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Evaluation of the DC Opportunity Scholarship Program

Second Year Report on Participation

ies NATIONAL CENTER FOR EDUCATION EVALUATION AND REGIONAL ASSISTANCE
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Evaluation of the DC Opportunity Scholarship Program

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1. Introduction

By fall 2005, the DC Opportunity Scholarship Program (OSP), the first federally funded voucher program in the United States, was in its second year serving low-income students in the nation's capital. More than 5,800 students have applied to the Program over the 2 years, and about 2,300 of them—eligible public school students who participated in a lottery to determine scholarship award—are the subject of a rigorous impact evaluation mandated by the Program statute. While the most important questions for the evaluation are about the Program's effectiveness in improving student outcomes, data are still being collected for that analysis and will be presented in a 2007 report. This document from the study team provides a brief update to the first report to Congress[1] by describing the schools and students who applied to and became participants in the Program for the 2005-06 school year. The analysis indicates that by fall 2005, the Program was operating at capacity, with more than 1,700 students using scholarships at 60 of 68 participating private schools.

1.1 The Program

The *District of Columbia School Choice Incentive Act of 2003* was passed by Congress in January 2004. The Act provided funds for District of Columbia Public Schools' (DCPS) improvement activities and charter school facility acquisitions. Most notably, the statute established what is now called the DC Opportunity Scholarship Program—the first Federal government initiative to provide K-12 education scholarships, or vouchers, to families to send their children to private schools of choice.

The DC Opportunity Scholarship Program has the following programmatic elements:

- 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 of choice.
- 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 schools they are attending.
- If there are more eligible applicants than available scholarships or open slots in private schools, applicants are to be awarded scholarships and admission to private schools by random selection, for example, by lottery.
- In making these 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.
- Private schools participating in the Program must be located in the District and must agree to Program requirements regarding nondiscrimination in admissions, fiscal accountability, and cooperation with the evaluation.

[1] To access the first evaluation report, see http://www.ed.gov/ies/ncee.

Implementation of the OSP, as analyzed in this report, took place over an 18-month period from April 2004 to September 2005. In late March 2004, the Washington Scholarship Fund (WSF), a 501(c)3 organization in the District of Columbia, was selected by the U.S. Department of Education (ED) to implement the OSP, under the supervision of the Office of Innovation and Improvement in ED and the Office of the Mayor of the District of Columbia. Since then, the WSF has worked with its implementation partners[2] to finalize the Program design, establish protocols, recruit applicants and schools, and place scholarship winners in participating private schools.

The funds appropriated for the OSP are sufficient to support approximately 1,700-1,800 students, depending on the cost of the participating private schools that they attend. Between students recruited during the first year who are continuing to use their scholarships and those awarded and using scholarships from the second year of recruitment, the Program is now operating at full capacity.

1.2 The Mandated Evaluation

The Act requires that this 5-year scholarship pilot Program be rigorously evaluated by an independent research team, using the "strongest possible research design for determining the effectiveness" of the Program and addressing a specific set of student comparisons and topics (Section 309):

- ***Impact Analysis***. Central to the evaluation is an impact analysis that compares outcomes of eligible applicants (students and their parents) from public schools randomly assigned to receive or not receive a scholarship through a lottery. Such random assignment experimental designs are widely viewed as the best methods for identifying the independent effect of programs on subsequent outcomes.[3] Thus, the impact analysis will be the source of the reliable, causal evidence on Program effectiveness called for in the legislation (see appendix A for a more comprehensive description of the evaluation and its technical approach).
- ***Performance Reporting.*** The Act also specifies a comparison of students participating in the scholarship Program with students in the same grades in the DCPS system, as a way of tracking general student progress and Program performance.[4] Such a comparison would draw upon what we call the "OSP recipient sample," which comprises all students offered a scholarship, including students who were already attending private schools at the point of application and public school

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[2] The WSF has joined with Capital Partners for Education, DC Parents for School Choice, and Fight for Children—all District-based nonprofit organizations, to assist in client recruitment and implementation activities.

[3] For examples, see the What Works Clearinghouse, *WWC Study Review Standards, 7* (http://www.whatworks.ed.gov/reviewprocess/studv_standards_final.pdf); Thomas D. Cook and Monique R. Payne, "Objecting to the Objections to Using Random Assignment in Educational Research," in *Evidence Matters: Randomized Trials in Education Research,* eds. Frederick Mosteller and Robert Baruch (Washington, DC: Brookings, 2002).

[4] DCPS students who did not apply to the scholarship Program are likely to be quite different from those who applied and are participating in the OSP—in ways we can observe and ways we cannot. Comparing outcomes between participants and nonapplicants is, therefore, not a reliable measure of Program effects.

applicants who were automatically awarded scholarships.[5,6] However, since the passage of the legislation and the first year of OSP implementation, DCPS has been in transition to a new academic assessment that differs from its earlier test, which the evaluation is required to use for its main outcomes measurement.[7] The divergence between the new DCPS assessment and the evaluation assessment means that comparing the academic performance of all scholarship recipients and other DCPS students is no longer possible, although this analysis was performed for students who participated in the Program's first year when the same assessment was used (see the first report to Congress).

- ***Response of Schools.*** Through descriptive analyses, the evaluation will assess how DC public and private schools are changing during the implementation of the OSP, in part by examining the extent to which the schools are experiencing significant losses or gains in student enrollment during this period.

1.3 Summary of Key Findings on Program Participation

Over the first 18 months of implementation that ended September 2005, applications were accepted in essentially two waves: in spring 2004 for fall 2004 enrollment, which we call "cohort 1," and through spring 2005 for fall 2005 enrollment, which we call "cohort 2." A total of 5,818 students applied, and 4,047 of them were deemed eligible for the OSP. By fall 2005, 2,454 students had been awarded scholarships, most by lottery because they were in grades for which there were more applicants than slots in participating private schools (table 1-1).

Table 1-1. OSP applicants by program status, cohorts 1 and 2

	Cohort 1	Cohort 2	Total
Applicants	2,692	3,126	5,818
Eligible applicants	1,848	2,199	4,047
In impact sample	492	1,816	2,308
Scholarship recipients	1,366	1,088	2,454
Scholarship users in initial year of receipt	1,027	797	1,824
Scholarship users in fall 2005	919	797	1,716

NOTES: Applicants entering grades 6-12 in cohort 2 who did not participate in baseline testing were not included in the eligible applicant figure. The initial year of receipt is fall 2004 for cohort 1 and fall 2005 for cohort 2.

SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

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[5] Automatic scholarship awards were given only in the first year of Program implementation to all students applying from public schools designated "in need of improvement" under the 2002 reauthorization of the *Elementary and Secondary Education Act* and to all public school applicants entering grades K-5 that were not oversubscribed and therefore not subject to award by lottery.

[6] The "recipient sample" is different from the "impact sample," which is limited to public school applicants who were subject to scholarship award by lottery and thus were randomly assigned to the "treatment" (scholarship) or "control" (nonscholarship) group.

[7] See Section 309(a)(3)(B) for the provision that stipulates that the evaluation use the same assessment as DCPS administered to public school students in the first year the OSP was operating. This requirement was intended to ensure that the impact analysis could be based on a consistent measure of student achievement and not subject to changes in the key outcome measure throughout the evaluation period.

In addition:

- **Private Schools.** Ten new schools agreed to participate in the Program in the second year of operation, raising the total from 58 who signed on in the first year to 68 in fall 2005. The new schools tend to have smaller class sizes, higher regular tuitions, and smaller minority populations and are less likely to be Catholic-affiliated than the population of schools that have participated in the OSP from the start (table 2-1). Of the 68 schools participating in year 2, 60 were serving scholarship students in fall 2005. The remaining eight schools had no OSP students in fall 2005. Although systematic data were not collected as to the reason(s) each school did not serve OSP students, program implementation staff reported that the most common reasons include that schools either: (1) determined that none of the current scholarship recipients met their entrance criteria, (2) had no scholarship recipients choose their school during the placement phase, or (3) filled their vacant slots before OSP recipients could be placed. [section 2]
- **Background of Applicants.** About 44 percent of total public school applicants to the Program came from a public school that was designated as SINI between 2003 and 2005. Eleven percent came from the worst performing public schools, those in which the percentage of students who reached the "proficient" benchmark on the DCPS assessment placed them in the bottom quartile of all DCPS schools. On the other hand, almost one-quarter of all applicants are from the highest performing (top quartile of) DCPS schools based on proficiency rates. [section 3]
- **Impact Sample.** A large subset of applicants during the first 2 years—2,308—were public school students who applied to be in grades for which there were more applicants than there were slots in participating private schools. Thus, a lottery determined whether they received a scholarship offer. These students were randomly assigned such that 1,387 received scholarships (treatment group) and 921 did not receive scholarships (the control group). They make up the "impact sample." Preliminary power analyses indicate that the impact sample is sufficient in number for the evaluation to be able to statistically detect meaningful and policy relevant differences in subsequent outcomes between the two groups. [section 3]
- **Characteristics of Treatment vs. Control Groups.** The treatment and control groups are statistically similar on all but 2 out of 15 important baseline characteristics that could be measured (table 3-5). In year 2, for students entering grades K-5, the average family income and years of mother's education are somewhat higher for the control group than for the treatment group. These differences are small and likely due to random chance, particularly since multiple small-scale lotteries were run for each grade band during year 2 in order to accommodate early and late applicants.[8] In estimating Program impacts, the evaluation will use baseline measures of student background factors to control for these pre-Program differences. [section 3]
- **Scholarship Use Rates.** Overall, 1,824 (74 percent) students who were awarded scholarships used them the initial year to attend a private school, although the rate was slightly lower for the impact sample of randomized public school applicants

[8] The smaller the number of students randomized at any time, the higher the odds of obtaining some statistical differences between the treatment and control groups.

(71 percent or 982 students). Lower use among the impact sample reflects two factors: (1) the group excludes students already attending private schools at the time of application, whose use rates are substantially higher than those for public school recipients and (2) the sample includes a higher proportion of older students, a group that was more constrained in their choice of schools under the Program and who experienced substantially lower use rates. Among cohort 1 students, there is an 8 percentage point decline in use between the first and second years of scholarship award for both the overall and impact sample groups. Taking these use patterns into account, in September 2005, a total of 1,716 students were enrolled in private schools of their parent's choosing by way of Opportunity Scholarships. [section 3]

1.4 Organization of This Report

The remainder of this report provides some additional details about participation as of the second year of Program implementation. Section 2 focuses on the DC private schools that offered to accept scholarship students during the first and second years of implementation. Section 3 updates the number of students that applied to and were awarded scholarships as part of the Program, including the subset of applicants who are the focus of the upcoming impact analysis.

For this report, which is descriptive, as well as for the later impact analysis reports, we will use several tests for calculating statistical significance, or the level of confidence that evaluators have that a difference between groups did not occur merely by chance. For most of the comparisons that we make, we use the "Student's *t* test." The *t* test is commonly used when the factor being considered, such as test scores, tends to be distributed continuously on a normal, bell-shaped curve. Unlike some significance tests, the *t* test incorporates information about the distribution of values in both comparison groups, and not just the overall population, and thus is a more precise measure of statistical significance than the *Z* test, for example.[9] When the characteristic in question is not normally distributed—such as gender, which is an either/or and not a more-or-less—we use the "chi-squared" test of statistical significance. All group differences that are mentioned in this report are statistically significant at least beyond the traditional 95% confidence level using a two-tailed statistical test.

[9] See Russell A. Langley, *Practical Statistics Simply Explained* (New York: Dover, 1970), 160-165.

2. Participating Schools

The DC Opportunity Scholarship Program seeks to enable low-income parents in the District to send their children to private schools as an alternative to the public school or schools otherwise available to them. As such, one important characteristic of the Program is the composition of the set of DC private schools that chose to accept OSP students.

Over half of District private schools have agreed to participate in the OSP.

- 58 (53 percent) of the 109 private elementary and secondary schools in DC in 2004 agreed to participate in the Program in the first year of implementation.
- 68 (65 percent) of the 104 District private schools in 2005—including all the schools that participated in the first year—chose to participate in the OSP during the second year of implementation.
- Of the 68 participating schools in fall 2005, 60 (88 percent) had OSP students enrolled at that time.

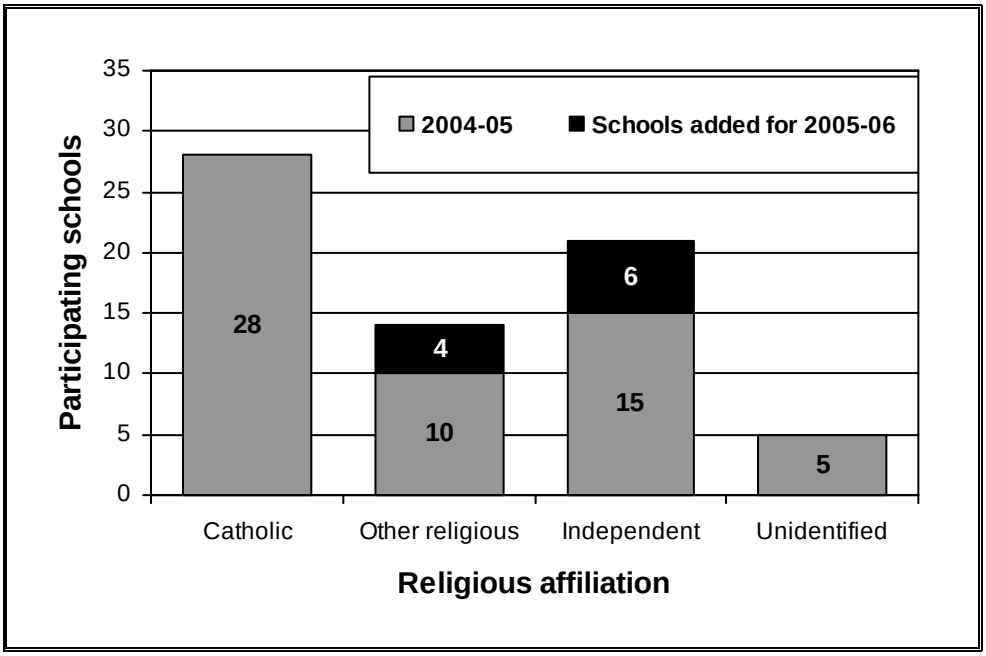
The religious status and affiliation of the participating schools varies (figure 2-1). Of the 63 participating schools for which a religious status could be determined, 28 (44 percent) are Catholic, 14 (22 percent) are formally affiliated with a religion besides Catholicism, and 21 (33 percent) are independent private schools. Perhaps because 28 of 30 DC private schools that identify themselves as Catholic all joined the Program the first year, none of the schools that joined in year 2 are; 4 of the 10 new private schools are non-Catholic religious schools, whereas the other 6 are independent. We were unable to determine the religious status of five schools, which all joined the Program in year 1.[10]

The 10 schools that joined the Program in year 2 are different in several other respects from the 58 schools that have participated from the start. On average, the new schools are more likely to charge tuition above the scholarship cap of $7,500, more likely to serve one or more high school grades, have a smaller percentage of racial minorities among their student populations, and are larger than the original group of participating schools (table 2-1). The average teacher/student ratios of the two groups of participating schools are statistically similar.

The 36 District private schools that are not currently participating in the Program differ from the total set of participating schools in some respects. Nonparticipating schools are more likely to charge average tuitions above the scholarship cap, have smaller enrollments, serve a smaller minority population, and have lower student/teacher ratios than the average among the participating schools (table 2-1). The group of nonparticipating private schools includes several highly specialized schools, such as a ballet school, as well as schools that exclusively serve students with significant disabilities.

[10] We were unable to identify conclusively the religious status of five participating private schools because the field for "Religious Affiliation" in the WSF School Directory database was blank for those schools, and visits to their websites did not provide sufficient information to determine their religious status.

Figure 2-1. Religious affiliation of participating schools by year



NOTE: Schools are defined as "participating" if they signed an agreement form to accept scholarship students. No schools dropped out of the Program between 2004-05 and 2005-06. Five Schools Did Not Identify Their Religious Affiliation In Either Year.

SOURCES: "School Directory, D.C. K-12 Scholarship Program, 2004-05 School Year," Washington Scholarship Fund, June 2004. "School Directory, D.C. Opportunity Scholarship Program, 2005-06 School Year," Washington Scholarship Fund, August 2005.

Table 2-1. Features of DC private schools by OSP participation status, years 1 and 2

Item	Participated year 1	New participants year 2	Total participants year 2	Nonparticipants year 2
Percent with average tuition[a] above $7,500	31.0[b]	88.9**	38.8	73.7**
Average size (student enrollment)	204.0	418.4**	236.0	137.6*
Percent serving high school[c]	17.2	50.0*	22.1	32.4
Average percent minority	81.2	35.3**	76.2	56.6*
Average student/teacher Ratio	10.9	8.5	10.6	7.8*
Total *N*	**58**	**10**	**68**	**36**

SOURCES: Data on participating private schools drawn from "School Directory, D.C. K-12 Scholarship Program, 2004-05 School Year," and "School Directory 2005-06, D.C. Opportunity Scholarship Program," Washington Scholarship Fund, June 2004 and April 2005, respectively. Data on both participating and nonparticipating private schools were also obtained from school websites.

[a] For schools that charge a range of tuitions, the midpoint of the range was selected. Tuition rates were unavailable for 8 of the participating private schools and 26 of the nonparticipating private schools.

[b] Three schools charged no tuition either because of foundation support or because the school serves groups such as DC-placed special education students funded by the government.

[c] Schools were classified as serving high schools if they enrolled students in any grade 9-12.

* The difference between groups is statistically significant at the 95 percent confidence level.

** The difference between groups is statistically significant at the 99 percent confidence level.

Of the 68 DC private schools that agreed to participate, 60 had OSP students enrolled in fall 2005. Eight schools were not serving OSP students at that time. Although systematic data were not collected as to the reason(s) each school did not serve OSP students, program implementation staff reported that the most common reasons include that schools: (1) determined that none of the current scholarship recipients met their entrance criteria,[11] (2) had no scholarship recipients choose their school during the placement phase, or (3) filled their vacant slots before OSP recipients could be placed.

[11] According to the Program statute, participating private schools are able to maintain any admittance criteria that they apply to non-OSP students in determining which OSP students are admissible. These criteria most typically include the completion of an interview with school staff, a placement test to determine their appropriate grade level, and an agreement to conform to the schools' codes of conduct. However, 13 percent of participating schools require standardized testing results to be a part of the admissions packet, for both OSP and non-OSP students.

3. Families/Students

How many and what kinds of people seek to avail themselves of an educational intervention signals both the levels and composition of client demand for such a program. This section describes the applicants to the Program, the numbers and types of applicants who were awarded scholarships, the size and composition of the randomized impact sample that will inform the evaluation, and the rates of scholarship usage and persistence in the Program.

3.1 Applicants

The Program received new applications on behalf of 3,126 students during the second year of implementation (table 3-1). In total, then, 5,818 distinct individuals applied for Opportunity Scholarships during the first two application periods. Over 2,000 new applicants were deemed eligible for the Program in cohort 2, for a total of 4,047 eligible applicants to date. This total represents approximately 10 percent of eligible low-income students in the District of Columbia, according to 2000 census figures.

Table 3-1. Number and percentage of applicants, by application status, cohorts 1 and 2

Measure	Cohort 1	Cohort 2	Total
Low-income students in DC	40,507	40,507	40,507
Applicants	2,692	3,126	5,818
Eligible applicants	1,848	2,199	4,047
Eligible applicants as percent of low-income students in DC	5	5	10

NOTE: Applicants entering grades 6-12 in cohort 2 who did not participate in baseline testing were not included in the eligible applicant figure.

SOURCES: Figure for low-income students is based on data from the U.S. Census, population of the District of Columbia ages 5 to 17 under 185 percent of the Federal poverty line in 2000. The exact number for 2004 and 2005 is likely to differ somewhat from this 2000 figure. Numbers of applicants and eligible applicants are from the DC Opportunity Scholarship Program applications.

Congress signaled its intention to ensure that the OSP was targeted to students most in need of educational options. In particular, Section 306 of the statute identifies students attending public schools officially designated as in need of improvement under NCLB as a group that should receive priority for the Program.[12] This priority was implemented in both how students were recruited to apply to the Program and how scholarships were awarded through lotteries (see also section 3.2). Because the lotteries must be conducted in the spring, before the

[12] Under NCLB, schools are designated as SINI if they have been conducting accountability testing consistent with the requirements of NCLB for at least 2 years and contained a student population or relevant subpopulation that failed to demonstrate Adequate Yearly Progress in both years.

District reports its SINI designations each August, the lottery priority group categories are always based on SINI designations that are a year behind.[13] For the purposes of examining applicant characteristics, however, it is more accurate to consider the designation for the school year in which a student applies to the OSP: 2004 SINI designations for cohort 1 (spring 2004 applicants) and the 2005 designations for cohort 2 (spring 2005 applicants). In evaluating the extent to which the Program is drawing students with educational needs, we also use another indicator of school performance—the proportion of each applicant's school that met the "proficient" benchmark on the DCPS assessment in 2005.

Based on these different measures of educational need (table 3-2):

- About 40 percent of applicants were from schools designated SINI for the year the students would be leaving those schools to participate in the OSP. For cohort 1, 37 percent were SINI, and for cohort 2 just under 43 percent were SINI. In total, 44 percent of OSP applicants were from schools designated as SINI between 2003 and 2005, a period when the number of SINI schools jumped from 15 to 101.
- About 11 percent of the applicants came from schools whose school-level proficiency rate in 2005 places them in the bottom quartile of all DCPS schools; nearly 30 percent were from the second quartile schools; and almost 36 percent were from third quartile schools. Just under one-quarter of all OSP applicants came from the highest performing public schools in the District, based on the proportion of the schools' students who met the proficiency benchmark.

3.2 Scholarships Awarded

The Program statute set two conditions for any lotteries conducted under the OSP. First, scholarships would be awarded by lottery when the Program is "oversubscribed"—that is, the number of eligible applicants exceeds the number of available slots in participating private schools. However, because the extent of oversubscription varied significantly by grade, in practice the determination of whether to hold a lottery was considered within grade bands: those applying for grades K-5, those applying for grades 6-8, and those applying for grades 9-12. Second, the statute specified that certain groups be given priority in any such lotteries, which led to the following classifications for the lottery:

- Applicants attending a public school in need of improvement under NCLB (highest priority);
- Non-SINI public applicants (middle priority); and
- Applicants already attending private schools (lowest priority).

[13] For example, SINI students in the cohort 1 (spring 2004) lottery had to be categorized based on SINI designations made in August 2003, using performance data from the 2002-03 school year. But 2 months after the lottery, more schools were designated SINI for 2004, and it is this later number that is a more accurate representation of which schools were low performing when cohort 1 was applying to the OSP.

Table 3-2. School-level academic performance designations and rates, public school attending at time of application to the OSP, cohorts 1 and 2

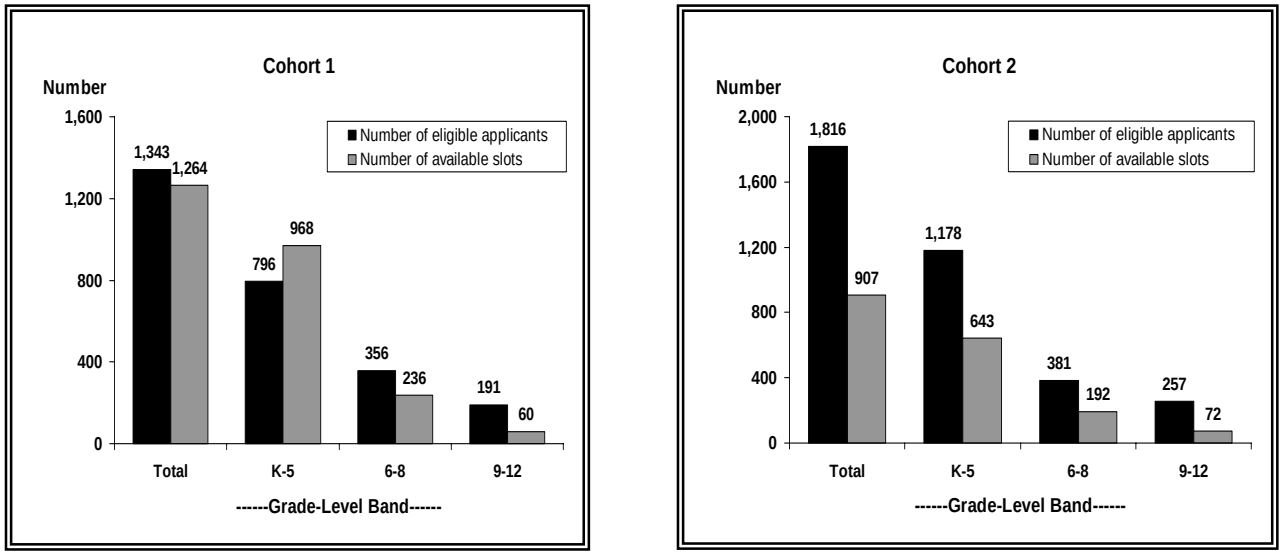
Percent of OSP applicants	Total	Cohort 1	Cohort 2
Attending SINI schools			
SINI in fall 2003 (*N*=15)	4.9	5.9	4.2
SINI in fall 2004 (*N*=90)	36.9	**37.1**	36.7
SINI in fall 2005 (*N*=101)	43.7	44.8	**42.8**
Attending schools in which the percent of students proficient in math and reading placed the school in the:			
Lowest quartile	11.2	11.4	11.1
Second quartile	29.5	30.3	28.9
Third quartile	35.6	35.2	35.9
Highest quartile	23.7	23.3	24.0
Percent missing	*21*	*19*	*22*
Sample size	3,159	1,343	1,816

NOTES: The figures in bold in the upper panel are those most relevant for each cohort. Proficiency rates were calculated based on a composite measure of reading and mathematics rates. The rates were calculated for each DCPS school, and the schools were sorted by rate. The proficiency rate for the school that was the "25th percentile" school in the distribution became the cut-point for the lowest quartile. The rates for the schools at the 50th and 75th percentiles became the cut-points for the second, third, and highest quartiles. As context for interpreting this distribution, schools that fell in the two lowest quartiles had fewer than 44 percent of their students achieving proficiency on the DCPS assessment.

SOURCES: The DC Opportunity Scholarship Program applications and the District of Columbia Public Schools website.

These conditions led to somewhat different scholarship award opportunities in years 1 and 2 of Program implementation (figure 3-1). In cohort 1, for example, there were more slots in participating schools than there were applicants for grades K-5; therefore, all eligible K-5 applicants automatically received scholarships, and no lotteries were conducted at that level. By contrast, in cohort 2 there were 1,178 K-5 applicants vying for scholarships to fill 643 slots, requiring a lottery to determine which students received an award. In both years, there was a high degree of oversubscription at the high school level and moderate oversubscription at the middle school level. In contrast to the first year, in year 2 there was sufficient demand from public school applicants that lotteries were conducted only for them; applicants who were already attending a private school (the lowest priority group) were not entered into a lottery.

Figure 3-1. Eligible public school applicants and available private school slots, by grade-level band, cohorts 1 and 2



SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

The design of the lotteries for cohort 2 applicants was similar to that for the first year, with the exception of how the priority treatment of SINI applicants was implemented. For cohort 1, the 79 eligible applicants from previously designated SINI schools were all automatically awarded scholarships, consistent with their status as the highest priority client group. For cohort 2, the 655 eligible applicants from previously designated SINI schools were all subject to lotteries, with scholarship award probabilities approximately one-third higher than non-SINI applicants within their respective grade bands (table 3-3).[14] Randomly assigning cohort 2 SINI students, but with a higher probability of award than non-SINI students, accomplished the dual objectives of including a significant number of SINI students in the experimental evaluation of Program impact while also treating them as the highest priority for access to the Program.

In general, the probability of receiving a scholarship through a lottery was based on the ratio of slots to applicants in each grade band. Given the likelihood that some students would choose not to use the scholarships that were awarded to them, based on the first-year experience, award probabilities were then adjusted to "over-award" scholarships by approximately 20 percent. As a result of the spring 2005 lotteries, 1,088 scholarships were awarded to cohort 2 eligible applicants—429 to applicants from SINI public schools and 659 to the larger group of applicants from non-SINI public schools (table 3-3).

[14] The cohort 2 SINI award probability advantage actually was 32.7 percent in K-5, 35.6 percent in 6-8, and 33.5 percent in 9-12. In the end, it did not equal exactly 33.3 percent in any of the grade bands because a handful of students deemed eligible on appeal were randomly assigned through small lotteries that are inherently less precise than are large lotteries.

Table 3-3. Probability of receiving a scholarship, by applicant type and grade-level band, cohort 2

Type of applicant	Grade-level band K-5	6–8	9–12	Total
SINI public				
Eligibles	277	184	194	655
Probability	78.3%	75.0%	38.1%	65.5%
Scholarships	217	138	74	429
Nonrecipients	60	46	120	226
Non-SINI public				
Eligibles	901	197	63	1,161
Probability	59.0%	55.3%	28.6%	56.8%
Scholarships	532	109	18	659
Nonrecipients	369	88	45	502
All public				
Eligibles	1,178	381	257	1,816
Probability	63.6%	64.8%	35.8%	59.9%
Scholarships	749	247	92	1,088
Nonrecipients	429	134	165	728
Private				
Eligibles	151	122	110	383
Probability	0%	0%	0%	0%
Scholarships	0	0	0	0
Nonrecipients	151	122	110	383
Total (public and private)				
Eligibles	1,329	503	367	2,199
Probability	56.4%	49.1%	25.1%	49.5%
Scholarships	749	247	92	1,088
Nonrecipients	580	256	275	1,111

NOTES: Eligible applicants already attending private school were given a zero probability of receiving a scholarship because the Program was rapidly filling with higher priority public school applicants. Applicants in cohort 2 entering grades 6-12 were required to participate in baseline testing prior to entry in the lottery. As a result, applicants who were deemed eligible based on income and residency but did not participate in the baseline testing are not included in the table.

SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

In total, then, after 18 months of Program implementation, the OSP has awarded scholarships to 2,454 students (table 3-4). The total awards to the three priority subgroups are:

- 508 scholarship awards to public school students attending schools designated as SINI the year before they entered the lottery;
- 1,730 scholarship awards to students in non-SINI public schools; and
- 216 scholarship awards to students attending private schools but otherwise eligible for the Program.

Table 3-4. Probability of receiving a scholarship, by applicant type and grade-level band, cohorts 1 and 2 combined

Type of applicant	Grade-level band			Total
	K - 5	6 – 8	9-12	
SINI public				
Eligibles	301	204	229	734
Probability	80.0%	77.4%	47.6%	69.2%
Scholarships	241	158	109	508
Nonrecipients	60	46	120	226
Non-SINI public				
Eligibles	1,673	533	219	2,425
Probability	77.9%	68.3%	28.3%	71.3%
Scholarships	1,304	364	62	1,730
Nonrecipients	369	169	157	695
All public				
Eligibles	1,974	737	448	3,159
Probability	78.3%	70.3%	38.2%	71.0%
Scholarships	1,545	522	171	2,238
Nonrecipients	429	215	277	921
Private				
Eligibles	390	278	220	888
Probability	33.9%	23.4%	8.6%	24.3%
Scholarships	132	65	19	216
Nonrecipients	258	213	201	672
Total (public and private)				
Eligibles	2,364	1,015	668	4,047
Probability	70.9%	57.8%	28.4%	60.6%
Scholarships	1,677	587	190	2,454
Nonrecipients	687	428	478	1,593

NOTES: The probability for applicants already attending private school was 42.8 percent for cohort 1 and zero percent for cohort 2. The probability for SINI public school applicants in all grade bands was 100 percent for cohort 1; see table 3-3 for the probabilities for cohort 2. The probability for non-SINI public school applicants in the K-5 grade band was 100 percent for cohort 1; see table 3-3 for the probability for cohort 2.

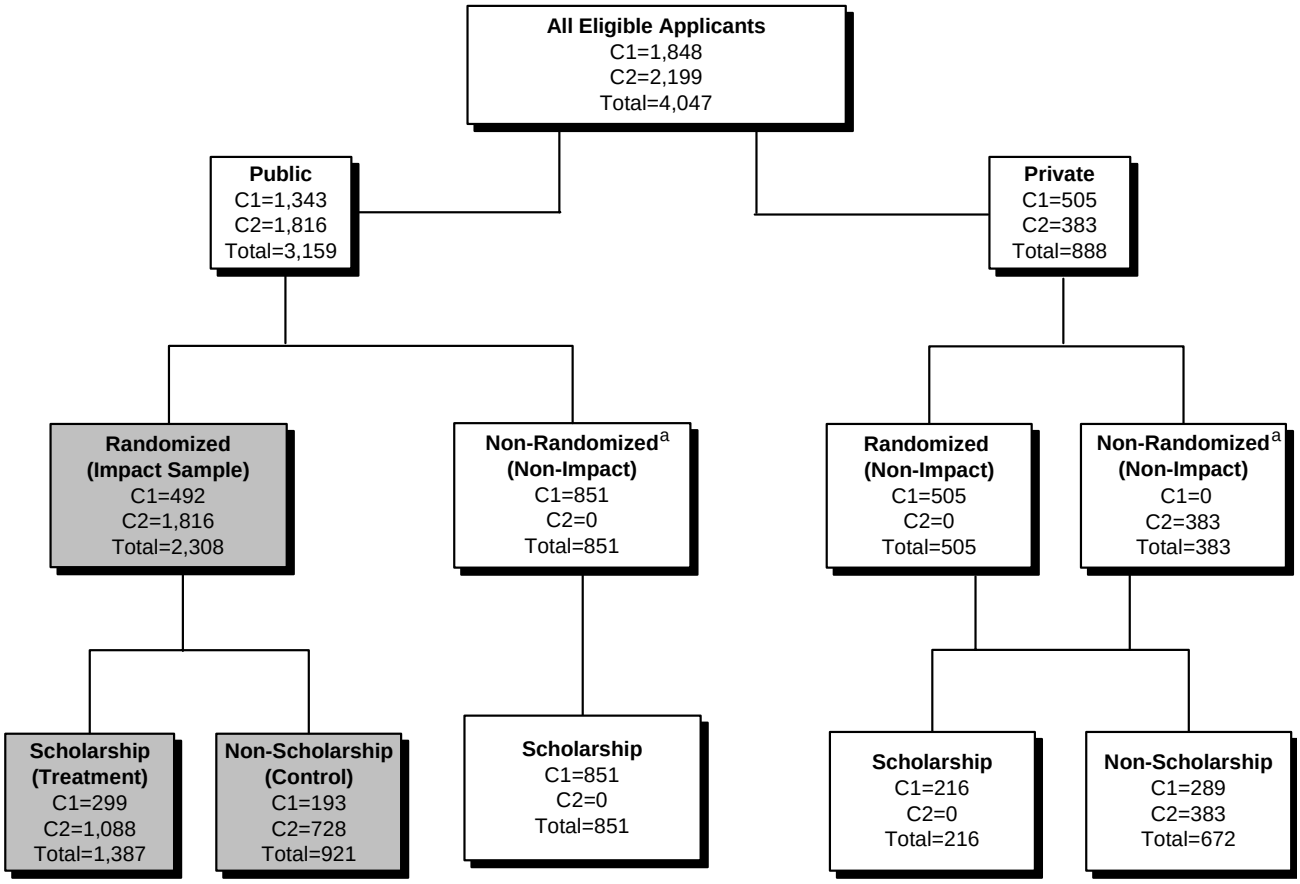
SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

3.3 Impact Sample

The impact sample is a direct result of the lotteries and the critical component of the legislatively required rigorous evaluation of the OSP. Impact evaluations compare the outcomes for a group of study participants, some of whom were randomly awarded access to the intervention (e.g., an OSP scholarship), and some of whom were randomly assigned to not receive access. The lotteries conducted for the OSP cohorts in years 1 and 2 satisfy these requirements. Since the intervention under consideration is an Opportunity Scholarship to attend a private school, the impact analysis should focus on the population of applicants for whom private schooling

represented a new opportunity. Thus, the impact sample for this evaluation will comprise all eligible applicants who were previously attending public schools (or were rising kindergartners) AND were subject to a lottery to determine whether they would receive an Opportunity Scholarship (figure 3-2, shaded).[15]

Figure 3-2. Construction of the impact sample from the applicant pool, cohorts 1 and 2



NOTES: C1 = Cohort 1 (applicants in spring 2004)
C2 = Cohort 2 (applicants in spring 2005)
Total = C1 and C2

[a]The group of applicants who were not randomly assigned includes: in cohort 1, public school applicants from SINI schools or who were entering grades K-5 (all received a 100 percent probability of getting a scholarship), and in cohort 2, private school applicants, the lowest priority group (all received a zero percent probability of award because it was clear the Program would be filled with higher priority public school applicants).

[15] The subgroups of eligible applicants to the Program who do not fit the criteria for the impact sample include eligible applicants in cohorts 1 and 2 who were already attending private schools (*n*=888) and two groups of public school applicants in cohort 1 who were all automatically awarded scholarships (*n*=851), specifically those from SINI public schools because of their high service priority and those applying for grades K-5 because there were sufficient private school slots in those grades to accommodate all of those applicants that year.

Overall

The impact sample group has the following characteristics:

- A total of 2,308 students, with 1,387 assigned by lottery to the treatment group and 921 assigned by lottery to the control group.[16]
- Approximately 21 percent of the impact sample is from cohort 1 (492 eligible applicants in grades 6-8 and 9-12), and the remaining 79 percent (all 1,816 eligible public school applicants) is from cohort 2.

The more than 2,300 students is a large study sample relative to the impact samples used in other evaluations of private school voucher programs (803 to 1,960 students).[17] Statistical computations based on reasonable assumptions about study response rates indicate that the impact sample will be sufficient to detect Program impacts of at least a moderate and educationally meaningful size.[18]

Treatment vs. Control Group Differences

An important strength of experimental methods of analysis is that the assignment of study participants to the treatment and control groups creates two analytic groups that are statistically similar. The treatment, in this case the offer of an Opportunity Scholarship, is provided to one group, and any subsequent differences in outcomes observed between the groups can be ascribed to the impact of that treatment. To ensure those conditions for the impact analysis, researchers must compare the characteristics of the treatment and control groups prior to the Program (at baseline) to see if the random assignment worked. Chance alone will occasionally generate baseline differences between the treatment and control groups about 1 time out of 20, but a properly executed lottery should produce analytic groups that are similar in almost all respects.

Analysis of the OSP groups at baseline suggests that a strong foundation for the impact analysis has been laid, although some statistical procedures will be used to further equate the groups. The

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[16] A total of five members (2.5 percent) of the cohort 1 randomized control group were awarded scholarships by lottery in the summer of 2005 as part of the control group follow-up lottery to reward control group members who cooperate with the evaluation testing requirements. Additional details regarding the follow-up control group lotteries are provided in Appendix A.

[17] William G. Howell and Paul E. Peterson, with Patrick J. Wolf and David E. Campbell, *The Education Gap: Vouchers and Urban Schools* (Washington: Brookings, 2002), 44.

[18] According to our power analysis in the first year report, an initial impact sample of 2,201 students should be sufficient to detect even rather modest test score impacts of 0.15 standard deviations both after 1 year and after 3 years of Program operation. See Patrick Wolf, Babette Gutmann, Nada Eissa, Michael Puma, and Marsha Silverberg, *Evaluation of the DC Opportunity Scholarship Program: First Year Report on Participation* (Washington, DC: U.S. Government Printing Office, 2005), A-4. To place this estimated effect size in context, an effect of 0.15 equates to a Normal Curve Equivalent (NCE) difference of 3.15 NCE points, since one standard deviation on the SAT-9 is 21/06 NCEs. Converting NCEs to a change in percentile ranks depends on where on the overall distribution the observed change occurs. For example, if the control group was, on average, at the 20th percentile, a gain of 3.15 NCEs would bring it up to about the 24th percentile. Such a gain is likely to be considered modest but educationally meaningful, and the capability to detect even a modest educational change is a clear strength of the impact evaluation going forward.

lotteries were conducted within grade bands, so any comparisons between the treatment and control group need to be made within such groupings. Those comparisons for cohort 1 determined that the first-year lotteries worked as designed in producing statistically similar analytic groups.[19] However, for cohort 2, the treatment and control groups within grade-bands differ from each other to a statistically meaningful extent in 2 of 15 comparisons (table 3-5). For students entering grades K-5, the average family income of members of the control group is $1,287 higher than that of the treatment group, and the average years of their mother's education is also slightly higher among elementary students randomly assigned to the control group. The treatment and control groups within the K-5 grade-band are statistically similar regarding race, ethnicity, and gender, and the analytic groups within the junior high and high school grade bands are indistinguishable on all factors measured.

The presence of 2 statistically significant differences out of 15 comparisons between the treatment and control groups is most likely due to random chance. In year 2, multiple, small-scale lotteries were conducted for each grade band, and the odds of getting differences between groups with random assignment increases when the samples are small.[20] In any case, because we have nearly complete baseline measures for student background factors, we can control for any measurable post-lottery differences between the analytic groups in the course of estimating subsequent Program impact.

3.4 Scholarship Usage

A large portion of the applicant pool and the impact sample received scholarships through the lotteries. These students are expected to use the funds to attend a participating private school of their choice. However, Opportunity Scholarship usage rates have varied by cohort, participant sample, and over time (table 3-6):

- Nearly three-quarters of OSP recipients use the scholarship the first year the students receive them. These initial usage rates are in the mid to high-range of usage rates reported by previous school voucher studies.[21]

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[19] Wolf et al., *Evaluation of the DC Opportunity Scholarship Program*, 41-43.

[20] To enable more parents to begin their school search early, within each grade band there was an early lottery for students deemed eligible by mid-spring and a late lottery for students not confirmed eligible until early summer. Students entering the same grade and with the same priority characteristics were assigned the exact same probability of winning a scholarship regardless of which scholarship lottery they entered.

[21] The initial scholarship usage rate was 82 percent in New York City, 78 percent in Dayton, Ohio, and 68 percent in Washington, DC, for the earlier experimental evaluation of private scholarship programs in those three cities. Voucher usage rates were 61 percent in the first year of the Milwaukee Parental Choice Program and estimated to be about 50 percent in the first year of the Cleveland Scholarship and Tutoring Program. See Howell et al., *The Education Gap,* 44; John F. Witte, *First Year Report: Milwaukee Parental Choice Program* (University of Wisconsin-Madison, November 1991), 3; John F. Witte, Andrea B. Bailey, and Christopher A. Thorn, *Second Year Report: Milwaukee Parental Choice Program* (University of Wisconsin-Madison, December 1992), 8; Paul E. Peterson and Bryan C. Hassel, eds., *Learning From School Choice* (Washington, DC: Brookings, 1998), 360.

Table 3-5. Characteristics of treatments versus controls, cohort 2 impact sample

Grade-band/characteristics	Treatments	Controls	Difference
K-5			
Percent African American	95.0	92.3	2.7
Percent missing	*9*	*7*	
Percent Hispanic (any race)	11.1	9.3	1.8
Percent missing	*6*	*7*	
Percent Female	50.7	53.4	-2.7
Percent missing	*0*	*0*	
Average annual family income	15,872	17,159	-1,287*
Percent missing	*0*	*0*	
Average years of mother's education	12.6	12.7	-0.1**
Percent missing	*15*	*14*	
Sample size	749	429	
6-8			
Percent African American	93.2	92.3	0.9
Percent missing	*10*	*10*	
Percent Hispanic (any race)	15.8	12.6	3.2
Percent missing	*8*	*5*	
Percent female	48.6	54.5	-5.9
Percent missing	*0*	*0*	
Average family income	18,296	17,232	1,064
Percent missing	*0*	*0*	
Average years of mother's education	12.5	12.4	0.1
Percent missing	*19*	*11*	
Sample size	247	134	
9-12			
Percent African American	92.8	93.6	-0.8
Percent missing	*10*	*5*	
Percent Hispanic (any race)	11.0	6.7	4.3
Percent missing	*11*	*10*	
Percent female	44.6	46.7	-2.1
Percent missing	*0*	*0*	
Average family income	18,577	18,431	146
Percent missing	*0*	*0*	
Average years of mother's education.	12.5	12.7	-0.2
Percent missing	*21*	*21*	
Sample size	92	165	

SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

* Statistically significant at the 95 percent confidence level.

** Statistically significant at the 99 percent confidence level.

- The impact sample overall has a slightly lower rate of initial use (71 percent), which can be attributed to two factors: (1) the group excludes students already attending private schools at the time of application, whose use rates are substantially higher than those for public school recipients and (2) the sample includes a higher proportion of older students, a group that was more constrained in their choice of schools under the Program and who experienced substantially lower use rates.
- Program attrition has been modest for cohort 1 students who initially used their scholarships. About 3 percent of the OSP recipient sample and 5 percent of the impact sample left the Program during the academic year after having initially used a scholarship to attend a private school in fall of 2004. Another 5 and 3 percent, respectively, left the Program during the transition to their second year. Thus, in cohort 1, a total of 64 percent of the OSP recipient sample and half of the impact sample have used their scholarships continuously since fall of 2004.

Table 3-6. Scholarship usage rates, OSP recipient and impact samples, cohorts 1 and 2

	Cohort 1		Cohort 2		Totals	
Persistence	**OSP recipient sample**	**Impact sample**	**OSP recipient sample**	**Impact sample**	**OSP recipient sample**	**Impact sample**
Number received scholarships	1,366	299	1,088	1,088	2,454	1,387
Percent initially used	75	62	73	73	74	71
Percent used all year 1	72	57	NA	NA	NA	NA
Percent used year 2	67	54	NA	NA	NA	NA
Percent used years 1 and 2	64	50	NA	NA	NA	NA

NOTE: The OSP recipient sample consists of all students offered scholarships, regardless of the award mechanism (automatic or lottery) or priority status (SINI public, non-SINI public, private). NA signifies that information is not yet available.

SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

As noted above, scholarship usage rates also are associated with the type of school the scholarship recipients previously attended and the grade they are entering. Across both cohorts, initial use of scholarships was lowest among students from SINI schools (69 percent) and, not surprisingly, highest for students who were already attending private schools when they applied for a scholarship (88 percent) (table 3-7). Scholarship usage rates differed even more based on grade band. Initial usage was highest among students entering K-5 (79 percent), somewhat lower among students entering 6-8 (69 percent) and lowest among students entering 9-12 (51 percent).

Table 3-7. Scholarship usage rates, by school type at time of application and grade band, cohorts 1 and 2 combined

Student characteristics	Number received scholarships	Percent initially used scholarships
School type at application		
SINI public	508	69
Non-SINI public	1,730	74
Private	216	88
Grade band		
K-5	1,677	79
6-8	587	69
9-12	190	51

SOURCES: The DC Opportunity Scholarship Program applications and the Program operator's files.

Scholarship usage rates are important to the evaluation of a policy intervention for two reasons. First, usage rates send an initial signal regarding the level of enthusiasm of clients toward the program and their ability to navigate the steps necessary for their children to make use of the program. Second, one of the impact questions the evaluation is designed to address is "what is the impact of actually attending a private school via a scholarship program?" When program usage and persistence rates are relatively high, the approach for determining program efficacy can more clearly stem from the randomly assigned groups of students. The lower the rates of use, the more the evaluators have to deviate from the random assignment and the greater the potential for inaccurate estimates. That important issue and the other central research questions associated with this evaluation will be addressed with initial outcome data in the next report to Congress.

Appendix A: Congressionally Mandated Evaluation

Section 309 of the *District of Columbia School Choice Incentive Act of 2003* describes the requirements for an independent evaluation of the DC Opportunity Scholarship Program. The Secretary of Education is to ensure the following:

- "The evaluation is conducted using the strongest possible research design for determining the effectiveness" of the school choice program; and
- "The results of the evaluation regarding the impact of the program on the participating students and nonparticipating students and schools in the District are disseminated widely."

Early on, the Institute of Education Sciences determined that the foundation of the DC Opportunity Scholarship Program evaluation would be a randomized controlled trial (RCT) that compared outcomes of eligible applicants (students and their parents) randomly assigned to receive or not receive a scholarship.[22] 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 requirement to use random selection to determine who receives a scholarship. In addition, the law clearly specified that such a comparison in outcomes be made.[23] This component represents the **impact analysis** and will provide evidence on the effectiveness of the Program.[24]

The law also called for the evaluation to track Program progress in other ways. For example, the evaluation was to compare the achievement of students participating in the scholarship Program to the achievement of students in the same grades in the DC Public Schools (DCPS). However, that performance reporting is no longer possible, since DCPS is in the process of changing its assessment away from the SAT-9 used when the OSP began and which the evaluation continues to administer. The evaluation will address other issues the statute requires, such as the experiences of DC schools during the period of Program implementation.

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[22] RCTs are commonly referred to as the "gold standard" for evaluating educational interventions; when mere chance determines which eligible applicants receive access to school choice, the students who apply but are not admitted make up an ideal "control group" for comparison with the school choice "treatment group." Both groups of participants are equally motivated to obtain new educational options, and nothing except a random draw distinguishes those who receive the opportunity from those who do not. Therefore, any differences in the two groups in subsequent years can be attributed to the impact of the Program. In contrast, the results of school choice studies that are not based on RCTs must be interpreted and used more cautiously because comparisons between the applicants and a group of students who chose not to apply will likely reflect not only the impact of the Program but also differences between the groups in motivation and other unmeasured characteristics.

[23] See 309(a)(4)(A)(ii).

[24] The RCT approach was also used by researchers conducting impact evaluations of the New York City; Dayton, Ohio; and Washington, DC, private scholarship programs.

Research Questions

Based on guidance in the statute, the research team plans to conduct a comprehensive and rigorous RCT evaluation of the impact of the OSP on participating students and families. Specifically, the evaluation will address the following research questions:

1. **What is the impact of the Program on student academic achievement?** The law places high priority on examining whether the Program—the availability and offer of scholarships—improves the academic achievement of eligible students. This question can be addressed most rigorously by comparing the academic achievement of student applicants randomly assigned by a lottery to receive and not receive scholarships.

2. **What is the impact of attending private versus public schools?** Because some students offered scholarships will choose not to use them, the research team will use accepted statistical methods to examine the effects for students who take the scholarship offer and successfully enroll in a private school.

3. **What is the impact of the Program on other student measures?** The law calls for examining other indicators of student and school success, including persistence, retention, graduation, and, if possible, college enrollment. In addition, the legislation requires the evaluation to assess the school safety of students who receive the scholarships relative to those who did not receive scholarships.

4. **What effect does the Program have on student and parent satisfaction with the educational options available in the District and with children's actual school experiences?** A key desired outcome of scholarship programs is an increase in both the school choices possible and parents' and students' satisfaction with the choices they have made. These issues will be examined by comparing the satisfaction and reasons for applying to the DC Opportunity Scholarship Program among applicants assigned by lottery to receive scholarships and those assigned to not receive scholarships.

5. **To what extent is the Program influencing public schools and expanding choice options for parents in Washington, DC?** Scholarship programs have been hypothesized to affect not only the students who receive the scholarships but also the broader population of public schools and students. Theory suggests that these broader outcomes could occur if a significant number of students move from public to private schools. The public schools might experience a reduction in per-student funding that affects their offerings, a change in average student performance, or they may respond to the competition for students by changing curricula, adopting new themes or missions, and modifying existing policies and practices to make the public schools more attractive to students with schooling options. Choice programs might also affect the larger population of private schools, beyond those in which the programs' participants are currently enrolled; if choice programs are successful, additional private

schools may choose to participate, new schools may be established to meet enrollment demand, or existing schools might expand capacity. These issues will be explored with descriptive analyses based primarily on school surveys.

Data Collection

To answer these questions, the evaluation will draw on different types of data—some available from DCPS, some collected for the purposes of this study. These data will include pre-Program (baseline) measures of family background and student achievement. The baseline measures allow us to verify that students randomly assigned to the scholarship and nonscholarship groups were, in fact, similar before the Program; the measures also enable us to create subgroups of students whose impacts we might want to examine separately, such as students with low prior achievement. Additional data collected will include annual "in-Program" measures (e.g., parents' satisfaction with their children's school, students' academic achievement), which will serve as outcomes for the rigorous evaluation of Program impacts (table A-1).

Impact Analysis

It is well-known that the independent effects of school choice on student outcomes are difficult to estimate. Perhaps the most significant difficulty faced by researchers is selection bias—the self-selection of families to even seek out a new school choice for their child, and the mutual student/school decision process that selects students into different types of schools. Because this bias is generally a result of unmeasurable factors, most researchers have preferred the use of an RCT to a dependence on nonexperimental (nonrandomized) statistical methods. Since the DC Opportunity Scholarship Program provides for the random distribution of scholarships through a lottery, we will use RCT methods to estimate most Program impacts.

Impact Analysis Sample

The RCT approach rests on random assignment or, in the case of the DC Opportunity Scholarship Program evaluation, a lottery to create two statistically equivalent groups of students from among Program applicants: a "treatment" group that receives a scholarship, and a "control" group that does not receive a scholarship. Because the two groups are generated from the same pool of applicants, they are equally likely to be motivated to participate in the Program and to reap any benefits from it. And as long as the pool of applicants is sufficiently large, the random assignment of students into treatment and control groups should produce groups that are similar in other characteristics, both those we can observe and measure (e.g., family income, prior academic achievement) and those we cannot (e.g., motivation to succeed). The random assignment ensures that all observed and unobserved characteristics are equally represented in both groups.

Table A-1. Data sources

Data source[a]	Description
Student assessments	• Baseline, or pre-Program measures of student achievement for public school applicants will come primarily from the SAT-9 standardized assessment administered by DCPS as part of its spring testing program.[b] • Each spring after the baseline year, the study will administer the SAT-9 to all students who were offered scholarships as well as all members of the control group who did not receive a scholarship. • DCPS test score data will be obtained for all public school students in those years, to compare with the scores of students participating in the Program.[c]
Parent surveys	• Surveys of parents (of all applicants) will be conducted in all 4 years of impact evaluation data collection. • Topics will include reasons for applying, satisfaction with school choices, perceptions of school safety, educational climate, and offerings.
Student surveys	• Each year after baseline, surveys will be conducted with all applicants who are in grades 4 and higher. • Topics will include students' satisfaction with their schools, perceptions of safety, and other characteristics of their school program and environment.
Principal surveys	• Surveys will be conducted each year of principals of all public and private schools then operating in DC, including public charter schools. • Topics will include school organization, safety, climate, principals' awareness of and response to the DC Opportunity Scholarship Program, and, for private school principals, why they are/are not participating.

[a] The research team originally planned to administer surveys to teachers, but this plan was dropped due to the higher than expected cost of student testing and the resistance of private school administrators to the burden of teacher-level data collection.

[b] Baseline achievement will be collected only for applicants from public schools; applicants who were already attending private schools will not be included in the impact analysis. For public school applicants who did not participate in regular DCPS testing in the year they applied to the Program (e.g., particularly children below grade 3), the study will administer the equivalent DCPS assessment to these students as soon as possible after application. All other data will be collected for all applicants, both from public and private schools.

[c] Beginning in spring of 2006, DCPS will administer a customized, criterion-referenced achievement test (CRT) for its accountability testing instead of the SAT-9. Since scores on the CRT are not directly comparable to scores on the SAT-9 administered for purposes of the evaluation, the performance report, which relies on DCPS accountability testing data for comparison, will be less reliable and informative than if the District were continuing to administer the SAT-9. The switch from the SAT-9 to the CRT will not affect the impact analysis, since members of both the treatment and control groups will be administered the same test (the SAT-9) by the evaluation team for purposes of determining Program impact.

According to the statute, the random assignment that is the means to create the treatment and control groups can only be used to help allocate scholarships under particular circumstances. As a result of these conditions, the impact analysis sample will:

- **Exclude applicants already enrolled in private school when they applied to the OSP.** The statute contained no provision to exclude from the Program students who were currently enrolled in private schools but otherwise eligible to participate.[25] A substantial number of private school students did apply to the Program, especially in the first year of Program implementation. However, because those students intended to use the DC Opportunity Scholarship to *continue* to attend private schools, measuring the difference in outcomes between private school applicants who did and did not receive a scholarship through the lottery would likely only answer the question of whether a different type or amount of scholarship funds affects student outcomes. While that question is of some policy interest, it is not the main focus of the evaluation as specified in the legislation. Therefore, applicants already enrolled in private schools when they applied are not part of the impact analysis sample.
- **Exclude any students who were originally assigned to the control group by lottery but subsequently were awarded scholarships by way of follow-up lotteries.** The participants in the impact evaluation all sought scholarships to attend private schools. To encourage those randomly assigned to the control group (nonrecipients) to turn out for follow-up data collection, a special lottery is held each spring in which the only eligible students are those originally assigned to the control group who subsequently cooperated with outcome data collection. Because they represent study-induced treatment crossover (i.e., participants assigned to the control condition who are subsequently offered the treatment) and were chosen at random, these students will be excluded from the subsequent impact analysis. Five scholarships were awarded to control group students by lottery in the summer of 2005, when the control group totaled 193 students. Ten scholarships will be awarded to control group students by lottery in the spring of 2006, when the control group will number 911 (e.g., 921 minus 5 control awardees minus 5 high school graduates). Ten or more new scholarships will be awarded to control group students each year after 2006. The number of scholarships awarded to members of the control group needs to be kept modest in the initial years of an experimental evaluation so that the control group remains sufficiently large to enable researchers to identify Program impact.
- **Include only public school applicants in grades where there are more applications than there are available private school slots.** A lottery is a fair and efficient way of distributing scholarships when there are too many applicants, but is inappropriate as an allocation device when sufficient scholarship funds and private school slots exist to accommodate all the eligible applicants at certain grade levels. In those grade levels, all applicants will receive scholarships, and they will be excluded from the impact sample.

[25] Some of these applicants from private schools were already relying on scholarship funds in order to attend those schools. However, the scholarships they were receiving may have been less generous than those available under the DC Opportunity Scholarship Program.

Thus, the impact evaluation of the DC Opportunity Scholarship Program depends on the extent to which large numbers of eligible DC families with public school students apply to the Program. The treatment and control groups must be of a sufficient size to allow us to detect and measure any difference in outcomes between the two groups (the "impact") with statistical certainty. A procedure called "power analysis" is used to determine the sample sizes necessary to enable the study to answer the central research questions and to measure Program effects that are large enough to be both meaningful in students' lives and relevant to policy debates about the efficacy of school choice interventions. At the end of the 18-month initial implementation period, we know the following about the impact sample and study power:

- Cohort 1 includes 492 eligible applicants who qualify as members of the impact sample. They all were entering grades 6-12 in fall of 2004. A total of 299 are members of the treatment group, and 193 are members of the control group.
- Cohort 2 includes 1,816 eligible applicants who qualify as members of the impact sample. They cover all eligible grades, K-12. A total of 1,088 are members of the treatment group, and 728 are members of the control group.
- The combined-cohort impact sample totals 2,308 students, of whom 1,387 are members of the treatment group, and 921 are members of the control group.
- Preliminary estimates of study power, given these participant numbers, suggest that the analysis will be able to detect even modest but educationally meaningful Program impacts for the combined-cohort sample and for the Cohort 2 sample alone.

General Statistical Approach: Estimating the Impact of the Offer of a Scholarship

Given appropriately sized treatment and control groups, the strategy for analyzing impacts is well established. To motivate the discussion of how we identify the effect of the scholarship Program on test scores, it is useful to begin with a simple representation of the selection problem as a missing data problem, using the potential outcomes approach. This approach defines causal effects in terms of *potential outcomes* or *counterfactuals.* Conceptually, the causal effect of treatment—the scholarship—is defined as the difference between the "outcome for individuals assigned to the *treatment* group" and the "outcome for the *treatment* group if it had not received the treatment," or:

(E.1) "$E(Y_i | X_i, T_i = 1)$" - "$E(Y_i | X_i, T_i = 0)$"

In the case of scholarships, the treatment effect—the effect of the scholarships on academic achievement—would be defined as the difference between "test scores for Program students" and "test scores for Program students if they had not received a scholarship." The fundamental problem is that a student is never observed simultaneously in both states of the world. What is observed is a student in the treatment group ($T_i = 1$) or in the control group ($T_i = 0$). The outcome in the absence of treatment, $E(Y_i | X_i, T_i = 0)$, is then the counterfactual—what would have occurred to those students receiving the scholarships if they had not received them.

If students receiving scholarships were identical to other students in both observable and unobservable characteristics, the counterfactual could be generated directly from an appropriately selected comparison group. Valid comparison groups are rarely found in practice, however. The random assignment of students into the Program generates the counterfactual from the control group—eligible applicants who did not receive a scholarship.[26] If correctly implemented, random assignment yields statistically equivalent groups and allows estimation of the Program impact through differences in mean outcomes between the two groups.

Consistent with this approach is the following basic analytic model of the effects of school choice scholarships on outcomes. Consider first the outcome equation for the test score of student *i* in year *t.* It is reasonable to assume that test scores (Y_{it}) are determined as follows:

$$(E.2) \quad Y_{it} = \alpha + \tau T_{it} + X_i \gamma + \varepsilon_{it} \text{ if } t>k \text{ (period after Program takes effect)}$$

Equation (E.2) estimates the effect of the *offer* of a scholarship on student outcomes. Under this model, commonly referred to as the "Intent to Treat" (ITT) estimation, 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 uses the scholarship to attend a private school. In E.2, T_{it} is equal to one if the student *has the opportunity to participate* in the scholarship Program (i.e., the award rather than the actual use of the scholarship) and equal to zero otherwise. X_i is a vector of student characteristics (measured at baseline) known to influence future academic achievement, such as prior test scores, mother's level of education, family income, etc. In this model, τ represents the effect of scholarships on test scores for students in the Program, conditional on X_i. With a properly designed RCT, using a judiciously chosen set of statistical controls for characteristics that predict future achievement should improve the precision of the estimated impact.[27] That treatment effect, τ, should be identical to the difference in mean outcomes between the treatment and the control groups.

Since the initial applicants were randomized within certain relevant subgroups, we will analyze Program impacts using a randomized block design. We are interested in how academic

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[26] See the following studies, which all use the same data from an evaluation of a New York City privately funded scholarship Program: William G. Howell, Patrick J. Wolf, David E. Campbell, and Paul E. Peterson, "School Vouchers and Academic Performance: Results from Three Randomized Field Trials," *Journal of Policy Analysis and Management* 21 (2000): 2; John Barnard, Constantine E. Frangakis, Jennifer L. Hill, and Donald B. Rubin, "Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City," *Journal of the American Statistical Association* 98 (2003): 462; Alan B. Krueger and Pei Zhu, *Another Look at the New York City School Voucher Experiment,* Working Paper Series, Education Research (Princeton, NJ: Princeton University, March 2003).

[27] For a spirited debate about the use of this technique in the context of school choice research, see William G. Howell and Paul E. Peterson, "Uses of Theory in Randomized Field Trials: Lessons from School Voucher Research on Disaggregation, Missing Data, and the Generalization of Findings," *American Behavioral Scientist* 47 (Jan. 2004): 634-657; Krueger and Zhu, *Another Look,* 658-698; Paul E. Peterson and William G. Howell, "Efficiency, Bias, and Classification Schemes: A Response to Krueger, A.B. and Zhu, P., '*Another Look at the New York City School Voucher Experiment*,'" Working Paper Series, Education Research (Princeton, NJ: Princeton University, March 2003): 699-717; Alan B. Kruger and Pei Zhu, "Inefficiency, Subsample Selection Bias, and Nonrobustness: A Response to Peterson, P.E. and Howell, W.G., '*Another Look at the New York City School Voucher Experiment*,'" ¯ Working Paper Series, Education Research (Princeton, NJ: Princeton University, March 2003): 718-728; Paul E. Peterson and William G. Howell, "Voucher Research Controversy: New Looks at the New York City Evaluation," *Education Next* 4 (Spring 2004): 73-78.

achievement (Y) is affected by the assignment into the scholarship Program within each block (B) or group of size n. The impacts are then estimated as:

$$(E.3) \quad Y_{ikt} = \mu + \tau\, T_{ikt}\, \Sigma^{b}_{j=2} \rho_j\, B_{ik} + X_{ik}\, \gamma + \varepsilon_{ik,t}$$

where

i = 1,.....,n observations and k=1,....,b blocks (defined by grade and priority status);
Y_{ji} is the outcome for student i in block j, at time t;
μ is the overall mean outcome (e.g., test score);
τ is the treatment (scholarship Program) effect;
ρ_j is the j^{th} block effect;
T_{it} is assignment into the scholarship Program;
B_{ji} is the block assignment;
X_{ji} represents observable characteristics, measured at baseline; and
ε_{ij} is the random error; independent, $N(0,\sigma_\varepsilon^2)$.

This analytical framework follows naturally from the group randomization and is easily implemented and interpreted. Y can be measured in several different dimensions, including test scores, school satisfaction, parental satisfaction, grade completion, including where appropriate, high school graduation, etc. μ is average outcome for all Program members, ρ_j is the average block effect, and τ is the effect of scholarships on academic achievement.[28]

Estimating the Impact of the Use of Scholarships

Even with a properly implemented RCT, we may expect that not all applicants placed by random assignment into the treatment (scholarship offer) group will actually use the scholarship at a private school. That is, some scholarship recipients may choose not to use their scholarship and instead attend a public school. This type of nonparticipation or underutilization of treatment services has been observed across all RCT settings, including medical trials, job training and health insurance experiments, as well as in previous school scholarship RCTs such as the one of the Milwaukee Parental Choice Program.

Policymakers are typically interested in the effect of scholarship *use* on student achievement, in addition to the offer of the scholarship. To estimate the impact, we will use a model commonly referred to as the "Impact of the Treated" (IOT), which statistically estimates the impact of actual scholarship use, relying on what is called the "Bloom Adjustment."[29] This is possible by using the original comparison of **all** treatment group members to **all** control group members but interpreting it in a different way. The new interpretation says that the treatment group's impact—how its outcomes differ from what would have transpired without a scholarship—has two components:

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[28] Depending on the extent to which the randomly assigned applicants are clustered in their schools, some adjustments to the standard error estimates may be necessary.

[29] Howard S. Bloom, "Accounting for No-Shows in Experimental Evaluation Designs." *Evaluation Review,* 8 (1984): 225-246.

- The impact on the decliners, who by definition do not participate in the Program even though offered a scholarship, which can logically be assumed to be zero.
- The impact on everyone else assigned to the treatment group—i.e., on the OSP participants who make up the rest of the experimentally determined treatment group.

This assumption alone—the presumption that the decliners remain unaffected by their assignment to the treatment group—makes it possible to translate the measured effect of the scholarship Program on the entire treatment group (which the experimental design provides directly as described above) as a way to assess the average effect of the Program on just the participants. It does not matter what the average effect would have been on the decliners had they participated. Nor does it matter whether decliners have different outcomes than participants due to "selection" or pre-existing differences. Thus, we will use a simple Bloom Adjustment to estimate the impact of the OSP on actual scholarship users.

Reports

The law requires the Secretary to submit to the Congress annual reports resulting from the independent evaluation and a final report not more than a year after the 5-year Program ends. These reports should provide the Congress, other policymakers, the research community, and the public at large with important new information about what happens to students, families, schools, and communities when educational options are expanded for urban low-income families through public policy. The next report, the first with results from the impact analysis, will be submitted in spring 2007.