with a voucher had an estimated score of 0.106 SD ($P \leq 0.001$) at baseline and 0.088 SD ($P \leq 0.001$) at pre-baseline above their matched peers remaining in public schools. In ELA, the estimates were 0.111 SD ($P \leq 0.001$) at baseline and 0.097 SD ($P \leq 0.001$) at pre-baseline. In both mathematics and ELA, these baseline and pre-baseline estimates reveal that students who switch from public to private schools with a voucher scored slightly higher while enrolled in a public school compared with their public school peers. The pre-baseline and baseline estimates were roughly the same, providing little evidence of divergent pretreatment trends; rather, there was a consistent levels difference in test scores between matched voucher and public students.

The estimates for the impact by year after receiving a voucher and switching from a public to a private school appear in the third through sixth columns of results in Table 2. In the first year, voucher students scored an average of −0.146 SD ($P \leq 0.001$) below their matched public school peers in mathematics. This average loss increased to −0.172 SD ($P \leq 0.001$) in the second year and then remained consistent at −0.168 SD ($P \leq 0.001$) in the third year, and then −0.173 ($P \leq 0.010$) in the fourth year after receiving a voucher and attending a private school.

The lower part of the mathematics panel of Table 2 shows separate estimates by each cohort of students based on the total number of years observed receiving a voucher. The yearly estimates above have a changing sample, whereas the cohort analysis keeps a consistent sample. To produce these findings, we constrained our sample to include only voucher and public school students who have one, two, three, or four total years of posttreatment data within each cell.

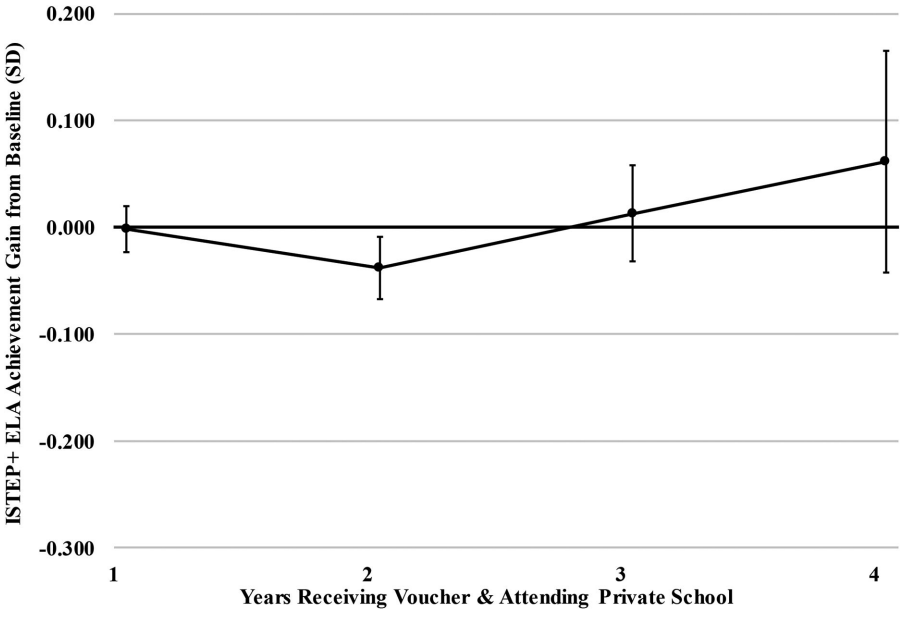
For students who were observed using vouchers for all four years, their mathematics achievement was 0.167 SD ($P \leq 0.001$) higher at baseline compared with their peers who remained in public schools. This difference was larger than what we found for all voucher students in the first row of Table 2. The cohort of students using vouchers for four total years scored −0.245 SD ($P \leq 0.001$) lower than their matched public school peers in year one. By year four, the negative effect shrunk to −0.173 SD ($P \leq 0.010$). An important caveat for these estimates is that we only observed a small number of students who receive a voucher and attend a private school for a total of four years (175 total students). For all other cohorts, we did not observe any meaningful differences with the overall first-, second-, and third-year estimates.

In ELA (Table 2), most of the value-added estimates for voucher students were not statistically significant, indicating that there were no differences in ELA achievement gains from baseline when comparing voucher students with their matched public school peers. The one exception was the estimate for students in the second year revealing that voucher students scored −0.038 SD ($P \leq 0.010$) below their public school peers. Although statistically significant, the estimate was quite small and disappears immediately in the third year. The second-year achievement loss in ELA was even larger (−0.153 SD, $P \leq 0.001$) for the cohort of students receiving a voucher for all four years; however, the loss also immediately dissipated. In addition to the estimates in Table 2, we display our main yearly effects in Figures 1 and 2.



Source: Authors' calculations.

Figure 1. Mean Math Achievement Gain from Baseline for Indiana Voucher Students.



Source: Authors' calculations.

Figure 2. Mean ELA Achievement Gain from Baseline for Indiana Voucher Students.

Robustness Checks

We subjected our preferred main effect estimates in math and ELA to a series of robustness checks. We fully describe the rationale behind each of the robustness checks in Appendix B and display the results for math in Appendix Table B1 and for ELA in Appendix Table B2.[29] Here, we provide a brief synopsis. In total, we estimated seven alternative models to assess the robustness of the main voucher effects. The robustness checks include: (1) controlling only for baseline achievement; (2) using a polynomial specification of baseline and pre-baseline achievement; (3) adding additional controls for prior achievement in the off subject; (4) including structural and nonstructural school change indicators; (5) constraining our results to only those students making structural school changes; (6) keeping students who exited private schools in the treatment group; and (7) taking out the Indianapolis urban area schools. The robustness checks largely revealed consistent findings with our preferred yearly estimates, primarily differing by no more than +/−0.020 SD for any one year.

One notable difference is when we restricted our sample to only students making structural changes (that is, changing schools due to normal grade progression) using a voucher to switch to a private school. In math, we observed a slightly lower first-year achievement loss (−0.127 SD, $P \leq 0.001$). However, the second- (−0.195 SD, $P \leq 0.001$), third- (−0.220 SD, $P \leq 0.001$), and fourth-year (−0.261 SD, $P \leq 0.050$) losses became increasingly larger when compared to the overall voucher effects. We believe more motivated families are more likely to make nonstructural moves (changing schools for any other reason), hence, the greater overall losses for students making structural moves. Despite the magnitude differences of these estimates, the large standard errors suggest that these are not statistically different from our preferred estimates.

Subgroup Effects

We found consistent evidence of modest annual achievement losses from baseline in math and null effects in ELA when comparing voucher students with their matched public school peers. However, these average estimates for all voucher students may either differ or be consistent across various subgroups of students. Therefore, we disaggregated the results further by student sex, race/ethnicity, student baseline ELL, and special education status;[30] whether a student remains enrolled in a private school; type of private school; and the location in which a voucher student previously attended a public school (a proxy for locality of their residence). We calculated these estimates by introducing interactions of the student subgroup with the voucher indicator in our preferred model. The main voucher effect in these models represents the voucher impact for the reference group (e.g., males), while the voucher effect for the interacted group (e.g., females) is calculated through the linear combination of the main effect and the estimate of the interaction.

In Tables 3 and 4, we display the results of our heterogeneity analysis by student subgroups for math and ELA, respectively. The first column in Tables 3 and 4 contains the baseline test scores indicating how much higher or lower baseline

[29] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[30] Prior research indicated that students are less likely to be classified as special education once enrolled in private schools (Wolf, Witte, & Fleming, 2012). We descriptively find a similar bias in Indiana (though not for ELL classification). For our subgroup results, we estimated differences based on a student's classification as a special education student at baseline, so we had minimal concern about this potential bias.

Table 3. Subgroup effects of Indiana voucher program on math achievement.

	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
Male	0.154$^{***,(s)}$	−0.143***	−0.164***	−0.164***	−0.144*
[n = 1,590]	(0.023)	(0.016)	(0.022)	(0.035)	(0.070)
Female	0.063$^{**,(s)}$	−0.149***	−0.179***	−0.173***	−0.205*
[n = 1,760]	(0.022)	(0.014)	(0.019)	(0.031)	(0.089)
White	0.090***	−0.151***	−0.211$^{***,(s)}$	−0.200$^{***,(s)}$	−0.239***
[n = 1,542]	(0.024)	(0.016)	(0.021)	(0.035)	(0.070)
Black	0.132***	−0.156***	−0.162***	−0.183***	−0.167***
[n = 795]	(0.031)	(0.022)	(0.032)	(0.057)	(0.119)
Latino/a	0.108***	−0.124***	−0.089$^{**,(s)}$	−0.067$^{(s)}$	0.013
[n = 759]	(0.033)	(0.023)	(0.031)	(0.043)	(0.113)
Other race/ethnicity	0.119	−0.147***	−0.205***	−0.295$^{***,(s)}$	–
[n = 254]	(0.067)	(0.038)	(0.053)	(0.081)	
Non-English language learner	0.098***	−0.143***	−0.178***	−0.194$^{***,(s)}$	−0.250$^{***,(s)}$
[n = 2,931]	(0.017)	(0.012)	(0.016)	(0.027)	(0.060)
English language learner	0.172***	−0.174***	−0.133***	−0.051$^{(s)}$	0.150$^{(s)}$
[n = 412]	(0.043)	(0.030)	(0.038)	(0.052)	(0.142)
Non-Special education	0.092$^{***,(s)}$	−0.146***	−0.185***	−0.190***	–
[n = 3,077]	(0.017)	(0.012)	(0.015)	(0.024)	
Special education	0.245$^{***,(s)}$	−0.133***	−0.141*	−0.095	–
[n = 263]	(0.056)	(0.040)	(0.064)	(0.098)	
Continue in private	0.137$^{***,(s)}$	−0.140***	−0.167***	−0.168***	–
[n = 2,870]	(0.017)	(0.012)	(0.015)	(0.024)	
Exit & return to public	−0.072$^{(s)}$	−0.183***	−0.232***	−0.172	–
[n = 480]	(0.040)	(0.029)	(0.052)	(0.160)	
Catholic	0.099***	−0.123$^{***,(s)}$	−0.144$^{***,(s)}$	−0.127$^{***,(s)}$	−0.165*
[n = 1,804]	(0.022)	(0.014)	(0.019)	(0.027)	(0.068)
Other private	0.114***	−0.175$^{***,(s)}$	−0.210$^{***,(s)}$	−0.252$^{***,(s)}$	−0.193*
[n = 1,546]	(0.023)	(0.017)	(0.023)	(0.044)	(0.098)
Urban	0.115***	−0.154***	−0.168***	−0.157***	−0.191*
[n = 1,866]	(0.022)	(0.015)	(0.021)	(0.035)	(0.077)
Suburban	0.091**	−0.114***	−0.167***	−0.216***	−0.048
[n = 773]	(0.032)	(0.024)	(0.032)	(0.045)	(0.102)
Town/rural	0.098**	−0.164***	−0.176***	−0.092	−0.181
[n = 707]	(0.033)	(0.025)	(0.031)	(0.049)	(0.159)

Notes: $^{*}P \leq 0.050$; $^{**}P \leq 0.010$; $^{***}P \leq 0.001$. ISTEP+ Math achievement measured in SD units, relative to the Indiana state mean and SD in each grade and year. We estimated subgroup effects by including subgroup interactions in our preferred model. Number of voucher students within each subgroup in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses. $^{(s)}$Indicates significant differences between subgroups ($P \leq 0.050$). Missing cells in fourth year indicate <15 students.
Source: Authors' calculations.

achievement is for different student groups (e.g., baseline estimates for black students receiving a voucher compared with their black peers remaining in public schools). In the subsequent columns of Tables 3 and 4 are the annual effects showing how much higher or lower student groups from baseline score in their first, second, third, and fourth year after receiving a voucher to attend a private school compared with their matched peers remaining in public schools (e.g., how much

Table 4. Subgroup effects of Indiana voucher program on ELA achievement.

	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
Male	0.169$^{***,(s)}$	0.013	−0.020	0.000	0.096
[n = 1,585]	(0.023)	(0.015)	(0.022)	(0.033)	(0.076)
Female	0.060$^{**,(s)}$	−0.017	−0.053**	0.025	0.025
[n = 1,763]	(0.022)	(0.014)	(0.019)	(0.031)	(0.060)
White	0.112***	−0.031$^{*,(s)}$	−0.083$^{***,(s)}$	−0.034$^{(s)}$	0.009
[n = 1,538]	(0.025)	(0.016)	(0.022)	(0.033)	(0.050)
Black	0.152$^{***,(s)}$	0.051$^{*,(s)}$	0.006$^{(s)}$	0.054	0.096
[n = 796]	(0.030)	(0.021)	(0.030)	(0.050)	(0.098)
Latino/a	0.065$^{*,(s)}$	0.006	0.011$^{(s)}$	0.074$^{(s)}$	0.137
[n = 760]	(0.030)	(0.021)	(0.028)	(0.041)	(0.121)
Other race/ethnicity	0.106	−0.019	−0.034	−0.030	–
[n = 254]	(0.062)	(0.041)	(0.058)	(0.084)	
Non-English language learner	0.118***	−0.003	−0.047**	−0.011$^{(s)}$	0.038
[n = 2,929]	(0.017)	(0.011)	(0.016)	(0.024)	(0.047)
English language learner	0.053	−0.011	0.020	0.125$^{*,(s)}$	0.167
[n = 412]	(0.036)	(0.029)	(0.039)	(0.056)	(0.151)
Non-Special education	0.088$^{***,(s)}$	−0.003	−0.045**	0.009	–
[n = 3,080]	(0.016)	(0.011)	(0.015)	(0.023)	
Special education	0.337$^{***,(s)}$	−0.013	0.047*	0.035	–
[n = 258]	(0.058)	(0.039)	(0.052)	(0.092)	
Continue in private	0.138$^{***,(s)}$	0.000	−0.031*	0.020$^{(s)}$	–
[n = 2,870]	(0.017)	(0.012)	(0.015)	(0.022)	
Exit & return to public	−0.048$^{(s)}$	−0.017	−0.121**	−0.194$^{(s)}$	–
[n = 478]	(0.042)	(0.026)	(0.045)	(0.105)	
Catholic	0.101***	0.036$^{**,(s)}$	0.010$^{(s)}$	0.056$^{*,(s)}$	0.039
[n = 1,804]	(0.021)	(0.014)	(0.018)	(0.025)	(0.061)
Other private	0.123***	−0.049$^{***,(s)}$	−0.104$^{***,(s)}$	−0.074$^{(s)}$	0.115
[n = 1,544]	(0.024)	(0.016)	(0.023)	(0.042)	(0.097)
Urban	0.118***	0.001	−0.023	0.027	0.028
[n = 1,863]	(0.021)	(0.015)	(0.021)	(0.033)	(0.080)
Suburban	0.098*	0.011	−0.025	0.002	0.157
[n = 772]	(0.032)	(0.021)	(0.027)	(0.036)	(0.089)
Town/rural	0.106**	−0.021	−0.089**	0.013	0.044
[n = 709]	(0.034)	(0.023)	(0.032)	(0.055)	(0.072)

Notes: $^{*}P \leq 0.050$; $^{**}P \leq 0.010$; $^{***}P \leq 0.001$. ISTEP+ Math achievement measured in SD units, relative to the Indiana state mean and SD in each grade and year. We estimated subgroup effects by including subgroup interactions in our preferred model. Number of voucher students within each subgroup in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses. $^{(s)}$Indicates significant differences between subgroups ($P \leq 0.050$). Missing cells in fourth year indicate <15 students.
Source: Authors' calculations.

higher or lower black students scored from baseline through one, two, three, and four years after receiving a voucher compared with their black peers who stayed in public schools). The estimates in Tables 3 and 4 are marked with an "$^{(s)}$" if the differences in the value-added effects were significantly different, for example, between male and female voucher students, between black and white voucher students, or other subgroups of voucher students. Looking across student subgroups, the results were generally consistent with our overall findings. The losses

in math were widespread across most subgroups. Similarly, we found mostly null impacts in ELA across the same subgroups. We highlight some notable differences below.

First, there were some significant differences in the estimates for Latino/a and black voucher students. Latino/a students experienced significant mathematics losses in the first year of using a voucher compared with their matched Latino/a public school peers, but these losses dissipated over time. By years three and four, the Latino/a voucher estimates were no longer statistically significant, revealing that Latino/a voucher students and their matched Latino/a public school peers did not differ in their math scores after several years in the program. In addition, white voucher students' mathematics losses were significantly greater than those experienced by Latino/a voucher students as indicated with the "$^{(s)}$" by the Latino/a estimates in Table 3 for the second and third years of using a voucher. In ELA, black voucher students experienced a small gain from baseline compared to their matched public school peers in the first year (0.051 SD, $P \leq 0.050$), which dissipates after two years and remerges, albeit statistically insignificant, three and four years post-baseline. Latino/a students experience positive, but statistically insignificant gains from baseline in all four years. Also, in the first two years, black voucher students score significantly higher than white students, who experienced an average loss relative to their public school peers in the first (−0.036 SD, $P \leq 0.050$) and second (−0.084 SD, $P \leq 0.001$) year of receiving a voucher. Latino/a students also score higher than white students in years three and four post-baseline.

Second, there were significant differences in the mathematics estimates for students classified at baseline as ELL or special education. Although ELL and special education students experienced annual losses in mathematics over the first two years of receiving a voucher compared with their matched public school peers, there were no statistically significant differences at the 5 percent significance level (denoted "NS") between these student groups by the time ELL students used a voucher for three (−0.051, NS) or four years (0.150, NS) or a special education student used a voucher for three years (−0.095, NS) (see Table 3). In ELA, ELL students experienced a statistically significant gain compared to their public school peers in the third year of receiving a voucher and attending a private school (0.0125, $P \leq 0.050$). We also observed a positive ELA achievement gain for special education students after two years of receiving a voucher and attending a private school (0.047 SD, $P \leq 0.050$). Although the estimate after three years is consistent in magnitude (0.035SD, NS), the estimate is no longer statistically significant, primarily due to the small number of special education students observed receiving a voucher through three years.

Third, students who exited the voucher program and returned to a public school experienced larger losses in both subjects than their voucher peers. This is most evident in mathematics after one (−0.183 SD, $P \leq 0.001$) or two years (−0.232 SD, $P \leq 0.001$) and after two years in ELA (−0.121, $P \leq 0.010$). This is a signal that students who left the voucher program and returned to a public school are among the lowest performers in private schools.

Fourth, we also note differences by type of private school, whether Catholic or other religious or nonreligious private. Although voucher students who attended a Catholic school experienced significant average losses compared to their matched peers across all four years, students attending other private schools experience even greater losses in the first three years (−0.175 SD, $P \leq 0.001$; −0.210 SD, $P \leq 0.001$; and −0.252, $P \leq 0.001$, respectively). The difference between Catholic and other private school voucher students is more pronounced in ELA, as the difference between the two groups is statistically significant through three years, with voucher students attending other private schools experiencing an average achievement loss from baseline of between 0.050 to 0.100 SD in each year.

DISCUSSION

Although vouchers in Indiana are not randomly assigned, this study was able to make use of state administrative longitudinal student records to investigate the impacts of the Indiana voucher program on student achievement. Public and private school students in Indiana, including all students receiving a voucher, have taken the same assessment for several years, even prior to the implementation of the voucher program. This feature is a significant benefit to researchers, allowing for apples-to-apples comparisons of student achievement outcomes and the ability to capture longitudinal impacts.

Generally, we found that low-income voucher students in Indiana experienced similar average achievement in ELA after attending a private school as did their matched peers who remained in public schools. In math, voucher students experienced a substantial average achievement loss after attending a private school in comparison with their public school peers. The losses in math primarily accumulate during the first two years of attending a private school and persist. We found these overall results are consistent across several robustness checks and between most student subgroups. We also found no evidence of different pretreatment trends between voucher and public students; the validity of our estimates hinges on this key assumption being satisfied.

This study is one of a small, but growing number of studies to estimate such losses in math achievement for students who use a voucher to attend a private school. Our findings align with recent research on the Louisiana Scholarship Program (see Abdulkadiroglu, Pathak, & Walters, 2018; Mills & Wolf, 2017) the Ohio EdChoice Scholarship Program (Figlio & Karbownik, 2016), and the DC Opportunity Scholarship Program (Dynarski et al., 2017, 2018). We found a smaller average overall loss in math; however, we similarly found that the largest losses in math achievement occur during the first two years of receiving a voucher and attending private school. We also found that these losses persist through four years of receiving a voucher and attending a private school, notably different from prior studies.

We also did not find statistical evidence that voucher students experience an improvement in their average achievement after baseline the longer they are enrolled in a private school. One might expect that students and their private schools would adjust to better meet the educational needs of voucher students. Collectively, this does not appear to be the case. However, there may be notable differences among private schools that is worthy of additional research to examine whether some private schools help students adjust to a greater degree than others. Incorporating additional years of testing data, which will also include new students, will help to reduce the noise in the third- and fourth-year estimates. Additional information from teachers and principals also will help to shed more light on these trends—and possible explanations for them—as the program continues to grow.

We did find some differential effects by some student subgroups. In math, Latino/a students, by the third year of using a voucher, do not score differently from their matched public school Latino/a peers. White students experience consistent losses across all years post-baseline in mathematics and the first two years in ELA. The results in math are also similar across several other subgroups of students, except for students classified as ELLs or special education at baseline. The consistency of findings across most student subgroups raises questions about the mechanisms that may explain these negative effects in mathematics, such as the mathematics curriculum, instruction, or teacher quality in private schools not being as robust as is found in public schools.

In investigating how voucher effects may differ across types of private schools, we found negative effects in mathematics for both Catholic and other private schools as well as even greater negative effects for other private schools in both math and ELA

achievement. These mathematics findings for Catholic and other private schools mirror those we found in previous work on student transfers from public to Catholic and other private schools in Indianapolis (see Berends & Waddington, 2018). This is a relevant comparison as it takes place in the same state, includes a handful of the same students, and the mechanism of switching from public to private schools is the same. Further research should explore the possible variability of impacts between Catholic and other private schools as school-average effects in both subjects are highly variable between schools. Some schools were more racially and ethnically diverse prior to the implementation of the voucher program, and therefore may have been better equipped to educate a demographically (and perhaps, academically) diverse group of students.

Our research has its limitations. Although our use of exact matching is a strong methodological approach to examine impacts with longitudinal data and is based on prior within-study comparisons, it has drawbacks as described earlier in this paper. We only drew generalizations about students using vouchers to switch from public to private schools. Our modeling strategy aligns well with the original intent of the voucher program—to increase access to private schools for low-income public school families—to estimate the program's effects. However, after the law changed prior to the 2013/2014 school year, which no longer required voucher students to move from a public to private school, over half of all students participating in the voucher program in 2014/2015 never attended an Indiana public school (Indiana Department of Education, 2018). For these students, we would find it challenging to establish an equivalent baseline and assess meaningful voucher effects. We also found it challenging to estimate effects for non-low-income voucher recipients (e.g., those receiving a "half" voucher) as we did not have a fine-grained measure of income to identify comparable public students who would be eligible.

Second, Indiana does not have a common assessment system in the pre-K through second grade levels in public and private schools. Because we used third, fourth, or fifth grade as a baseline for most students, our research focuses on voucher program effects in the upper elementary and middle school grades. In 2014/2015, nearly 50 percent of the students receiving vouchers statewide were in the K-4 grade levels (Indiana Department of Education, 2018), meaning that current ISTEP+ testing does not capture a significant number of students receiving vouchers. As a result, we may never know whether the ICSP had a significant impact (positive or negative) on students in earlier grade levels.

Voucher programs are designed to provide new learning opportunities, for which achievement gains should serve as a proxy for any program's success for students and schools. Our results do not provide robust support that the ICSP has been successful to date at improving student achievement for low-income students who use a voucher to switch from a public to a private school. Although academic achievement outcomes are important for researchers, policymakers, and practitioners to consider, parents make schooling decisions for their children based on a multitude of factors, including academics, location, safety, and religion. Therefore, researchers need to examine outcomes beyond test scores (e.g., educational attainment, engagement, social and emotional learning, character, civic participation, and other nonacademic outcomes). Additional data on these other student outcomes need to be collected and analyzed to provide a more complete picture. In an era of expanding school choice, policymakers must draw from recent findings about statewide voucher programs that there is more to learn about the impacts of large-scale parental choice programs on American families.

R. JOSEPH WADDINGTON is an Assistant Professor in the Department of Educational Policy Studies and Evaluation at the University of Kentucky, 131 Taylor Education Building, Lexington, KY 40506 (e-mail: rjwaddington@uky.edu).

MARK BERENDS is a Professor of Sociology at the University of Notre Dame, 4110A Jenkins Nanovic Halls, Notre Dame, IN 46556 (e-mail: mberends@nd.edu).

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Appendix A

Description of Data Restrictions for Analysis

In the process of creating our analytical sample, we made several decisions that pared down the sample of voucher and public school comparison students. We describe these decisions below and include the number of voucher students removed in parentheses. We have no evidence that public school comparison students were differentially excluded because of these decisions.

Between the 2011/2012 and 2014/2015 school years, there were a total of 19,120 unique students who received vouchers statewide in grades 3 through 8. We first removed any students whom we observed with multiple observations within a given year or who transferred schools within the same year (29). We do not have information from IDOE that allows us to tell which school these students attended first or in which one they were tested. Next, we removed all voucher students (3,101) who had only one total year of testing data. These are mostly 3rd-grade students receiving a voucher in 2014/2015. We also removed all students (3,371; mostly fourth graders in 2014/2015) with only two years of test data as our models include controls for a pre-baseline and baseline year. We removed all students (850) who are missing both math and ELA test scores in any given year as they have a gap in their achievement trajectories. Last, we removed all students who at some point attended alternative public schools throughout the state (13).

Based on these decisions, we have 11,756 unique voucher students for whom we have at least three consecutive years of test scores in our data. Similarly, we have 556,919 unique students attending an Indiana public school (traditional public, charter, or magnet) with at least three consecutive years of test scores in our data, never received a voucher, and remain enrolled in a public school through the duration of our available data. We provide descriptives for all voucher and public school students not part of our analytic sample in Tables A1 and A2. We include other relevant dimensions of the voucher student analytic sample in Table A3.

Table A1. Comparison of all voucher and non-voucher private school students.

	Voucher students						Non-Voucher students	
	Public→ Private Matched low-income	Public→ Private Unmatched low-income	Public→ Private Non-Low income	Private→ Public →Private	Private→ Public	Always private	Catholic	Other private
Students	3,363	520	501	209	649	6,514	31,482	12,609
		Baseline year			*All years*		*All years*	
Female	0.526	0.521	0.470	0.559	0.516	0.508	0.495	0.505
Black	0.238	0.181	0.106	0.119	0.249	0.102	0.029	0.053
Latino/a	0.226	0.301	0.068	0.150	0.162	0.156	0.066	0.025
Other race/ethnicity	0.076	0.175	0.072	0.119	0.114	0.078	0.064	0.061
Received FRPL	0.951	0.850	0.000	0.571	0.852	0.633	0.140	0.157
ELL	0.123	0.134	0.014	0.027	0.153	0.147	0.049	0.026
Special education	0.079	0.083	0.084	0.036	0.178	0.127	0.087	0.079
Retained	0.008	0.035	0.004	0.000	0.068	0.034	0.012	0.018
Mean math score	−0.244	−0.252	0.114	–	–	–	–	–
Mean ELA score	−0.220	−0.246	0.200	–	–	–	–	–
			All years with voucher				*All years*	
Mean math score	−0.373	−0.394	−0.073	−0.249	−0.604	−0.146	0.387	0.230
Mean ELA score	−0.205	−0.241	0.147	0.008	−0.416	0.012	0.500	0.364

Notes: Table displays voucher and private school students with at least three years of test scores. Proportions of students reported for demographic characteristics. Mean ISTEP+ Math and ELA scores measured in SD units, relative to the Indiana state mean and SD within each grade and year. For voucher students moving from public to private schools (the first three columns), demographic and achievement characteristics reported in baseline year as well as achievement in all years receiving a voucher. For all other voucher students and non-voucher private school students, demographic and achievement characteristics reported across all years (e.g., ever received FRPL, ever classified as ELL, aggregate achievement).
Source: Authors' calculations.

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Table A2. Comparison of all public school students.

	Matched low-income public	Unmatched low-income public	Non-Low-Income public in matching cell	Outside of matching cell public
Students	34,587	22,961	41,099	458,272
		Baseline year		*All years*
Female	0.527	0.509	0.505	0.493
Black	0.252	0.281	0.057	0.098
Latino/a	0.168	0.141	0.022	0.088
Other race/ ethnicity	0.027	0.030	0.026	0.084
Received FRPL	0.922	0.930	0.000	0.550
ELL	0.114	0.085	0.019	0.073
Special education	0.008	0.125	0.129	0.160
Retained	0.065	0.020	0.007	0.021
Attended charter	0.065	0.113	0.035	0.033
Attended magnet	0.020	0.054	0.023	0.024
Mean math score	−0.335	−0.332	0.358	–
Mean ELA score	−0.333	−0.325	0.362	–
		All years		
Mean math score	−0.310	−0.326	0.371	0.052
Mean ELA score	−0.307	−0.323	0.344	0.039

Notes: Table displays public school students with at least three years of test scores and who have never received a voucher or attended a private school. Proportions of students reported for demographic characteristics. Mean ISTEP+ Math and ELA scores measured in SD units, relative to the Indiana state mean and SD within each grade and year. For public students within a matching cell (the first three columns), demographic and achievement characteristics reported in baseline year as well as achievement across all years of available data. For unmatched public school students, demographic and achievement characteristics reported across all years (e.g., ever received FRPL, ever classified as ELL, aggregate achievement).
Source: Authors' calculations.

Table A3. Additional descriptive information for matched analytical sample of voucher students.

	Number of students	Proportion of students
Total matched low-income voucher students	3,363	1.000
Ever enrolled in Catholic school	1,824	0.542
Ever enrolled in other private school	1,561	0.464
Observed receiving voucher 2011–2012 (first year program)	804	0.239
Observed receiving voucher 2012–2013	1,506	0.448
Observed receiving voucher 2013–2014	1,911	0.568
Observed receiving voucher 2014–2015	1,951	0.580
Continue receiving voucher & remain enrolled in private school	2,861	0.851
Stop receiving voucher & remain enrolled in private school	21	0.006
Stop receiving voucher & return to public school	481	0.143
One total year receiving voucher	1,504	0.447
Two total years receiving voucher	1,079	0.321
Three total years receiving voucher	604	0.180
Four total years receiving voucher	176	0.052
Year of first voucher receipt		
2011–2012	804	0.239
2012–2013	963	0.286
2013–2014	890	0.265
2014–2015	706	0.210
Grade of first voucher receipt		
Grade 4	58	0.017
Grade 5	1,125	0.335
Grade 6	974	0.290
Grade 7	720	0.214
Grade 8	486	0.145
Grade retention upon private school entry	160	0.048

Notes: Table displays all matched voucher students who are part of our analytical sample. Students continuing to receive vouchers are those observed receiving a voucher through the final year of the available data (2014/2015), eighth grade, or leaving the data set (e.g., moving to another state or a non-reporting private school). Students returning to public school are observed in a traditional public, charter, or magnet school in at least one year after receiving a voucher and attending private school.
Source: Authors' calculations.

Appendix B

Robustness Checks

In this appendix, we detail the rationale behind each of the seven robustness checks of the yearly voucher effects estimated from our preferred regression model. Each set of estimates comes from the same preferred model, with any additions to the model or changes to the sample described below. We display the results for math in Table B1 and for ELA in Table B2.

Our first three robustness checks involve alternative specifications of prior achievement as a control in our preferred model. In the first, we only controlled for baseline achievement. Although this is a common approach within educational research, we provided detailed reasons for why we also included a control for pre-baseline achievement in our preferred model in the main methods section of the paper. We found minimal differences with our preferred model when including only baseline achievement. In math, the fourth-year loss is slightly larger than the preferred model (−0.202 SD vs. −0.173 SD) and in ELA, the second-year loss is slightly smaller than the preferred model and not statistically significant (−0.029 SD vs. −0.038 SD). This finding, along with the estimated null achievement gain from

Table B1. Robustness checks of main voucher effects on math achievement..

	Pre-Baseline	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
Preferred model	0.088***	0.106***	−0.146***	−0.172***	−0.168***	−0.173**
	(0.016)	(0.016)	(0.011)	(0.015)	(0.024)	(0.059)
	[3,350]	[3,350]	[3,350]	[1,848]	[771]	[175]
Baseline prior achievement only	–	0.106***	−0.143***	−0.169***	−0.169***	−0.202***
		(0.016)	(0.012)	(0.016)	(0.025)	(0.061)
		[3,350]	[3,350]	[1,848]	[771]	[175]
Polynomial prior achievement	0.088***	0.106***	−0.145***	−0.171***	−0.172***	−0.176**
	(0.016)	(0.016)	(0.011)	(0.015)	(0.024)	(0.058)
	[3,350]	[3,350]	[3,350]	[1,848]	[771]	[175]
Off-Subject prior achievement included	0.088***	0.106***	−0.152***	−0.178***	−0.174***	−0.185**
	(0.016)	(0.016)	(0.011)	(0.015)	(0.024)	(0.059)
	[3,301]	[3,301]	[3,301]	[1,827]	[764]	[173]
Mobility indicators included	0.088***	0.098***	−0.124***	−0.192***	−0.176***	−0.202***
	(0.016)	(0.016)	(0.012)	(0.016)	(0.024)	(0.060)
	[3,350]	[3,350]	[3,350]	[1,848]	[771]	[175]
Structural changes only	0.131***	0.148***	−0.127***	−0.195***	−0.220***	−0.261*
	(0.032)	(0.030)	(0.020)	(0.023)	(0.038)	(0.125)
	[1,132]	[1,132]	[1,132]	[728]	[329]	[70]
Keep exit students in treatment	0.085***	0.103***	−0.146***	−0.156***	−0.151***	−0.160**
	(0.016)	(0.016)	(0.011)	(0.014)	(0.021)	(0.051)
	[3,350]	[3,350]	[3,350]	[2,200]	[1,049]	[264]
No Indianapolis urban	0.081***	0.098***	−0.160***	−0.177***	−0.179***	−0.175**
	(0.019)	(0.019)	(0.013)	(0.017)	(0.027)	(0.065)
	[2,455]	[2,455]	[2,455]	[1,381]	[599]	[143]

Notes: $^{*}P \leq 0.050$; $^{**}P \leq 0.010$; $^{***}P \leq 0.001$. ISTEP+ Math achievement measured in SD units, relative to the Indiana state mean and SD within each grade and year. Number of voucher students contributing to each yearly estimate in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.
Source: Authors' calculations.

Table B2. Robustness checks of main voucher effects on ELA achievement.

	Pre-Baseline	Baseline	First year with voucher	Second year with voucher	Third year with voucher	Fourth year with voucher
Preferred model	0.097***	0.111***	−0.002	−0.038**	0.013	0.062
	(0.015)	(0.016)	(0.011)	(0.015)	(0.023)	(0.053)
	[3,348]	[3,348]	[3,348]	[1,843]	[777]	[176]
Baseline prior achievement only	–	0.111***	0.004	−0.029	0.018	0.062
		(0.016)	(0.011)	(0.016)	(0.024)	(0.053)
		[3,348]	[3,348]	[1,843]	[777]	[176]
Polynomial prior achievement	0.097***	0.111***	−0.002	−0.038**	−0.013	0.048
	(0.015)	(0.016)	(0.011)	(0.014)	(0.022)	(0.049)
	[3,348]	[3,348]	[3,348]	[1,843]	[777]	[176]
Off-Subject prior achievement included	0.097***	0.111***	−0.008	−0.047***	0.004	0.037
	(0.015)	(0.016)	(0.011)	(0.014)	(0.022)	(0.049)
	[3,300]	[3,300]	[3,300]	[1,823]	[770]	[174]
Mobility indicators included	0.097***	0.104***	0.016	−0.043**	−0.004	0.038
	(0.015)	(0.016)	(0.012)	(0.015)	(0.023)	(0.052)
	[3,348]	[3,348]	[3,348]	[1,843]	[777]	[176]
Structural changes only	0.167***	0.189***	0.010	−0.010	0.008	0.098
	(0.027)	(0.027)	(0.019)	(0.023)	(0.034)	(0.093)
	[1,134]	[1,134]	[1,134]	[727]	[331]	[70]
Keep exit student in treatment	0.091***	0.102***	−0.003	−0.030***	−0.002	0.051
	(0.015)	(0.016)	(0.011)	(0.014)	(0.021)	(0.041)
	[3,348]	[3,348]	[3,348]	[2,195]	[1,056]	[266]
No Indianapolis urban	0.093***	0.112***	−0.018	−0.060***	−0.003	0.059
	(0.018)	(0.019)	(0.013)	(0.017)	(0.026)	(0.054)
	[2,455]	[2,455]	[2,455]	[1,376]	[606]	[144]

Notes: $^{*}P \leq 0.050$; $^{**}P \leq 0.010$; $^{***}P \leq 0.001$. ISTEP+ ELA achievement measured in SD units, relative to the Indiana state mean and SD within each grade and year. Number of voucher students contributing to each yearly estimate in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.
Source: Authors' calculations.

baseline in years one, three, and four, is why we described our overall findings in ELA as not statistically meaningful.

In the second robustness check involving an alternative specification of prior achievement, we included polynomials of baseline and pre-baseline achievement. The polynomial for each includes a first-, second-, and third-order term. This flexible specification reflects a similar approach used by Dobbie and Fryer (2017). We did not find any meaningful differences in the voucher effects when compared to our preferred model.

The third robustness check involved including baseline and pre-baseline achievement scores (and their third-order polynomials) in the off subject (e.g., pretreatment math achievement for ELA models and vice versa) as additional controls. This decision was influenced by the growing value-added modeling literature (for review, see Koedel, Mihaly, & Rockoff, 2015). Again, we found no meaningful differences with our preferred model.

Moving away from alternative specifications of prior achievement, we included two mobility indicators to distinguish students (voucher or public) who changed schools from the previous year in our next robustness check. The first indicates whether a student made a structural move due to normal grade progression. The second indicates whether a student made a nonstructural move (switching schools

for any other reason we did not observe). Both variables indicate a switch only in the school year immediately after the switch took place (t), even though the switch takes place between years t and $t-1$. There is a negative association between mobility and student achievement (see Schwartz, Stiefel, & Cordes, 2017), so these indicators may explain away part of the first-year voucher effect as all voucher students in our analysis made a transition to a private school in the first year. In math, we found a first-year achievement loss of 0.022 SD less after controlling for mobility. Yet, the loss in later years remained roughly the same or larger in magnitude. We found no meaningful differences in the ELA results.

In our next robustness check, we constrained our sample to only include voucher students making a structural move to a private school and their matched public school peers. Families with higher motivation or other resources may be more likely to make nonstructural moves, which may correlate with higher achievement outcomes for voucher students. Voucher students making structural moves have even higher baseline achievement in both subjects than their matched peers. While the first-year achievement loss in math is slightly lower, the second-, third-, and fourth-year losses became increasingly larger compared to the overall voucher effects. The lower achievement of voucher students initially making a structural move suggests our hypothesis may be valid, though unsurprising with a program designed to encourage mobility. In ELA, we did not find any meaningful differences, except for the second-year loss, which is not statistically significant for voucher students making structural moves to private schools.

In the next robustness check, we kept students who exit private schools or the small number of students who have stopped receiving a voucher yet remained in private schools as part of the treatment condition. Several unobserved reasons (time-varying confounding) may have influenced a student's decision to switch back to a public school or to stop receiving a voucher. This approach stems from the work by Robins, Hernán, and Brumback (2000) and Sobel (2012). Largely, we found no meaningful differences compared to our preferred model. However, we note that the second- and third-year losses in math and second-year loss in ELA are approximately 0.010 to 0.020 SD smaller than the preferred model, suggesting that exiting voucher students marginally improve their achievement after returning to a public school.

The final robustness check stems from our own prior work. We previously found that students attending Catholic and other private schools in Indianapolis, of whom at least half are voucher students, experienced annual achievement losses in math (Berends & Waddington, 2018). To ensure these students were not driving results, we removed all students in the Indianapolis urban area from the analysis. We observed similar findings in both subjects as our preferred model, suggesting that the voucher student loss in math achievement was statewide and even slightly larger in magnitude (by 0.010 to 0.020 SD) outside of Marion County.