RESEARCH REPORT

The Effect of the DC School Voucher Program on College Enrollment

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Executive Summary

Washington, DC's, Opportunity Scholarship Program (OSP), the only federally funded voucher program in the United States, has provided private-school scholarships to low-income students in DC since 2004. The program is small, but it has received significant attention in national debates over private school choice and has been the subject of rigorous evaluations mandated by Congress.

This study is the first look at OSP participants' college enrollment outcomes, measured using administrative data from the National Student Clearinghouse. I measure the effect of the OSP by comparing the college enrollment rates of students who were offered a scholarship in lotteries held in 2004 and 2005 with those of students who applied but did not win a scholarship.

I find that students who won scholarships to attend private schools were not significantly more or less likely to enroll in college than students who did not. These are the first experimental estimates of the impact of a publicly funded private school choice program on college enrollment.

The Effect of the DC School Voucher Program on College Enrollment

The number of publicly funded private-school scholarships awarded to US children has increased rapidly, from less than 150,000 in 2004 to more than 400,000 in 2017 (EdChoice 2017). School voucher programs are almost exclusively enacted by state and local governments. The only federal program is Washington, DC's, Opportunity Scholarship Program (OSP), which was authorized by Congress in 2004. Congress specifically cited concerns about the DC public school system, including lackluster performance on the National Assessment of Educational Progress, when it created OSP.[1]

The OSP has been a political football in national debates over private school choice. The program was closed to new participants under the Obama administration and a Democratic Congress in 2009, and then reauthorized in 2011 and 2017 by a Republican-led Congresses as part of appropriations bills. The OSP is currently part of legislation that provides $45 million to DC schools, with $15 million going to the voucher program and $30 million split between the traditional school district and public charter schools. This three-sector arrangement dates to the creation of the program, and the notion that investments should be made in traditional public and charter schools while giving low-income students immediate alternatives was instrumental in gaining the support of local officials (Hsu 2004).

The DC program is one of a small number of private school choice programs that have now been in place long enough that student outcomes can be tracked beyond high school. An earlier evaluation of the OSP found that it increased the rate at which participants graduated from high school, but this analysis was only possible for a subset of participants and relied on information reported by parents (Wolf et al. 2013).

More broadly, existing research on private school choice programs is limited but points toward more positive effects on educational attainment (amount of schooling completed) than achievement (scores on standardized tests). The handful of studies examining the longer-term effects of private school choice programs have found positive effects on college enrollment rates among participants in Milwaukee (Cowen et al. 2013) and Florida (Chingos and Kuehn 2017) and among subgroups of participants in New York City (Chingos and Peterson 2015).[2]

In this study, I examine the college enrollment rates of students who applied for a voucher during the first two years of the program (2004 and 2005) and who are now of college-going age. Like the earlier studies, I take advantage of lotteries to ensure that the results are not biased by selection into

the program. This is the first study to examine the impact of a publicly funded private school choice program on college enrollment using a randomized experiment.

I find that the program had no detectable impact, positive or negative, on the rates at which participating students enrolled in college or the types of colleges they attended. However, the results are not precise enough to rule out modest positive or negative effects.

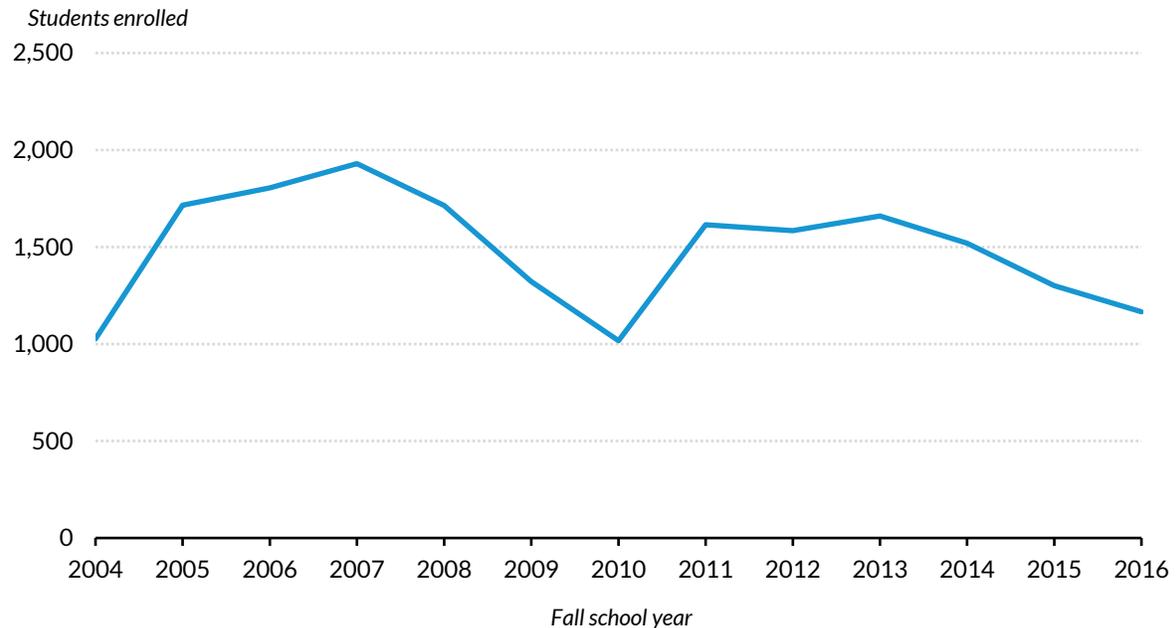# Background on the Opportunity Scholarship Program

The DC Opportunity Scholarship Program, created by an act of Congress in January 2004, provides scholarships to low-income families (defined as those making no more than 185 percent of the federal poverty level) to attend private schools. Scholarships are only available to DC residents attending participating DC private schools. Participating schools must agree to requirements including nondiscrimination in admissions, fiscal accountability, and the provision of data and information for evaluation purposes (Wolf et al. 2005).

Figure 1 shows that the program has enrolled between 1,000 and 2,000 students each year since its inception in 2004–05, with a peak of 1,930 in 2007–08, and 1,166 enrolled in the most recent year for which data are available (2016–17). Scholarship amounts were initially capped at $7,500 (about $9,700 in 2017 dollars); the maximum is now $8,653 for elementary and middle school and $12,981 for high school.[3]

Enrollment in the program is small relative to public school enrollment in DC. OSP enrollment has never exceeded 3 percent of total enrollment (district, public charter, and OSP) and is currently closer to 2 percent.[4] This largely reflects the fact that program funding can only accommodate a limited number of students (roughly 1,200 scholarships per year in recent years).[5]

FIGURE 1

Enrollment in DC Opportunity Scholarship Program

2004–16

Students enrolled

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Source: "School Choice: District of Columbia, Opportunity Scholarship Program," EdChoice, accessed January 25, 2018, https://www.edchoice.org/school-choice/programs/district-of-columbia-opportunity-scholarship-program/.

The law that created the OSP mandated that scholarships be allocated by lottery if there were more applicant families than funding could accommodate or school spaces available. Two Congressionally mandated evaluation efforts have used these lotteries to compare the outcomes of students awarded scholarships to those who applied but did not win the lottery.

The first evaluation tracked 2,308 students who applied for a scholarship beginning in 2004 or 2005 through 2008–09 (Wolf et al. 2010). Three years after application, students who were offered a scholarship scored significantly higher on reading tests than those who were not (Wolf et al. 2013). This difference persisted for at least four years after application but was only statistically significant at the 90 percent level.

Estimated positive impacts on reading scores were smaller for students who initially attended schools designated as in need of improvement (SINI), whom Congress gave priority in the lottery. But the difference in impacts (for students who attended SINI versus not SINI schools at baseline) was not statistically significant. There was no significant impact on math scores.

Parents reported higher levels of satisfaction and perceptions of school safety if their children won the scholarship lottery, but there were no significant differences in how students perceived the quality or safety of their school (Kisida and Wolf 2015; Wolf et al. 2013).

Among the subsample of about 500 applicants who were expected to graduate from high school by 2008–09, Wolf and colleagues (2010) report that the offer of a scholarship increased high school graduation rates (as reported by parents) by 12 percentage points, a 17 percent increase relative to the control group. The estimated impact of using a scholarship was 21 percentage points.

The second Congressionally mandated evaluation effort is tracking the outcomes of 1,771 participants from the 2012, 2013, and 2014 lotteries. Data from these students' first school year following the lottery indicate that receiving a scholarship reduced test scores in reading and math, although only the math effects are statistically significant (Dynarski et al. 2017). Impacts on parent and student satisfaction were generally positive but statistically insignificant, whereas impacts on parents' (but not students') perceptions of school safety were positive and significant.

These overall findings mask significant heterogeneity across students based on the type of school they were initially attending and their grade level at the time of application. Students attending SINI schools experienced negligible impacts on test scores, whereas students attending other schools experienced large negative impacts. Students in elementary school experienced large negative impacts, whereas students in middle or high school experienced modest positive impacts (although the latter were not statistically significant).

A potentially important limitation of existing research on the OSP is that it has relied on voluntary participation by students and families in data collection efforts, including standardized testing and surveys. Wolf and colleagues (2010) tested 70 percent of eligible students at least four years after application and Dynarski and colleagues (2017) tested about 75 percent of students in their study of students' first post-lottery year. The parent survey used to measure high school graduation in Wolf and colleagues (2010) had a response rate of 63 percent.

These are high response rates, but they still require the investigators to assume that nonrespondents do not differ in unmeasured ways between the treatment and control groups. This assumption cannot be tested directly, and research indicates that nonresponse can bias estimates of treatment effects in randomized evaluations of school vouchers, and that commonly used reweighting techniques may not mitigate this bias (Chingos and Peterson 2015).

Data and Methods

The present study builds on existing work on the OSP by using administrative records to measure the college enrollment patterns of participants in the first two lotteries. I track the college enrollment outcomes of a subset of 1,594 students who applied for a scholarship in 2004 or 2005 and are now old enough to have potentially graduated from high school and enrolled in college.

Urban Institute researchers worked with the current OSP administrator, Serving Our Children (SOC), to reconstruct baseline files from the original lottery applications that the Washington Scholarship Fund (the original OSP administrator) used in 2004 and 2005. These applications did not include the rich baseline surveys (or measures of student achievement) that the first Congressionally mandated evaluation used. It did include basic demographic information, including student race and ethnicity, gender, age, whether the parents or guardians were married, whether parents or guardians owned their home, and family income. Urban and SOC put in place strict procedures to ensure that applicants' personally identifiable information was never released to anyone outside SOC.

I used these data to identify students who met the same criteria used to construct the study sample for the original evaluation. These were students who were attending a public school at baseline (or were entering kindergarten) and who were subject to the lottery because they applied for a grade that was oversubscribed and were not part of a priority group guaranteed to win a scholarship.[6]

I reconstructed baseline records for 2,282 students who met these requirements, which is 99 percent of the evaluation sample size of 2,308 reported in Wolf and colleagues (2010). I grouped students based on the year of application, grade they were entering (as reported on the application), and whether they were attending a SINI school at baseline.[7] These groupings affected the likelihood that the student won the lottery, which I adjust for throughout the analysis using the same base weights used by Wolf and colleagues (2010, table A.4).

For this study, I limit the sample of students to those who can be observed for at least two years following their expected graduation from high school (assuming on-time progression from the grade they were entering at the time of their OSP application). This two-year window permits the observation of college enrollment outcomes of students who repeated a grade before high school graduation or who did not enroll in college immediately afterward.

A total of 1,601 students met this criterion, and the complete data on name and date of birth needed to match to administrative records on college enrollment were available for 1,594 (99.6 percent) of these students. The students included in this study were entering grades 3–12 at the time of

application; the 681 students who were entering grades K–2 in 2004 and 2005 are not yet old enough to be observed for at least two years after expected high school graduation.

These records were matched to administrative data on college enrollment maintained by the National Student Clearinghouse (NSC), a nonprofit organization that collects student-level enrollment and degree data from colleges and universities making up 97 percent of enrollment in the US. Coverage rates exceed 95 percent in all sectors of higher education (as of fall 2016), except the for-profit sector, where the coverage rate is 71 percent. These coverage rates have increased over time; in fall 2006, the coverage rate was 89 percent.[8]

NSC data have been used extensively in education research (see, e.g., Chingos and Peterson 2015; Dynarski, Hemelt, and Hyman 2015). These data are nearly universal and do not require contacting students to obtain information. Administrative data from NSC are thus more complete and accurate that can typically be obtained by survey.

I use the NSC data to measure whether students in the OSP lotteries enrolled in college within a given number of years after expected high school graduation. I measure college enrollment overall as well as by sector (two- versus four-year and public versus private) and whether the student ever enrolled full time.[9] The NSC data also include information on degree receipt, but too few students are old enough to have potentially received a college degree, so I do not use this information in the present study.

I report the effect of winning the OSP lottery as the difference between the average outcome of the treatment group (won the lottery) and the control group (lost the lottery). All differences are weighted as described above and are estimated as marginal effects from probit regressions with standard errors clustered by family. I report results with and without control variables derived from the scholarship application.

These estimates are "intent to treat" (ITT) in that they capture the effect of being offered a scholarship, when in fact many students who were offered a scholarship did not use one. Among all students who won the lottery, 70 percent used a scholarship for at least one year.[10] I only report ITT estimates throughout this study, but the effect of using a scholarship for at least one year can be calculated by dividing the ITT estimates by 0.7.[11]

Student Characteristics and Program Participation

Given random assignment to treatment, we would expect the treatment and control groups to be similar on observed characteristics. This is generally the case for the limited set of student characteristics captured on the scholarship application (table 1). A joint significant test fails to reject the null of no difference with p = 0.45.

The descriptive statistics also show that OSP applicants are a very disadvantaged group, with an average family income of about $18,000 and less than 20 percent from married two-parent families. Almost all are black or Hispanic, and about one quarter were attending a public charter school at the time of application.[12]

TABLE 1

Descriptive Statistics

|	Control	Treatment	Difference	*p*-value of difference
Race and ethnicity				
Black	88%	87%	-0.4%	0.82
Hispanic	8%	11%	2.7%	0.07
Not black or Hispanic	5%	3%	-2.2%	0.03
Female	51%	49%	-2.0%	0.43
Parents/guardians married	19%	19%	-0.3%	0.86
Parent/guardian owns home	14%	14%	0.7%	0.70
Age	12.3	12.4	0.1	0.54
Income missing	19%	18%	-0.1%	0.96
Family income	$18,640	$17,860	-$780	0.21
Charter at baseline	26%	26%	-0.3%	0.87
Observations (unweighted)	648	946		

Source: Author's calculations from OSP application data.
Notes: Summary statistics are weighted by baseline weights. A joint significance test of all of the listed variables fails to reject the null of no difference with p = 0.45.

Serving Our Children also provided data on which school years each treatment-group student used their scholarship through 2016–17. As noted, 27 percent of students who were offered a scholarship never used it. Figure 2 shows that 13, 14, and 15 percent of students used their scholarship for one, two, and three years, respectively. About 30 percent of students used a scholarship for four or more years.

FIGURE 2

Years of Participation in OSP

Treatment-group students

Years of OSP participation
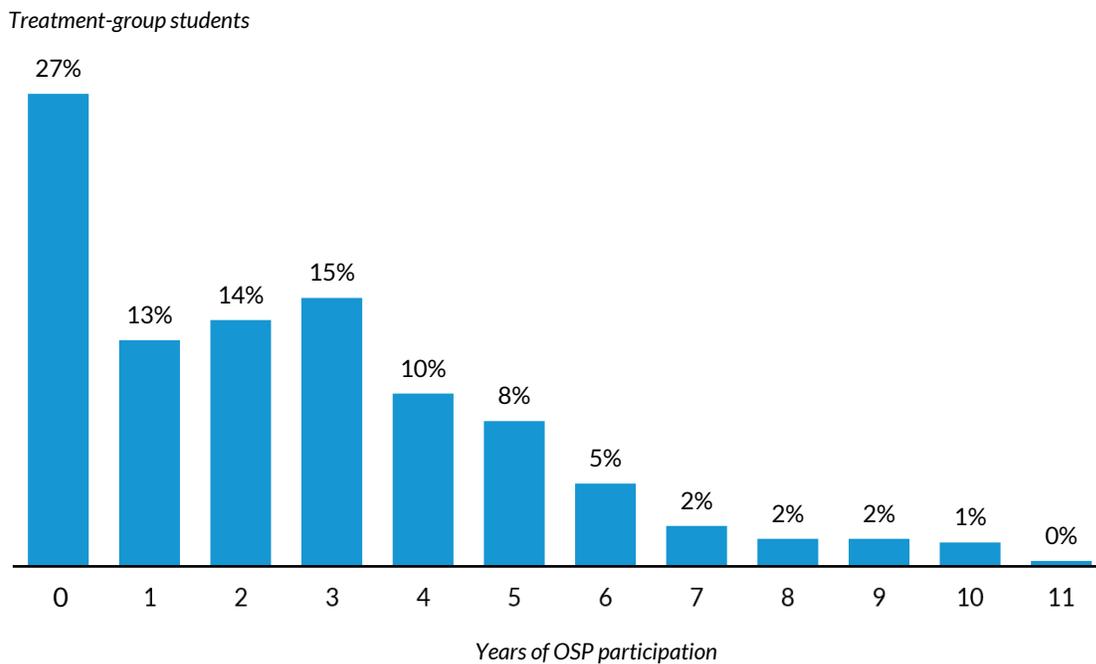
Source: Author's calculations from OSP administrative data.

Students who entered the OSP in later grades had fewer remaining years in school in which they could have participated in the program. The average lottery winner used the scholarship for 39 percent of their remaining years in K–12 schools. The corresponding percentage for students who used a scholarship for at least one year is 55 percent.[13]

Wolf and colleagues (2010) list several reasons students did not use their scholarship or stopped participating, including a lack of space at a desired private school, a lack of services for students with special needs, admission to a preferred charter school, a child's private school being converted into a public charter school, or losing eligibility through a rise in family income or moving out of DC.
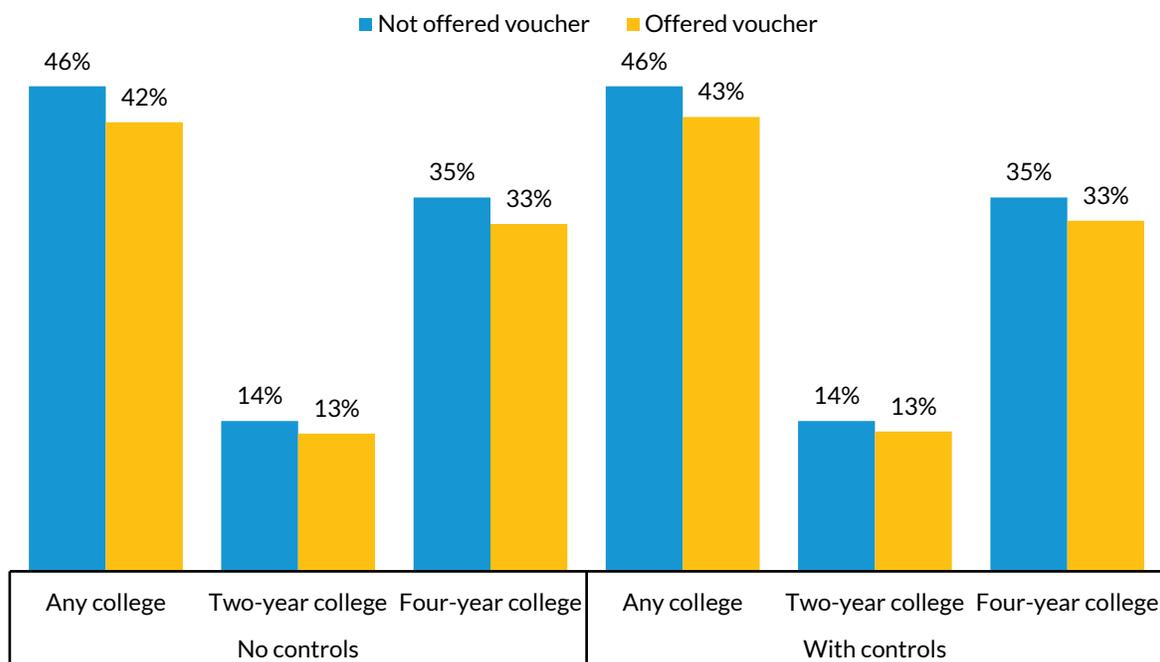
Results

My main findings are reported in figure 3 and detailed regression results are provided in table A.1. Overall, students offered a scholarship were somewhat less likely to enroll in college within two years of expected graduation from high school: 42 percent did compared with 46 percent of applicants who

lost the lottery. None of these differences are statistically distinguishable from zero at conventional levels.

This pattern holds for both two- and four-year colleges and for four-year public and four-year private colleges (table A.1). Adding control variables has little impact on the results, as would be expected given random assignment.

Effect of OSP Voucher Offer on College Enrollment within Two Years of Expected High School Graduation

Source: Author's calculations from linked OSP and NSC data.
Note: For detailed regression results, see table A.1.

I observe a subset of students for more than two years after expected high school graduation, with fewer students observed for longer periods of time. Figure 4 reports the effect of winning the OSP lottery on college enrollment within two, three, four, and five years of expected high school graduation. None of the estimates are statistically distinguishable from zero, but the modest negative estimate for the full sample becomes a small positive estimate for those observed for at least five years.

This result suggests that the negative estimate in the short run merely reflects a delay in when some students enter college, perhaps because private schools are more likely to hold them back for multiple grades or their entrance into college is more likely to be delayed for other reasons.

The change in results may also be in part because of the change in the sample of students examined. I find slightly less negative two-year impacts for students observed for three through five years than for students observed for two years, mostly because of differences in the control group's enrollment rate (table A.2).[14]

FIGURE 4

Effect of OSP Voucher Offer on College Enrollment within 1–5 Years of High School Graduation

Effect of scholarship offer (with 95 percent confidence intervals)

Enrollment within X years of expected high school graduation

Source: Author's calculations from linked OSP and NSC data.
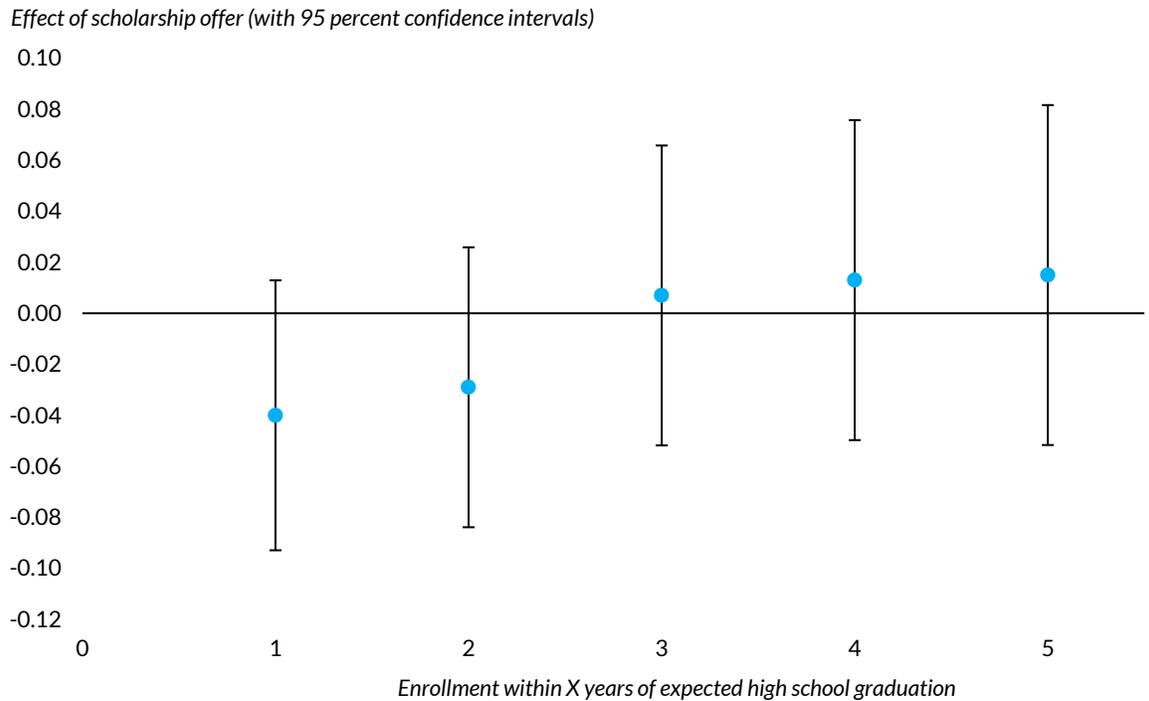**Note:** For detailed regression results, see table A.2 specifications with maximum possible sample size for each outcome.

It is important to interpret these results in light of the imprecision of the estimates. For example, the three-year result reported in figure 4 cannot rule out a zero effect at the 95 percent confidence level, but it also cannot rule out a negative effect of 5 percentage points or a positive effect of 6 percentage points. Such effects would roughly correspond to 10 percent changes relative to the control group's college enrollment rate of 46 percent.

These results reflect the outcomes of everyone in the treatment group, regardless of how long they used a scholarship to attend private school or if they used it at all. This is necessary in the context of a randomized experiment with limited baseline data, as it is not possible to randomize whether a student uses a scholarship that is offered to them and how long they use it. Controlling for a rich set of baseline data on student characteristics (including test scores), the association between scholarship usage and college enrollment can be informative (see, e.g., Chingos and Kuehn 2017). But this is not the case in this context given the limited data available from the application.[15]

Finally, I estimate the effect of winning a scholarship for subgroups of students, defined based on demographic characteristics and the types of schools they attended at the time of application. There is no compelling evidence of effect heterogeneity, although the effects are often imprecisely estimated given the reduced sample size (table A.3). None of the estimated effects are significantly different from zero. For example, I find no evidence of more positive effects among students from SINI public schools than students from other schools.

Conclusions

This study provides the first experimental evidence on the effect of a publicly funded private school choice program on college enrollment. I find that students who won a scholarship to attend a private school in Washington, DC, enrolled in similar types of colleges at similar rates as students who were not offered a private school voucher.

No significant difference between scholarship winners and losers still suggests a possible productivity benefit of the private sector, as the maximum voucher amount ($9,700 in 2017 dollars) was less than half of average per-pupil spending in public schools (roughly $20,000 in 2017 dollars in 2004–05, and more in later years).[16] Additionally, some studies find that competition from private school choice programs boosts outcomes in public schools (e.g., Figlio and Hart 2014), but this was unlikely in the DC context because of the small size of the program (Wolf et al. 2010).[17]

The lack of a positive impact on college enrollment may be surprising in light of prior evidence indicating a large positive effect of the program on high school graduation rates (Wolf et al. 2010). One possible explanation is that the earlier study only included students who would have graduated by 2008–09, but when I limit the sample to approximately the same group of students I find similarly insignificant effects on college enrollment.[18]

I am not able to test other possible explanations for the divergence in findings, such as differential graduation standards in public versus private high schools or bias resulting from survey nonresponse.[19] And both findings are somewhat imprecisely estimated.

This study has a number of limitations besides imprecision in the estimates. A necessary drawback of examining longer-term outcomes is that they reflect the experiences of earlier cohorts of students. Results for more-recent participants (which will not be measurable for many years) may differ because of changes in the voucher program or changes in the public-school system. For example, the performance of DC's public school system on the National Assessment of Educational Progress has increased markedly in recent years (even controlling for changes in demographics).[20] This may be why earlier evaluations of OSP found positive impacts on test scores (Wolf et al. 2013) but a more recent evaluation found negative one-year effects (Dynarski et al. 2017).

The large and growing charter sector also complicates the interpretation of the results, as there are many students in both the treatment and control groups who attend public schools of choice. The share of DC public school students enrolled in charters has grown from less than 20 percent when OSP was launched to more than 40 percent today.[21] In the most recent OSP evaluation, 42 percent of the control group was enrolled in a charter school compared with 15 percent of the treatment group (Dynarski et al. 2017).

These limitations aside, this study shows that students who won private-school scholarships from the nation's only federally funded school voucher program were not significantly more or less likely to enroll in college than students who did not win a scholarship. But given the significant changes to the DC education system over the period since the students in this study applied for scholarships in 2004 and 2005, it will be important to continue to track the outcomes of more recent program participants in K–12 schools and beyond.

Appendix

Effect of Scholarship Offer on College Enrollment within Two Years of Expected High School Graduation

	Any college	Four-year college	Two-year college	Full-time enrollment	Four-year public college	Four-year private college
Without controls	-0.034	-0.025	-0.012	-0.030	-0.026	-0.013
	(0.028)	(0.026)	(0.019)	(0.027)	(0.024)	(0.019)
With controls	-0.029	-0.022	-0.010	-0.023	-0.022	-0.012
	(0.028)	(0.026)	(0.019)	(0.027)	(0.023)	(0.018)
Control mean	0.458	0.353	0.142	0.353	0.249	0.133
Observations	1,594	1,594	1,594	1,594	1,594	1,594

Notes: Robust standard errors adjusted for clustering on family appear in parentheses. Treatment estimates are marginal effects from probit regressions. Controls include race and ethnicity (black, Hispanic, with neither or missing as omitted category), gender, whether parents of guardians were married, whether parents or guardians owned home, age, natural log of family income (with missing coded as zero), and whether income was missing. All models are weighted using baseline weights.
** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

TABLE A.2

Effect of Scholarship Offer on College Enrollment within One to Five Years of Expected High School Graduation

	Enrollment at any colleges within				
	1 year	2 years	3 years	4 years	5 years
Expected high school graduation 2005–15 (N = 1,594)	-0.040 (0.027) 0.395	-0.029 (0.028) 0.458			
Expected high school graduation 2005–14 (N = 1,421)	-0.022 (0.028) 0.371	-0.015 (0.030) 0.436	0.007 (0.030) 0.46		
Expected high school graduation 2005–13 (N = 1,243)	-0.022 (0.031) 0.381	-0.011 (0.032) 0.441	0.008 (0.032) 0.469	0.013 (0.032) 0.484	
Expected high school graduation 2005–12 (N = 1,105)	-0.017 (0.032) 0.366	-0.012 (0.034) 0.433	0.006 (0.034) 0.463	0.012 (0.034) 0.478	0.015 (0.034) 0.492

Notes: For each regression, I report the coefficient (marginal effect from probit regression), standard error (adjusted for clustering on family), and control mean. All models include controls listed in notes to table A.1 and are weighted using baseline weights.
** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Subgroup Analysis, Effect of Scholarship Offer on Enrollment at Any College within Two Years of Expected High School Graduation

	No controls		With controls			
	Estimate	Std error	Estimate	Std error	C mean	Obs.
Black	-0.044	(0.030)	-0.038	(0.030)	0.460	1,393
Hispanic	-0.004	(0.088)	-0.012	(0.093)	0.420	160
Not black or Hispanic	0.164	(0.146)	0.092	(0.164)	0.483	60
Female	-0.034	(0.040)	-0.037	(0.040)	0.508	789
Male	-0.030	(0.038)	-0.027	(0.038)	0.407	805
School ever SINI	-0.055	(0.039)	-0.038	(0.039)	0.479	852
School never SINI	-0.012	(0.039)	-0.017	(0.039)	0.437	742
Entering grades 3–5	-0.080	(0.049)	-0.070	(0.050)	0.517	489
Entering grades 6–8	0.015	(0.043)	0.019	(0.043)	0.420	700
Entering grades 9–12	-0.065	(0.053)	-0.065	(0.054)	0.455	405
Charter at baseline	-0.033	(0.057)	-0.045	(0.058)	0.529	405
Not charter at baseline	-0.034	(0.032)	-0.028	(0.033)	0.434	1,189

Notes: Robust standard errors adjusted for clustering by family appear in parentheses. Controls include those listed in table A.1. All models are weighted using baseline weights.

** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Notes

1. DC Parental Choice Incentive Act of 2003, H.R. 2556, 108th Cong. (2003).

2. Chingos and Kuehn (2017) discusses this literature in more detail.

3. Wolf et al. (2005) and Serving Our Children, http://servingourchildrendc.org/.

4. Author's calculations from Common Core of Data Public School Universe.

5. Of the $15 million annual appropriation, about $12 million is for scholarships (the remaining $3 million is for program administration and evaluation), which averages roughly $10,000 per student.

6. In the 2004 lottery, only the 492 students from non-SINI schools entering grades 6–8 could be included in the lottery. There were enough scholarships to accommodate all applicants entering grades K–5 and those currently attending SINI schools. In the 2005 lottery, all students applying from public schools were subject to the lottery and thus included in the evaluation.

7. I impute grade entering for seven students using their date of birth. The SINI status of the school attended at baseline is measured using the name of the baseline school reported on the application and lists of SINI schools provided by Serving Our Children. (SINI statuses for a given school year were not released until after the lottery was completed, so we use the status that was available at the time of the lottery and thus would have been used to grant priority in the lottery: 2003 SINI status in the 2004 lottery and 2004 SINI status in the 2005 lottery.) I also measure whether the baseline school was ever SINI between 2003 and 2005, which I use in the subgroup analysis but not to construct the randomization strata or base weights.

8. "NSC Enrollment Percentages," April 11, 2012, https://nscresearchcenter.org/wp-content/uploads/NSC_COVERAGE.xlsx. Coverage rates are also high in Washington, DC, and the surrounding states (Maryland and Virginia). In fall 2016 (2006), coverage rates were 99.1 percent (94.8 percent) in DC, 97.6 percent (91.0 percent) in Maryland, and 97.4 percent (94.7 percent) in Virginia.

9. These outcomes variables are binary variables that indicate whether a student ever enrolled in a college of a given type within the specified window (i.e., they are not mutually exclusive across college types).

10. This statistic is weighted using the base weights. The unweighted share of lottery winners who ever used a scholarship is 73 percent.

11. This Bloom adjustment requires the assumption that winning the lottery had no impact on students who did not ever use a scholarship.

12. Students with missing race and ethnicity data are included in the "not black or Hispanic" grouping.

13. These calculations assume that students progress on time through 12th grade, when in reality at least some repeat grades. The statistics in the text are weighted by the base weights; unweighted statistics are similar (39 and 53 percent).

14. This could reflect improvements in the DC public school system, which younger cohorts (those observed for fewer years after expected high school graduation) potentially attended for a greater number of more-recent years. But below I find no consistent evidence of differential effects for students who entered the program in earlier versus later grades.

15. For example, I find that students who were offered a scholarship but did not use it enrolled in college at significantly lower rates than the control group. This relationship attenuates only slightly when controls are included, suggesting a strong bias from unmeasured characteristics given that it is unlikely that winning the lottery has a negative effect on students among students who never use the scholarship. These results are available from the author upon request.

16. National Center for Education Statistics, Digest of Education Statistics, table 236.70, https://nces.ed.gov/programs/digest/d16/tables/dt16_236.70.asp?current=yes. Fourteen percent of treatment-group students attended a private school that charged more for tuition than the scholarship amount (Wolf et al. 2010, table ES-1). Private schools may spend more than they receive in tuition because of other revenue sources (e.g., the Catholic church), and public schools face costs (e.g., special education and district administration) that private schools do not. But previous attempts to adjust for these differences find that they narrow the estimated cost differences between sectors but do not eliminate them (Howell and Peterson 2002).

17. I do not consider the effects of the additional funding provided to public schools as part of the legislation authorizing the OSP. Thirty million dollars works out to roughly $441 per student enrolled in a DC public school in 2014–15.

18. These results are available from the author upon request.

19. Wolf et al. (2010, table C-2) report that the treatment impact for nonrespondents would have to be -21 percentage points to explain the entire positive impact they report.

20. Kristin Blagg and Matthew M. Chingos, "Does Gentrification Explain Rising Student Scores in Washington, DC?" *UrbanWire* (blog), Urban Institute, May 24, 2016, https://www.urban.org/urban-wire/does-gentrification-explain-rising-student-scores-washington-dc.

21. Author's calculations from Common Core of Data Public School Universe.

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About the Author

Matthew M. Chingos is director of the Urban Institute's Education Policy Program, which undertakes policy-relevant research on issues from prekindergarten through postsecondary education. Current research projects examine universal prekindergarten programs, school choice, student transportation, school funding, college affordability, student loan debt, and personalized learning.