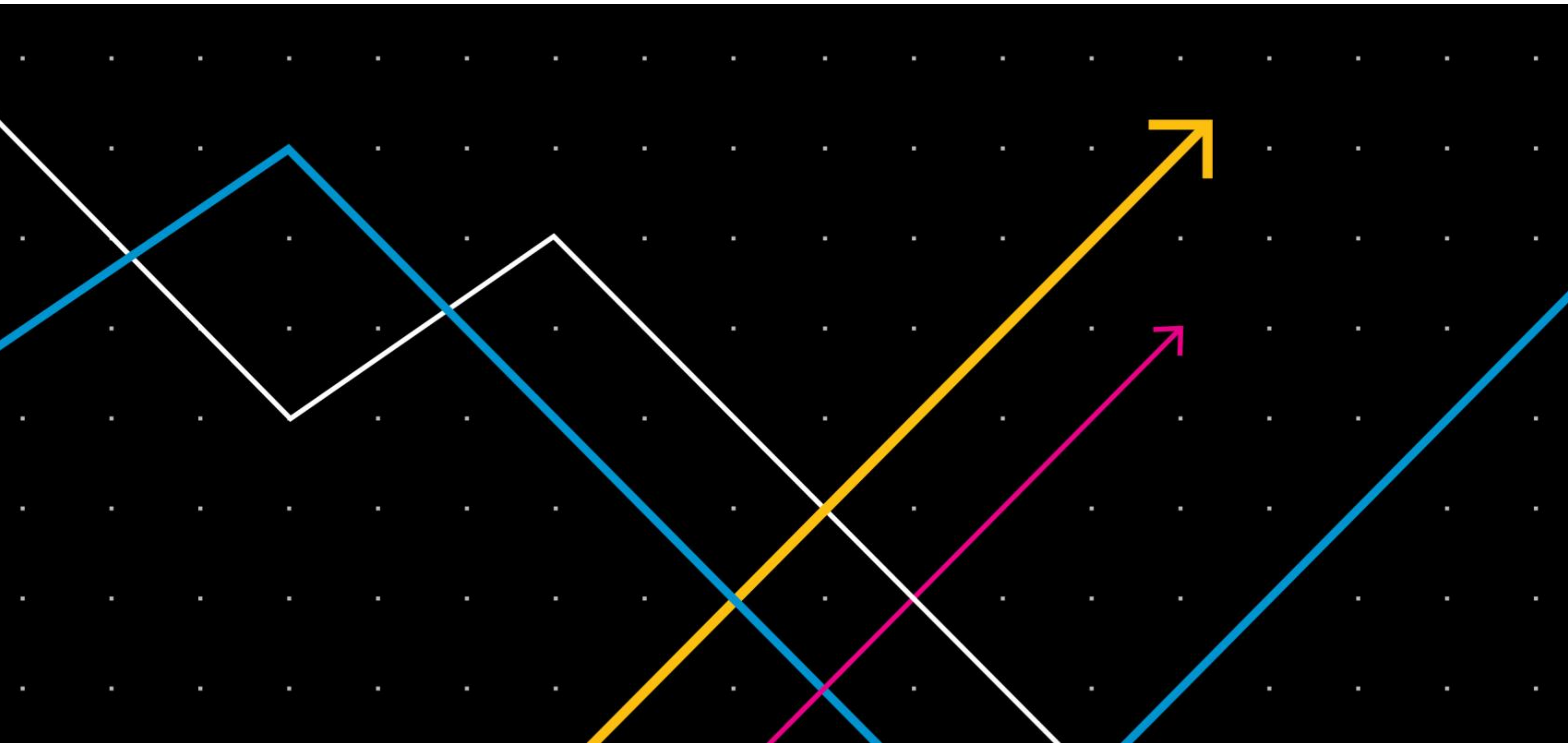

RESEARCH REPORT

The Effects of Ohio's EdChoice Voucher Program on College Enrollment and Graduation

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Executive Summary

More than a million US students now participate in private school choice programs because of recent growth in vouchers, tax credit scholarships, education savings accounts, and tax credits. Given the rapid expansion in these programs, it is important to understand how they affect the students who participate in these programs and the students who remain in public schools.

Ohio's Educational Choice Scholarship Program (EdChoice), which was enacted 20 years ago, provides a rare opportunity to study the long-term impacts of a statewide voucher program on both private and public school students' enrollment in and graduation from college. Prior research on EdChoice found that the program modestly improved the test scores of public school students but harmed the achievement of students who used a voucher to attend private school.

In this study, we track the college enrollment and degree attainment of more than 6,000 students who first participated in EdChoice between 2008 and 2014 and compare them with the outcomes of more than 500,000 students with similar demographic characteristics and student achievement who remained in public schools. About 1,400 of these EdChoice students are old enough for us to track through potential graduation with a bachelor's degree.

We find that students who used an EdChoice scholarship to attend private school were substantially more likely to enroll in and graduate from college than similar students who remained in public schools. For college enrollment, the impact of EdChoice participation was 15 percentage points, which represents a 32 percent increase over the public school enrollment rate of 48 percent. For college graduation, the impact was 9 percentage points, corresponding to a 60 percent increase above the public school rate of 15 percent.

Compared with public school students, EdChoice students were especially more likely to enroll in four-year and selective colleges. For example, EdChoice students were 15 percentage points (50 percent) more likely to enroll in four-year colleges but were only 4 percentage points (16 percent) more likely to enroll in two-year colleges.

For both enrollment and graduation, we find suggestive evidence that effects are largest for the approximately 60 percent of EdChoice students who remained in the program for at least four years. But because students who remained in the program for at least four years have characteristics that make them more likely to have graduated from college absent the voucher, we caution that these results are at greater risk of bias than our main findings.

Estimated enrollment impacts are strongest for male students, Black students, students with below-median test scores before leaving public school, and students who spent the most time in poverty during their childhood. For example, EdChoice participation increased Black students' college enrollment by 18 percentage points (37 percent), compared with a 13 percentage-point (26 percent) increase for white students. The EdChoice impact on college graduation was 10 percentage points for both groups, but that increase corresponds to a 136 percent increase for Black students compared with 79 percent for white students.

We also find that the EdChoice voucher program had positive estimated impacts on students who remained in public schools. Students in public schools who were eligible for EdChoice experienced modestly higher college attendance and graduation rates, even though gains in standardized test scores appear limited. The effects are particularly pronounced for Black students and students from low-income families.

Taken together, these findings indicate that Ohio's school voucher program had the largest benefits for the primarily low-income students who used the program to attend private school and that students who remained in public schools also saw improvements in their chances of getting to and through college. More generally, the fact that EdChoice participation appears to have decreased state test performance while boosting long-run outcomes indicates that state tests might not be an ideal metric for evaluating private school quality, given curricular differences between sectors and different incentives to perform on state exams between public schools that faced accountability for their students' performance on these exams versus private schools that did not.

The EdChoice program has changed substantially since the period this study covers. The students we follow into college joined EdChoice when the program was largely limited to low-income students from struggling public schools. The program has more than quintupled in size since then, and all students statewide are now eligible. This study includes very few higher-income students and includes no students who did not previously attend public school, so it is unclear whether the positive results we find will hold for these students.

Our results add to a growing evidence base that voucher programs can improve important long-run outcomes for low-income students even if those programs reduce test scores in the short run. At the same time, the significant differences—in both eligibility and scale—between the targeted programs that have produced this encouraging evidence and the universal programs currently being expanded across the country mean that more evidence is needed to verify that these positive results will continue.

The Effects of Ohio’s EdChoice Voucher Program

The number of US students in private school choice programs surpassed 1 million for the first time last year.[1] The recent growth in voucher, tax credit scholarship, education savings account, and tax credit programs reflects the enactment of new policies and the expansion of existing programs. In many states, eligibility for private school choice programs has been expanded from lower-income families to all students statewide.

Given the rapid expansion in private school choice, it is important to understand how these programs affect the students who participate—in most cases, to attend private schools—and the students who remain in public schools. Prior research on the effects of these programs on public school students suggests modest positive effects on public school students’ test scores in Florida and Ohio (Figlio and Hart 2014; Figlio, Hart, and Karbownik 2023; Figlio and Karbownik 2016), but we know of no studies that evaluate these programs’ longer-run effects on public school students.

Meanwhile, research about these programs’ effects on voucher participants themselves presents a puzzle. Studies of programs in Indiana, Louisiana, and Ohio found negative impacts on participants’ state test scores (Abdulkadiroğlu, Pathak, and Walters 2018; Erickson, Mills, and Wolf 2021; Figlio and Karbownik 2016; Mills and Wolf 2017; Waddington and Berends 2018), but results are more favorable regarding college enrollment and degree attainment. Erickson, Mills, and Wolf (2021) show no statistically significant effects of voucher participation on college enrollment in Louisiana, and Chingos and coauthors (2019) found positive effects on college enrollment and degree attainment in Florida and Milwaukee.

These studies raise the question of whether state test scores are a useful metric to gauge the performance of private schools, which often have different curricula from public schools and might face different incentives to concentrate on state examinations. But these studies are not conclusive, in part because the divergent findings between short- and long-run outcomes typically come from programs in different states.[2]

We study the effects of Ohio’s Educational Choice (EdChoice) voucher program on the longer-run college outcomes of private and public school students affected by the voucher. We contribute to research on these programs in two principal ways. For one, we present the first evidence to our knowledge of a school voucher program’s effects on public school students’ college outcomes. In addition, this is only the second study to our knowledge to estimate the long-term effects of a statewide private school choice program that has previously been shown to reduce participants' test scores, and the first study to be able to follow students as far as on-time college graduation.

Erickson, Mills, and Wolf (2021) found that the Louisiana voucher program had large negative impacts on math and reading achievement but no statistically significant impacts on college enrollment, but they were able to investigate only college enrollment within six months of high school graduation. The Louisiana evaluation was limited to oversubscribed schools, and the small sample sizes led to large standard errors. As a consequence, the authors cannot rule out, at the 95 percent confidence level, effect sizes of –9 percent to 25 percent for college enrollment relative to the control group mean. We bring to bear a larger sample size that permits greater precision of estimates, as well as longer-run college outcomes than were possible to study at the time of the Louisiana analysis.

We track the college enrollment of more than 6,000 students who first participated in EdChoice between 2008 and 2014, compared with the outcomes of more than 500,000 students with similar demographic characteristics and student achievement who remained in public schools. We find the EdChoice students were substantially more likely to enroll in college than matched comparison students (64 versus 48 percent), especially at four-year colleges and more selective colleges.

For the approximately 1,400 students who are old enough to have potentially graduated from college with a four-year degree, we find substantial positive impacts of EdChoice participation on bachelor's degree attainment (23 versus 15 percent). For both enrollment and graduation, we find suggestive evidence that effects are largest for the approximately 60 percent of EdChoice students who remained in the program for at least four years, though these students tend to be stronger performing. Enrollment impacts are strongest for male students, Black students, students with below-median test scores before leaving public school, and students who spent the most time in poverty during their childhood.

We also find that the EdChoice voucher program had positive impacts on students who remained in public schools. Students in public schools who were eligible for the EdChoice voucher program experienced modestly higher college attendance and graduation rates, even though gains in standardized test scores appear limited. The effects are particularly pronounced for Black students and students from low-income families.

Taken together, these findings indicate that the EdChoice voucher's previously noted small positive effects on public school students appear to continue beyond test scores to college outcomes and that private school participants in the EdChoice program have positive college outcomes, in contrast to the large negative short-run test score outcomes previously found.

Ohio's EdChoice Program

Ohio enacted what is now the Educational Choice Scholarship Program (EdChoice) in 2005, and students first used scholarships from this program to attend private schools beginning in the 2006–07 school year.[3]

The program has expanded significantly over the years. At first, eligibility was limited to students who previously attended public schools that received the lowest rating under the state's accountability system three years in a row. In 2006, the legislature expanded eligibility to students attending schools that received either of the two lowest ratings in at least two of the preceding three years.

In 2013–14, Ohio launched EdChoice Expansion, which provides scholarships to economically disadvantaged students regardless of their public school's accountability rating. This program gradually expanded (one grade per year) from kindergarten students in 2013–14 to all K–12 students in 2020–21 (Glover, Redmond, and Vitale 2019).

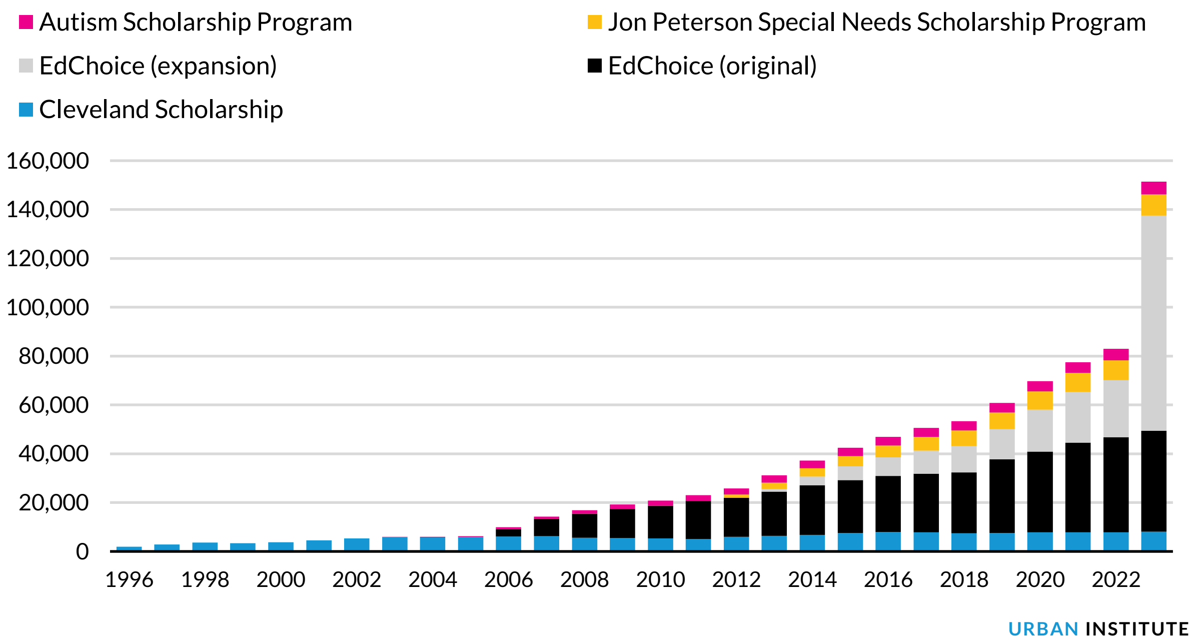
Since 2023–24, all students have been eligible for EdChoice Expansion regardless of income (though families with incomes above 450 percent of the federal poverty level receive less than the full scholarship amount). Importantly, our study does not speak to the effects of the new universal voucher program. Our study covers students who first participated in the original EdChoice program between 2007–08 and 2013–14. During this period, eligible students received vouchers worth up to $4,250 for grades K–8 and $5,000 for high school. For students from families earning below 200 percent of the federal poverty level, private schools had to accept the scholarship as full payment for tuition.

Figure 1 shows the rapid expansion in EdChoice participation, from 3,071 students in 2006–07 to 62,289 in the original and expansion programs in 2022–23 and 129,541 in 2023–24 following the elimination of means testing. Ohio has several other voucher programs, including the Autism Scholarship Program, the Jon Peterson Special Needs Scholarship Program, and the Cleveland Scholarship (launched in 1996 as the state's first voucher initiative).[4] As of 2014, the last year covered by our study, 20,263 students were receiving a voucher through the original EdChoice program.

FIGURE 1

Number of Students Participating in Ohio Voucher Programs

Voucher use in Ohio has grown rapidly



Source: "School Choice in Ohio," EdChoice, accessed March 26, 2025, https://www.edchoice.org/school-choice/state/ohio/.

EdChoice students were required to take all state exams through 2018–19, after which participating private schools could choose either the state tests or a state-approved alternative (Glover, Redmond, and Vitale 2019, 38).[5] Figlio and Karbownik (2016) used propensity score matching to estimate the impact of EdChoice participation on state test performance for students who left public schools that barely qualified for the voucher program (i.e., relatively higher performing compared with all eligible schools). The researchers found large negative impacts on test performance. The same report also studied the program's effects on public school students and found small improvements in math and reading scores for students attending eligible public schools relative to students attending schools that just missed eligibility.

Data and Methods

For our main analysis on EdChoice's effects on participating students, we compare the college enrollment and graduation outcomes of students who used an EdChoice voucher to attend private school with those of matched students (based on their prior test scores, demographics, and attended schools) who remained in public school. We use linked student-level data on demographics, state test performance, public school enrollment, and EdChoice participation from the Ohio Department of Education and Workforce linked to college enrollment and attainment data from the National Student Clearinghouse.

We identify 6,243 EdChoice participants with complete background information data who were enrolled in public schools in grades 3–8 between 2007–08 and 2013–14. These students were old enough to enroll in college within two years of expected high school graduation and appear in our extract of National Student Clearinghouse data, which run through fall 2020. A subset of 1,650 students were old enough to be observed for at least six years following high school graduation, which is the sample we use to examine bachelor's degree attainment. Table 1 shows the number of students in each entering cohort included in our analysis.

TABLE 1

Number of EdChoice Students Included in Analysis by Entering Cohort

School year	Grades						
(spring)	3	4	5	6	7	8	Total
2008	182	173	295	298	176	378	1,502
2009	153	176	252	232	106	347	1,266
2010	1	163	279	234	107	345	1,129
2011	0	0	298	235	134	346	1,013
2012	0	0	0	131	72	291	494
2013	0	0	0	1	90	356	447
2014	0	0	0	0	0	392	392
Total	**336**	**512**	**1,124**	**1,131**	**685**	**2,455**	**6,243**

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: Cells indicate the number of EdChoice students included in study sample, by the school year and grade from the year before their initial participation in EdChoice. Shaded cells indicate cohorts that are also in the graduation sample (*N* = 1,650).

Table 2 shows the characteristics of EdChoice participants, measured before they participated in the program. Participating students had test scores well below the state average (e.g., –0.24 standard deviations in reading) but well above that of nonparticipating students (e.g., –0.42 standard deviations in reading), suggesting positive selection into the program based on prior academic performance, a phenomenon Figlio and Karbownik (2016) also documented. Both values are negative because we restrict the potential comparison sample to public schools that sent at least one student to a private school through the voucher program during our data window. This improves computational capacity of the propensity score matching and does not affect our results. In select specifications, we also match within rather than across public schools, and thus, schools that never lose students to private schools through the voucher program do not contribute to the identifying variation. EdChoice students were also more likely to be Black or Hispanic and less likely to receive special education services.

We match each EdChoice participant to nonparticipants with similar prior test scores, demographics, and attended schools using propensity score matching.[6] This matching procedure does not fully eliminate the potential that voucher participants are differentially selected on unmeasurable characteristics, but it reduces the concern. Later in this report, we present evidence about the potential range of values one might reasonably expect given this differential selection. Our baseline specification matches with replacement and uses a bandwidth of 0.0005, probit, and Epanechnikov kernel to estimate average treatment effect on the treated (ATT), but in numerous robustness checks reported below, we show that the exact specification of the matching algorithm plays little role in shaping our findings. We cluster the standard errors at the level of the school that a student attended the year immediately before private school enrollment.[7] Table 2 shows that this matching procedure produces a comparison group of students with similar test scores and demographic characteristics to the EdChoice participants.

TABLE 2

Demographic Characteristics and Prior Achievement

	Raw			Matched		
	Treated	Untreated	Difference	Treated	Untreated	Difference
College attendance sample						
Reading score (std.)	-0.24	-0.42	0.18	-0.24	-0.24	0.00
Math score (std.)	-0.35	-0.45	0.10	-0.35	-0.35	0.00
Female	0.51	0.50	0.01	0.51	0.51	0.00
Black	0.62	0.46	0.16	0.62	0.62	0.00
Hispanic	0.06	0.04	0.02	0.06	0.07	0.00
Other	0.07	0.07	0.00	0.07	0.07	0.00
Low income	0.73	0.71	0.02	0.74	0.73	0.01
ELL	0.04	0.03	0.02	0.04	0.04	0.00
Special education	0.07	0.16	-0.30	0.07	0.07	-0.01
College graduation sample						
Reading score (std.)	-0.21	-0.55	0.34	-0.25	-0.28	0.03
Math score (std.)	-0.35	-0.60	0.25	-0.38	-0.41	0.04
Female	0.48	0.50	-0.01	0.49	0.48	0.01
Black	0.69	0.51	0.18	0.67	0.66	0.01
Hispanic	0.05	0.04	0.01	0.05	0.06	0.00
Other	0.05	0.06	-0.01	0.06	0.06	0.00
Low income	0.69	0.73	-0.04	0.70	0.69	0.01
ELL	0.02	0.03	-0.10	0.02	0.02	-0.01
Special education	0.05	0.17	-0.38	0.06	0.07	-0.04

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: ELL = English language learner. Bandwidth = 0.0005. The panel A raw sample has 6,245 observations in the treated group (students who attended private school) and 608,358 observations in the control group (students who did not attend private school), while the matched sample has 6,216 observations in the treated group and 571,517 observations in the control group. This means that at the bandwidth of 0.0005, we do not find matches for 27 observations. The panel B raw sample has 1,650 observations in the treated group (students who attended private school) and 87,678 observations in the control group (students who did not attend private school), while the matched sample has 1,411 observations in the treated group and 77,791 observations in the control group. This means that at the bandwidth of 0.0005, we do not find matches for 239 observations. Private school students are students who enrolled in private school in grades 4 to 9 for whom we observe public school records with valid test scores in grades 3 to 8. Public school students are in grades 3 to 8 with valid test scores and background information who are enrolled in schools from which at least one student enrolled in private school during our sample period. This leads to modest negative sample selection even in the raw data. We make this restriction because our matching is within schools, and thus, public schools that never send any student to private school do not contribute to the identifying variation. Other than that, the college attendance sample is limited to public school years (spring) 2007, 2008, 2009, and 2010 in grades 4 and above; 2011 in grades 5 and above; 2012 in grades 6 an above; and 2013 in grade 8, while the college graduation sample is limited to public school years (spring) 2007 in grades 5 and above, 2008 in grades 6 and above, 2009 in grades 7 and above, and 2010 in grade 8. This ensures that students in our data have up to two years to enroll in college and up to six years to graduate from college, given their expected normal progress in high school and the end of our college outcomes in 2020.

We also calculate EdChoice's effects on the college enrollment outcomes of students who remained in public schools using the regression discontinuity method originally introduced in Figlio and Karbownik (2016). This method compares the outcomes of students at public schools that were just barely eligible for their students to receive vouchers based on their accountability rating with the outcomes of students at schools that were just barely ineligible.

We use the performance index (PI) score as our running variable in a regression discontinuity design. We use the second-best PI assigned to a school between 2007–08 and 2013–14. This means that in the first year of observations, it is the second-best PI from 2003–04 to 2005–06, while in the last year of observations, it is the second-best PI from 2009–10 to 2011–12. The cutoff for academic watch versus continuous improvement status that determines voucher eligibility was 80 points, and in our main analysis, we choose a narrow bandwidth of 3 points to the left and to the right of this threshold. But our results are robust to alternative bandwidth choices. We cluster standard errors at the school level, yielding 365 and 135 clusters in college attendance and graduation analyses, respectively. Thus, this analysis likewise does not suffer from too few clusters.

One complication of our setting is that schools could enter the neighborhood of the cutoff from the left (treatment) or the right (control) in multiple years. This means we stack together multiple school-year quasi experiments. We address this potential issue in two ways. First, in addition to the pooled sample, we present results where we select the school only the first or the last time we observe it in the regression discontinuity sample. This means each school is observed in the regression exactly once. Second, in the pooled analysis, we include grade-by-year fixed effects to control for the stacked design.

Empirically, we use local-linear regression to estimate our parameter of interest, which will be unbiased, assuming that potential outcomes are smooth through the eligibility cutoff. We provide supportive evidence for this assumption by performing balancing and density tests. Appendix table A.8 presents the results of balancing check for first observed, last observed, and pooled samples and for college attendance and graduation samples. We consider gender, race, poverty, special education status, and prior-year test scores as balancing variables. In line with the identifying assumption, we do not find any statistically significant discontinuities in the background characteristics. Out of 42 coefficients, none are statistically significant at conventional levels, though we acknowledge that test scores in the college graduation sample are imprecisely estimated because of extremely small sample sizes. Consistent with this evidence, appendix figure A.1 shows that the density of the running variable is smooth (especially for college graduation) through the cutoff, though we are concerned about the density mass right after the cutoff for the college attendance sample. In fact, the test Cattaneo, Jansson, and Ma (2020) proposed rejects the null of smooth distribution in both cases, and thus, we address this through a donut hole approach in robustness checks.

Estimated Effects on EdChoice Participants

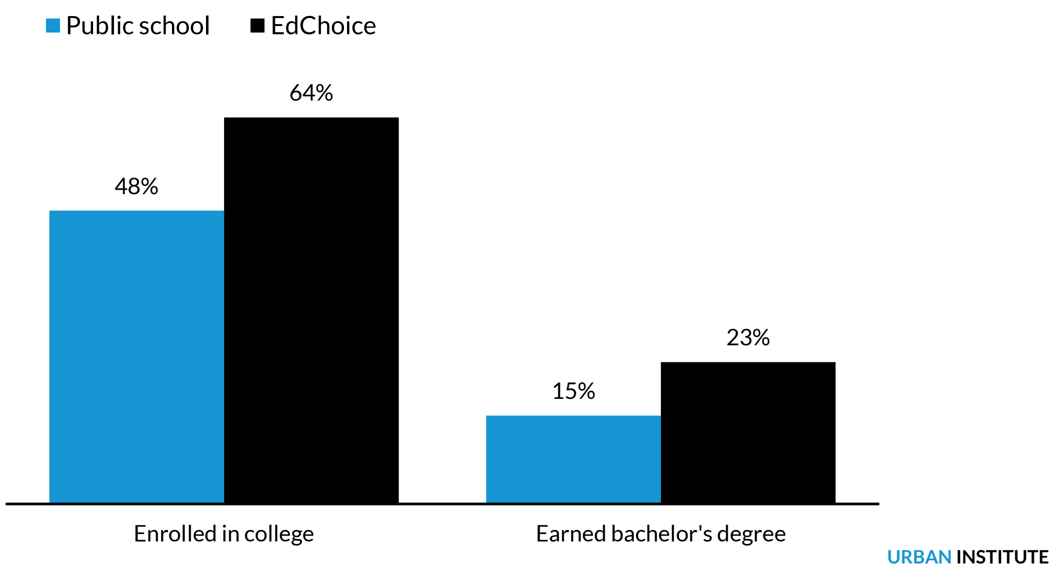
EdChoice participants were significant more likely to enroll in college and earn bachelor's degrees (figure 2). Sixty-four percent of EdChoice scholarship users enrolled in any college within two years of their expected graduation, compared with 48 percent of comparison students who remained in public schools. This difference of 15 percentage points represents a 32 percent increase. (Full regression results are in appendix table A.1; effect sizes do not precisely match the differences between the blue and black bars in the figures because of rounding.)

For the smaller group of students who are old enough to be observed through their potential college graduation (at least six years following high school), 23 percent of EdChoice students earned a bachelor's degree compared with 15 percent of comparison students.[8] The effect size of 9 percentage points represents a 60 percent increase relative to the control group.

FIGURE 2

Share of Students Enrolling in or Completing College

EdChoice participants are more likely to enroll in college and earn four-year degrees



Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: Enrollment is within two years of expected high school graduation, and bachelor's degree attainment is measured within six years of expected high school graduation.

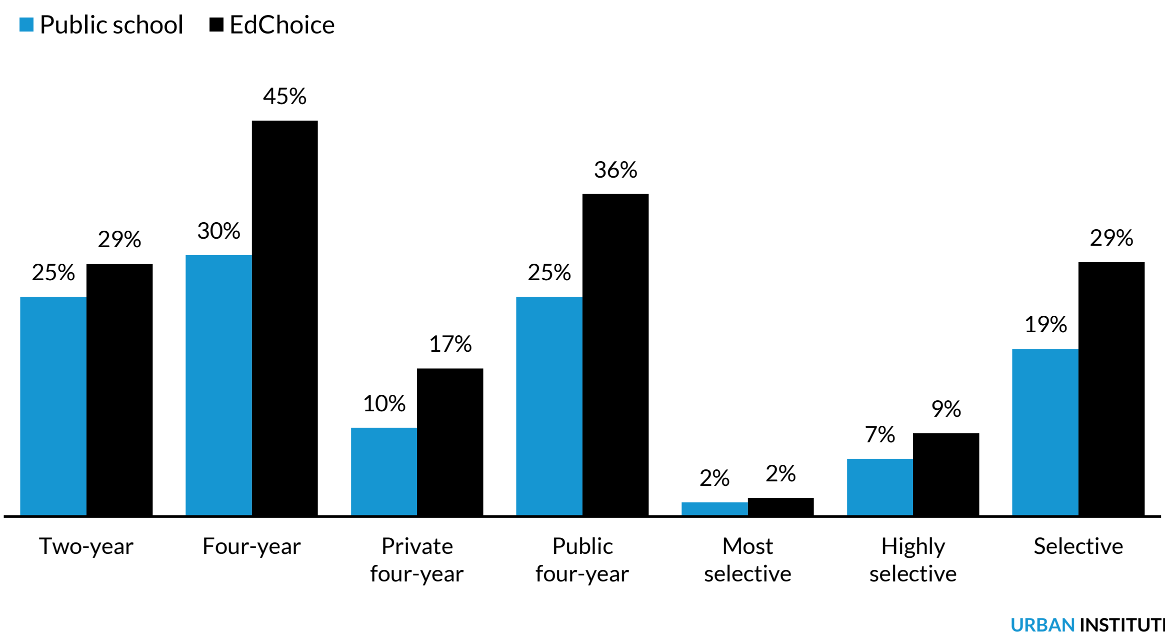
Compared with nonparticipants, EdChoice students were especially more likely to enroll in four-year and selective colleges (figure 3), though estimated impacts across all sectors were statistically significant (appendix table A.1).[9] For example, EdChoice students were 15 percentage points more likely to enroll in four-year colleges (45 versus 30 percent) but only 4 percentage points more likely to enroll in two-year colleges (29 versus 25 percent). This might partially explain the substantial impact of EdChoice participation on bachelor's degree completion.

We find suggestive evidence that the effects on college enrollment and graduation are largest for the approximately 60 percent of EdChoice participants who remain in the program for at least four years (figure 4; appendix table A.2). These EdChoice students are 21 percentage points (44 percent) more likely to enroll in college than comparison students in public schools, compared with effects of 5 to 10 percentage points for EdChoice students who participated for one to three years. Likewise, bachelor's degree attainment effects are 16 percentage points (161 percent) for the long-term participants compared with statistically insignificant effects of up to 2 percentage points for the shorter-term participants.

FIGURE 3

Share of Students Enrolling in Different Types of Colleges

Enrollment impacts are largest at four-year and selective colleges

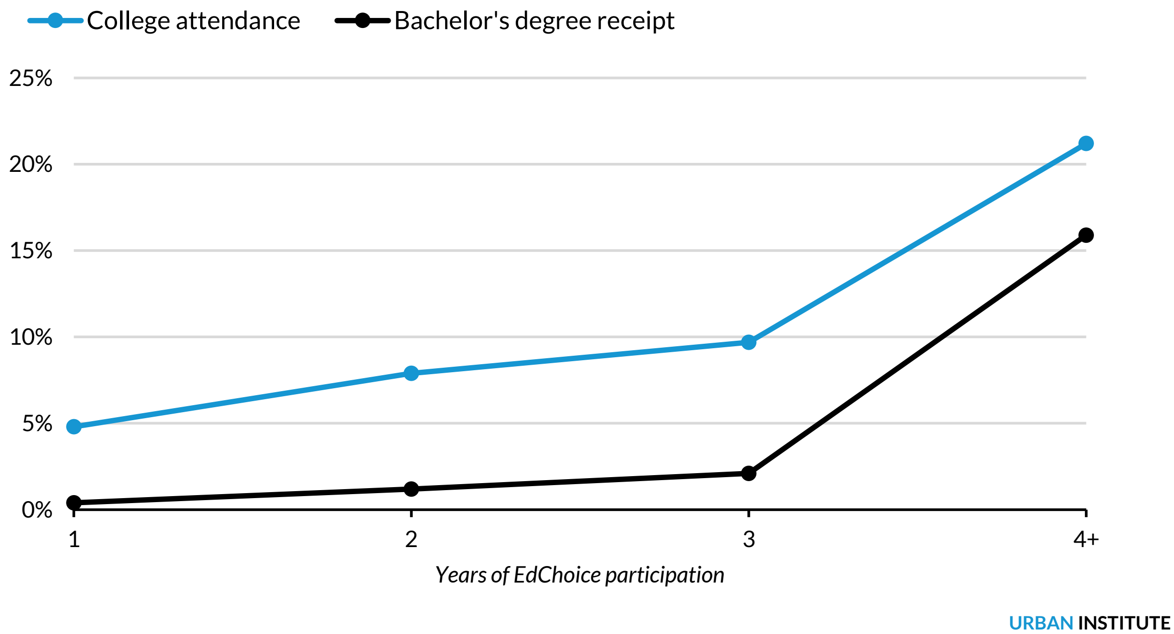


Source: Authors' calculations from Ohio Department of Education and Workforce data.
Notes: Results reflect any enrollment in the listed type of college within two years of expected high school graduation.

FIGURE 4

Impact of EdChoice Participation, by Years of Program Participation

Students who stayed in EdChoice for four or more years saw the largest benefits



Source: Authors' calculations from Ohio Department of Education and Workforce data.
Note: The values on the vertical axis are percentage points.

This evidence is consistent with prior studies finding that longer participation in private school choice programs is linked to larger increases in college enrollment and graduation (Chingos, Monarrez, and Kuehn 2019), but we characterize the evidence as suggestive, given that the long-term participants are positively selected in terms of their pretreatment characteristics (appendix table A.3). For example, in the college attendance sample, 82 percent of students who participated for four or more years were identified as low income at some point in the data, compared with 96 percent of one-year participants. And long-term EdChoice participants had higher math and reading scores before participation than those who did not remain in the program as long. Our matching procedure adjusts for these observed differences, but the substantial selection we observe increases the risk that there are differences on unmeasured characteristics correlated with college outcomes.

We next examine whether EdChoice participation effects vary by students' baseline characteristics, including demographics and test scores. We find more evidence of such heterogeneity for college enrollment than for bachelor's degree receipt (appendix table A.4). For example, enrollment impacts are larger for boys than for girls and for Black students than for white students, but there is little such variation in the estimated graduation impacts.[10] Enrollment impacts are much larger for students with below-median reading and math scores, but graduation impacts do not vary with reading scores and are modestly larger among students with above-median math scores.

Relative changes in postsecondary outcomes yield different inferences about heterogeneity in impacts than the absolute changes. For example, bachelor's degree attainment increased 136 percent for Black students and 79 percent for white students. Put differently, the relative shift in college graduation among Black students was 1.7 times that of white students, even though the absolute shifts were similar for the two groups.

We might expect results to vary by geography, given differences in such factors as private school availability and transportation options between large cities, smaller towns, and rural areas. Smaller communities tend to have fewer private school options and are particularly less likely to have several convenient private school options. One might therefore reasonably expect that students would be more likely to find better private school matches in larger communities with more options and that private schools in larger communities are more likely to face competitive pressure. Consistent with this potential expectation, we find some evidence that EdChoice impacts are bigger in larger metropolitan areas, especially for college graduation compared with college enrollment (appendix table A.5).

Finally, we confirm that our main results are robust to a wide range of alternative methodologies, control group construction approaches, and samples (appendix table A.6). We first confirm that our results are not affected by excluding 2006–07 from the main analysis.

Subsequent columns test the sensitivity of results to the choice of schools included in the control group. Our results are invariant to both excluding schools for which we do not observe four years of school performance information and schools that are further away from the eligibility cutoff (either below 77 points or above 82 points on the performance index). The next two columns limit the control group to schools where students were ineligible for the voucher: either all schools with PI scores greater than 80 or schools that just barely missed the eligibility cutoff with PI scores between 80 and 82. In this last sample, we find similar college attendance but larger college graduation effects, but the treated sample is relatively small in this instance, and we have relatively more unmatched students. This makes sense, given that these schools, on average, have higher-achieving and more affluent students, and thus, it is harder to find matches for voucher-eligible students.

Our next set of robustness checks addresses the sensitivity of the matching procedure. Our findings are unchanged if we use logit instead of probit in matching, use triangular kernel instead of Epanechnikov kernel, include score fixed effects instead of continuous score variable, use nearest neighbor with exact matching rather than propensity score matching, use a doubly robust estimator with ex-post entropy balancing, or match individuals exactly on either a limited set (school, year, and grade) or full set of variables used in the propensity score construction. This last analysis is not our preferred approach because it greatly restricts the sample of matched individuals (necessitated because of the continuous nature of our test score measures), thus increasing the standard errors and potentially limiting external validity. The final column presents average treatment effects rather than average treatment effects on the treated, with somewhat smaller estimates, as expected, that nonetheless remain statistically significant at conventional levels.

A final sensitivity check pertains to our choice of bandwidth for propensity score matching. In the preferred specification, the bandwidth is 0.0005, the optimal bandwidth computed for our college graduation sample. Appendix figure A.3 presents the ATT estimates for college attendance and graduation outcomes for bandwidths ranging from 0.00001 to 0.25. The results are largely unchanged in terms of effect sizes and statistical significance, though with more restrictive bandwidths, we find matches for fewer students attending private schools. Appendix figure A.4 presents the nearest neighbor matching estimates of ATT for unique match (one nearest neighbor) all the way to 10 matches. The results are invariant to this manipulation.

One natural concern with propensity score matching methods is that they account for selection on observables but are silent about selection on unobservables. To address this issue, to the extent possible, we rely on two approaches. First, we compute the Oster delta (Oster 2019), which is the ratio of the magnitude of selection on unobservables to the magnitude of selection on observables. It is common in the literature to assume that a delta of 1 or above suggests a robust result. Second, we compute the Rosenbaum and Becker-Caliendo gammas, which provide a degree to which our results are sensitive to bias, increasing odds of exposure to EdChoice (Becker and Caliendo 2007; Rosenbaum 2002). It is commonly assumed in the literature that a gamma of 2 or higher suggests a robust finding. Appendix table A.11 presents these results along with alternative estimators and computations needed to execute these analyses. For college attendance, the Oster delta is approximately 1, suggesting that our results are robust, but for college graduation, the delta is only about 0.5. This means it is enough that selection on unobservables is only half as strong as on observables to nullify our findings. All the gamma bounds range from 1.5 to 2.0, which means our finding is insensitive to a bias that would increase odds of exposure to EdChoice by 50 to 100 percent. Overall, we conclude that our findings are likely robust to selection on unobservables, but we acknowledge that the estimates regarding college graduation appear to be more sensitive than those regarding college attendance, likely because of the smaller analytic sample size.

Effects on Public School Students

Figlio and Karbownik (2016) previously found that the threat of students using EdChoice vouchers to attend private schools improved the test scores of public school students. These kinds of effects potentially capture several mechanisms, including competitive pressure on public schools by virtue of their students being eligible for a voucher, as well as re-sorting of students across schools (which can have implications for class size and peer effects, as well as changes in the curricular or social match between students and schools).

We find evidence that this same effect of EdChoice vouchers extends to college-going outcomes among students who remained in public schools (figure 5; appendix table A.7). Non-EdChoice students who attended public schools that were eligible for the program were about 3 percentage points more likely to enroll in college and 6 percentage points more likely to earn a bachelor's degree than nonparticipants at public schools that were not eligible.

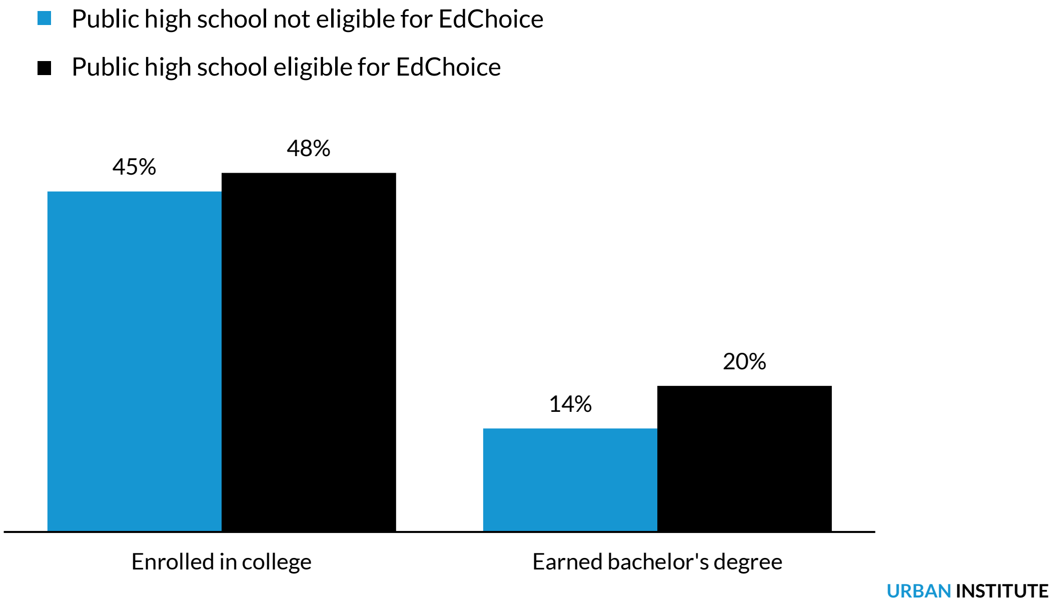
We also examine heterogeneity in the effects on public school students for the two college outcomes, which we perform for two samples: a larger one, where we do not require observing test scores for three consecutive years, and a smaller one where we do (appendix table A.9). The latter sample matches our pooled sample from appendix table A.7. Irrespective of the sample and outcome, we consistently find larger effects for Black students and low-income students. On the other hand, there is no consistent pattern by gender or special education needs.

Similar to the participation effects, we also conduct extensive robustness testing of our results, overall and for Black students and low-income students for whom we documented particularly large gains. Our results are robust to including quadratic terms in the regression discontinuity estimation and to two nonparametric approaches (Calonico et al. 2017; Kolesár and Rothe 2018), though the college attendance estimate using RdRobust, albeit of similar magnitude, is not statistically significant at conventional levels (appendix table A.10). The results are further unaffected by excluding observations close to the cutoff (except of college attendance when we drop +/-1 points bandwidth), alleviating our concerns about potential violations of the density smoothness. In appendix figure A.2, we further show that estimates are comparable when we change the bandwidth, though we acknowledge that the estimates are modestly smaller and not consistently statistically significant at higher bandwidths for college attendance.

FIGURE 5

Share of Students Enrolling in or Completing College

Availability of vouchers increases college enrollment and graduation among students attending public schools



Source: Authors' calculations from Ohio Department of Education and Workforce data. See appendix table A.7 (pooled sample with controls).

Implications for School Choice Policy

Our findings indicate that Ohio's school voucher program had large and lasting positive effects on the primarily low-income students who used EdChoice scholarships to attend private school, with especially large impacts for students who remained in the program for several years. (Again, we caution against overinterpreting these dosage effects because students who attended private schools via EdChoice vouchers for longer periods tended to be higher-performing students, so some of this apparent positive effect might be attributable to differential selection.) EdChoice participants were substantially more likely to enroll in college and to earn bachelor's degrees than similar students who remained in public schools, even though earlier research found negative impacts on test scores (Figlio and Karbownik 2016).

We also find evidence that allowing students to use public funding to attend private schools did not harm outcomes for public school students in Ohio. We find small increases in college enrollment and graduation of public school students associated with the EdChoice program, complementing evidence of increases in more contemporaneous test scores previously documented by Figlio and Karbownik (2016).

A key limitation of this study is that vouchers were not allocated by random lotteries, so we must assume that the rich set of baseline characteristics (including prior test scores) that we match on capture the differences between participants and nonparticipants that affect college enrollment and success. We think the case for this assumption is strengthened by the fact that prior research in Ohio found negative impacts of EdChoice participation on test scores (Figlio and Karbownik 2016), whereas positive selection on unobservables would lead us to expect the opposite.

More generally, the fact that EdChoice participation appears to have decreased state test performance while boosting long-run outcomes indicates that state tests might not be an ideal metric for evaluating private school quality, given curricular differences between sectors and differential incentives to perform on state exams between public schools that faced accountability for their students' performance on these exams versus private schools that did not. But this comparison is based on different research methodologies applying to different samples of students, so more research is needed to verify this disconnect in impacts between short- and long-run outcomes.

Another important limitation of any study that examines long-run outcomes is that the results necessarily reflect the conditions of the voucher program at a previous time, rather than the voucher program as it currently exists. In this case, the most recent EdChoice participants included in the analysis joined the program more than a decade ago when it was still largely targeted to students at low-performing public schools. As a result, the findings might not accurately forecast the impacts of newer programs that are open to all students and include expenses other than private school tuition (e.g., education savings accounts).

In Ohio, the number of EdChoice students more than doubled in 2023 following the elimination of the income cap for families to receive a voucher. But private school enrollment has not matched the increase in voucher use, suggesting that students already enrolled in private schools have played a large part in the increase in voucher users.[11] Our study includes very few higher-income students and includes no students who did not previously attend public school, so it is unclear whether the positive results we find will hold for these students.

Understanding who participates in private school choice programs today and how they fare in school and beyond will be important to evaluate and improve these programs. But they will be harder to study for several reasons. First, students who never attend public school cannot be included in an evaluation like this one because matching on baseline test scores is critical to our methodology (and already limits us from including students who first participate in EdChoice before fourth grade). Second, it will be more difficult to conduct credible research on the effects of universal programs on public school students because there is less variation in public schools' exposure to programs that are widely available. And third, research will not be possible in the many states that have yet to make data available.[12]

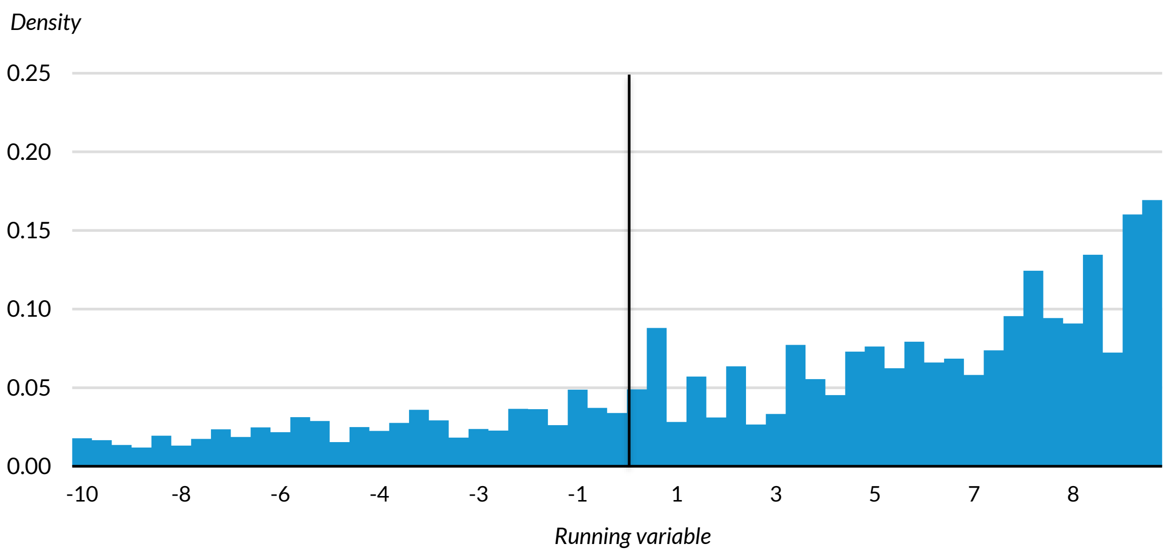
In conclusion, we see reasons for both optimism and caution. The case for optimism is that our results add to a growing evidence base that voucher programs can improve important long-run outcomes for low-income students, even if those programs reduce test scores in the short run. But the significant differences between the targeted programs that have produced this encouraging evidence and the universal programs currently being expanded across the country mean that more evidence is needed to verify that these strong results will continue.

Appendix

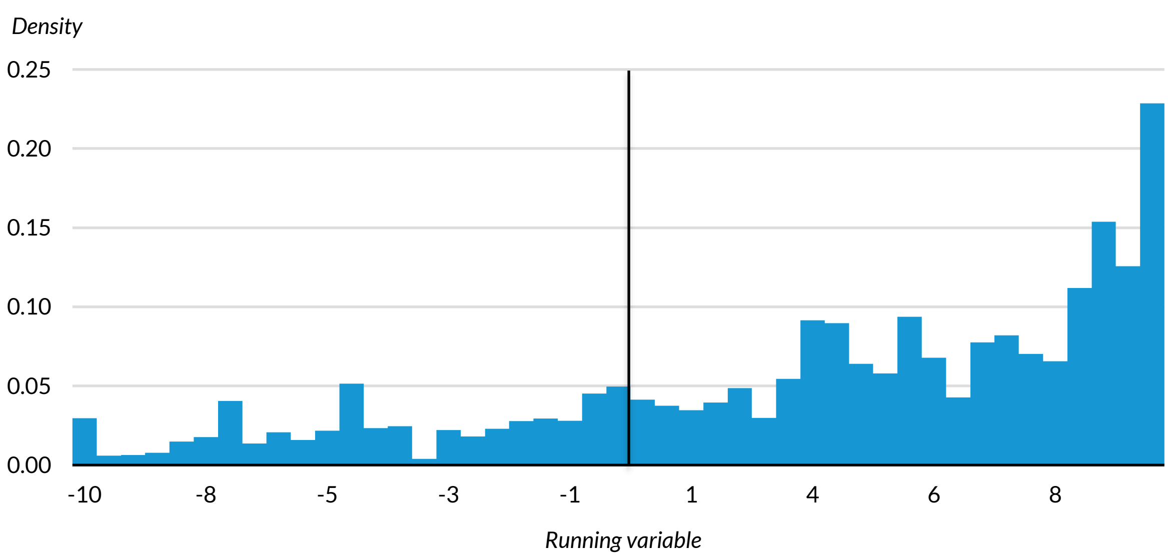
FIGURE A.1

Regression Discontinuity Sample: Density of Distributions

College attendance



College graduation



URBAN INSTITUTE

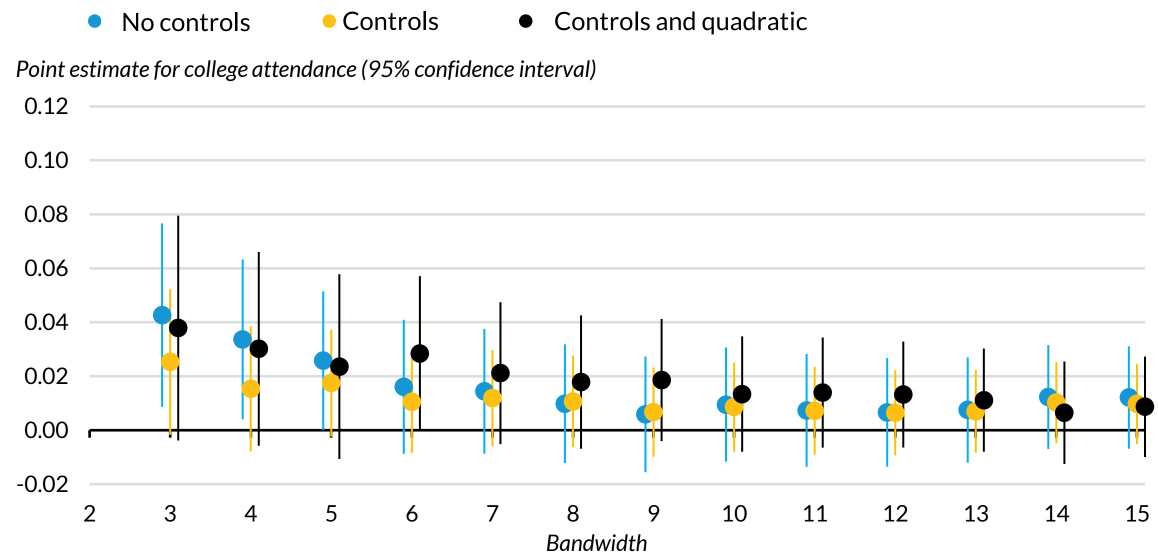
Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: This figure presents the number of observations around the performance index of 80, the EdChoice cutoff. The sample is restricted to +/- 10 performance index points. The pooled sample is as described in panels C of table A.7. Panel A presents the college attendance sample, while panel B presents college graduation sample.

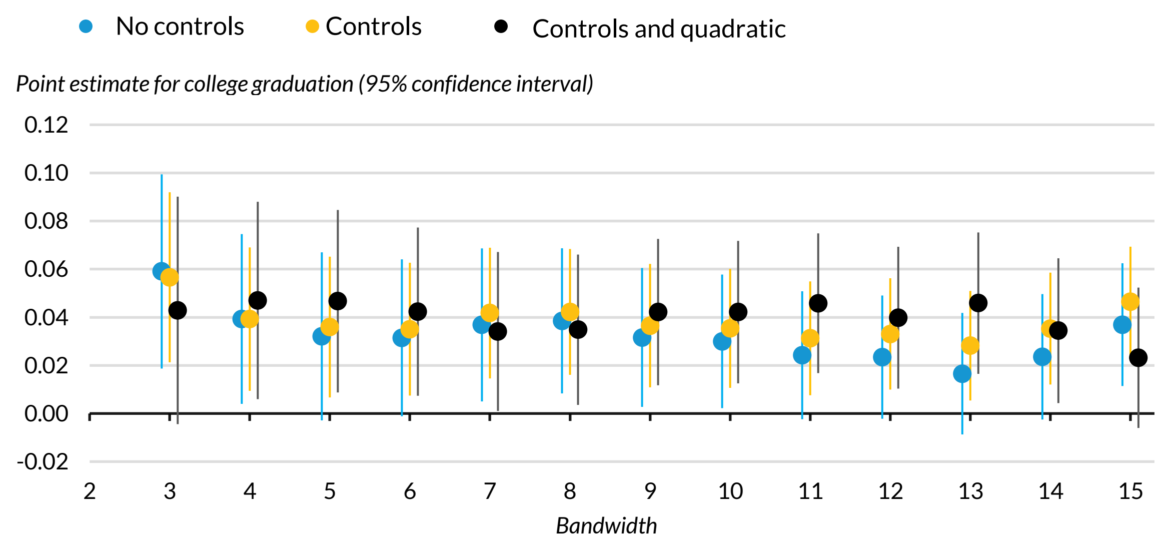
FIGURE A.2

Regression Discontinuity: Robustness to Alternative Bandwidths

College attendance



College graduation



URBAN INSTITUTE

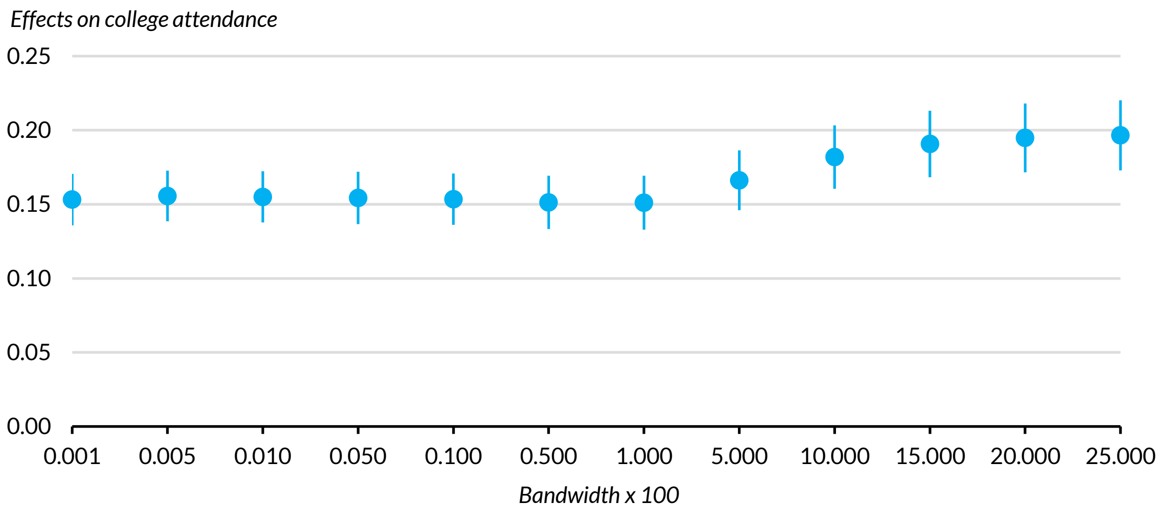
Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: This figure presents robustness analysis for local linear estimates for regression discontinuity design used to study competitive effects of EdChoice. The sample is based on those described in panels C of appendix table A.7. Panel A estimates college attendance effects, while panel B estimates college graduation effects. Blue circle estimates do not include additional controls, yellow circles include demographic controls, while black circles include both demographic controls and quadratic fit. The baseline sample is limited to public schools with performance index scores within 3 points of 80 (the qualifying cutoff), while subsequent estimates expand the bandwidth to +/- 15 performance index points. Ninety-five percent confidence intervals are based on standard errors clustered at the public school level.

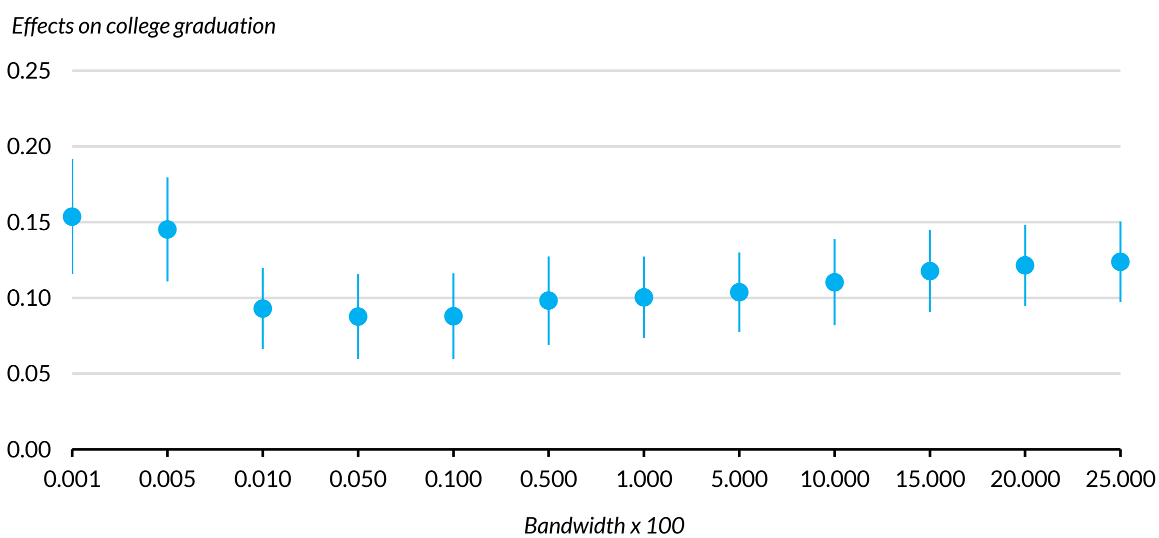
FIGURE A.3

Robustness of EdChoice Participation Effects: Bandwidth Choice in Propensity Score Matching

College attendance



College graduation



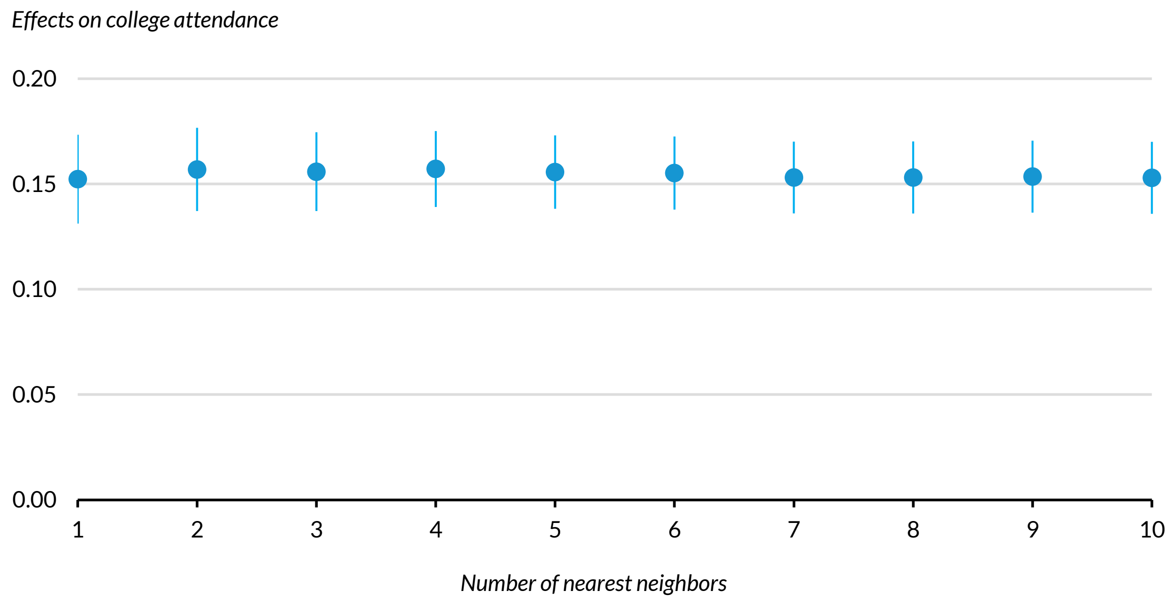
URBAN INSTITUTE

Source: Authors' calculations from Ohio Department of Education and Workforce data.
Notes: Propensity score estimates based on the first column (college attendance) and the last column (college graduation) in table A.1. Sensitivity to different bandwidth choice in propensity score matching ranges from 0.00001 to 0.25. Panel A presents estimates for college attendance, while panel B presents estimates for college graduation. Ninety-five percent confidence intervals are based on standard errors clustered at the baseline public school level.

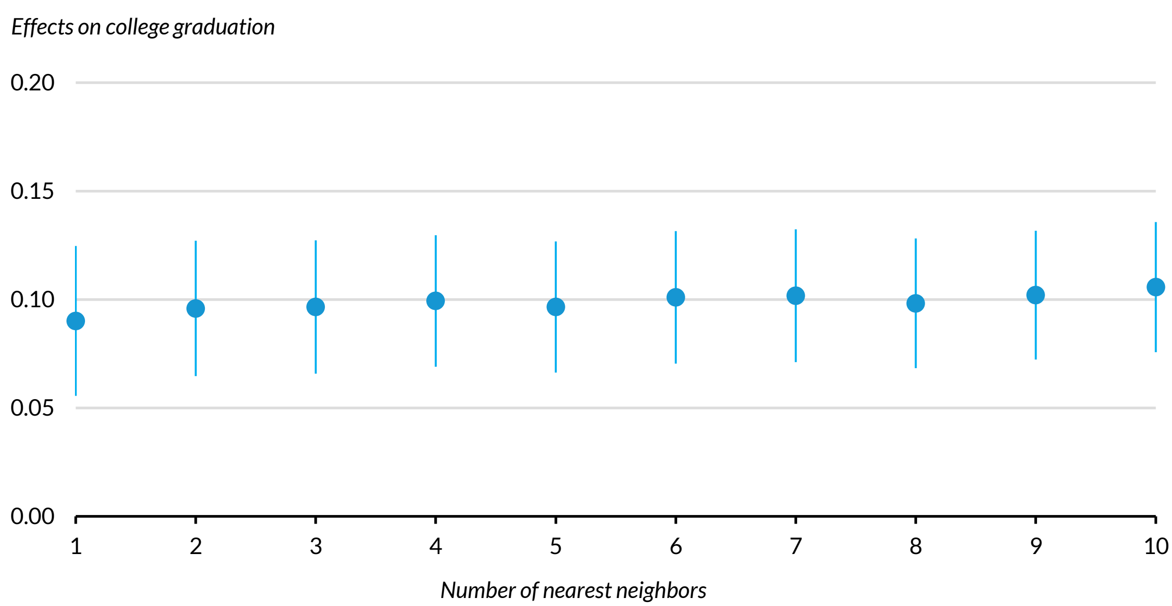
FIGURE A.4

Robustness of EdChoice Participation Effects: Number of Nearest Neighbors

College attendance



College graduation



URBAN INSTITUTE

Source: Authors' calculations from Ohio Department of Education and Workforce data.
Notes: Nearest neighbor estimates are based on column 10 of table A.6. Sensitivity to different number of neighbors chosen in nearest neighbor matching ranges from 1 to 10. Panel A presents estimates for college attendance, while panel B presents estimates for college graduation. Ninety-five percent confidence intervals are based on standard errors clustered at the baseline public school level.

TABLE A.1

Estimated Effects of EdChoice Participation on College Attendance and Graduation

	Attendance								Graduation
	Any college	Two-year	Four-year	Private four-year	Public four-year	Most selective	Highly selective	Selective	Bachelor's
Private school attendance	0.154***	0.037***	0.152***	0.067***	0.116***	0.005***	0.029***	0.098***	0.088***
	(0.009)	(0.007)	(0.010)	(0.005)	(0.009)	(0.002)	(0.004)	(0.007)	(0.014)
Mean of Y (public)	0.484	0.248	0.295	0.100	0.248	0.016	0.065	0.189	0.146
N (treated)	6,217	6,217	6,217	6,217	6,217	6,217	6,217	6,217	1,394
N (comparison)	571,564	571,564	571,564	571,564	571,564	571,564	571,564	571,564	9,882

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: College attendance is measured within two years of expected high school graduation; college graduation is measured within six years of expected high school graduation. Results from propensity score matching account for low-income status, English language learner status, special education status, gender, race or ethnicity, test scores, year, grade, and school. Selectivity is based on the 2004 Barron's Index and includes only four-year institutions.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.2

EdChoice Participation Effects, by Length of Participation

		Years in Private School			
	Baseline	**1**	**2**	**3**	**4+**
College attendance					
Private school attendance	0.154***	0.048***	0.079***	0.097***	0.212***
	(0.009)	(0.017)	(0.019)	(0.022)	(0.011)
Mean of Y (public)	0.484	0.480	0.481	0.481	0.484
Matched treated	6,217	1,028	860	643	3,685
Unmatched treated	26	3	1	1	22
Used controls	571,559	369,519	343,051	264,707	481,134
Unused controls	36,794	228,620	253,952	331,572	119,805
Observations	614,596	599,170	597,864	596,923	604,646
Graduation with bachelor's degree					
Private school attendance	0.088***	0.004	0.012	0.021	0.159***
	(0.014)	(0.016)	(0.022)	(0.029)	(0.020)
Mean of Y (public)	0.099	0.097	0.097	0.097	0.099
Matched treated	1,385	269	236	112	764
Unmatched treated	265	9	22	20	218
Used controls	77,743	46,717	47,638	29,562	65,909
Unused controls	9,934	40,011	38,928	56,939	21,002
Observations	89,327	87,006	86,824	86,633	87,893

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: College attendance is measured within two years of expected high school graduation; college graduation is measured within six years of expected high school graduation. Results from propensity score matching account for low-income status, English language learner status, special education status, gender, race or ethnicity, test scores, year, grade, and school. Selectivity is based on the 2004 Barron's Index and includes only four-year institutions.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.3

Characteristics of EdChoice Participants, by Length of Program Participation

	College Attendance Sample					College Graduation Sample				
	All	**1**	**2**	**3**	**4+**	**All**	**1**	**2**	**3**	**4+**
Female	50.5	49.7	47.7	50.2	51.4	48.5	44.6	44.2	47.7	50.8
White	24.4	20.0	20.0	25.3	26.6	20.7	15.1	18.2	18.2	23.2
Black	62.4	68.5	68.2	62.3	59.5	68.7	70.9	73.6	72.0	66.4
Hispanic	6.5	4.5	6.0	5.7	7.3	5.2	5.4	4.7	3.8	5.4
Other	6.6	7.1	5.8	6.7	6.7	5.5	8.6	3.5	6.1	5.0
Ever low income	87.2	96.3	93.6	92.9	82.2	82.8	92.1	90.7	87.1	77.6
Ever special education	4.8	3.3	2.9	3.0	6.0	2.0	1.8	1.2	0.8	2.4
Ever English learner	12.1	16.1	16.6	17.2	9.1	8.8	15.1	11.6	7.6	6.5
Math test scores (std.)	-0.35	-0.60	-0.58	-0.47	-0.21	-0.35	-0.62	-0.60	-0.41	-0.20
Reading test scores (std.)	-0.24	-0.50	-0.47	-0.37	-0.09	-0.21	-0.52	-0.48	-0.28	-0.05
Observations	6,243	1,031	861	644	3,707	1,650	278	258	132	982

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: Low-income students are identified based on participation in the free and reduced-price lunch program (by the students or members of their households), being on public assistance, or having filed a Title I application. Math and reading scores are standardized by grade, subject, and year across all public school students in Ohio to have mean zero and unit standard deviation.

TABLE A.4

EdChoice Participation Effects, by Baseline Student Characteristics

		Gender		Race		Left Public School in Grade		Median of Baseline Reading Scores		Median of Baseline Math Scores		Share of Years in Poverty		
	Baseline	Female	Male	White	Black	3–5	6–8	Below	Above	Below	Above	< 25%	25–75%	> 75%
College attendance														
PS attendance	0.163*** (0.009)	0.148*** (0.010)	0.188*** (0.012)	0.130*** (0.014)	0.176*** (0.012)	0.134*** (0.012)	0.179*** (0.010)	0.190*** (0.010)	0.113*** (0.010)	0.182*** (0.010)	0.120*** (0.010)	0.101*** (0.014)	0.160*** (0.014)	0.170*** (0.011)
Mean of Y (public)	0.484	0.547	0.422	0.496	0.471	0.464	0.493	0.382	0.715	0.386	0.720	0.739	0.507	0.399
Matched treated	6,211	3,131	3,052	1,506	3,849	1,968	4,206	4,061	2,107	4,277	1,899	1,097	1,340	3,702
Unmatched treated	32	22	38	20	49	4	65	17	58	22	45	48	33	23
Used controls	571,557	246,246	240,353	166,309	259,345	153,857	381,740	371,948	115,121	384,354	110,257	52,240	72,718	329,358
Unused controls	36,796	55,206	66,548	90,489	21,822	39,601	33,155	49,693	71,591	45,934	67,808	65,823	32,833	55,381
Observations	614,596	304,605	309,991	258,324	285,065	195,430	419,166	425,719	188,877	434,587	180,009	119,208	106,924	388,464
Graduation with bachelor's degree														
PS attendance	0.100*** (0.013)	0.104*** (0.019)	0.098*** (0.017)	0.103*** (0.028)	0.102*** (0.017)	N/A N/A	N/A N/A	0.104*** (0.013)	0.106*** (0.026)	0.089*** (0.013)	0.107*** (0.025)	0.114*** (0.038)	0.082*** (0.027)	0.103*** (0.017)
Mean of Y (public)	0.099	0.121	0.077	0.131	0.075	N/A	N/A	0.045	0.250	0.048	0.269	0.270	0.085	0.056
Matched treated	1,394	631	691	273	897	N/A	N/A	883	446	934	386	230	267	779
Unmatched treated	256	169	159	68	237	N/A	N/A	144	177	176	154	124	114	136
Used controls	77,795	32,655	33,584	19,407	38,306	N/A	N/A	53,210	14,375	56,158	11,396	5,697	8,435	44,247
Unused controls	9,882	10,778	10,660	14,586	6,314	N/A	N/A	11,700	8,392	11,485	8,638	9,663	6,847	12,788
Observations	89,327	44,233	45,094	34,334	45,754	N/A	N/A	65,937	23,390	68,753	20,574	15,714	15,663	57,950

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: N/A = not applicable; PS = private school. College attendance is measured within two years of expected high school graduation; college graduation is measured within six years of expected high school graduation. Results from propensity score matching account for low-income status, English language learner status, special education status, gender, race or ethnicity, test scores, year, grade, and school. Selectivity is based on the 2004 Barron's Index and includes only four-year institutions. The results in this table are based on models without demographic controls.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.5

EdChoice Participation Effects, by Size of City or Town

		METROPOLITAN AREA DEFINITIONS					
		Metropolitan Statistical Areas			County Population		
	Baseline	Large metro (≥ 1 million people)	Medium metro (500,000 to 1 million people)	Other	≥ 400,000 people	200,000 to 400,000 people	< 200,000 people
College attendance							
Private school attendance	0.146*** (0.010)	0.144*** (0.012)	0.159*** (0.015)	0.126*** (0.025)	0.154*** (0.009)	0.111*** (0.030)	0.124*** (0.024)
Mean of Y (public)	0.483	0.500	0.481	0.425	0.487	0.477	0.470
Matched treated	5,655	2,638	1,962	1,015	4,251	838	529
Unmatched treated	28	7	24	37	24	41	0
Used controls	519,413	294,334	153,413	71,402	388,534	68,712	61,744
Unused controls	34,425	10,523	13,029	11,137	11,812	12,263	10,773
Observations	559,521	307,502	168,428	83,591	404,621	81,854	73,046
Graduation with bachelor's degree							
Private school attendance	0.076*** (0.015)	0.120*** (0.019)	0.106*** (0.029)	-0.019 (0.021)	0.102*** (0.018)	0.048* (0.025)	-0.067*** (0.023)
Mean of Y (public)	0.099	0.105	0.090	0.096	0.098	0.086	0.120
Matched treated	1,226	494	437	227	892	179	101
Unmatched treated	235	94	141	68	201	76	12
Used controls	71,676	39,920	21,779	7,297	56,203	7,100	3,686
Unused controls	9,089	3,344	4,241	4,184	3,600	4,622	5,554
Observations	82,226	43,852	26,598	11,776	60,896	11,977	9,353

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: College attendance is measured within two years of expected high school graduation; college graduation is measured within six years of expected high school graduation. Results are from propensity score matching that account for low-income status, English language learner status, special education status, gender, race or ethnicity, test scores, year, grade, and school. Selectivity is based on the 2004 Barron's Index and includes only four-year institutions. We define metro areas in two ways. One is based on metropolitan statistical areas where we define large metros as areas with more than 1 million residents (Butler, Brown, Clermont, Cuyahoga, Delaware, Fairfield, Franking, Geauga, Hamilton, Hocking, Lacke, Licking, Lorain, Madison, Medina, Morrow, Perry, Pickaway, Union, and Warren Counties), between 500,000 and 1 million residents (Carroll, Fulton, Greene, Lucas, Miami, Montgomery, Ottawa, Stark, Summit, Wood counties), and the reminder of the state. Another is based on counties by population with three groups: above 400,000 people (Cuyahoga, Frankling, Hamilton, Lucas, Montgomery and Summit Counties), between 200,000 and 400,000 people (Butler, Clermonth, Lake, Lorain, Mahoning, Stark, Trumbull, and Warren Counties), and below 200,000 people (i.e., the remainder of the state).

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.6

Robustness of EdChoice Participation Effects

		Modify Sample					Matching Specification							
	Baseline	Include SY 06–07	Limit to schools with PI score	Limit PI score to 77–82	Limit PI score to ≥ 80	Limit PI score to 80–82	Logit	Triangular kernel	Score FE	Nearest neighbor 1	Ex-post entropy balanced	Exact match limited	Exact match full	ATE
College attendance														
Private school attendance	0.154*** (0.009)	0.153*** (0.009)	0.149*** (0.011)	0.139*** (0.021)	0.142*** (0.015)	0.166*** (0.024)	0.151*** (0.009)	0.154*** (0.009)	0.150*** (0.009)	0.152*** (0.011)	0.157*** (0.008)	0.148*** (0.010)	0.112** (0.057)	0.093*** (0.019)
Mean of Y (public)	0.484	0.485	0.498	0.422	0.559	0.411	0.484	0.484	0.484	0.484	0.484	0.484	0.484	0.484
Matched treated	6,217	7,767	4,524	1,171	2,368	781	6,187	6,217	6,191	6,217	6,217	3,778	148	6,217
Unmatched treated	26	58	17	19	2,173	3,760	56	26	52	26	26	2465	6095	26
Used controls	571,564	694,829	441,891	77,405	259,361	37,640	571,558	571,564	562,522	6,043	571,564	33,493	176	571,564
Unused controls	36,794	31	34,856	18,909	27,585	4,034	36,800	36,794	45,836	602,315	36,794	574,865	608,182	36,794
Observations	614,601	702,685	481,288	97,504	291,487	46,215	614,601	614,601	614,601	614,601	614,601	614,601	614,601	614,601
Graduation with bachelor's degree														
Private school attendance	0.088*** (0.014)	0.092*** (0.010)	0.113*** (0.016)	0.137*** (0.037)	0.141*** (0.050)	0.198*** (0.069)	0.083*** (0.013)	0.088*** (0.014)	0.104*** (0.013)	0.081*** (0.017)	0.091*** (0.013)	0.107*** (0.017)	0.098 (0.066)	0.070*** (0.023)
Mean of Y (public)	0.099	0.100	0.106	0.095	0.166	0.091	0.099	0.099	0.099	0.099	0.099	0.099	0.099	0.099
Matched treated	1,385	2,264	917	208	235	78	1,330	1,385	1,343	1,385	1,385	715	44	1,385
Unmatched treated	265	425	83	17	765	922	320	265	307	265	265	935	1,606	265
Used controls	77,744	117,657	52,111	10,033	15,708	2,203	77,631	77,744	72,744	1,344	77,744	6,761	67	77,744
Unused controls	9,934	256	7,796	3,850	5,973	1,844	10,047	9,934	14,934	86,334	9,934	80,917	87,611	9,934
Observations	89,328	120,602	60,907	14,108	22,681	5,047	89,328	89,328	89,328	89,328	89,328	89,328	89,328	89,328

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: ATE = average treatment effect; FE = fixed effects; PI = performance index in state accountability system; SY = school year. College attendance is measured within two years of expected high school graduation; college graduation is measured within six years of expected high school graduation. Results from propensity score matching account for low-income status, English language learner status, special education status, gender, race or ethnicity, test scores, year, grade, and school. Selectivity is based on the 2004 Barron's Index and includes only four-year institutions.

TABLE A.7

Regression Discontinuity Estimates of Effects of EdChoice Program on Public School Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	First-Year Effects				Second-Year Effects				Third-Year Effects				College Effects	
	Reading score (std.)		Math score (std.)		Reading score (std.)		Math score (std.)		Reading score (std.)		Math score (std.)		Attendance / graduation	
Panel A1. College attendance sample with three-year test scores panel (first observed sample)														
Below cutoff	-0.037	-0.006	-0.038	-0.006	-0.011	0.011	0.026	0.053	0.013	0.037	0.035	0.066*	0.034*	0.020
	(0.051)	(0.033)	(0.055)	(0.039)	(0.049)	(0.030)	(0.058)	(0.037)	(0.050)	(0.029)	(0.059)	(0.038)	(0.020)	(0.016)
Control mean	-0.347		-0.372		-0.341		-0.375		-0.342		-0.373		0.462	
Observations	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067
Panel A2. College graduation sample with three-year test scores panel (first observed sample)														
Below cutoff	0.030	0.072	0.151	0.197*	0.054	0.101	0.115	0.155	-0.006	0.037	0.012	0.053	0.059***	0.057***
	(0.082)	(0.072)	(0.113)	(0.101)	(0.096)	(0.075)	(0.130)	(0.111)	(0.075)	(0.063)	(0.105)	(0.090)	(0.021)	(0.018)
Control mean	-0.276		-0.346		-0.230		-0.297		-0.221		-0.262		0.138	
Observations	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625
Panel B1. College attendance sample with three-year test scores panel (last observed sample)														
Below cutoff	-0.016	0.005	0.003	0.024	0.024	0.038	0.059	0.076**	0.048	0.062*	0.055	0.070*	0.037*	0.018
	(0.052)	(0.034)	(0.056)	(0.039)	(0.051)	(0.031)	(0.062)	(0.037)	(0.052)	(0.034)	(0.062)	(0.037)	(0.019)	(0.015)
Control mean	-0.341		-0.364		-0.342		-0.373		-0.336		-0.368		0.463	
Observations	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067	60,067
Panel B2. College graduation sample with three-year test scores panel (last observed sample)														
Below cutoff	0.030	0.070	0.152	0.195*	0.054	0.100	0.116	0.153	-0.007	0.035	0.012	0.051	0.059***	0.057***
	(0.082)	(0.073)	(0.113)	(0.101)	(0.096)	(0.075)	(0.129)	(0.110)	(0.075)	(0.062)	(0.105)	(0.089)	(0.021)	(0.018)
Control mean	-0.275		-0.346		-0.23		-0.297		-0.221		-0.262		0.139	
Observations	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625	6,625
Panel C1. College attendance sample with three-year test scores panel (pooled sample)														
Below cutoff	-0.016	0.009	-0.013	0.013	0.010	0.027	0.052	0.073**	0.030	0.049	0.045	0.068**	0.043**	0.025*
	(0.043)	(0.031)	(0.045)	(0.035)	(0.042)	(0.029)	(0.048)	(0.033)	(0.044)	(0.031)	(0.050)	(0.035)	(0.017)	(0.014)
Control mean	-0.37		-0.394		-0.37		-0.401		-0.369		-0.4		0.454	
Observations	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735	82,735
Panel C2. College graduation sample with three-year test scores panel (pooled sample)														
Below cutoff	0.031	0.071	0.152	0.195*	0.055	0.101	0.117	0.155	-0.006	0.036	0.012	0.050	0.059***	0.057***
	(0.082)	(0.072)	(0.113)	(0.100)	(0.096)	(0.074)	(0.129)	(0.110)	(0.075)	(0.063)	(0.105)	(0.090)	(0.021)	(0.018)
Control mean	-0.278		-0.348		-0.232		-0.3		-0.222		-0.264		0.138	
Observations	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646	6,646
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Source: Authors' calculations from Ohio Department of Education and Workforce data.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.8

Results of Balance Tests for Estimated Effects of EdChoice Program on Public School Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Background Characteristics					Lagged Test Scores (t-1)	
	Female	White	Black	Poor	Non–special education	Reading	Math
Panel A1. College attendance sample with three-year test scores panel (first observed sample)							
Below cutoff	-0.012	-0.087	0.101	-0.019	-0.007	-0.063	-0.024
	(0.010)	(0.072)	(0.070)	(0.039)	(0.012)	(0.056)	(0.060)
Control mean	0.494	0.590	0.296	0.701	0.845	-0.475	-0.502
Observations	60,067	60,067	60,067	60,067	60,067	20,394	20,394
Panel A2. College graduation sample with three-year test scores panel (first observed sample)							
Below cutoff	-0.028	-0.080	0.102	-0.039	-0.032	0.274	0.152
	(0.022)	(0.115)	(0.107)	(0.057)	(0.022)	(0.357)	(0.344)
Control mean	0.498	0.732	0.205	0.575	0.864	-0.970	-0.895
Observations	6,625	6,625	6,625	6,625	6,625	80	80
Panel B1. College attendance sample with three-year test scores panel (last observed sample)							
Below cutoff	-0.011	-0.082	0.086	-0.025	-0.004	-0.042	0.005
	(0.010)	(0.072)	(0.069)	(0.036)	(0.011)	(0.047)	(0.051)
Control mean	0.495	0.593	0.294	0.704	0.845	-0.451	-0.477
Observations	60,067	60,067	60,067	60,067	60,067	31,536	31,536
Panel B2. College graduation sample with three-year test scores panel (last observed sample)							
Below cutoff	-0.026	-0.077	0.098	-0.039	-0.034	0.269	-0.021
	(0.023)	(0.115)	(0.107)	(0.057)	(0.022)	(0.314)	(0.311)
Control mean	0.498	0.732	0.206	0.575	0.864	-0.994	-0.889
Observations	6,625	6,625	6,625	6,625	6,625	101	101
Panel C1. College attendance sample with three-year test scores panel (pooled sample)							
Below cutoff	-0.007	-0.091	0.101	-0.022	-0.003	-0.034	-0.009
	(0.009)	(0.065)	(0.064)	(0.034)	(0.011)	(0.039)	(0.041)
Control mean	0.496	0.560	0.320	0.731	0.844	-0.452	-0.480
Observations	82,735	82,735	82,735	82,735	82,735	43,062	43,062
Panel C2. College graduation sample with three-year test scores panel (pooled sample)							
Below cutoff	-0.025	-0.078	0.099	-0.039	-0.033	0.269	-0.021
	(0.022)	(0.115)	(0.107)	(0.057)	(0.022)	(0.314)	(0.311)
Control mean	0.498	0.731	0.206	0.576	0.864	-0.994	-0.889
Observations	6,646	6,646	6,646	6,646	6,646	101	101

Source: Authors' calculations from Ohio Department of Education and Workforce data.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.9

Heterogeneity of Estimated Effects of the EdChoice Program on Public School Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Gender		Race		Low Income		Special Education		Grades	
	Female	Male	White	Black	Yes	No	No	Yes	3–5	6–8
Panel A. College attendance (cross-section sample)										
Below cutoff	0.031*	0.040**	0.017	0.042*	0.035**	0.022	0.030*	0.056***	0.030*	0.035
	(0.018)	(0.018)	(0.020)	(0.021)	(0.017)	(0.020)	(0.018)	(0.020)	(0.018)	(0.022)
Control mean	0.510	0.382	0.423	0.479	0.394	0.598	0.485	0.246	0.450	0.443
Observations	84,366	86,030	81,598	67,954	130,770	39,626	141,285	29,111	57,574	112,822
Panel B. College graduation (cross-section sample)										
Below cutoff	0.027	0.010	0.003	0.060***	0.016	-0.002	0.023	0.007		
	(0.020)	(0.012)	(0.018)	(0.016)	(0.010)	(0.023)	(0.016)	(0.008)		
Control mean	0.148	0.088	0.132	0.095	0.066	0.211	0.134	0.025		
Observations	16,375	17,106	17,799	12,413	22,529	10,952	27,898	5,583	N/A	N/A
Panel C. College attendance (test scores college sample)										
Below cutoff	0.040**	0.041**	0.011	0.067***	0.041**	0.025	0.034*	0.074***		
	(0.018)	(0.018)	(0.022)	(0.023)	(0.017)	(0.022)	(0.018)	(0.022)		
Control mean	0.517	0.393	0.441	0.473	0.397	0.610	0.491	0.256		
Observations	41,148	41,587	42,681	29,660	62,152	20,583	69,389	13,346	N/A	N/A
Panel D. College graduation (test scores college sample)										
Below cutoff	0.072**	0.048**	0.045*	0.113***	0.047***	0.057	0.072***	0.005		
	(0.028)	(0.022)	(0.026)	(0.025)	(0.017)	(0.037)	(0.024)	(0.019)		
Control mean	0.169	0.107	0.148	0.103	0.072	0.228	0.154	0.034		
Observations	3,291	3,355	4,558	1,566	4,036	2,610	5,644	1,002	N/A	N/A

Source: Authors' calculations from Ohio Department of Education and Workforce data.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.10

Robustness of Estimated Effects of the EdChoice Program on Public School Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Linear			Nonparametric		Donut		
	Baseline	Quadratic	Nonpanel	RdRobust	RdHonest	Exclude +/- 0.25	Exclude +/- 0.50	Exclude +/- 1.00
Panel A. College attendance sample								
Below cutoff	0.025*	0.038*	0.017	0.018	0.049***	0.035*	0.026	-0.008
	(0.014)	(0.021)	(0.012)	(0.012)	(0.008)	(0.015)	(0.017)	(0.031)
Bandwidth	3	3	3	6.7	2.6	3	3	3
Observations	82,735	82,735	170,396	196,328	61,256	75,895	69,972	50,565
Panel B. College graduation sample								
Below cutoff	0.057***	0.043*	0.012	0.040***	0.055***	0.060**	0.060*	0.126**
	(0.018)	(0.024)	(0.011)	(0.015)	(0.017)	(0.024)	(0.034)	(0.060)
Bandwidth	3	3	3	6.0	2.6	3	3	3
Observations	6,646	6,646	33,481	16,300	5,077	5,938	5,285	3,920
Bandwidth imposed	Yes	Yes	Yes	No	No	Yes	Yes	Yes

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: Results correspond to the pooled sample with the controls specification reported in table A.7.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE A.11

Assessing Selection on Observables

	(1)	(2)	(3)	(4)
	College attendance		College graduation	
kmatch estimate	0.163***	0.154***	0.100***	0.088***
	(0.009)	(0.009)	(0.013)	(0.014)
OLS estimate	0.169***	0.158***	0.105***	0.103***
	(0.007)	(0.007)	(0.012)	(0.012)
psmatch2 estimate	0.169***	0.152***	0.114***	0.089***
	(0.009)	(0.009)	(0.016)	(0.016)
Oster delta	1.14	0.94	0.46	0.48
Rosenbaum bounds gamma	1.95	1.80	2.00	1.55
Becker-Caliendo gamma	1.95	1.80	1.85	1.70
Additional controls	No	Yes	No	Yes

Source: Authors' calculations from Ohio Department of Education and Workforce data.

Notes: OLS = ordinary least squares. Columns 1 and 2 present estimates for college attendance, while columns 3 and 4 present estimates for college graduation. Each row is based on a different estimator: our preferred kmatch in row 1, simple OLS in row 2, and psmatch2 estimate in row 3. Odd-numbered columns do not include demographic controls, while even-numbered columns control for demographic characteristics (indicator for being low income, indicator for non–English language learner student, indicator for non–special education student, indicator for female student, and racial or ethnic indicators). Subsequent rows report the Oster delta, the Rosenbaum bounds gamma, and the Becker-Caliendo gamma. See Sascha O. Becker and Marco Caliendo, "Sensitivity Analysis for Average Treatment Effects," *Stata Journal* 7 (2007): 71–83, https://doi.org/10.1177/1536867X0700700104; Emily Oster, "Unobservable Selection and Coefficient Stability: Theory and Evidence," *Journal of Business and Economic Statistics* 37, no. 2 (2019): 187–204, https://doi.org/10.1080/07350015.2016.1227711; and Rosenbaum, Paul R. Rosenbaum, *Observational Studies* (New York: Springer, 2002). Standard errors are clustered at the baseline public school level. Average treatment effects on the treated are reported.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Notes

1 EdChoice, "1 Million Students Now Using Private School Choice," press release, June 18, 2024, https://www.edchoice.org/media/1-million-students-now-using-private-school-choice/.

2 There is short- and long-run evidence on city-specific programs in Milwaukee, New York City, and Washington, DC, but only the studies of Milwaukee used state tests rather than norm-referenced tests (Chingos et al. 2019; Chingos and Peterson 2015).

3 See Figlio and Karbownik (2016) for a more detailed history of the EdChoice program and citations to original sources.

4 All K–12 students who reside in the Cleveland Metropolitan School District are eligible for a voucher, and students must take the Cleveland scholarship if they are deemed eligible, rather than Ohio's statewide EdChoice scholarship. For this reason, we exclude Cleveland from the analysis. See "Cleveland Scholarship Program," EdChoice, accessed March 26, 2025, https://www.edchoice.org/school-choice/programs/ohio-cleveland-scholarship-program/.

5 See also "List of Approved Assessments," Ohio Department of Education and Workforce, last updated March 17, 2025, https://education.ohio.gov/topics/list-of-approved-assessments.

6 Specifically, we use test scores in mathematics and reading, school year, school grade, school identifier—as well as indicators for being in poverty, not being an English language learner, not receiving special education services, female, and four racial indicators (white, Black, Hispanic, other)—to construct propensity score. All these covariates are measured before enrollment in private school. In alternative specifications, we also match exactly on school year, school grade, school identifier, and all other variables used to construct the propensity score.

7 We include 671 and 293 schools (clusters) in college attendance and graduation analyses; thus, our analysis does not suffer from too few clusters.

8 We identify bachelor's degrees based on degree titles in the National Student Clearinghouse data. We count students who received higher degrees (e.g., degrees such as a master's for which a bachelor's is a prerequisite) as having earned a bachelor's degree.

9 Enrollment by sector captures any enrollment within two years of expected high school graduation, so a student can enroll in multiple types of colleges (e.g., transferring from a two-year to a four-year college and vice versa). We define college selectivity based on the 2004 Barron's Selectivity Index: most selective colleges have index values of 1 or 2; highly selective colleges have index 1, 2, or 3l and selective colleges have index 1, 2, 3, or 4. Schools with a Barron's Index of less than 4 are classified as nonselective.

10 Sample sizes for racial or ethnic groups other than Black and white students are too small to produce meaningful results.

11 Kendall Crawford and Zack Carreon, "School Voucher Use Has Surged in Ohio. But Private School Enrollment Isn't Rising with It," WOSU, June 17, 2024, https://www.wosu.org/2024-06-17/school-voucher-use-has-surged-in-ohio-but-private-school-enrollment-isnt-rising-with-it.

12 John Kristof, Alli Aldis, and Colyn Ritter, "Who Is Using School Choice? It's a Harder Question to Answer Than You Might Think," EdChoice, August 27, 2024, https://www.edchoice.org/engage/who-is-using-school-choice-its-a-harder-question-to-answer-than-you-might-think/.

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