

Evaluation of the DC Opportunity Scholarship Program: Experimental Impacts After at Least Four Years

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This paper summarizes the results reported in Patrick Wolf, Babette Gutmann, Michael Puma, Brian Kisida, Lou Rizzo, Nada Eissa, and Matthew Carr, *Evaluation of the DC Opportunity Scholarship Program: Final Report* (NCEE 2010-4018), Washington, DC: National Center for Education Evaluation and Regional Assistance, Institute of Education Sciences, U.S. Department of Education. The content of this paper is solely the responsibility of the authors and does not necessarily represent any official position of the U.S. Department of Education or any author-affiliated institutions.

Abstract:

The *District of Columbia School Choice Incentive Act of 2003*,¹ passed by the Congress in January 2004, established the first federally funded, private school voucher program in the United States. The purpose of the scholarship program was to provide low-income parents, particularly those whose children attend schools identified for improvement or corrective action under the *Elementary and Secondary Education Act*, with “expanded opportunities to attend higher performing schools in the District of Columbia.” This evaluation compares the outcomes of 2,300 eligible applicants randomly assigned to receive an offer (treatment group) or not receive an offer (control group) of an OSP scholarship through a series of lotteries. Data on outcomes—test scores, high school graduation, perceptions of school safety and satisfaction—were collected annually over four or five years.

Based on analysis of the final spring 2009 data we find that the Program significantly improved students’ chances of graduating from high school. The offer of an OSP scholarship raised students’ probability of completing high school by 12 percentage points overall. On average, after at least four years students who were offered (or used) scholarships had reading scores that were statistically higher than those who were not offered scholarships, while math scores were statistically similar to those who were not offered scholarships. Additionally, the OSP raised parents’, but not students’, ratings of school safety and satisfaction. Parents were more satisfied and felt school was safer if their child was offered or used an OSP scholarship. The Program had no effect on students’ reports on school conditions.

¹ Title III of Division C of the *Consolidated Appropriations Act*, 2004, P.L. 108-199.

EVALUATION OF THE DC OPPORTUNITY SCHOLARSHIP PROGRAM: FINAL REPORT ²

The *District of Columbia School Choice Incentive Act of 2003*,³ passed by the Congress in January 2004, established the first federally funded, private school voucher program in the United States. The purpose of the scholarship program is to provide low-income parents, particularly those whose children attend schools identified for improvement or corrective action under the *Elementary and Secondary Education Act*, with “expanded opportunities to attend higher performing schools in the District of Columbia” (Sec. 303). According to the statute, the key components of the Program include:

- To be eligible, students entering grades K-12 must reside in the District and have a family income at or below 185 percent of the federal poverty line.
- Participating students receive scholarships of up to \$7,500 to cover the costs of tuition, school fees, and transportation to a participating private school.
- Scholarships are renewable for up to 5 years (as funds are appropriated), so long as students remain eligible for the Program and remain in good academic standing at the private school they are attending.
- In a given year, if there are more eligible applicants than available scholarships or open slots in private schools, applicants are to be awarded scholarships by random selection (e.g., by lottery).
- In making scholarship awards, priority is given to students attending public schools designated as in need of improvement (SINI) under the *No Child Left Behind (NCLB) Act* and to families that lack the resources to take advantage of school choice options, operationally defined as students attending public school at the time of application.

² This paper summarizes the content of the sixth of a series of annual reports mandated by Congress. We gratefully acknowledge the contributions of a significant number of individuals in its preparation and production. Marsha Silverberg of the Institute for Education Sciences is the Contract Officer’s Representative for this project and has contributed greatly to the content and successful execution of the study. Staff from the Washington Scholarship Fund provided helpful information and have always been available to answer our questions. We also have benefited from the advice of a Technical Working Group comprising Julian Betts, Thomas Cook, Jeffrey Henig, William Howell, Guido Imbens, Rebecca Maynard, and Larry Orr. The challenging task of assembling the analysis files was capably undertaken by Yong Lee, Quinn Yang, and Yu Cao at Westat. The management and conduct of the data collection was overseen by Babette Gutmann and performed by Juanita Lucas-McLean and Bonnie Ho of Westat. Expert editorial and production assistance was provided by Evarilla Cover and Saunders Freeland of Westat.

³ Title III of Division C of the *Consolidated Appropriations Act*, 2004, P.L. 108-199.

- Private schools participating in the Program must be located in the District of Columbia and must agree to requirements regarding nondiscrimination in admissions, fiscal accountability, and cooperation with the evaluation.

As part of this legislation, Congress mandated a rigorous evaluation of the impacts of the Program, now called the DC Opportunity Scholarship Program (OSP). This final evaluation report presents the longer term effects of the Program on families who applied and were given the option to move from a public school to a participating private school of their choice.

DC Opportunity Scholarship Program

The OSP is operated by the Washington Scholarship Fund (WSF), a 501(c)3 organization based in the District of Columbia, under contract to the U.S. Department of Education's Office for Innovation and Improvement. To date, there have been five rounds of applications to the OSP (table 1). Applicants in spring 2004 (cohort 1) and spring 2005 (cohort 2) represent the majority of Program applicants; the evaluation sample was drawn from these two groups.⁴ There were a smaller number of applicants in spring 2006 (cohort 3), spring 2007 (cohort 4), and spring 2008 (cohort 5) who were recruited and enrolled by WSF in order to keep the Program operating at capacity each year.⁵ These relatively late enrollees are not formally part of this impact analysis. Applicants in spring 2009 (cohort 6) were recruited and their eligibility was determined. Eligible applicants were not enrolled in the Program when it was determined that federal funding would not be provided for new students for the 2009-10 school year.

⁴ Descriptive reports on each of the first 2 years of implementation and cohorts of students have been previously prepared and released (Wolf, Gutmann, Eissa, Puma, and Silverberg 2005; Wolf, Gutmann, Puma, and Silverberg 2006) and are available on the Institute of Education Sciences' website at <http://ies.ed.gov/ncee>.

⁵ Because the influx of cohort 2 participants essentially filled the Program, the WSF recruited and enrolled a much smaller number of students in each succeeding year, primarily to replace OSP students who left the Program between the second and fifth year of implementation. WSF limited applications to students entering grades K-6 for cohort 3 and grades K-7 for cohorts 4 and 5 because there were few slots available in participating high schools, as large numbers of students from cohorts 1 and 2 advanced to those grades. Applications also were limited to students previously attending public schools or rising kindergarteners, since public school students are a higher service priority of the Program than are otherwise eligible private school students.

Table 1. OSP Applicants by Program Status, Cohorts 1 Through 6, Years 2004-09

| | Cohort 1 (Spring 2004) | Cohort 2 (Spring 2005) | Total Cohort 1 and Cohort 2 | Cohort 3 (Spring 2006), Cohort 4 (Spring 2007), and Cohort 5 (Spring 2008) | Cohort 6 (Spring 2009) | Total, All Cohorts |
|---|---------------------------|---------------------------|-----------------------------------|---|---------------------------|-----------------------|
| Applicants | 2,692 | 3,126 | 5,818 | 2,034 | 628 | 8,480 |
| Eligible applicants | 1,848 | 2,199 | 4,047 | 1,284 | 216 | 5,547 |
| Scholarship awardees | 1,366 | 1,088 | 2,454 | 1,284 | NA | 3,738 |
| Scholarship users in initial year of receipt | 1,027 | 797 | 1,824 | 1,057 | NA | 2,881 |
| Scholarship users fall 2005 | 919 | 797 | 1,716 | NA | NA | 1,716 |
| Scholarship users fall 2006 | 788 | 684 | 1,472 | 333 | NA | 1,805 |
| Scholarship users fall 2007 | 678 | 581 | 1,259 | 671 | NA | 1,930 |
| Scholarship users fall 2008 | 496 | 411 | 909 | 807 | NA | 1,714 |
| Scholarship users fall 2009 | 386 | 319 | 705 | 617 | NA | 1,322 |

NOTES: NA means “not applicable” because scholarships could not be awarded or used in a given year. Because most participating private schools closed their enrollments by mid-spring, applicants generally had their eligibility determined based on income and residency, and the lotteries were held prior to the administration of baseline tests. Therefore, baseline testing was not a condition of eligibility for most applicants. The exception was applicants entering the highly oversubscribed grades 6-12 in cohort 2. Those who did not participate in baseline testing were deemed ineligible for the lottery and were not included in the eligible applicant figure presented above, though they were counted in the applicant total. In other words, the cohort 2 applicants in grades 6-12 had to satisfy income, residency, and baseline testing requirements before they were designated eligible applicants and entered in the lottery.

The initial year of scholarship use was fall 2004 for cohort 1, fall 2005 for cohort 2, fall 2006 for cohort 3, fall 2007 for cohort 4, and fall 2008 for cohort 5.

SOURCES: OSP applications and WSF’s enrollment and payment files.

Among the applicants, those determined eligible for the Program represent just over 10 percent of all children in Washington, DC, who meet the OSP’s eligibility criteria, according to 2000 Census figures.⁶ During fall 2009, a total of 1,322 students were using Opportunity Scholarships to attend participating private schools.

Mandated Evaluation of the OSP

In addition to establishing the OSP, Congress mandated that an independent evaluation of it be conducted, with annual reports on the progress of the study. The legislation indicated that the evaluation should analyze the effects of the Program on various academic and nonacademic outcomes of concern to policymakers and use “...the strongest possible research design for determining the effectiveness” of the Program.⁷

⁶ See previous evaluation reports, including Wolf, Gutmann, Puma, Rizzo, Eissa, and Silverberg 2007, p. 8.

⁷ *District of Columbia School Choice Incentive Act of 2003*, Section 309 (a)(2)(A).

This evaluation was developed to be responsive to these requirements. In particular, the foundation of the evaluation is a randomized controlled trial (RCT) that compares outcomes of 2,300 eligible applicants (students and their parents) randomly assigned to receive an offer (treatment group) or not receive an offer (control group) of an OSP scholarship through a series of lotteries.⁸ This decision was based on the mandate to use rigorous evaluation methods, the expectation that there would be more applicants than funds and private school spaces available, and the statute's requirement that random selection be the vehicle for determining who receives a scholarship. An RCT design is widely viewed as the best method for identifying the independent effect of programs on subsequent outcomes (e.g., Boruch, de Moya, and Snyder 2002, p. 74; Cook and Campbell 1979, p. 56). Random assignment has been used by researchers conducting impact evaluations of other scholarship programs in Charlotte, NC; New York City; Dayton, OH; and Washington, DC (Greene 2001; Howell, Wolf, Campbell, and Peterson 2002; Mayer et al. 2002).

The recruitment, application, and lottery process conducted by WSF with guidance from the evaluation team created the foundation for the evaluation's randomized trial and determined the group of students for whom impacts of the Program are analyzed. Because the goal of the evaluation was to assess both the short-term and longer term impacts of the Program, it was necessary to focus the study on early applicants to the Program (cohorts 1 and 2) whose outcomes could be tracked over at least four years during the evaluation period. During the first two years of recruitment, WSF received applications from 5,818 students. Of these, approximately 70 percent (4,047 of 5,818) were eligible for the Program (table 1). Of the total pool of eligible applicants, 2,308 students who were rising kindergarteners or currently attending public schools entered lotteries (492 in cohort 1; 1,816 in cohort 2), resulting in 1,387 students assigned to the treatment condition and 921 assigned to the control condition. These students constitute the evaluation's impact analysis sample and represent three-quarters of all students in cohorts 1 and 2 who were not already attending a private school when they applied to the OSP.⁹

Data Collection

The evaluation gathers information annually from students and families in the study, as well as from their schools, in order to address the key research questions. These data include:

⁸ The law clearly specified that such a comparison in outcomes be made (see Section 309 (a)(4)(A)(ii)).

- **Student assessments.** Measures of student achievement in reading and math for public school applicants come from the Stanford Achievement Test-version 9 (SAT-9)¹⁰ administered by either the District of Columbia Public Schools (DCPS) (cohort 1 baseline) or the evaluation team (cohort 2 baseline and all follow-up data collection). The evaluation testing took place primarily on Saturdays, during the spring, in locations throughout DC arranged by the evaluators. The testing conditions were similar for members of the treatment and control groups.
- **Parent surveys.** The OSP application included baseline surveys for parents applying to the Program. These surveys were appended to the OSP application form and therefore were completed at the time of application to the Program. Each spring after the baseline year, surveys of parents of all applicants were conducted at the Saturday testing events, while parents were waiting for their children to complete their outcome testing. The parent surveys provide the self-reported outcome measures for parental satisfaction and safety.
- **Student surveys.** Each spring after the baseline year, surveys of students in grades 4 and above were conducted at the outcome testing events. The student surveys provide the self-reported outcome measures for student satisfaction and safety.
- **Principal surveys.** Each spring, surveys of principals of all public and private schools operating in the District of Columbia were conducted. Topics include self-reports of school organization, safety, and climate; principals' awareness of and response to the OSP; and, for private school principals, why they are or are not OSP participants.
- **Parent Follow Up surveys.** In the summer of 2009, parents of students who had turned 16 by June 30, 2009, were contacted (by telephone) to determine if their child was still enrolled in high school or had graduated from high school. The parent follow-up surveys provided the self-reported outcome measure for educational attainment (that is, high school graduation) analyzed for the first time in this report.

Several methods were used to encourage high levels of response to the final year of data collection in spring 2009 (year 5 for cohort 1 and year 4 for cohort 2). Study participants were invited to at least five different data collection events if they were a member of the treatment group and at least six different data collection events if they were a member of the control group. Impact sample members received payment for their time and transportation costs if they attended a data collection event. The events were held on Saturdays except for one session that was staged on a weeknight. Multiple sites throughout DC were used for these events, and participants were invited to the location closest to their residence. When the address or telephone number of a participant was inaccurate, such cases were submitted to the tracing office at Westat and subject to intensive efforts to update and correct the contact information. Treatment and control group students were tested under the same conditions.

¹⁰ Stanford Abbreviated Achievement Test (Form S), Ninth Edition. San Antonio, TX: Harcourt Educational Measurement, Harcourt Assessment, Inc., 1997.

After these initial data collection activities were completed, the test score response rate¹¹ for 2009 was 63.5 percent. The treatment group response rate was 63.9 percent, and the control group response rate was 62.7 percent, a response rate differential of 1.2 percentage points lower for the control group compared to the treatment group. Although that differential was not statistically significant, to reduce the likelihood of nonresponse bias and increase the generalizability of the study results, a random subsample of half of the nonrespondents in both the treatment and control groups was drawn and subjected to intensive efforts at nonrespondent conversion. Since these initial nonrespondents were selected at random, each one that was successfully converted to a respondent counts double in the analysis, as he or she “stands in” for an approximately similar initial nonrespondent that was not subsampled (see Kling, Ludwig, and Katz 2005; Sanbonmatsu, Kling, Duncan, and Brooks-Gunn 2006). The “effective” response rate after subsample conversion is the number of actual respondents prior to the subsample plus two times the number of subsampled respondents, all divided by the total number of students in the impact sample.

As a result of the subsample conversion process, the final effective test score response rate for 2009 data collection was 69.5 percent, and the differential rate of response between the treatment and control groups was reduced to 0.1 percentage points higher for the treatment group.¹² The effective parent survey response rate was 66.1 percent.¹³ The effective student survey response rate was 66.5 percent.¹⁴

¹¹ A total of 296 students initially in the impact sample (202 in cohort 1 and 94 in cohort 2) were forecasted to have graduated from high school before the spring of 2009, based on their grade upon Program application at least four years after random assignment. The Stanford Achievement Test that is mandated as the evaluation test does not have a version for students beyond 12th grade. As a result, these “grade outs” were not invited to data collection events, and therefore are not counted in the main set of response rate calculations presented in this report.

¹² Specifically the overall effective response rates were 69.5 percent for the treatment group and 69.4 percent for the control group. Prior to drawing the subsample, response rates for the control group were 50.6 percent (cohort 1) and 64.3 percent (cohort 2). Response rates (after drawing the subsample) for the control group were 53.1 percent (cohort 1) and 67.6 percent (cohort 2). After subsample weights were applied, the effective response rates for the control group were 54.6 percent (cohort 1) and 71.2 percent (cohort 2). Prior to drawing the subsample, response rates for the treatment group were 52.9 percent (cohort 1) and 66.6 percent (cohort 2). Response rates (after drawing the subsample) for the treatment group were 59.6 percent (cohort 1) and 68.5 percent (cohort 2). After subsample weights were applied, the effective response rates for the treatment group were 65.5 percent (cohort 1) and 70.5 percent (cohort 2). There were 296 impact sample students awarded scholarships who were no longer eligible for data collection by spring of 2009; these students are excluded from the main response rate calculations.

¹³ Specifically, the overall effective response rates were 65.6 percent for the treatment group and 67.0 percent for the control group. Response rates (after drawing the subsample) for the control group were 49.4 percent (cohort 1) and 65.0 percent (cohort 2). After subsample weights were applied, the effective response rates for the control group were 50.9 percent (cohort 1) and 69.1 percent (cohort 2). Response rates (after drawing the subsample) for the treatment group were 53.7 percent (cohort 1) and 65.0 percent (cohort 2). After subsample weights were applied, the effective response rates for the treatment group were 59.3 percent (cohort 1) and 67.1 percent (cohort 2).

¹⁴ Specifically, the overall effective response rates were 67.3 percent for the treatment group and 65.1 percent for the control group. Response rates (after drawing the subsample) for the control group were 50.6 percent (cohort 1) and 64.5 percent (cohort 2). After subsample weights were applied, the effective response rates for the control group were 52.2 percent (cohort 1) and 67.2 percent (cohort 2). Response rates (after drawing the subsample) for the treatment group were 59.2 percent (cohort 1) and 66.3 percent (cohort 2). After subsample weights were applied, the effective response rates for the treatment group were 65.1 percent (cohort 1) and 68.0 percent (cohort 2).

The public school principal survey response rate was 75 percent, and the private school principal survey response rate was 72 percent. The response rate for the parent follow-up survey was 63.2 percent.

Missing outcome data create the potential for nonresponse bias in a longitudinal evaluation such as this one, if the nonrespondent portions of the sample are different between the treatment and control groups. Response rates for the various data collection instruments all differed by less than 2.5 percent between the treatment and control groups. Study respondents in the final year of data collection were statistically similar to the overall impact sample on 13 of 14 baseline (pre-Program) characteristics before nonresponse weights were applied—a difference predicted by chance—and indistinguishable from each other on all features after applying weights to adjust for nonresponse. More important, treatment group respondents did not differ significantly from control group respondents on any of the 14 baseline covariates examined, either before or after the application of the nonresponse weights. Thus, the impact results presented here do not appear affected by participant nonresponse and do appear to be generalizable to the original sample of students in the study.

Participation in the OSP

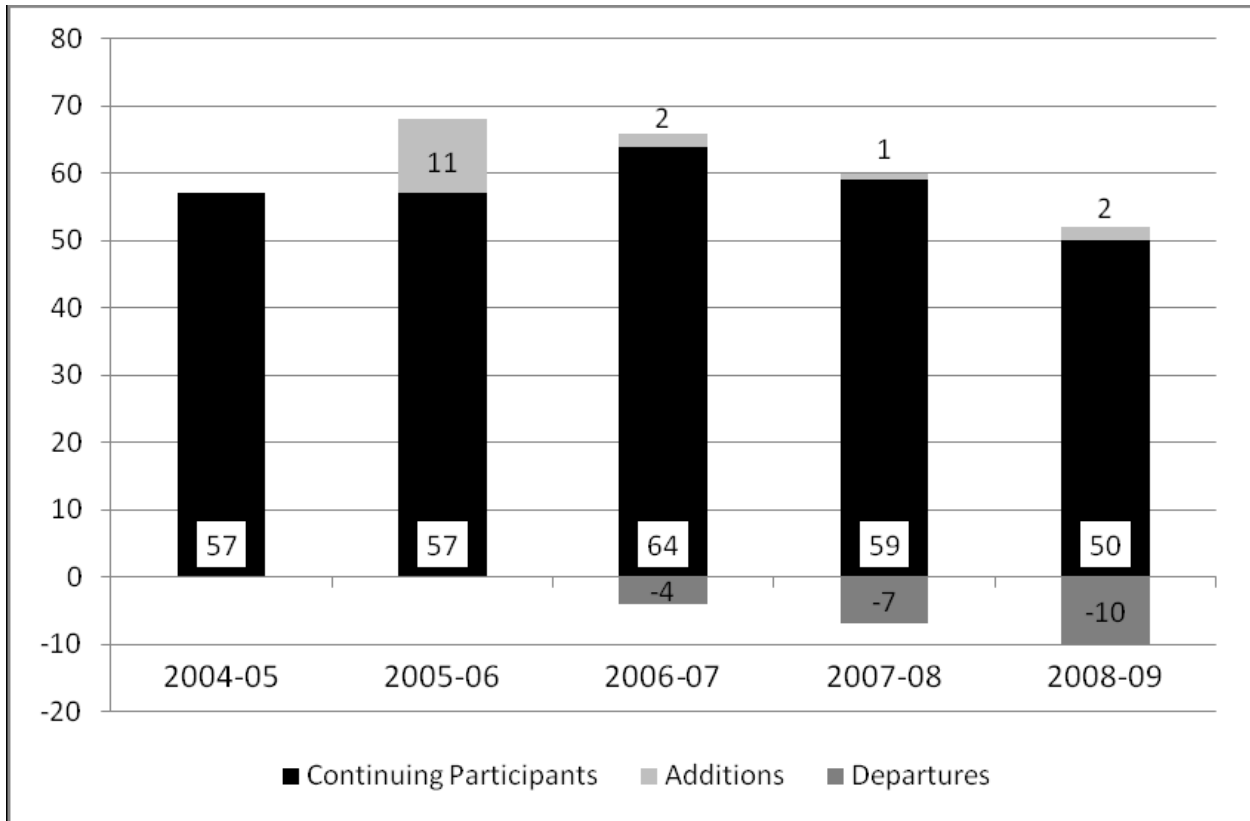
In interpreting the impacts of the OSP, it is useful to examine the characteristics of the private schools that participate in the Program and the extent to which students offered scholarships (the treatment group) moved into and out of them.

School Participation

The private schools participating in the OSP represent the choice set available to parents whose children received scholarships. A total of 52 of 90 private schools in the District of Columbia were participating in the Program at the start of the 2008-09 school year, the final year for which data were collected for this evaluation. That count of participating private schools was down from the peak of 68 achieved in 2005-06. A total of 73 different private schools participated in the OSP at some point during the first five years of the Program (figure 1).¹⁵ Among them, 52 percent consistently participated in all five years, while 48 percent partially participated, including seven former Catholic private schools that enrolled a total of 112 treatment group students in 2007-8; but subsequently left the Program and operated as public charter schools in 2008-09.

¹⁵ This figure represents a net loss of nine schools since the prior year. Eleven schools stopped participating while two new schools participated for the first time in 2008-09. The total number of private schools operating in DC declined from 109 in 2004-05 to 90 in 2008-09.

Figure 1. Number of Participating OSP Private Schools, 2004-05 through 2008-09



SOURCE: OSP School Directory information, 2004-05, 2005-06, 2006-07, 2007-08, 2008-09, WSF.

Among these participating schools (table 2):

- Consistent participants enrolled an average of 23 OSP students per school per year; partial participants enrolled an average of 16 students per school per year. Catholic schools that converted to public charter schools, a subset of partial participants, enrolled an average of 41 OSP students per school per year in the Program.
- Forty-six percent of schools that partially participated charged tuition in excess of the \$7,500 maximum scholarship award, while 37 percent of consistent participants charged more than this amount. No Catholic schools that converted to public charter schools charged above \$7,500.

Table 2. Features of Private Schools Participating in the OSP by Participation Status, 2004-05 through 2008-09

| | Percent of All Participating Schools | Average School Size | Average Yearly Scholarship Users | Percent with Tuition Over \$7,500 |
|---------------------------------|--------------------------------------|---------------------|----------------------------------|-----------------------------------|
| Consistent participants | 52.1 | 251.0 | 23.4 | 36.8 |
| All partial participants | 47.9 | 238.2 | 16.3 | 45.7 |
| Catholic to charter conversions | 9.6 | 176.3 | 41.0 | 0.0 |
| Other partial participants | 38.4 | 262.2 | 10.1 | 57.1 |

NOTES: Average school size is the average of the school size data from the National Center for Education Statistics' Private School Survey, 2005-06 and 2007-08. Average tuition represents the average tuition charged across all years that a school participated in the Program.

SOURCES: OSP School Directory information, 2004-05, 2005-06, 2006-07, 2007-08, 2008-09, WSF. National Center for Education Statistics' Private School Survey, 2005-06 and 2007-08.

Among the schools that participated in 2008-09:¹⁶

- Fifty-four percent (28) were faith-based, with a majority of them (15) the parochial schools of the Catholic Archdiocese of Washington;
- Fifty percent charged an average tuition above the OSP's scholarship cap of \$7,500;¹⁷
- The average participating school had a total student population of 286 students in 2008-09;
- Twenty-three percent served high school students;¹⁸
- The average minority percentage among the student body was 66 percent; and
- The average student/teacher ratio was 9:4.

Schools Attended by Scholarship Users in 2008-09

Not all of the schools that agreed to participate in the Program served OSP students every year. During the 2008-09 school year, which represented five years after application for cohort 1 and four years after for cohort 2, OSP students were enrolled in 40, and the impact sample's treatment students in

¹⁶ Information was obtained for all 52 participating schools from records of the WSF regarding whether the schools were faith-based, charged tuition above \$7,500, and served high school. The data regarding school size (valid $N = 43$), percent minority students (valid $N = 39$) and student/teacher ratio (valid $N = 39$) were drawn from the National Center for Education Statistics' Private School Survey, last administered in 2007-08.

¹⁷ For schools that charge a range of tuitions, the midpoint of the range was selected.

¹⁸ Schools were classified as serving high school students if they enrolled students in any grade 9-12.

38, of the 52 schools available to them.²⁵ Since participating schools varied in how many slots they committed to the Program, OSP students were clustered in certain schools; this was also true of the students in the impact sample's treatment group.

The schools that offered the most slots to OSP students, and in which OSP students and the impact sample's treatment group were clustered, have characteristics that differed somewhat from the typical participating OSP school. Fourteen percent of treatment group students were attending a school that charged tuition above the statutory cap of \$7,500 during the year represented by this evaluation report (table 3), even though 50 percent of participating schools charged tuitions above that cap in 2008-09. Although 54 percent of all participating schools were faith-based (29 percent part of the Catholic Archdiocese of Washington), 80 percent of the treatment group attended a faith-based school, with most of them (53 percent) attending the 15 participating Catholic parochial schools (figure 2).

Table 3. Features of Participating OSP Private Schools Attended by the Treatment Group in 2008-09

| Characteristic | Weighted Mean | Highest | Lowest | Valid <i>N</i> |
|---|---------------|----------|---------|----------------|
| Charging over \$7,500 tuition (percent of treatment students attending) | 14.2% | NA | NA | 38 |
| Tuition | \$7,252 | \$29,607 | \$4,500 | 38 |
| Enrollment | 292.1 | 1,097 | 16 | 31 |
| Student <i>N</i> | 465 | | | |

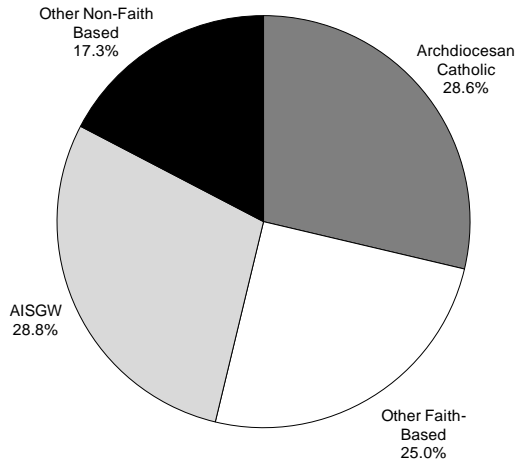
NOTES: "Valid *N*" refers to the number of schools for which information on a particular characteristic was available. When a tuition range was provided, the mid-point of the range was used. The weighted mean was generated by associating each student with the characteristics of the school he/she was attending and then computing the average of these student-level characteristics.

SOURCES: OSP School Directory information, 2008-09, WSF; National Center for Education Statistics' Private School Survey, 2007-08.

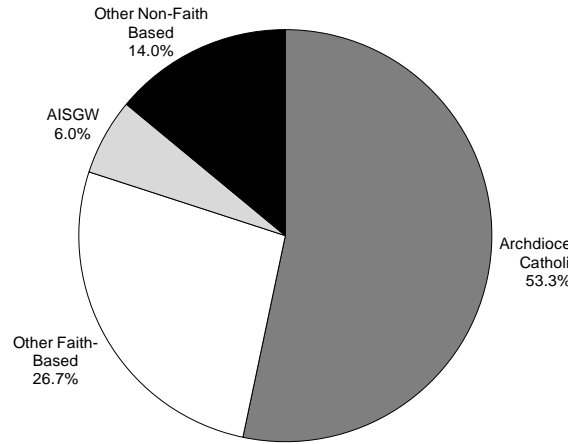
²⁵ The source for student enrollment in participating schools is the WSF OSP payment file for 2008-09.

Figure 2. Religious Affiliation of Participating Schools

Percent of Participating Schools, 2008-09



Percent of Students Attending Participating Private Schools, 2008-09



NOTES: School $N = 52$ for percent of participating private schools in 2008-09. School $N = 38$ and Student $N = 465$ for percent of students in the treatment group attending participating private schools in 2008-09. AISGW is an acronym for the Association of Independent Schools of Greater Washington.

SOURCES: OSP School Directory information, 2004-05, 2005-06, 2006-07, 2007-08, Washington Scholarship Fund; OSP payment file for 2008-09, Washington Scholarship Fund.

Schools Attended by the Treatment Group in Relation to Those of the Control Group in 2008-09

While the characteristics of the participating private schools are important considerations for parents, how those characteristics differ from the public school options available to parents matters more. How different are the school conditions? Students in the treatment and control groups did not differ significantly regarding the proportion attending schools that offered a separate library, gyms, individual tutors, music programs, and after-school programs (table 4). However, there were some notable differences between students in the treatment and control groups in some aspects of the schools they attended.

- Students in the treatment group were less likely than those in the control group to attend a school that offered special programs for students who may be academically challenged; these include programs or services for non-English speakers and for students with learning problems;
- Students in the treatment group were less likely to be in schools with special programs for advanced learners; and
- Students in the treatment group were less likely than those in the control group to attend a school with a cafeteria facility, a nurse’s office, counselors, and art programs.

Table 4. Characteristics of School Attended by the Impact Sample, Year of Application and 2008-09

| Percentage of Students Attending a School with: | Baseline Year | | | 2008-09 | | |
|---|---------------|---------|------------|-----------|---------|------------|
| | Treatment | Control | Difference | Treatment | Control | Difference |
| Separate Facilities: | | | | | | |
| Computer lab | 72.02 | 71.87 | .16 | 94.72 | 91.47 | 3.25** |
| Library | 80.00 | 77.15 | 2.85 | 77.22 | 78.98 | -1.75 |
| Gym | 60.24 | 60.45 | -.21 | 67.93 | 71.03 | -3.09 |
| Cafeteria | 86.26 | 87.52 | -1.27 | 75.87 | 90.96 | -15.09*** |
| Nurse's office | 87.52 | 89.33 | -1.81 | 49.85 | 82.45 | -32.61*** |
| Percent missing | 7.05 | 7.12 | -.07 | 1.09 | 4.67 | -3.59 |
| Programs: | | | | | | |
| Special program for non-English speakers | 45.95 | 40.21 | 5.75* | 32.01 | 57.41 | -25.40*** |
| Special program for students with learning problems | 65.35 | 65.80 | -.45 | 75.21 | 90.41 | -17.90*** |
| Special program for advanced learners | 37.14 | 31.92 | 5.23 | 37.72 | 48.88 | -11.17*** |
| Counselors | 79.68 | 77.50 | 2.19 | 77.42 | 87.14 | -9.73*** |
| Individual tutors | 36.93 | 38.00 | -1.07 | 58.49 | 62.83 | -4.34 |
| Music program | 68.87 | 69.38 | -.51 | 93.32 | 90.63 | 2.69 |
| Art program | 70.75 | 67.43 | 3.33 | 83.80 | 92.10 | -8.30*** |
| After-school program | 83.07 | 83.22 | -.15 | 91.45 | 88.33 | 3.13 |
| Percent missing | 7.29 | 7.47 | -.18 | 1.09 | 4.67 | -3.59 |
| Sample size (unweighted) | 1,060 | 586 | 474 | 1,060 | 586 | 474 |

*Statistically significant at the 90 percent confidence level.

**Statistically significant at the 95 percent confidence level.

***Statistically significant at the 99 percent confidence level.

NOTES: Data are weighted. Baseline year means presented here differ from those presented in previous reports due to the exclusion of grade-outs.

SOURCES: OSP applications, Impact Evaluation Parent Survey (for school attended), and Impact Evaluation Principal Survey.

Student Participation

As has been true in other school choice programs, some students offered scholarships never used them, while others used their scholarships to attend a participating private school at some point

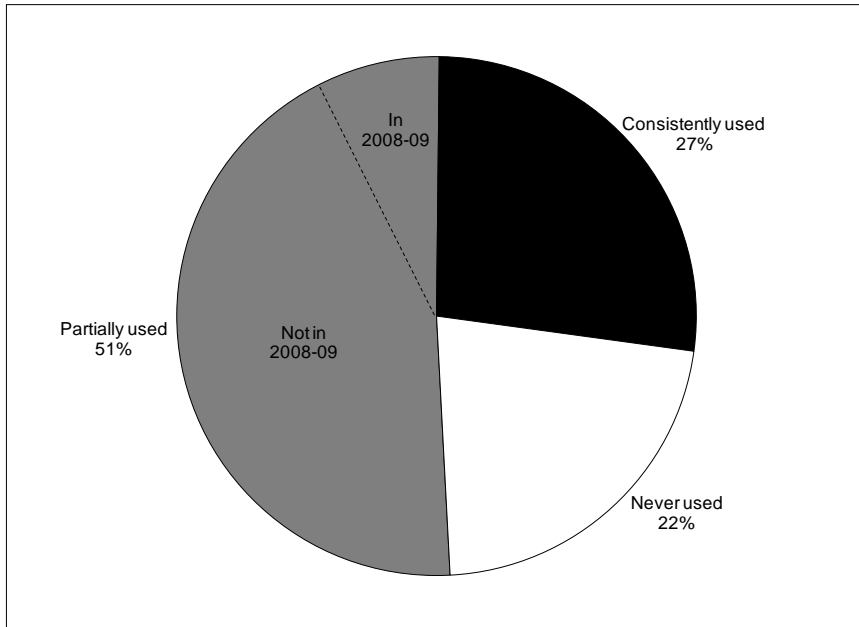
during the four- to five-year period. And over the years, some students lost their eligibility for the Program. For example, by 2008-09, a total of 94 of the 1,387 members of the treatment group were no longer eligible to receive scholarships because they had “graded out” of the Program, which means that they would have moved beyond 12th grade. Looking across the remaining members of the impact sample’s treatment group who had four (cohort 1) or five (cohort 2) years of potential Program participation (figure 3):

- 351 treatment group students (27 percent) used their scholarship during all years available to them after the scholarship lottery.
- 660 treatment students (51 percent) used their scholarships, but not consistently, during the school years after the scholarship award. Among these students are an estimated 147 who may have been forced by circumstances to stop using their scholarship. Students could become “forced-decliners” because the school they continued to attend converted from a participating Catholic school to a public charter school (estimated for 35 treatment students),¹⁹ their family income grew to exceed the Program’s income limit (confirmed for 21 treatment students), their family moved out of DC (confirmed for 29 students), or they may have faced a lack of space for them in a participating high school when they transitioned from 8th to 9th grade (estimated for 62 treatment students).²⁰ Among the students who partially used their scholarship over at least four years after random assignment, 17 percent (9 percent of eligible treatment group students overall) used their OSP scholarship in 2008-09.
- The remaining 282 out of 1,293 (22 percent) never used the OSP scholarships offered to them.

¹⁹ Based upon survey data, 35.9 percent of 97 treatment group students who used a scholarship to attend one of these Catholic schools in grades K-7 in 2007-08 continued to attend the same school when it converted to a public charter school in 2008-09.

²⁰ The estimate of the number of students forced to decline their scholarships due to the lack of high school slots was calculated by comparing the higher rate of scholarship continuation for 7th graders moving to 8th grade with the lower rate of scholarship continuation for 8th graders moving to 9th grade. The difference between those two continuation rates, applied to the number of OSP students moving from 8th to 9th grade, generates the estimate of forced decliners due to high school slot constraints of 62 (20 in year two plus 30 in year three plus 12 new cases in 2008-09). It is impossible to know for certain if all 62 of these students declined to use the scholarship solely or primarily because of high school slot constraints, and not for other reasons, or if some treatment students were forced to decline their scholarship at the very start due to high school slot constraints. It also is impossible to know if some students declined to even attempt to renew their scholarships because they knew their family exceeded the income limit, or how many treatment students moved out of DC and never informed the evaluators that they had “moved out” of Program eligibility. Therefore, the total estimate of 147 forced decliners for 2008-09 is simply an estimate based on the limited data available.

Figure 3. Proportion of K-12 Treatment Group Using Their OSP Scholarship Award Through 2008-09



NOTES: Students were identified as scholarship users based upon information from WSF’s payment files. Because some schools use a range of tuitions and some students had alternative sources of funding, students were classified as full users if WSF made payments on their behalf that equaled at least 80 percent of the school’s annual tuition. Otherwise students were identified as partial users (1 percent to 79 percent of tuition paid) or nonusers (no payments).

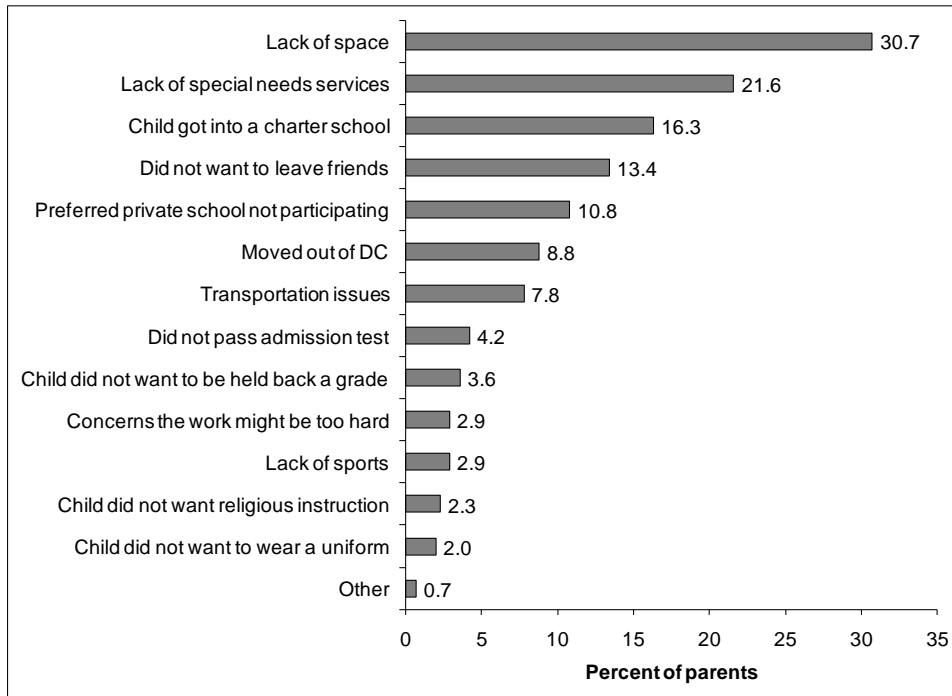
By the 2008-09 school year, 94 treatment group students had “graded out” and are not included in this figure. Of these grade-outs, 19 fully used their scholarships over all of the years that were available to them, 22 partially used their scholarships, and 53 never used their scholarships.

Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment.

SOURCE: WSF’s payment files.

Across the years, the most common reasons given by parents for never using an OSP scholarship that was awarded to their child was a lack of space at their preferred private school, the absence of special needs services, and that their child was admitted to a preferred public charter school (figure 5).

Figure 5. Reasons Given by Parents of Treatment Students for Never Using an OSP Scholarship

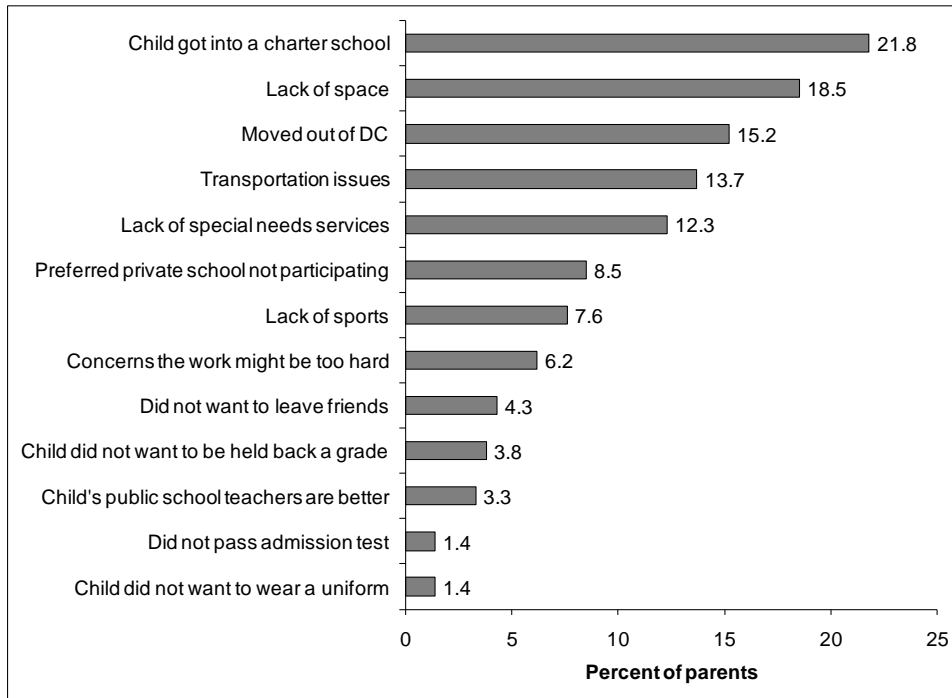


NOTES: Responses are unweighted. Respondents were able to select multiple responses each year, and some respondents participated in data collection for multiple years. Percentages given represent the sum of all responses obtained across years one through four of data collection (i.e., longitudinal responses) divided by the sum of all respondents ($N = 306$) across all of those same years (i.e., longitudinal respondents). As a result, this figure includes initial responses from parents of students who subsequently graded out of the Program. Categories with responses from fewer than three parents in any year are collapsed into the “Other reasons” category for confidentiality reasons.

SOURCE: Impact Evaluation Parent Surveys.

Among students who initially used a scholarship but then left the Program, the most common reasons for leaving were that the child was admitted to a preferred public charter school, a lack of space at their preferred private school, and that the family moved out of DC (figure 6).

Figure 6. Reasons Given by Parents of Treatment Students for Not Continuing to Use an OSP Scholarship



NOTES: Responses are unweighted. Respondents were the parents of treatment students who used a scholarship in a previous year but not in a subsequent year ($N = 211$). The reasons for not using were drawn from the parent responses the first year after their child stopped using a scholarship. Respondents appear in the data only one time (i.e., unique respondents), though they may have provided multiple reasons for not continuing to use a scholarship. This figure includes initial responses from parents of students who subsequently graded out of the Program.

SOURCE: Impact Evaluation Parent Surveys.

Overall Movement Into and Out of Private and Public Schools

Where did students who declined to participate in the OSP attend school instead? Children in the treatment group who never used the OSP scholarship offered to them, or who did not use the scholarship consistently, could have remained in or transferred to a public charter school or a traditional DC public school, or enrolled in a non-OSP-participating private school. The same alternatives were available to the impact sample's control group; they could remain in their current DC public school (traditional or charter), enroll in a different public school, or try to find a way to attend a participating or nonparticipating private school. These choices could affect program impacts because traditional public, public charter, and private schools are presumed to offer different educational experiences and because previous studies suggest that switching schools has an initial short-term negative effect on student achievement (Hanushek, Kain, and Rivkin 2004).

The members of the impact sample were all attending DC public schools or were rising kindergarteners in the year they applied to the OSP. Of the students who were not entering kindergarten, approximately three-fourths were attending traditional DC public schools, while the remaining one-fourth were attending public charter schools. At least four years after random assignment, there was substantial variation across educational sectors (table 5).

Table 5. Percentage of the Impact Sample Still in K-12 by Type of School Attended: At Baseline and 2008-09

| | Baseline | | 2008-09 | | |
|------------|-------------|---------|-------------|---------|---------|
| | Public | | Public | | Private |
| | Traditional | Charter | Traditional | Charter | |
| Treatment | 75.1 | 24.9 | 26.6 | 18.4 | 55.0 |
| Control | 74.6 | 25.4 | 53.2 | 35.3 | 11.5 |
| Difference | .5 | -.5 | -26.6 | -16.9 | 43.5 |

NOTES: Baseline year means presented here differ from those presented in previous reports due to the exclusion of grade-outs. The longitudinal statistics presented in this table exclude data from students who were rising kindergarteners at baseline to reduce the risk of compositional bias across the years examined. As a result, the type of school attended reported here may vary slightly from other cross-sectional descriptions of school attended found in this report. Student $N = 1,375$. Percent missing baseline: Treatment = 4.7, Control = 7.9; percent missing year four or year five: Treatment = 19.4, Control = 25.5. Data are unweighted and represent actual responses. Given the rates of missing data, readers are cautioned against drawing firm conclusions.

Results for cohort 1 are based on five years after random assignment and for cohort 2 on four years after random assignment.

SOURCES: Program applications and Impact Evaluation Parent Surveys.

These data show how assignment to treatment (being offered a scholarship) is not perfectly correlated with private school attendance and that assignment to the control group (no scholarship offer) does not necessarily entail attendance at a traditional public school. A number of school choices are available in DC to parents who seek alternatives to their neighborhood public school, and many members of the control group availed themselves of school choice options even if they were not awarded an Opportunity Scholarship. 35 percent of the control group still in grades K-12 in 2008-09 were enrolled in public charter schools, and 12 percent attended a private school. Similarly, among the treatment group still in grades K-12 in 2008-09, 45 percent of them chose not to use their scholarship. Twenty-seven percent of the K-12 treatment group attended a traditional public school in 2008-09, and 18 percent attended a public charter school.²¹

²¹ The subset of survey respondents in the treatment group is disproportionately comprised of treatment users; that is why the rates of treatment group members attending private schools presented here are significantly higher than the overall scholarship usage rates presented in other sections of the report. It is necessary to rely on survey respondents — in both the treatment and control groups — for the descriptive comparison provided here because WSF’s payment files, which are used to calculate the Program-wide scholarship usage rates, do not contain any information on the types of schools attended by treatment nonusers or control group members.

School-switching is both an inherent aspect of the K-12 experience and a component part of the scholarship program treatment. Students in inner-city school districts with limited grade-range schools (e.g. K-5, 6-8, 9-12) and many public charter schools will tend to switch schools frequently during their educational careers. Such school mobility is a natural aspect of the counterfactual demonstrated by our control group. Moreover, public school students offered private school scholarships must switch schools, at least initially, in order to avail themselves of such an intervention. In the context of an experimental evaluation that begins when all of the students are in public schools, separating the effect of the school choice intervention from the effect of an action *necessitated by use of the school choice intervention* (i.e. switching schools) would produce only a partial and misleading estimate of the true and total impact of the intervention. As a result, we merely describe the school-switching that the students in our evaluation experienced. We do not control for the effects of school switching after random assignment because doing so would undermine the validity of our impact estimates.

Impact of the Program After at Least Four Years: Key Outcomes

Research Methodology

The statute that authorized the OSP mandated that the Program be evaluated with regard to its impact on student test scores and school safety, as well as the “success” of the Program, which, in the design of this study, includes satisfaction with school choices. This final report on the OSP examines the effects of the Program on students and their parents near the end of the 2008-09 school year. The analysis is both consistent with and different from that presented in prior evaluation reports examining shorter-term impacts. It is consistent in that impacts are presented in two ways: the impact of the *offer* of an OSP scholarship, derived straight from comparing the average outcomes of the treatment and control groups, and (2) the impact of *using* an OSP scholarship, statistically adjusting for students who declined to use their scholarships. Like the earlier reports, the final estimates provide impacts on achievement, safety, and satisfaction.

Two parts of the analysis are different this year. First, in previous analyses, the two cohorts of students in the impact sample had the potential to experience the same number of years in the Program (e.g., three years after application). In spring 2009, the last year evaluation data were collected, cohort 1 students who applied in 2004 (14 percent of the sample) could have used their scholarships for five years while cohort 2 students who applied a year later (86 percent of the sample) could only have used their scholarship for four years. For this reason, we refer to impacts as “after at least four years” since a small

portion of the sample—both treatment and control—were in the study a year longer.²² Another important difference is that for the first time we are able to estimate the impacts of the Program on educational attainment. Most students who applied to the Program were in grades K-5. But by 2009, 22 percent of the impact sample (approximately 500 students) had aged to the point that they could have completed 12th grade and graduated from high school. This number of students was sufficient to reliably estimate impacts on this outcome; this is the first time random assignment has been used to estimate the causal relationship between a school voucher program (or private schooling) and educational attainment, thus providing a more rigorous estimate than previous studies that have addressed this issue. There are some limitations to this analysis, however: it is based on parent reports rather than school administrative records, and it represents a relatively small share of the study sample.

As with previous experimental analyses of the impacts of voucher or voucher-like programs (e.g. Howell et al. 2002), the estimates of the impact of the scholarship offer, called “intent-to-treat” or ITT, include in the treatment average the outcomes for all scholarship recipients who provided outcome data, including recipients who never used their scholarships. In other words, scholarship “decliners” remain in the study, as full members of the treatment group, for the purposes of generating these experimental estimates of the impact of the scholarship offer on student outcomes.

Because the RCT approach has the important feature of generating comparable treatment and control groups, we used a common set of analytic techniques, designed for use in social experiments, to estimate the Program’s impact on test scores and the other outcomes listed above. These analyses began with the estimate of simple mean differences using the following equation, illustrated using the test score of student i in year t (Y_{it}):

$$(1) Y_{it} = \alpha + \tau T_{it} + \varepsilon_{it} \quad \text{if } t > k \text{ (period after Program takes effect),}$$

where T_{it} is equal to 1 if the student *has the opportunity to participate* in the OSP (i.e., the award rather than the actual use of the scholarship) and is equal to 0 otherwise. Equation (1) therefore estimates the effect of the **offer** of a scholarship on student outcomes. Under this ITT model, all students who were randomly assigned by virtue of the lottery are included in the analysis, regardless of whether a member of the treatment group used the scholarship to attend a private school or for how long.

²² Combining the two cohorts in this way was necessary to ensure that the sample size (number of students) for analysis was sufficient to detect impacts of a policy-relevant size and to provide results that could be applied to both cohorts. We were unable to collect data from cohort 1 in their fourth year after application because the legislative decision to extend the OSP and the evaluation came too late.

Proper randomization renders experimental groups approximately comparable, but not necessarily identical. In the current study, some modest differences, almost all of which are not significant, exist between the treatment group and the control group counterfactual at baseline (Wolf et al. 2007, p. 13). The basic regression model can, therefore, be improved by adding controls for observable baseline characteristics to increase the reliability of the estimated impact by accounting for minor differences between the treatment and control groups at baseline and improving the precision of the overall model. This yields the following equation to be estimated:

$$(2) Y_{it} = \alpha + \tau T_{it} + X_i \gamma + \delta_R R_{it} + \delta_M M_{it} + \varepsilon_{it}.$$

where X_i is a vector of student and/or family characteristics measured at baseline and known to influence future academic achievement, and R_{it} and M_{it} refer to **baseline** reading and mathematics scores, respectively.²³ In this model, τ —the parameter of sole interest—represents the effect of scholarships on test scores for students in the Program, conditional on X_i and the baseline test scores. The δ 's reflect the degree to which test scores are, on average, correlated over time. With a properly designed RCT, baseline test scores and controls for observable characteristics that predict future achievement should improve the precision of the estimated ITT impact.

Impact of Using a Scholarship

To estimate the magnitude of the impact of actually using a scholarship, if offered one, we employed conventional Bloom adjustments (Bloom 1984), re-scaling the ITT impacts over the subgroup of treatment members who actually used their scholarships. For the scholarship awardees in the OSP impact sample that provided final year outcome test scores, 86 percent had used a scholarship for all or part of the four or five years after they were offered a scholarship by lottery. We view these “ever users” as the proper focus of the IOT analysis because doing so requires that we make no assumptions regarding how much exposure to the OSP treatment actually counts. Treatment group members are classified as users if they ever used a scholarship for any amount of time. The 14 percent of the treatment students in the final year respondent sample who did not use their scholarships are treated the same as scholarship users for purposes of determining the effect of the offer of a scholarship, so as to preserve the integrity of

²³ The consistent set of covariates used to generate impact estimates were: student's baseline reading scale score, student's baseline math scale score, student attended a school designated SINI 2003-05 indicator, student's age (in months) at the time of scholarship application, student's forecasted entering grade for the next school year, student's gender, student's race (African-American indicator), special needs indicator, mother employed part-time or full-time indicator, household income, total number of children in student's household, the number of months the family has lived at its current address, and the number of days from September 1 to the date of outcome testing for each student. Some missing baseline data were imputed by fitting stepwise models to each covariate using all of the available baseline covariates as potential predictors.

the random assignment, even though scholarship decliners likely experienced no impact from the Program. Fortunately, there is a way to estimate the impact of the OSP on the average participant who actually used a scholarship, or what we refer to as the IOT estimate. This approach does not require information about why 14 percent of the individuals declined to use the scholarship when awarded, or how they differ from other families and children in the sample. But if one can assume that decliners experience zero impact from the scholarship Program, which seems reasonable given that they did not use the scholarship, it is possible to avoid these kinds of assumptions about (or analyses of) selection into and out of the Program.

This is possible by using the original comparison of **all** treatment group members to **all** control group members (i.e., the ITT estimates described above) but re-scaling it to account for the fact that a known fraction of the treatment group members did not actually avail themselves of the treatment and therefore experienced zero impact from the treatment. The average treatment impact that was generated from a mix of treatment users and nonusers is attributed only to the treatment users, by dividing the average treatment impact by the proportion of the treatment group who used their scholarships. For this report, depending on the specific outcome being rescaled, this “Bloom adjustment” (Bloom 1984) will increase the size of the ITT impacts that were statistically significant by 14-72 percent, since the percentage of treatment users among the population of students that provided valid scores on the various test and survey outcomes ranged from 58-88 percent.²⁴

Subgroup Analyses

The main focus of this study was on the impacts of the OSP on the overall group of students who were randomly assigned. The study provides additional consideration of the programmatic impacts on policy-relevant subgroups of students. The subgroups were designated prior to data collection and include students who were attending SINI versus non-SINI schools at application, those performing relatively higher or lower at baseline, and girls or boys. Earlier reports also considered elementary versus high school students, and those from application cohort 1 or cohort 2.

²⁴ Additionally, the data suggest that 2.9 percent of the control group were likely able to enroll in a private school because of the existence of the OSP. This hypothesis is derived from the fact that 20.2 percent of the control group students without treatment siblings are attending private schools, whereas 23.1 percent of the control group overall is in private schools. Since the 20.2 percent rate for controls without treatment siblings could not have been influenced by “Program-enabled crossover,” we subtract that “natural crossover rate” from the overall rate of 23.1 percent to arrive at the hypothesized Program-enabled crossover rate of 2.9 percent. To adjust for the fact that this small component of the control group may have actually received the private-schooling treatment by way of the Program, the estimates of the impact of scholarship use include a “double-Bloom” adjustment.⁴² We rescale the pure ITT impacts by an amount equal to the treatment decliner rate (~15 percent), as described above plus the estimated Program-enabled crossover rate (~2.9 percent) to generate the IOT estimates.

Previous Analyses

The evaluation of the impact of the OSP is a longitudinal study, in that it tracks the outcomes of a sample of students over multiple years of their potential participation in the Program. Three earlier reports described impacts one, two, and three years after students applied to the OSP and were randomly assigned by lottery to either the treatment or control group.²⁵

Leading up to this report we found that after two and three years, the full sample of students awarded scholarships scored significantly higher in reading achievement. Students awarded scholarships performed significantly higher in mathematics after one year, but scored at similar levels in math after two and three years. Across the first three years, parents consistently rated their child's school as safer and gave it a higher grade if their child was offered or used a scholarship. There were no impacts on students' own views of school safety and satisfaction during the first three years.

Impacts on Attainment

The evaluation considers the outcome of educational attainment because the OSP statute requires such an assessment,²⁶ and prior research has linked enrollment in private schools, particularly Catholic schools, to higher rates of high school graduation as well as higher college enrollment rates (Evans and Schwab 1995; Grogger and Neal 2000; Neal 1997; Warren 2010). This evaluation is the first to use random assignment to estimate the causal relationship between a school voucher program or private schooling and educational attainment, thus providing a more rigorous estimate than previous studies.

Educational attainment is most commonly measured as achievement of certain educational milestones, most notably high school graduation, college enrollment, and college graduation. Because 51 percent of the impact sample applied to enter the Program in grades K-5, and another 31 percent applied to enter grades 6-8, by four or five years later few of these students were potential candidates for high school graduation. However, combining those older middle school applicants with the 18 percent of the impact sample that entered the Program in grades 9-12 provided sufficient data to determine the impact of the OSP on high school graduation rates. There were too few students in the sample, however, for whom college enrollment could be confirmed at the time of data analysis.

²⁵ See Wolf et al. 2007, table ES-2, Wolf et al. 2008, xxii-xxiv, Wolf et al. 2009, xxvi-xxx.

²⁶ Title III of Division C of the Consolidated Appropriations Act, 2004, P.L. 108-199, Section 309(4)D.

This attainment analysis focused on students in the impact sample who were forecasted to have been seniors in high school during or before the 2008-09 school year and therefore had the opportunity to graduate prior to the summer of 2009.²⁷ A parent follow-up survey asked parents whether these students had received high school diplomas by the end the 2008-09 school year. Differences in parental reports of student educational attainment between the treatment and control groups were then measured via the evaluation's standard regression method of analysis.

The attainment impact analysis revealed (table 6):

- The offer of an OSP scholarship raised students' probability of graduating by 12 percentage points. The high school graduation rate was 82 percent for the treatment group compared to 70 percent for the control group. Using a scholarship increased the graduation rate by 21 percentage points.
- A positive impact of 13 percentage points for students who came from SINI 2003-05 schools, the subgroup of students for whom the statute gave top priority. The high school graduation rate was 79 percent for the SINI 2003-05 members of the treatment group compared to 66 percent for the SINI 2003-05 members of the control group. Using a scholarship increased the graduation rate by 20 percentage points.
- The offer of an OSP scholarship led to a positive impact for students who applied to the Program with relatively higher levels of academic performance (14 percentage points) and female students (20 percentage points). Using a scholarship to attend a participating private school increased the graduation rate by 25 and 28 percentage points, respectively.
- There was no statistically significant evidence of impacts on graduation rates for students who applied to the Program from not SINI 2003-05 schools, with relatively lower levels of academic performance, and male students.²⁸

²⁷ As such, the sample for the impact analysis of the OSP on educational attainment differs from the overall impact sample used for other elements of the evaluation in that it is limited to older applicants. The attainment experiences of impact sample students below grade 8 at baseline are as yet unknowable and could conceivably differ from those reported for the older applicants here.

²⁸ None of the subgroup interaction effects themselves were statistically significant, which means that we cannot say with confidence that the impact of the OSP on educational attainment was different across the various subgroup pairs.

Table 6. Impact Estimates of the Offer and Use of a Scholarship on Students Forecasted in Grade 12 or Above by 2008-09: Percent with High School Diploma, 2008-09

| High School Diploma | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | <i>p</i> -value of estimates |
|---------------------|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| Full sample | .82 | .70 | .12*** | .26 | .21*** | .46 | .01 |
| SINI 2003-05 | .79 | .66 | .13** | .28 | .20** | .43 | .01 |
| Not SINI 2003-05 | .89 | .82 | .07 | .19 | .21 | .54 | .46 |
| Difference | -.10 | -.16 | .06 | .13 | | | .59 |
| Lower performance | .60 | .49 | .12 | .23 | .20 | .40 | .12 |
| Higher performance | .93 | .79 | .14** | .35 | .25** | .61 | .02 |
| Difference | -.33 | -.30 | -.03 | -.06 | | | .80 |
| Male | .71 | .66 | .07 | .14 | .14 | .30 | .26 |
| Female | .95 | .75 | .20*** | .46 | .28*** | .65 | .01 |
| Difference | -.24 | -.08 | -.15 | -.34 | | | .18 |

**Statistically significant at the 95 percent confidence level.

***Statistically significant at the 99 percent confidence level.

NOTES: Means are regression adjusted using a consistent set of baseline covariates. Impact estimates are reported as marginal effects. Effect sizes are in terms of standard deviations. Valid $N = 316$, including SINI 2003-05 $N = 231$, Not SINI 2003-05 $N = 85$, Lower performance $N = 105$, Higher performance $N = 211$, Male $N = 167$, Female $N = 149$. Sample weights used.

Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. High school graduation determined via parental self-reports.

Impacts on Student Achievement

The mandate to assess the Program’s effect on student test scores is consistent with the stated purpose of the Program and the priority Congress placed on having the OSP serve students from low-performing schools. Academic achievement as a measure of Program success is also well aligned with parents’ stated priorities in choosing schools (Wolf et al. 2005, p. C-7).

The primary analysis indicates a statistically significant overall impact of the Program on reading achievement after at least four years, but not in math. That is, the average reading test scores of the treatment group as a whole were significantly higher than those of the control group as a whole. Students scored an average of 3.9 scale score points higher (2.8 months of additional learning) in reading than students in the control group. The calculated impact of using a scholarship was 4.8 scale score points (3.4 months of additional learning) (tables 7 and 9).²⁹

²⁹ As in previous years, we re-calculated achievement impacts using two alternate estimation approaches. The first approach used exactly the same analytic approach as the main analysis but applied that approach to a sample of respondents for which enough of the “latest-to-respond” treatment group students were excluded from the sample so that the treatment group response rate exactly matched the pre-subsample control group response rate of 63.5 percent. The second approach used the exact same

Table 7. Impact Estimates of the Offer and Use of a Scholarship on the Full Sample: Academic Achievement, 2008-09

| Student Achievement | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | <i>p</i> -value of estimates |
|---------------------|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| Reading | 649.15 | 645.24 | 3.90* | .11 | 4.75* | .13 | .06 |
| Math | 644.06 | 643.36 | .70 | .02 | .85 | .03 | .71 |

*Statistically significant at the 90 percent confidence level.

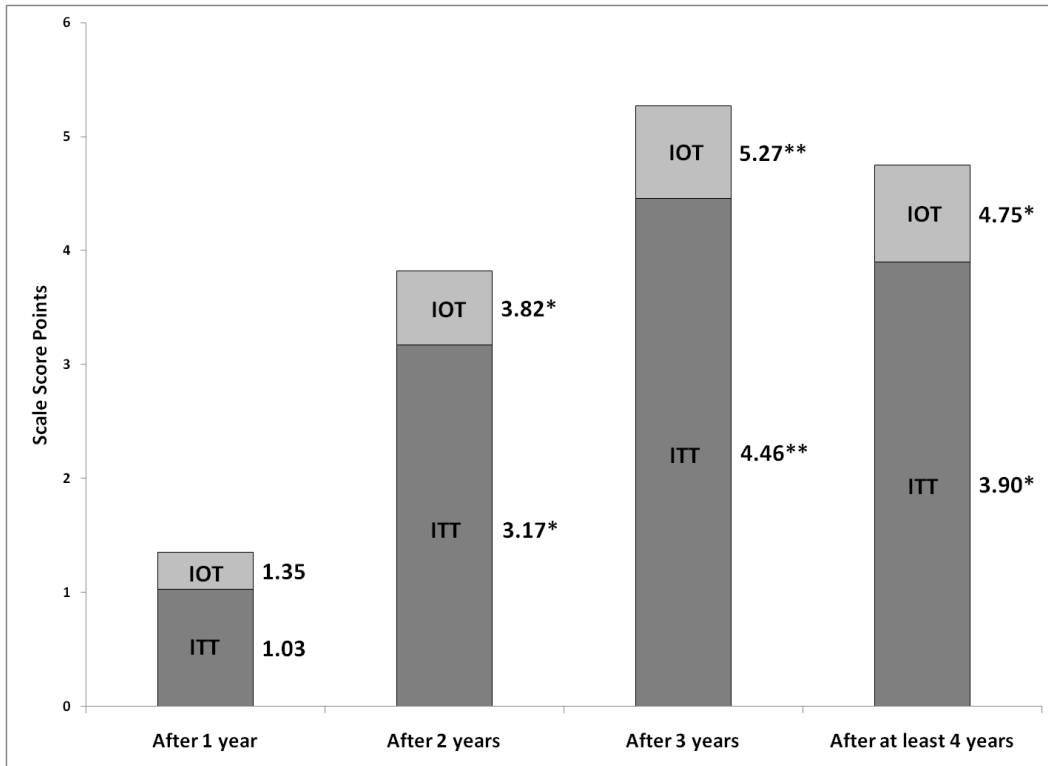
NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Impacts are displayed in terms of scale scores. Effect sizes are in terms of standard deviations. Valid *N* for reading = 1,328; math = 1,330. Separate reading and math sample weights used.

The achievement impacts at least four years after random assignment can be viewed clearly and placed in the context of impacts estimated in prior years in figures 7 and 9.³⁰

respondent sample as the main analysis but generated an alternative set of robust standard errors by clustering on the school the student attended instead of on the student’s family. The two sensitivity tests yielded results that were consistent with the main analysis. The overall impact estimate for reading was larger (4.80 scale score points) and statistically significant when drawn from the trimmed sample; the overall reading estimate was also statistically significant when the statistical model was modified to control for similarities among students who attended the same school as opposed to controlling for similarities among students who are from the same family.

³⁰ The scale score mean and standard deviation (SD) for the SAT-9 norming population varies by grade and is 463.8 (SD = 38.5) for kindergarteners tested in the spring, compared to 652.1 (SD = 39.1) for fifth graders and 703.6 (SD = 36.5) for students in 12th grade. Because scale scores on a given test are vertically equated, it is possible to combine observations across grades in the estimation of overall test score impacts. The estimation of those impacts, through regression equations, and the calculation of effect sizes produce average effects and effect sizes across the entire group that are weighted by the proportion of students in each grade

Figure 7. Impact of the OSP on Reading Achievement Overall, by Years After Application



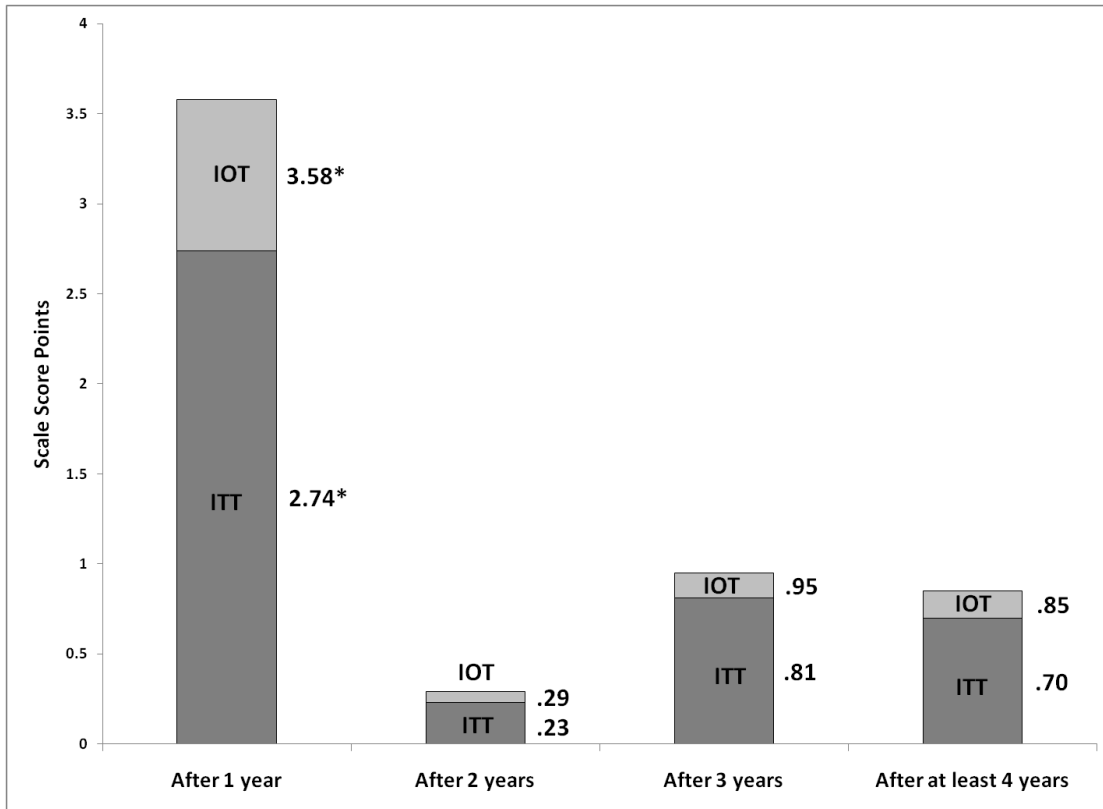
* Statistically significant at the 90 percent confidence interval.

** Statistically significant at the 95 percent confidence interval.

NOTE: Differences between each year's cumulative impact estimates have not been tested to determine their statistical significance.³¹

³¹ The samples of students who provided outcome data each year varied somewhat. Therefore, readers are cautioned not to draw conclusions about differences in impacts across the years. The difference in samples across the years was greatest between the sample after three years and the sample at least four years, due to the fact that 296 students had graded-out of the study at least four years after random assignment.

Figure 8. Impact of the OSP on Math Achievement Overall, by Years After Application



* Statistically significant at the 90 percent confidence interval.

NOTE: Differences between each year's cumulative impact estimates have not been tested to determine their statistical significance.

Subgroup Impacts on Student Achievement

The offer of a scholarship, and the use of a scholarship, had a statistically significant positive impact on reading achievement at least four years after random assignment for one-half of the student subgroups, including at least two subgroups who applied with a relative advantage in academic preparation.³² There were no impacts on math achievement for any of the six subgroups examined, as was true for the full impact sample (tables 8 and 9). The subgroups with positive reading impacts include:

- Students in the treatment group who had not attended a SINI 2003-05 school prior to the Program. These students scored an average of 5.8 scale score points higher (3.5 months of additional learning) in reading than students in the control group from not SINI 2003-05 schools (the impact of the offer of a scholarship); the calculated impact of using a scholarship for this group was 7.0 scale score points (4.2 months of additional learning).
- Students in the treatment group who entered the Program in the higher two-thirds of the applicant test-score performance distribution scored an average of 5.2 scale score points higher in reading (3.9 months of additional learning) than similar students in the control group; the impact of using a scholarship for this group was 6.1 scale score points (4.6 months of additional learning).
- Female students in the treatment group, who scored an average of 5.3 scale score points higher in reading (3.4 months of additional learning) than female students in the control group; the impact of using a scholarship was 6.2 scale score points (4.0 months of additional learning).

There was no evidence of a subgroup impact in reading for students who applied from a school designated SINI between 2003 and 2005. The analysis also did not show evidence of subgroup impacts for students who entered the Program in the lower one-third of the applicant test-score performance distribution or for male students.

³² The subgroup reading impacts were confirmed by the sensitivity tests in all cases.

Table 8. Impact Estimates of the Offer and Use of a Scholarship on Subgroups At Least Four Years After Application: Academic Achievement

| Reading | | | | | | | |
|--------------------------------|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|----------------------|
| Student Achievement: Subgroups | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | p-value of estimates |
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| SINI 2003-05 | 657.49 | 656.41 | 1.08 | .03 | 1.33 | .04 | .76 |
| Not SINI 2003-05 | 643.25 | 637.45 | 5.80** | .16 | 6.99** | .19 | .02 |
| Difference | 14.24 | 18.96 | -4.72 | -.13 | | | .27 |
| Lower performance | 629.45 | 628.27 | 1.18 | .04 | 1.54 | .05 | .74 |
| Higher performance | 657.79 | 652.61 | 5.18** | .15 | 6.08** | .18 | .04 |
| Difference | -28.34 | -24.34 | -4.00 | -.11 | | | .35 |
| Male | 642.78 | 640.33 | 2.45 | .07 | 3.07 | .09 | .44 |
| Female | 654.64 | 649.38 | 5.27** | .15 | 6.24** | .18 | .05 |
| Difference | -11.86 | -9.05 | -2.81 | -.08 | | | .50 |

| Math | | | | | | | |
|--------------------------------|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|----------------------|
| Student Achievement: Subgroups | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | p-value of estimates |
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| SINI 2003-05 | 656.67 | 657.05 | -.38 | -.01 | -.47 | -.02 | .90 |
| Not SINI 2003-05 | 635.31 | 633.89 | 1.42 | .04 | 1.71 | .05 | .56 |
| Difference | 21.36 | 23.16 | -1.80 | -.05 | | | .65 |
| Lower performance | 632.39 | 631.16 | 1.24 | .04 | 1.61 | .05 | .71 |
| Higher performance | 649.08 | 648.59 | .49 | .01 | .58 | .02 | .83 |
| Difference | -16.69 | -17.44 | .75 | .02 | | | .85 |
| Male | 639.59 | 640.70 | -1.11 | -.04 | -1.38 | -.04 | .68 |
| Female | 648.04 | 645.64 | 2.40 | .07 | 2.84 | .08 | .37 |
| Difference | -8.44 | -4.94 | -3.50 | -.10 | | | .35 |

**Statistically significant at the 95 percent confidence level.

NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Impacts are displayed in terms of scale scores. Effect sizes are in terms of standard deviations. Total valid *N* for Reading = 1,328, including: SINI 2003-05 *N* = 520, Not SINI *N* = 808, Lower performance *N* = 435, Higher performance *N* = 893, Male *N* = 649, Female *N* = 679. Total Valid *N* for Math = 1,330, including SINI 2003-05 *N* = 516, Not SINI 2003-05 *N* = 814, Lower performance *N* = 435, Higher performance *N* = 895, Male *N* = 649, Female *N* = 681.

Table 9. Estimated Impacts in Months of Schooling From the Offer and Use of a Scholarship for Statistically Significant Reading Impacts After At Least Four Years

| Student Achievement: Reading | Impact of the Scholarship Offer (ITT) | | Impact of Scholarship Use (IOT) | |
|---------------------------------|---------------------------------------|---------------------|---------------------------------|---------------------|
| | Effect Size | Months of Schooling | Effect Size | Months of Schooling |
| Full sample | .11 | 2.77 | .13 | 3.38 |
| Not SINI 2003-05 | .16 | 3.49 | .19 | 4.21 |
| Higher performance | .15 | 3.90 | .18 | 4.58 |
| Female | .15 | 3.37 | .18 | 4.00 |

NOTES: Treatment impacts were converted to months based upon the average monthly increase in reading scale scores for the control group across all years.

Impacts on Reported Safety and an Orderly School Climate

School safety is a valued feature of schools for the families who applied to the OSP. A total of 17 percent of cohort 1 parents at baseline listed school safety as their most important reason for seeking to exercise school choice—second only to academic quality (48 percent) among the available reasons (Wolf et al. 2005, p. C-7). A separate study of why and how OSP parents choose schools, which relied on focus group discussions with participating parents, found that school safety was among their most important educational concerns (Stewart, Wolf, and Cornman 2005, p. v).

There are no specific tests to evaluate the safety of a school as there are for evaluating student achievement. There are various indicators of the relative orderliness of the school environment, such as the presence or absence of property destruction, cheating, bullying, and drug distribution, to name a few. Students and parents can be surveyed regarding the extent to which such indicators of disorder are or are not a problem at their or their child’s school. The responses then can be consolidated into an index of safety and an orderly school climate and analyzed, as we do here and has been done in other school choice studies.

Parent Self-Reports

Overall, the parents of students offered an Opportunity Scholarship in the lottery subsequently reported their child’s school to be safer and more orderly than did the parents of students in the control group. The impact of the offer of a scholarship on parental perceptions of safety and an orderly school climate was .48 on a 10-point index of indicators of school safety and orderliness, an effect size of 0.14 standard deviations (table 10). The impact of using a scholarship was .58 on the index, with an effect size of .17 standard deviations.

Table 10. Impact Estimates of the Offer and Use of a Scholarship on the Full Sample and Subgroups: Parent Perceptions of Safety and an Orderly School Climate, 2008-09

| Safety and an Orderly School Climate: Parents | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | |
|---|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | <i>p</i> -value of estimates |
| Full sample | 7.94 | 7.47 | .48** | .14 | .58** | .17 | .02 |
| SINI 2003-05 | 7.45 | 7.35 | .10 | .03 | .13 | .04 | .77 |
| Not SINI 2003-05 | 8.28 | 7.55 | .73*** | .22 | .88*** | .27 | .01 |
| Difference | -.83 | -.20 | -.63 | -.19 | | | .16 |
| Lower performance | 7.84 | 7.36 | .48 | .14 | .61 | .18 | .20 |
| Higher performance | 7.99 | 7.51 | .48* | .15 | .57* | .17 | .06 |
| Difference | -.15 | -.15 | -.00 | -.00 | | | .99 |
| Male | 8.05 | 7.67 | .38 | .12 | .48 | .14 | .20 |
| Female | 7.87 | 7.31 | .56* | .17 | .67* | .20 | .06 |
| Difference | .18 | .36 | -.18 | -.05 | | | .68 |

*Statistically significant at the 90 percent confidence level.

**Statistically significant at the 95 percent confidence level.

***Statistically significant at the 99 percent confidence level.

NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Effect sizes are in terms of standard deviations. Valid $N = 1,224$, including: SINI 2003-05 $N = 472$, Not SINI 2003-05 $N = 752$, Lower performance $N = 400$, Higher performance $N = 824$, Male $N = 597$, Female $N = 627$. Parent survey weights were used.

This impact of the offer of a scholarship on parental perceptions of safety and an orderly school climate was observed for three of the subgroups of students examined: parents of students from not SINI 2003-05 schools experienced a positive increase of .22 standard deviations, parents of students who entered the Program in the higher two-thirds of the applicant test-score performance distribution experienced a positive increase of .15 standard deviations, and female students experienced a positive increase of .17 standard deviations (table 10).³³ There was no evidence of an impact on parents of students from SINI 2003-05 schools, parents of students who entered the Program with relatively lower levels of academic achievement, and parents of male students (table 10).

Student Self-Reports

The students in grades 4-12 who completed surveys paint a different picture about school safety at their school than do their parents. While parent safety was a measure of parental perceptions, the student index of school climate and safety asked students if they personally had been a victim of theft, drug-dealing, assaults, threats, bullying, or taunting or had observed weapons at school. On average,

³³ This impact of the offer of a scholarship on parental perceptions and student reports of safety and an orderly school climate, in the overall sample and for subgroups, were confirmed by the sensitivity tests in all cases.

reports of school climate and safety by students offered scholarships through the lottery were not statistically different from those of the control group (table 11). That is, there was no evidence of an impact from the offer of a scholarship or the use of a scholarship on students' reports. No statistically significant findings were evident across the subgroups analyzed.

Table 11. Impact Estimates of the Offer and Use of a Scholarship on the Full Sample and Subgroups: Student Reports of Safety and an Orderly School Climate, 2008-09

| Safety and an Orderly School Climate: Students | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | <i>p</i> -value of estimates |
|--|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| Full sample | 6.16 | 6.01 | .15 | .07 | .18 | .09 | .33 |
| SINI 2003-05 | 6.27 | 6.01 | .26 | .12 | .32 | .15 | .21 |
| Not SINI 2003-05 | 6.08 | 6.01 | .06 | .03 | .08 | .04 | .76 |
| Difference | .19 | -.00 | .20 | .10 | | | .51 |
| Lower performance | 6.08 | 5.80 | .28 | .12 | .37 | .17 | .36 |
| Higher performance | 6.19 | 6.11 | .09 | .04 | .10 | .05 | .62 |
| Difference | -.11 | -.31 | .20 | .10 | | | .58 |
| Male | 6.04 | 5.93 | .12 | .05 | .14 | .06 | .66 |
| Female | 6.25 | 6.07 | .18 | .09 | .21 | .11 | .32 |
| Difference | -.21 | -.15 | -.06 | -.03 | | | .85 |

NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Effect sizes are in terms of standard deviations. Valid $N = 1,054$, including: SINI 2003-05 $N = 502$, Not SINI 2003-05 $N = 552$, Lower performance $N = 333$, Higher performance $N = 721$, Male $N = 505$, Female $N = 549$. Student survey weights were used. Survey given to students in grades 4-12.

Impacts on School Satisfaction

Economists have long used customer satisfaction as a proxy measure for product or service quality (see Johnson and Fornell 1991). While not specifically identified as an outcome to be studied, it is an indicator of the “success of the Program in expanding options for parents,” which Congress asked the evaluation to consider.³⁴ Satisfaction is also an outcome studied in the previous evaluations of K-12 scholarship programs, all of which concluded that parents tend to be significantly more satisfied with their child’s school if they have had the opportunity to select it (see Greene 2001, pp. 84-85). Satisfaction of both parents and students was measured by the percentage that assigned a grade of A or B to their child’s or their school.

³⁴ Section 309 of the District of Columbia School Choice Incentive Act of 2003.

Parent Self-Reports

At least four years after random assignment, parents overall were more satisfied with their child's school if they had been offered a scholarship and if their child used a scholarship to attend a participating private school. A total of 76 percent of treatment parents assigned their child's school a grade of A or B in 2009 compared with 68 percent of control parents—a difference of 8 percentage points (impact of the offer of a scholarship) (table 12); the impact of using a scholarship was a difference of 10 percentage points in parent's likelihood of giving their child's school a grade of A or B. The effect sizes of these impacts were .18 and .22, respectively.

For each of the six subgroups of parents, those in the treatment group were more satisfied than their counterparts in the control group. These differences, however, were not statistically significant at the subgroup level for parents of scholarship students from SINI 2003-05 schools or who were male — two groups that also did not demonstrate significant achievement gains from the Program, though the SINI 2003-05 students did show significant benefits in terms of educational attainment. Parents of students from not SINI 2003-05 schools, parents of students who had higher and lower test-score performance at baseline, and parents of female students were significantly more likely to give their child's school a grade of A or B if they were in the treatment group. The effect sizes ranged from .17 to .27 standard deviations for the offer of, and from .20 to .32 standard deviations for the use of, a scholarship in these groups.³⁵

³⁵ The school satisfaction impacts estimated for both parents and students, in the overall sample and for subgroups, were confirmed by the sensitivity tests in all cases.

Table 12. Impact Estimates of the Offer and Use of a Scholarship on the Full Sample and Subgroups: Parent Reports of Satisfaction with Their Child’s School, 2008-09

| Parents Who Gave Their School a Grade of A or B | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | <i>p</i> -value of estimates |
|---|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| Full sample | .76 | .68 | .08*** | .18 | .10*** | .22 | .00 |
| SINI 2003-05 | .69 | .66 | .03 | .07 | .04 | .08 | .51 |
| Not SINI 2003-05 | .81 | .69 | .12*** | .27 | .15*** | .32 | .00 |
| Difference | -.12 | -.02 | -.10 | -.21 | | | .12 |
| Lower performance | .70 | .61 | .10** | .20 | .12** | .25 | .05 |
| Higher performance | .79 | .71 | .08** | .17 | .09** | .20 | .03 |
| Difference | -.08 | -.10 | .02 | .04 | | | .75 |
| Male | .75 | .67 | .08* | .17 | .10* | .21 | .06 |
| Female | .77 | .68 | .09** | .19 | .10** | .22 | .03 |
| Difference | -.02 | -.01 | -.01 | -.01 | | | .90 |

*Statistically significant at the 90 percent confidence level.

**Statistically significant at the 95 percent confidence level.

***Statistically significant at the 99 percent confidence level.

NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Impact estimates are reported as marginal effects. Effect sizes are in terms of standard deviations. Valid $N = 1,227$, including: SINI 2003-05 $N = 475$, Not SINI 2003-05 $N = 752$, Lower performance $N = 402$, Higher performance $N = 825$, Male $N = 594$, Female $N = 633$. Parent survey weights were used.

Student Self-Reports

As was true with the school safety and climate measures, students had a different view of their schools than did their parents. At least four years after random assignment, there were no significant differences between the treatment group and the control group in their likelihood of assigning their schools a grade of A or B (table 13).³⁶ Student reports of school satisfaction were statistically similar between the treatment and control groups for all six subgroups examined.³⁷

³⁶ Only students in grades 4-12 were administered surveys, so the satisfaction of students in early elementary grades is unknown.

³⁷ These results were confirmed by both sensitivity tests.

Table 13. Impact Estimates of the Offer and Use of a Scholarship on the Full Sample and Subgroups: Student Reports of Satisfaction with Their School, 2008-09

| Students Who Gave Their School a Grade of A or B | Impact of the Scholarship Offer (ITT) | | | | Impact of Scholarship Use (IOT) | | <i>p</i> -value of estimates |
|--|---------------------------------------|--------------------|-------------------------------|-------------|---------------------------------|-------------|------------------------------|
| | Treatment Group Mean | Control Group Mean | Difference (Estimated Impact) | Effect Size | Adjusted Impact Estimate | Effect Size | |
| Full sample | .69 | .72 | -.03 | -.06 | -.03 | -.07 | .43 |
| SINI 2003-05 | .66 | .66 | .00 | .00 | .00 | .00 | 1.00 |
| Not SINI 2003-05 | .71 | .76 | -.05 | -.12 | -.06 | -.15 | .29 |
| Difference | -.05 | -.10 | .05 | .11 | | | .46 |
| Lower performance | .71 | .67 | .04 | .08 | .05 | .11 | .54 |
| Higher performance | .68 | .73 | -.06 | -.13 | -.07 | -.16 | .14 |
| Difference | .04 | -.06 | .09 | .20 | | | .18 |
| Male | .70 | .73 | -.03 | -.07 | -.04 | -.09 | .55 |
| Female | .68 | .71 | -.02 | -.05 | -.03 | -.06 | .62 |
| Difference | .01 | .02 | -.01 | -.02 | | | .91 |

NOTES: Results are for cohort 1 five years after random assignment and cohort 2 four years after random assignment. Means are regression adjusted using a consistent set of baseline covariates. Impact estimates are reported as marginal effects. Effect sizes are in terms of standard deviations. Valid $N=1,001$, including: SINI 2003-05 $N=478$, Not SINI 2003-05 $N=523$, Lower performance $N=319$, Higher performance $N=682$, Male $N=480$, Female $N=521$. Student survey weights were used. Survey given to students in grades 4-12.

Conclusions

The results of this analysis of a school choice program emerged from an evaluation structured as a Randomized Control Trial (RCT) at least four years after random assignment to the treatment (offer of a scholarship) or control (not offered) group. Because mere chance determined whether eligible applicants received the treatment of a scholarship offer or a place in the control group, any subsequent differences between the outcomes of the two groups that are statistically significant can be attributed to the program intervention.

On average, students are performing higher in reading if they were offered an Opportunity Scholarship. However, the impact of the OSP on student test scores in math is not significantly different from zero. Parents are much more satisfied with their children’s school and view it as safer as a result of the scholarship offer.

The OSP had a significant positive impact on parent-reported high school graduation rates. Overall, 82 percent of those offered scholarships graduated compared to 70 percent of those who were not offered scholarships, a difference of 12 percentage points. The impact on graduation of actually using a scholarship to attend private school was 21 percentage points. Similar benefits extended to the high-

priority SINI 2003-05 students, those who were higher performing when they entered the Program, and female participants. These results provide support for prior research suggesting that private schools provide students with an educational climate that encourages school completion either through the faculty and school environment or by having similarly motivated and achieving peers (Evans and Schwab 1995; Grogger and Neal 2000; Neal 1997; Warren 2010).

It is important to note that the findings regarding the impacts of the OSP reflect the particular Program elements that evolved from the law passed by Congress and the characteristics of students, families, and schools—public and private—that exist in the Nation’s capital. The same program implemented in another city could yield different results, and a different scholarship program in Washington, DC, might also produce different outcomes.

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