

## **School Vouchers: Results from Randomized Experiments**

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## School Vouchers: Results from Randomized Experiments

In the past decade much has been learned about the way in which school vouchers impact low-income families and their children.<sup>1</sup> Ten years ago, the empirical information available about this widely debated question came primarily from a seriously flawed public-school choice intervention attempted in Alum Rock, California during the 1960s.<sup>2</sup> But in the early and mid-1990s, new voucher programs sprouted across the country in such cities as Milwaukee, Dayton, Cleveland, Indianapolis, San Antonio, Washington, D. C. and New York City. Initially, many of the evaluations of these innovations were limited by the quality of the data or the research procedures employed. Often, planning for the evaluation began after the experiment was

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<sup>2</sup> R. J. Bridge and J. Blackman, *A Study of Alternatives in American Education: Vol. 4. Family Choice in Education* (Santa Monica, CA: Rand Corporation, 1978); Richard Elmore, "Choice as an Instrument of Public Policy: Evidence from Education and Health Care," in W. Clune & J. Witte, eds., *Choice and Control in American Education: Vol. 1. The Theory of Choice and Control in American Education* (New York: Falmer, 1990), pp. 285-318.

underway, making it impossible to gather baseline data or to ensure the formation of an appropriate control group. As a result, the quality of the data collected was not as high as researchers normally would prefer.<sup>3</sup>

Despite their limitations, these early evaluations provided program operators and evaluation teams with opportunities to learn the problems and pitfalls accompanying the study of school vouchers. Subsequent evaluations of voucher programs in Dayton, New York and Washington, D. C. have been designed in such a way as to allow for the collection of higher-quality information about student test-score outcomes and parental assessments of public and private schools. Because vouchers in these cities were awarded by lot, program evaluations could be designed as randomized field trials. Prior to conducting the lotteries, the evaluation team collected baseline data on student test scores and family background characteristics. One and two years later, the evaluation team again tested the students and asked parents about their children's school experiences.<sup>4</sup> Any statistically significant differences between students offered a voucher

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<sup>3</sup> Disparate findings have emerged from these studies. For example, one analysis of the Milwaukee choice experiment found test score gains in reading and math, particularly after students had been enrolled for three or more years, while another study found gains only in math, and a third found gains in neither subject. Jay P. Greene, Paul E. Peterson, and Jiangtao Du, "School Choice in Milwaukee: A Randomized Experiment," in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Washington, D. C.: Brookings, 1998), pp. 335-56; Cecilia Rouse, "Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program," Department of Economics, Princeton University, 1997; John F. Witte, "Achievement Effects of the Milwaukee Voucher Program," paper presented at the 1997 annual meeting of the American Economics Association. On the Cleveland program, see Jay P. Greene, William G. Howell, and Paul E. Peterson, "Lessons from the Cleveland Scholarship Program," in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Washington, D. C.: Brookings, 1998), pp. 357-92; Kim K. Metcalf, William J. Boone, Frances K. Stage, Todd L. Chilton, Patty Muller, and Polly Tait, "A Comparative Evaluation of the Cleveland Scholarship and Tutoring Grant Program: Year One: 1996-97," School of Education, Smith Research Center, Indiana University, March 1998. Greene, Peterson, and Du, 1998 report results from analyses of experimental data; the other studies are based upon analyses of non-experimental data.

<sup>4</sup> Results from the Dayton evaluation after one year are reported in William G. Howell and Paul E. Peterson, "School Choice in Dayton, Ohio: An Evaluation After One Year," Paper prepared for the Conference on Charters, Vouchers and Public Education, 2000, (Program on Education Policy and Governance, Kennedy School of Government, Harvard University, Cambridge). Website address: <http://data.fas.harvard.edu/pepg/>. First-year results for Washington are reported in Patrick J. Wolf, William G. Howell and Paul E. Peterson, "School Choice in Washington, DC: An Evaluation after One Year," (Paper prepared for the Conference on Charters, Vouchers and Public Education, 2000, sponsored by the Program on Education Policy and Governance, Kennedy School of Government, Harvard University, Cambridge, MA; Website address: <http://data.fas.harvard.edu/pepg/>. First-year results from the New York City evaluation are reported in Paul E. Peterson, David E. Myers, William G. Howell, and Daniel P. Mayer, "The Effects of School Choice in New York City," in Susan B. Mayer and Paul E. Peterson, eds., *Earning and Learning: How Schools Matter* (Washington, D.C.: Brookings, 1999), Ch. 12.

and those not offered a voucher may be attributed to experiences at school, because average student initial abilities and family backgrounds are similar.

This paper reports the estimated effects on students and families of 1) the offer of a voucher on students and parents; and 2) a switching from a public to a private school. Students who were evaluated entered private school in grades 2-5 in New York City and grades 2-8 in Dayton (and other parts of Montgomery County, Ohio) and Washington, D. C.<sup>5</sup> Specifically, the evaluation estimates the impact of the program on student test scores on the Iowa Test of Basic Skills (ITBS) in reading and mathematics as well as their combined performance in both subject areas. Scores range between 0 and 100 National Percentile Ranking (NPR) points; nationally, the median student's performance is at the 50th percentile.

### **The Three Voucher Programs**

The design of the three voucher programs was similar in key respects, thereby allowing the evaluation team to combine results from the separate evaluations of these programs. All were privately funded; all were targeted at students from low-income families, most of whom lived within the central city; all provided partial vouchers which the family was expected to supplement from other resources. All students included in the evaluation had previously been attending public schools. However, the programs differed in size, timing and certain administrative details. In this section we describe the main characteristics of the School Choice Scholarships Foundation (SCSF) program in New York City, the Parents Advancing Choice in Education (PACE) program in the Dayton metropolitan area, and the Washington Scholarship Fund (WSF) program in Washington, D. C.

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<sup>5</sup> Baseline data from the D. C. and Dayton evaluations are reported in Paul E. Peterson, Jay P. Greene, William G. Howell and William McCready, "Initial Findings from an Evaluation of School Choice Programs in Dayton, Ohio and Washington, D. C." Paper prepared under the auspices of the Program on Education Policy and Governance, Harvard University, for presentation before the annual meetings of the Association of Public Policy and Management, New York City, NY October, 1998. The paper is available at <http://data.fas.harvard.edu/pepg/>. Baseline data for New York City are reported in Paul E. Peterson, David Myers, Josh Haimson, and William G. Howell, "Initial Findings from the Evaluation of the New York School Choice Scholarships Program," Program on

**SCSF Program in New York City**

In February 1997 SCSF announced that it would provide 1,300 scholarships worth up to \$1,400 annually for at least three years to children from low-income families currently attending public schools. The scholarship could be applied toward the cost of attending a private school, either religious or secular. After announcing the program, SCSF received initial applications from over twenty thousand students between February and late April 1997.

To be eligible for a scholarship, children had to be entering grades one through five, live in New York City, attend a public school at the time of application, and come from families with incomes low enough to qualify for the U. S. government's free school lunch program. To ascertain eligibility, students and an adult member of their family were asked to attend verification sessions during which family income and their child's public-school attendance were documented.

Subsequent to the lottery, SCSF assisted families in identifying possible private schools their children might attend. By the end of the second year, about 64 percent of these children were using a scholarship: 62 percent of the children had used a scholarship for two full years, 12 percent used one just in the first year, and 2 percent used a scholarship only in the second year.

**PACE Program in Dayton, Ohio**

In the spring of 1998, Parents Advancing Choice in Education (PACE), a privately-funded non-profit corporation, offered low-income families within the Dayton metropolitan area an opportunity to win a scholarship to help defray the costs of attending the school of their choice. Eligible applicants participated in a lottery in which winners were offered a scholarship that could be used at participating private and public schools in Dayton and in other parts of Montgomery County, Ohio. Students entering kindergarten through twelfth grade qualified. For

the 1998-99 school year, PACE offered scholarships to 515 students who were in public schools and 250 students who were already enrolled in private schools.

The program was announced in January 1998. Based on census data and administrative records, program operators estimated that approximately 32,000 students met the program's income and eligibility requirements. Interested families were asked to call PACE, which took preliminary applications from over 3,000 students. PACE asked applicants to attend sessions where administrators verified their eligibility for a scholarship, students took the Iowa Test of Basic Skills (ITBS), and parents completed questionnaires. Over 1,500 applicants attended these verification sessions in February, March and April 1998. The lottery was then held on April 29, 1998.

During the first year of the program, the PACE scholarships covered 50 percent of tuition at a private school up to a maximum award of \$1,200. Support was guaranteed for eligible students for at least four years; in addition, the program expects to support students through the completion of high school, provided funds remain available. Scholarship amounts were increased beginning in 1999 as a result of increased funds available to PACE and support for the program by the Children's Scholarship Fund, a nationwide school-choice scholarship program.

Among the public school students offered a scholarship, 54 percent used the scholarship to attend a private school in the program's first year. Thirty-three schools accepted students who had not previously been attending a private school; 201 of these students attended twelve Roman Catholic schools, 14 attended a Lutheran school, 34 attended three other Christian schools, and 14 attended four secular, non-public schools.<sup>6</sup>

### **WSF Program in Washington, D. C.**

The Washington Scholarship Fund (WSF), a privately-funded school voucher program, was originally established in 1993. At that time, a limited number of scholarships, which could be used at a private school of the family's choice, were offered to students from low-income families. By the fall of 1997, WSF was serving approximately 460 children at 72 private schools. WSF then received a large infusion of new funds from two philanthropists, and a major expansion of the program was announced in October 1997. Both general news announcements and paid advertising were used to publicize the enlarged school-choice scholarship program. WSF announced that, in the event that applications exceeded scholarship resources, winners would be chosen by lottery. The program expanded further in 1999 with support from the Children's Scholarship Fund.

To qualify, applicants had to reside in Washington, D. C. and be entering grades K-8 in the fall of 1998. WSF awarded recipients with incomes at or below the poverty line vouchers that equaled 60 percent of tuition or \$1,700, whichever was less. Families with income above the poverty line received smaller scholarships. The maximum amount of tuition support for high school students was \$2,200. WSF has said that it will attempt to continue tuition support to the children in its program for at least three years and hopefully, if funds are available, until they complete high school. No family with income more than two-and-a-half times the poverty line was eligible for support.

Over 7,500 telephone applications to the program were received between October 1997 and March 1998; in response to invitations sent by WSF, over 3,000 applicants attended verification and testing sessions. The lottery selecting scholarship winners was held on April 29, 1998. WSF announced that it expected to award over one thousand scholarships, with a majority going to students not previously in a private school.

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<sup>6</sup> Information provided to the evaluation team by Parents Advancing Choice in Education, January 2000.

Provided they gained admission, scholarship students could attend any private school in the Washington area. During the 1998-99 school year, students participating in the evaluation attended seventy-two different private schools. In order to assist families in finding a school, WSF made extensive efforts during the summer months of 1998 to inform scholarship recipients of private school options.

Of those students offered scholarships, 53 percent made use of them to attend a private school in the first year of the program. Of those who participated in the second year of the evaluation, 68 percent of the scholarship recipients attended Catholic schools, 20 percent attended other religious schools, 9 percent attended secular schools, and for 2 percent it was not possible to determine the school's affiliation.

At the end of the second year, 70 percent of the students in the control group attended a District of Columbia public school, 17 percent attended a charter school, 9 percent a religious school, one percent a private secular school, one percent a public school outside the District of Columbia, one percent a magnet school and one percent were home-schooled. The type of school attended by the remaining one percent could not be determined. Of those who declined the scholarship offered to them, 73 percent attended a District of Columbia school, 24 percent attended a charter school, one percent a public school outside the District of Columbia, and one percent attended a magnet school. For one percent of the decliners it was not possible to determine the type of school, and the remainder attended a variety of alternative schools.

### **Evaluation Procedures**

The evaluation procedures used in all three evaluations conform to those used in randomized field trials. The evaluation team collected baseline data prior to the lottery, administered the lottery, and then collected follow-up information one and two years later. The following section details the steps taken to collect the relevant information.



### Baseline Data Collection

During the eligibility verification sessions attended by voucher applicants, students took the Iowa Test of Basic Skills (ITBS) in reading and mathematics in order to provide baseline information on student performance prior to the beginning of the program. Students in kindergarten applying for a scholarship for first grade were not tested at baseline, however. The sessions took place during the months of February, March and April immediately prior to the voucher lottery. These sessions generally lasted about two hours. The sessions were held in private-school classrooms, where school teachers and administrators served as proctors under the overall supervision of the evaluation team and program sponsors. The producer of the ITBS graded the tests.<sup>7</sup> Students in grades four through eight also completed a short questionnaire inquiring about their school experiences.

While children were being tested, adults accompanying them filled out surveys that asked about their satisfaction with their children's schools, their involvement in their children's education, and their demographic characteristics. Parents completed these questionnaires in rooms separate from those used for testing. Administrators explained that responses to the questionnaire would be held in strict confidence and would be used for statistical purposes only. Respondents had considerable time to complete their surveys, and administrators were available to answer questions about the meaning of particular items. Information from these surveys has been reported elsewhere.<sup>8</sup>

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<sup>7</sup> The assessment used in this study is Form M of the Iowa Tests of Basic Skills, Copyright c 1996 by The University of Iowa, published by The Riverside Publishing Company, 425 Spring Lake Drive, Itasca, Illinois 60143-2079. All rights reserved.

<sup>8</sup> Howell and Peterson, 2000; Wolf, Howell and Peterson, 2000. Paul E. Peterson, David E. Myers, William G. Howell, and Daniel P. Mayer, "The Effects of School Choice in New York City," in Susan B. Mayer and Paul E. Peterson, eds., *Earning and Learning: How Schools Matter* (Washington, D.C.: Brookings, 1999), Ch. 12. For detailed results from the second-year evaluation of New York City's voucher program, see David Myers, Paul E. Peterson, Daniel Mayer, Julia Chou, and William P. Howell, "School Choice in New York City after Two Years: An Evaluation of the School Choice Scholarships Program," September 2000. Occasional Paper, Program on Education Policy and Governance, Taubman Center on State and Local Government, Kennedy School of Government, Harvard University). Report available at <http://data.fas.harvard.edu/pepg/>

Over 5,000 students participated in baseline testing in New York City. After vouchers were awarded, approximately 1000 families were selected at random from those who did not win the lottery to comprise a control group of approximately 960 families.<sup>9</sup>

In Dayton, 1,440 students were tested at baseline and 1,232 parent questionnaires were completed. Of the 1,440 students, 803 were not at the time attending a private school; of the 1,232 parent questionnaires, 690 were completed by parents of students who were not attending a private school. Follow-up testing information is reported only for students who were in public schools at the time of application.

In Washington, D. C., 2,023 students were tested at baseline; 1,928 parent surveys asking questions about each child were completed; 938 student surveys were completed. Of the 2,023 students tested, 1,582 were not attending a private school at the time of application for a scholarship; of the 1,928 parent questionnaires, 1,446 were completed by parents of students who were not then attending a private school. Follow-up testing and survey information was obtained only from families with children not in private schools at the time of application.

### **The Lottery**

The evaluation team conducted the lottery in May 1997 in New York City and April, 1998 in Dayton and D. C. Program operators notified lottery winners in May of the year in which the lottery was conducted. If a family was selected, all children in that family entering eligible grades were offered a scholarship. In order to ensure that an adequate number of scholarships were given to students not currently attending a private school, separate lotteries were held in Dayton and D. C. for students currently in public and private schools. This procedure also assured random assignment to test and control groups of those families

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<sup>9</sup> See Appendix. Exact procedures for the formation of the control group are described in Jennifer Hill, Donald B. Rubin and Neal Thomas, "The Design of the New York School Choice Scholarship Program Evaluation." Paper presented before the American Political Science Association annual meeting in Boston, MA, August 31, 1998.

participating in the evaluation. Only those students who were in public schools at the time of the lottery are included in these evaluations.

Because many more families applied for scholarships in New York City than originally had been anticipated, the evaluation team randomly selected families for vouchers through a two-stage procedure. As families applied for vouchers, they were formed into groups on the basis of their application date. During the early stages, all families were invited to eligibility assessment and data collection sessions. However, after it became clear that more families would be attending these sessions than could be accommodated, the evaluation team began randomly selecting applicants, inviting only those selected to attend the sessions. After the first stage was completed, families who attended these sessions and met the eligibility requirements were then randomly selected for the scholarship group or the control group. To ensure that all families from the different groups had the same chance of being selected for a voucher, the evaluation team adjusted the second-stage selection probabilities to reflect the differential chances of being invited to the verification sessions.

In New York City, the final lottery was held in mid-May 1997. Mathematica Policy Research (MPR) administered the lottery; SCSF announced the winners. Within the parameters established by SCSF, all applicants had an equal chance of winning the lottery. SCSF decided in advance to allocate 85 percent of the scholarships to applicants from public schools whose average test scores were less than the citywide median. Consequently, applicants from these schools, who represented about 70 percent of all applicants, were assigned a higher probability of winning a scholarship. In the information reported in the tables, results have been adjusted by weighting cases differentially so that they can be generalized to all eligible applicants who would have come to the verification sessions had they been invited, regardless of whether or not they attended a low-performing school.

Because vouchers were allocated by a lottery conducted by the evaluation team, those offered scholarships are not expected to differ significantly from members of the control group (those who did not win a scholarship). Baseline data confirm this expectation.<sup>10</sup> In D. C., there were no significant differences in baseline demographic characteristics or initial test scores. The baseline test scores of those entering grades two through eight who were offered a voucher averaged 30.4 national percentile points in reading and 23.8 in mathematics. Those not offered the scholarship scored, on average, 30.3 national percentile points in reading and 22.8 points in math. As in D. C., the demographic characteristics of those offered vouchers in Dayton and New York did not differ significantly from the characteristics of those who were not offered a voucher.<sup>11</sup> Estimated effects of the program on subsequent test scores adjust for baseline test scores.

### **Collection of Follow-up Information**

The second-year follow-up information was collected in New York City in the spring of 1999 and in Dayton and D. C. in the spring of 2000. Data collection procedures were similar across cities. Focus group conversations were recorded at a number of testing sessions in all cities.

Because test-score results from the second-year of the evaluation differ significantly between African American students and those from other ethnic backgrounds, the ethnic composition of the students participating in the evaluation is particularly salient. Forty-two per

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<sup>10</sup> For additional baseline information on Washington, D. C. and Dayton, Ohio, see Paul E. Peterson, Jay P. Greene, William G. Howell, and William McCready, "Initial Findings from an Evaluation of School Choice Programs in Washington, D. C. and Dayton, Ohio," Occasional Paper, Program on Education Policy and Governance, Kennedy School of Government, Harvard University, October 24, 1998, Appendix. Paper prepared under the auspices of the Program on Education Policy and Governance, Harvard University, for presentation before the annual meetings of the Association of Public Policy and Management, New York City, NY October, 1998. Available at <http://data.fas.harvard.edu/pepg/>; for New York City, see Paul E. Peterson, David Myers, Josh Haimson, and William G. Howell, "Initial Findings from the Evaluation of the New York School Choice Scholarships Program," Program on Education Policy and Governance, Taubman Center on State and Local Government, Kennedy School of Government, Harvard University, 1997. Report is available at <http://data.fas.harvard.edu/pepg/>.

<sup>11</sup> For a more extended discussion of the characteristics of applicants for the Dayton scholarship program, see Peterson, Greene, Howell and McCready, 1998.

cent of the students participating in the second year of the evaluation in New York City were African Americans. The percentages in Dayton and D. C. were 74 percent and 94 percent, respectively. Hispanic students participating in the second year of the evaluation constituted 51 percent of the total in New York City, 2 percent in Dayton, and 4 percent in Washington, D. C. Finally, 5 percent of the students participating in the evaluation in New York City were white. The percentages of whites in Dayton and D. C. were 24 percent and 1 percent, respectively. The remaining students came from a variety of other ethnic backgrounds.

**New York City.** To evaluate the effects of the SCSF program in New York City, Mathematica Policy Research (MPR) assembled two statistically equivalent groups of families: (1) a voucher group with 1,000 families and (2) a control group with 960 families. Procedures used to construct the two groups and to collect first-year follow-up information and the results from the evaluation of the first year of the program are described in the appendix.<sup>12</sup> For the second-year follow-up, families were invited in April, May and June of 1999 to attend sessions during which students again took the ITBS in mathematics and reading. Adult members of their family completed surveys that asked a wide range of questions about the educational experiences of their oldest child within the age range eligible for a scholarship. Students in grades three through six were also asked to complete short questionnaires.

Testing and questionnaire administration procedures were similar to those that had been followed during the baseline and first year follow-up sessions. Both the voucher students and students in the control group were tested in locations other than the school they were currently attending.

Sixty-six percent of the students included in the evaluation attended the second-year testing sessions in New York City. Sixty-nine percent of those offered vouchers, as compared to 62 percent of the students in the control group, participated in these sessions. This fairly high

response rate was achieved in part because SCSF conditioned the renewal of scholarships on participation in the evaluation. Also, non-scholarship winners selected to become members of the control group were compensated for their expenses and told that they could automatically reapply for a new lottery if they participated in these follow-up sessions. Detailed response rate information for the second follow-up survey and test, along with response rates for the baseline and the first follow-up surveys and test administrations are reported in the appendix.<sup>13</sup> On the whole, response rates for test and control groups were similar both in the overall response rate and in the demographic and initial test score characteristics of evaluation participants.

Commentators have nonetheless wondered whether the pattern of response in Years One and Two could have affected our findings. According to Richard Rothstein, "those with a voucher knew that its renewal might depend on their children's performance. thus, gains in scores of voucher recipients, compared with controls, may be exaggerated."<sup>14</sup> To forestall this possibility, parents were assured that performance on the test would have no affect on whether or not their voucher was to be continued. After two years this should have been self-evident, since at no time did any student or family or teacher or school official learn the test results for any child. But if parents and children misbelieve that their test performance could affect results, this incorrect perception should have affected the test and control groups equally , because control group members were told that participation in the evaluation made them eligible for participation in a new lottery limited only to those participating in the evaluation.

To adjust for survey non-response in our statistical analyses, we use an analytic model to predict non-response based on a variety of background characteristics. The predicted probability of not responding is then used to adjust the sample weights. Since response patterns are similar for test and control groups, weighted results do not differ substantially from unweighted results.

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<sup>12</sup> For New York, see appendix.

<sup>13</sup> Also, see Myers, Peterson, Mayer, Chou, and Howell, 2000.

**Dayton and Washington, D. C.** In D. C. and Dayton, to estimate the impact of a private school on student test performance after one and two years of attendance at these schools, the evaluation team collected follow-up information between late February and late April, 2000.

The procedures used to obtain follow-up data were essentially the same as those used to collect baseline data, except that data were collected only from students who had not been in private school at the time of the initial scholarship application. Students again took the ITBS in mathematics and reading. Caretakers accompanying the child completed surveys that asked a wide range of questions about the educational experiences of each of their children. Students in grades four through eight also completed a questionnaire that asked them about their experiences at school. Testing and questionnaire administration procedures were similar to those that had been followed at baseline.<sup>15</sup>

To obtain a high participation rate in the follow-up data collection effort, those who had declined the offer of a voucher and members of the control group were compensated for their expenses. They were also told in Washington, D. C. that they would be included in a new lottery, if they participated in the follow-up sessions. In Dayton, a second lottery was promised as a reward for participating in the first follow-up session but not for the second. Instead, families were given a higher level of compensation for participating in the follow-up session.

In D. C., follow-up survey information was obtained for 1,052 students from parents and caretakers. First-year follow-up test information was obtained from 995 students who had also been tested at baseline, a response rate of 63 percent. Of these students, 486 were members of the control group and 509 were members of the treatment group. In the second-year follow up,

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<sup>14</sup> Richard Rothstein, "Judging Vouchers Merits proves to be Difficult Task," *New York Times*, December 13, 2000, p. A25.

<sup>15</sup> Difficulties were encountered in the administration of the first-year test at the initial pilot session in Washington, D. C. Test booklets were not available at the testing site for scholarship students in grades 3-8. Copies of the test arrived eventually, but the amount of time available for testing may have been foreshortened. Significant effects on reading scores are not apparent, but significant effects on math performance are evident, probably because the math test was the last to be administered. Statistical adjustments in the test score analysis take into account the special circumstances of the pilot session.

the overall response rate was 50.3 percent. The response rate for those offered scholarships was 50.5 percent; the response rate for the control group was 50 percent.

In Dayton, in the first-year follow-up, 57 percent of the students in the control group and 56 percent of those offered scholarships returned to take the reading and math tests. In the second year, 49.5 percent of the students in the control group and 47.8 percent of those offered scholarships returned to take the reading and math tests. The Appendix compares the characteristics of participants and non-participants in the second-year follow-up sessions.

In Dayton and D. C., as in New York, baseline demographic and test score information was used to adjust for non-response to requests for participation in follow-up sessions.<sup>16</sup>

### **Data Analysis and Reporting Procedures**

The evaluation takes advantage of the fact that a lottery was used to award scholarships. As a result, it is possible to compare two groups of students that were similar, on average, except that members of the control group were not offered a scholarship. Any statistically significant differences between the two groups may be attributed to the school experience, not the child's initial ability or family background, which were essentially the same at baseline.

This report provides data that help answer two questions. The first question is as follows:

What was the impact on test-score performances of students from low-income families residing within a large central city one and two years after an *offer* of a voucher?

This question can be answered straightforwardly by comparing the test scores of those who were offered a scholarship with the test scores of the control group. Because scholarships were awarded at random, the two groups may be assumed to be, on average, equivalent statistically, save the offer of a scholarship. Any differences between the two groups can, then, be attributed to the scholarship offer.

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<sup>16</sup> The Appendix to this report compares the characteristics of participants and non-participants in the second-year follow-up sessions. For a discussion of the weighting procedures used in these evaluations, see Howell and Peterson, 2000; Wolf, Howell and Peterson, 2000; Myers, Peterson, Mayer, Chou, and Howell, 2000.



To compute program impacts on children's test scores, we estimated a statistical model that took into account students' scholarship or control-group status as well as baseline reading and math test scores. Baseline test scores were included to: 1) adjust for minor baseline differences between the treatment and control groups on the achievement tests; and 2) to increase the precision of the estimated impacts.

For some policy analysts, this first question is the crucial policy question: What happens when a school choice program is put into effect? How does the program impact the population of low-income families who were offered a school-choice scholarship? This query is similar to a question often asked in medical research: What will happen if a particular pill is marketed? How will the health of potential users be altered, whether or not all patients use the pill as prescribed?

This analytic strategy has certain methodological advantages because calculation of the impact of the scholarship offer is quite straightforward. However, generalization from these results alone has the important disadvantage of assuming that usage rates of scholarships are fixed when in fact they might be highly variable, depending upon the size of the scholarship, the time the scholarship is offered, and the marketing of the program as a whole. Also, if programmatic impacts are substantial, participation rates may increase with the passage of time.

For these reasons, most analysts want an answer to the second question as well:

What was the impact on test-score performances of students from low-income families residing within a large central city one and two years after switching from a public to a private school?

In medical research, the parallel question is: What are the consequences of actually taking a pill, as prescribed? In the case of the education intervention evaluated here, the answer to this second question requires a comparison between those switching from a public to a private school and a comparable control group who remained in a public school.<sup>17</sup>

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<sup>17</sup> To compute the program's impact on those who used a scholarship to attend a private school, we used an instrumental variable estimator, which provides an unbiased estimate of the effects of switching to a private school. This procedure is discussed in Joshua D. Angrist, Guido W. Imbens, and Donald B. Rubin, "Identification of Causal

Although we present information describing the effects of an *offer* of a scholarship, the text of this report will discuss, for the most part, the impact on students in the first and second years of a switch from a public to a private school. Second-year results compare those in private schools for two years with a control group that was not in private school for two years. Although most of the control-group students had never attended a private school, some had attended for one year.

### **Test Score Findings**

The results reported below provide the first information from randomized field trials on the effects of school vouchers over a two-year period of time from three sites. However, they build upon a body of research that has explored the differences between schooling for low-income minorities in the public and private sectors.

### **Prior Research**

Several studies have compared the test performance of students in public and private schools, and they usually find that low-income and African American students attending private schools outperform their public-school peers. Information on the effects of attendance at a Catholic high school are contained in a recent University of Chicago analysis of 12,000 students in the National Longitudinal Survey of Youth, conducted by the Department of Education. This report finds that, even when adjustments are made for family background, students from all racial and ethnic groups are more likely to go to college if they attended a Catholic school, but the effects are the greatest among urban minorities. The probability of graduating from college rises from 11 to 27 percent, if such a student attends a Catholic high school.<sup>18</sup> This study's findings are consistent with

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Effects using Instrumental Variables," *Journal of the American Statistical Association*, 91 (1996), 444-462. The procedure, widely used by statisticians to correct for selection effects, was used to estimate the effects of actual class size reduction in Tennessee. See Alan Krueger, "Experimental Estimates of Education Production Functions." *Quarterly Journal of Economics*, 114 (1999), 497-533.

<sup>18</sup> Derek Neal, "The Effects of Catholic Secondary Schooling on Educational Achievement," (Harris School of Public Policy, University of Chicago and National Bureau for Economic Research, 1996), p. 26.

other studies.<sup>19</sup> After reviewing the literature on school effects on learning, University of Wisconsin Professor John Witte concludes that studies of private schools "indicate a substantial private school advantage in terms of completing high school and enrolling in college, both very important events in predicting future income and well-being. Moreover, . . . the effects were most pronounced for students with achievement test scores in the bottom half of the distribution."<sup>20</sup>

Even the most careful of these studies, however, can take into account only observed family background characteristics. They cannot be sure that they have taken into account an intangible factor — the willingness of a family to pay for their child's tuition, and all that this implies about the importance they place on education. As a result, it remains unclear whether the findings from these studies describe actual differences between public and private schools or simply differences in the kinds of students and families attending them.<sup>21</sup> In the jargon of the research community, this is called the self-selection problem, a problem for researchers that arises when a population differentiates itself by freely selecting a particular situation, in this case, private school. How can one be sure that the findings are not due to the self-selected character of the population, not the education intervention?

The best solution to the self-selection problem is the random assignment of students to test and control groups. Until recently, evaluations of voucher programs have not utilized a random-assignment research design and therefore have not overcome the possible selection

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<sup>19</sup> William N. Evans and Robert M. Schwab, "Who Benefits from Private Education? Evidence from Quantile Regressions," (Department of Economics, University of Maryland, 1993); David Figlio and Joe Stone, "School Choice and Student Performance: Are Private Schools Really Better?" (University of Wisconsin Institute for Research on Poverty, 1977).

<sup>20</sup> John F. Witte, "School Choice and Student Performance," in Helen F. Ladd, ed., *Holding Schools Accountable: Performance-Based Reform in Education* (Washington, D. C.: Brookings, 1996), p. 167.

<sup>21</sup> Major studies finding positive educational benefits from attending private schools include James S. Coleman, Thomas Hoffer, and Sally Kilgore, *High School Achievement* (New York: Basic Books, 1982); John E. Chubb and Terry M. Moe, *Politics, Markets, and America's Schools* (Washington: Brookings 1990); Derek Neal, "The Effects of Catholic Secondary Schooling on Educational Achievement," (University of Chicago, Harris School of Public Policy and National Bureau for Economic Research, 1996). Critiques of these studies have been prepared by Arthur S. Goldberger and Glen G. Cain, "The Causal Analysis of Cognitive Outcomes in the Coleman, Hoffer, and Kilgore Report," *Sociology of Education*, vol. 55 (April-July 1982), pp. 103-22; Douglas J. Wilms, "Catholic School Effects

problems. Privately funded programs in Indianapolis, San Antonio, and Milwaukee admitted students on a first-come, first-served basis. And in the state-funded program in Cleveland, though scholarship winners were initially selected by means of a lottery, eventually all applicants were offered a scholarship, thereby precluding the conduct of a randomized experiment. The public Milwaukee program did award vouchers by a lottery, but data collection was incomplete.<sup>22</sup>

As a consequence, the findings presented here on New York, D. C., and Dayton provide a unique opportunity to examine the effects of school vouchers on students from low-income families who live in central cities. In contrast to prior studies, random assignment was conducted by the evaluation team, follow-up test-score information was obtained from about one-half to two-thirds of the students who participated in the lottery, and baseline data provided information that allowed the analysts to adjust for non-response.

### **Effects of the Voucher Offer**

The average estimates of the impacts of a voucher offer in all three cities provide a better indication of programmatic effects than do the results from any one city, because minor fluctuations in data collection may influence results in any one site. Also, when student performance is estimated on the basis of one-hour testing sessions, combined test-score performance of students on the reading and math tests is a better indicator of student achievement than either test separately. Theoretically, the more test items used to evaluate performance, the more likely it is that one will estimate performance accurately. Empirically, performances on the two tests are highly correlated with one another ( $r$  equals about .7). In addition, results from the two tests, when combined together, were found to be more stable across time and from place to place, indicating that combining results from the two tests reduces

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on Academic Achievement: New Evidence from the High School and Beyond Follow-up Study," *Sociology of Education*, 58 (1985), 98-114.

random, idiosyncratic variations in observations of student performance.<sup>23</sup> For these reasons, the average from all three cities of the effects of an offer, as presented in Table 1, provides the best available information on the effects of the voucher offer.

In the case of ethnic groups other than African Americans, no significant effects of a voucher offer were observed. For African Americans, however, statistically significant effects on combined test scores were observed after two years in all three cities. The average effect of an offer of a voucher in the three cities on combined test scores was 2.0 percentile points after one year, and 3.5 percentile points after two years. In math the average effect in the three cities was 3.2 points after one year, and 3.4 points after two years. In reading, the average effect in the three cities was 0.8 after one year and 3.6 after two years. Tables 2A, 2B, and 2C provide information by city on the effect of a simple offer of a voucher on test scores.

The effects of an offer, as reported in these tables, are considerable less than the size of the effects of switching to a private school, because only about one-half the students offered a voucher were using the voucher at the end of the second year. Low-income families in central cities are a highly mobile population and their resources are limited. The vouchers offered by the program sponsors in these three cities did not exceed \$1,700 and were often less. Presumably, in a larger, government-funded voucher program with an unlimited time span, voucher participation rates—and, therefore, voucher offer effects--would be larger and increase over time, especially if gains from vouchers were apparent to families and the number of spaces in private schools increased. It is thus of interest to determine the effects of switching to a private school.

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<sup>22</sup> Results from these evaluations are reported in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Brookings, 1998).

<sup>23</sup> This procedure was also employed in Krueger. 1999.

### **Effects of Switching from a Public to a Private School**

As can be seen in Table 3, overall results differ depending on whether the student is African American or from some other ethnic group. One finds no significant differences between the test-score performance of non-African American students switching from a public to a private school and the performance of students in the control group--either after one or two years. Nor were significant differences observed in the test-score performance of these students on reading and math tests, considered separately. Nor were significant differences observed for these students in any one of the three cities.

The effects of switching to a private school on African American students, however, differed markedly from the effects on students from other ethnic backgrounds. In the three cities, taken together, African American students who switched from public to private schools scored, after one year, 3.3 NPR points higher on the combined math and reading tests, and, after two years, 6.3 percentile points higher, than the African American students in the control group. As can be seen in Tables 4A, 4B, and 4C, these differences in combined test score performance were statistically significant in all three cities. These are the average results for the three cities combined, weighting each city in inverse proportion to the standard error of the estimate for that city. Unweighted estimates for the impact on black students after two years are slightly larger.<sup>24</sup>

Although the overall test performance is the most reliable information, Table 3 also shows that, for the three cities taken together, differences after two years are approximately the same for the reading and math tests. On average, African American students in the three cities who switched from public to private schools achieved 6.3 percentile points higher on the reading test and 6.2 points higher on the math test than did the African American students in the control group.

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<sup>24</sup> The average unweighted impact on combined test scores of switching schools is, for blacks, 2.7 after one year, 6.6 after two years. For math, unweighted impacts are 4.8 after one year, 6.5 after two years; for reading, 0.6 after one year, and 6.8 after two years.

The findings for each city are reported in tables 2A, 2B, and 2C. No effects on students from ethnic backgrounds other than African American were observed in any city. The largest differences between African American students who switched from public to private schools and those in the control group, were observed in Washington, D. C. In this city, black students attending private schools for two years scored 9.0 percentile points higher on the two tests combined than did students in the control group. The smallest differences after two years were observed in New York City. In this city, African American students attending private schools scored 4.3 percentile points higher on the reading and math tests combined. In Dayton, the difference in combined test-score performance was 6.5 percentile points, nearly at the mid-point between the differences observed in the other two cities.

The trend over time also varies from one city to the other. As can be seen in Table 2A, in New York City, substantial test-score differences between African American students in private and public schools appear at the end of the first year but then attenuate slightly in the second year. The combined score difference after two years is 4.3 percentile points, which is slightly but not significantly (in statistical terms) less than the 5.8 percentile points observed after one year. In this city, one may reasonably conclude that the initial gains from the school voucher program for African Americans are preserved but do not increase between year one and year two.

In Dayton, there seems to be a steady upward trend in the combined test score performance of African Americans in reading and math. Table 2C shows that African American students who switched from public to private school performed 3.3 percentile points higher on the combined test in year one and 6.5 percentile points higher in year two.<sup>25</sup>

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<sup>25</sup> If those whose scores jumped or dropped dramatically between baseline and year one are excluded from the analysis, then the gains in year one are larger than those reported here. See Howell and Peterson, 2000. Now that data are available for two years, we have chosen not to exclude these students from the analysis, because it is more difficult to justify such exclusions after two years than after just one. After all, students might make striking gains

In some ways the most striking results are for African Americans in Washington, D. C. As can be seen in Table 2B, in general, no significant differences were observed in year one, but a large impact of attendance at a private school was observed after two years. Also in D. C., clear differences between the impact of the program on older and younger students were observed in year one.<sup>26</sup> Younger students may have benefited slightly from the voucher program after one year, but older students did not. In fact the older students who switched to private schools performed poorly at the end of the first year. Many of these students were quite unhappy with their new school, as they communicated frankly in their questionnaire responses.<sup>27</sup> We think the low performance of these students in year one reflects more their discontent than their actual achievement level. By the end of the second year, the transformation in the test-score performance of these same students suggests that this discontent seems to have subsided. When data from the responses to the student questionnaire for the second year become available, it will be possible to ascertain whether or not this in fact was the case. In any case, by the end of year two, younger and older African American students were benefiting similarly from the switch to private schools. Younger students who had changed schools performed on the combined test 9.3 percentile points higher than those remaining in public schools. Older African American students in private school scored 10.3 percentile points higher.

### **Interpreting the Magnitude of the Effects**

These effects are moderately large. As can be seen in Table 5, black students who switch to private schools score, after one year, 0.17 standard deviations higher than the students in the control group. After two years, the size of the effect grows to 0.33 standard deviations, about a

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that are real—or suffer genuinely serious losses—over a two-year time period. Changes of this magnitude over one year seem less plausible. Given our decision not to exclude cases with significant changes in year two, it was desirable, for the sake of consistency, to apply the same framework to the analysis of year-one data.

<sup>26</sup> The D.C. program offered the only opportunity to examine the effect on test scores of an offer of a school voucher to older students. While vouchers were offered to middle-school students in Dayton, there were not enough cases to justify a separate analysis.



third of the difference in test score performances between blacks and whites. If this trend line should continue in subsequent years, even at an attenuated rate, it would eliminate the test-score difference between blacks and whites for those who switch to private schools.<sup>28</sup> Continuing evaluation of voucher programs may provide information on whether or not these gains can be consolidated and extended.

Another way of obtaining a sense of the magnitude of these effects is to compare them to the effects observed in an evaluation of a class-size reduction intervention conducted in Tennessee, the only other major education reform to be subjected to evaluation by means of a randomized field trial. The effects on African Americans of attendance at a private school shown here are larger than the estimated effect of a 7-student reduction in class size. According to a recent reanalysis of data from Tennessee, the class-size reduction effect for African Americans after two years was, on average, 4.9 percentile points, somewhat less than the 6.7 percentile effect of switching to a private school.<sup>29</sup>

It is also of interest to compare the size of the effects of the voucher intervention with the size of the effects reported in the RAND study entitled *Improving School Achievement* released in August 2000.<sup>30</sup> Identifying the most successful states, Texas and North Carolina, which have introduced rigorous accountability systems that involve state-wide testing, the study finds what it says are “remarkable” one-year gains [in math scores] in these states of “as much as 0.06 to 0.07 standard deviation[s] per year”—or 0.12 to 0.14 over two years. The two-year effects of the school voucher intervention on black students observed here are over twice as large.

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<sup>27</sup> Parent and student surveys corroborate this intuition. Black students in grades 6-8 who attended private schools expressed less satisfaction and lower morale, and reported a higher frequency of expulsions and fewer friends than students attending private schools in grade 2-5. See Wolf, Peterson, and Howell, 2000.

<sup>28</sup> Christopher Jencks and Meridith Phillips, eds., *The Black-White Test Score Gap* (Washington, D. C.: Brookings, 1999).

<sup>29</sup> Krueger, p. 525.

<sup>30</sup> Also, see Ann Flanagan, Jennifer Kawata and Stephanie Williamson. 2000. *Improving Student Achievement: What NAEP Test Scores Tell Us* (Santa Monica, CA: RAND Corporation, 2000), p. 59.

### **Explicit Controls for Family Background Characteristics**

When initially released, the results reported above have been questioned by interest groups and scholars who think that a randomized assignment to test and control groups does not adequately control for the family background characteristics of the students in the two groups. According to Bruce Fuller, for example, “The experimental group may have been biased as some of the most disadvantaged voucher winners did not switch to a private school, and therefore were excluded from the group (possibly boosting mean achievement levels artificially)”<sup>31</sup> Similarly, the People for the American Way have argued that this “study’s key finding improperly compares two dramatically different groups and may well reflect private school screening out of the most at-risk students”<sup>32</sup>

These criticisms are based upon an inaccurate characterization of our analysis. They misunderstand the design of the study and incorrectly suggest that we drop some students from the analysis. As mentioned previously, roughly half the students took the voucher that was offered to them (the takers) and about half did not (the decliners). As we state clearly in our reports, takers and decliners differed in a number of respects. Most notably, takers had higher family incomes in New York and D.C., but lower incomes in Dayton. The New York and D.C. findings are not surprising, given that the voucher awards did not cover all the costs of a private education. These additional costs were the reason most frequently given by families for not using the voucher. Presumably, take-up rates would rise if the monetary value of vouchers were increased.

The decliners are not dropped from the analysis, however. All members of the treatment groups were invited to follow-up testing sessions, whether or not they made use of their voucher,

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<sup>31</sup> Bruce Fuller, Luis Huerta, and David Ruenzel. 2000. “A Costly Gamble or Serious Reform? California’s School Voucher Initiative—Proposition 38.” Policy Analysis for California Education. University of California, Berkeley and Stanford University, p. 10.

<sup>32</sup> “Deception by the Numbers: Ten Reasons to Doubt the Latest Claims for Vouchers.” A document on the website of People for the American Way Foundation, p. 3.

and all who participated in the follow-up testing sessions are included in the analysis. To estimate the impact of switching from a public to a private school, the analytical technique is not to compare takers with the control group, as Fuller and the People for the American Way have suggested. Instead, the instrumental variable technique is used to obtain unbiased estimates of the effects of attending a private school.

In the absence of randomized field trials, analysts usually attempt to adjust for selection effects by controlling for initial test scores and family background characteristics. Such studies, however, cannot adjust for unobserved differences between treatment and control groups, which in the case of voucher programs might be considerable. The instrumental-variable model used here, random assignment to test and control groups, does adjust for unobserved differences between takers and decliners, because the instrumental variable, random assignment, is not correlated with the error term in the second-stage equation.<sup>33</sup>

Inasmuch as the instrumental variable effectively controls for selection effects, results are unlikely to vary materially when one also controls for family background characteristics. Our results are consistent with this expectation. We report for African Americans in table 6 results from estimations which control not only for initial test scores (as do the analyses in the previous tables) but also for mother's education, mother's employment status, family size, and whether or not the family received welfare. The estimated impacts on the test scores of African Americans of switching from a public to a private school in the three cities remain exactly the same—6.3 National Percentile Ranking (NPR) points, a statistically significant impact. Minor differences are observed when impacts within each individual city are estimated. When estimating effects in New York City without controlling for family background characteristics, the impact is estimated to be 4.4 NPR points; when family background controls are added, the impact is 4.2

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<sup>33</sup> The technique was first used in medical research, is now commonplace in econometric studies, and was employed by Alan Krueger in his analysis of the effects of class size on student performance in Tennessee, a study praised by

NPR points. In Dayton, Ohio, when controls are introduced, the point estimate drops from 6.5 to 5.9 NPR points. And in Washington, D.C., the estimated impact increases from 9.0 to 9.1 NPR points.

### **Parental Satisfaction**

Most studies of scholarship or voucher programs for low-income minority families have found that families receiving the scholarships are much more satisfied with their schooling than are families who remain in public schools.<sup>34</sup> The results from New York's second year confirm the earlier findings.<sup>35</sup> When asked to assess their school overall, families give higher marks to the private schools. Nearly 40 percent of the scholarship users give their school an “A” compared with less than a tenth of the control group do (Table 7).

We also examined parental satisfaction with specific dimensions of school life. On every aspect of a school about which parents were questioned, private-school parents were substantially more satisfied than control-group parents. The percent of parents “very satisfied” with a private school was significantly higher for all of the following: location of the school, school safety, teaching, parental involvement, class size, school facility, student respect for teachers, teacher communication with parents with respect to their child’s progress, extent to which child can observe religious traditions, parental support for the school, discipline, clarity of school goals, staff teamwork, teaching, academic quality, the sports program, and what is taught in school (Table 7). Forty-four percent of the private-school parents were very satisfied with the academic quality of the school as contrasted with just 5 percent of the control group. Similarly,

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many of the same people who have criticized our report, Alan Krueger, “Experimental Estimates of Education Production Functions.” *Quarterly Journal of Economics*, 114 (1999), 497-533.

<sup>34</sup> A summary of findings from earlier studies is available in Paul E. Peterson, “School Choice: A Report Card,” in Peterson and Hassel, *Learning from School Choice*, p. 18. Mark Schneider, Paul Teske, Melissa Marschall, and Christine Roch, “Tiebout, School Choice, Allocative and Productive Efficiency,” paper prepared for annual meetings of the American Political Science Association, 1998, finds higher levels of parental satisfaction within New York City public schools, when parents are given a choice of school.

<sup>35</sup> Parental and survey information after two years is currently available only for New York City; similar information for Washington, D. C. and Dayton, Ohio is currently being prepared for analysis.

47 percent of the private-school parents expressed the highest satisfaction with “what’s taught in school” compared with 7 percent of the control group.

The scholarship program had the smallest impact on parental satisfaction with schools’ sports programs. Less than a quarter of the scholarship parents were very satisfied with a school’s sports program compared with 6 percent of the control- group parents.

It may be hypothesized that the voucher program, like other innovations, has a Hawthorne effect, namely, the fact of innovation and change by itself enhances levels of parental satisfaction. If so, then the scholarship program might be expected to have a lesser impact on parental satisfaction after two years than after one. Parents may initially be impressed with the fact that they have a choice of school; with the passage of time, however, the initial impression may be moderated by the discovery that the school may not fully live up to its reputation.

Differences in the level of satisfaction between public and private schools did not, for the most part, change significantly between the first and second years of the voucher program. On all the specific dimensions of school life about which parents were asked, differences in the percent of public- and private-school parents claiming satisfaction did not change significantly from one year to the next. For example, the difference in private- and public-school parent satisfaction with the academic quality of the child's school was 40 percentage points at the end of the first year and 39 points at the end of the second--a statistically insignificant change. Nor were there any statistically significant changes from the first to the second year in parental responses to the 16 questions probing about satisfaction with other dimensions of school life, including class size, discipline, school safety, teaching, teacher-parent communication, and teaching values (See Table 7).

When, however, parents were asked the overall grade they would give the school attended by their child, we found some evidence of either an initial Hawthorne effect or else that the program’s impact was declining. Whereas 49 percent of private-school parents gave their

school an "A" at the end of the first year, only 38 percent did so at the end of the second year. Public-school parents who gave their school an "A" declined by only one percentage point, from 10 to 9 percent. In other words, the difference in private- and public-school parent willingness to give their school an "A" declined from 39 to 29 percentage points, a significant change (Table 7). Still, after two years, private-school parents were still much more likely to give their school an "A" than were control-group parents.

### **Testing the Sore Loser Hypothesis**

Some have hypothesized that applicants who are denied vouchers are so shattered by the experience that they no longer remain engaged in their child's education. New York Times columnist Richard Rothstein, for example, offers the suggestion that parents who lost the lottery were "sore losers":

Parents know if their children got vouchers and this knowledge can affect results. For example, volunteers for vouchers, already more dissatisfied with public schools than others, may have their hopes raised, then dashed when they were not selected for a voucher. Sorely disappointed, they may then demand less of their children in public school.<sup>36</sup>

The sore loser hypothesis is worth exploring, because, as we have just seen, parents with children in private school are much more likely to be satisfied with their child's school. Is it possible that the test score results that we have observed are due not so much to differences to a higher quality schooling in private school as to a deterioration in parental involvement in their child's education when choice is not available. If the latter, then the case for giving parents choice would become quite compelling.

To test this hypothesis, we first compared levels of parental satisfaction at baseline, after one year, and after two years. On each of these occasions, parents were asked: "How satisfied

are you with the following aspects of your child's current school?" Parents were given four options, "very satisfied" indicating the highest level of satisfaction; this response category proved to be the one that discriminated most clearly between parents of students at public and private schools.

Parents were asked this question about the following aspects of the school: teaching, school safety, parental involvement, class size, school facility, student respect for teachers, communication regarding student progress, freedom to observe religious traditions, and the school's location. In all cases, those not receiving the voucher were slightly *more* satisfied after one year after having been denied a scholarship than they said they were at the time they had applied (table 8). At the end of two years, control-group satisfaction levels had deteriorated somewhat so that on five of the nine items, they expressed slightly but not significantly less satisfaction than at baseline. In other words, there is very little evidence that the control-group parents were "sore losers." Given the fact that they knew they had only about a one in twenty chance of winning, their initial expectations were probably not unduly high.

Parental responses to questions about their relationships with their children cast further doubt on the hypothesis that control-group parents were sore losers who cared less for the education of their children after having lost the lottery. Parents were asked how often they helped their child with homework, talked with their child about school, attended school activities, and worked on school projects. In every case, the answers given by parents with children in private school for two years resembled those of the control group (Table 9). These findings are similar to those observed after one year.<sup>37</sup>

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<sup>36</sup> Richard Rothstein, "Judging Vouchers' Merits Proves to be Difficult Task," *New York Times*, December 13, 2000, p. A25.

<sup>37</sup> Peterson, Myers, and Howell, 1998, Table 13.

### **Possible Explanations for Private-School Impacts**

A variety of additional explanations for the impact of private schools on the test scores of African American students have been offered. We shall not attempt to adjudicate among these alternative explanations; however, we shall present data from the parent survey administered during the second-year follow-up in New York City that may shed light on the plausibility of alternative explanations.

#### **School Facilities.**

Do private schools provide superior instruction because they have more financial resources? Many lay observers have the impression of private education as that provided by an expensive day school catering to well-to-do families or an exclusive boarding school attended by college-bound "preppies." Students are taught in small classes in well appointed settings by high paid teachers. Giving expression to this point of view, Sandra Feldman, head of the American Federation of Teachers, attributed the positive effects of vouchers to the fact that students in private schools were taught in smaller classes. "I see it as a validation of the need for small class sizes," she observed.<sup>38</sup>

Yet most private schools are not prestigious prep schools but instead have a religious affiliation, modest tuition, and limited facilities. Nationwide, the average private school expenditures per pupil in 1993-94 were estimated at \$3,116, considerably less than public school expenditure per pupil, which was \$6,653.<sup>39</sup>

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<sup>38</sup> Jeff Archer, "N.Y. C. Voucher Students Post Modest Gains," *Education Week*, November 9, 1998,

<sup>39</sup> Andrew J. Coulson, *Market Education: The Unknown History* (New Brunswick: Social Philosophy and Policy Center and Transaction Publishers, 1999), p. 277.



Per pupil expenditures for both Catholic and public schools were available for schools in three boroughs of New York City.<sup>40</sup> In comparing expenditures, the amount spent by New York public schools for all items that did not clearly have a private-school counterpart was deducted. Among other things, deductions were taken for all monies spent on transportation, special education, school lunch, other ancillary services, and the cost of financing the far-flung bureaucracy that runs the city-wide, borough-wide, and district-wide operations of the New York City public schools.

Taking all these deductions from public school expenditures amounted to no less than 40 percent of the cost of running the New York City public schools. But even after taking all these and other deductions, public schools were still spending over \$5,000 per pupil each year, more than twice the \$2,400 spent on similar services in Catholic schools.

In Washington, D. C., median tuition at the private schools attended by the scholarship students included in the evaluation was \$2,600 in the year 1998-99. The average tuition was \$3,113 per year.<sup>41</sup> The average is substantially higher than the median because of the high tuition charged by a few independent schools, such as Sidwell Friends, the school attended by Chelsea Clinton, which charged over \$15,000 per year. Assuming that the ratio of tuition to total educational expenditure in Washington, D.C. is the same as in the three boroughs in New York City discussed previously, private educational expenditures, on average, exceed tuition by an estimated 28 percent or a total of \$3,988 in 1998-99.

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<sup>40</sup> Estimates are based on information about Catholic schools in three boroughs within New York City in an unpublished memorandum submitted to PEPG from the New York archdiocese in August 1999. Public-school expenditure by school for the City of New York is available on the Board of Education website.

<sup>41</sup> Private school tuition rates were estimated in part from information provided in Lois H. Coerper and Shirley W. Mersereau. *Independent School Guide for Washington, D.C. and Surrounding Area*. 11<sup>th</sup> ed. (Chevy Chase, MD: Independent School Guides, 1998). For schools not listed in this volume, information was obtained in telephone conversations with school staff. Some schools have a range of tuition charges, depending on the number of students from the family attending the school and other factors. The tuition used for this calculation is the maximum charged by the school. The tuition also includes all fees, except for the registration fee, which is ordinarily treated as partial payment toward tuition. Figures are weighted proportionate to the number of students in the evaluation attending a particular school. Public-school expenditure includes the costs of transportation and special education, which may not be provided by private schools.

Average per pupil costs of the public schools in Washington was \$8,812 in 1995-96, the last year for which reliable information was available.<sup>42</sup> However, this figure includes monies for ancillary costs, such as transportation, school lunch, capital costs and central administration, costs not incurred by all private schools. When public-school expenditures for services and programs comparable to those offered in private schools are considered, estimated average public-school per-pupil expenditure was \$7,653 in 1995-96. Presumably, per pupil expenditure was higher in 1998-99. But if public-school expenditure remained constant after 1996, the amount spent per pupil was an estimated 92 percent higher than those in the private schools attended by the average scholarship student.

Parental reports are consistent with these expenditure data. According to the parents, private schools were less likely to have a library, a nurse's office, child counselors, and special programs for non-English speakers and students with learning problems. The greatest difference was for programs for non-English speaking students. Forty-four percent of the private-school parents reported such a program in their school compared with 80 percent of the control-group parents. Most other differences were not as large; for example, 58 percent of the private-school families reported that their school had a program for the learning disabled compared with 74 percent of the parents in the control group (Table 9). Public schools are also larger. As estimated by New York parents, the effect of choosing the private sector was to reduce the average size of the school by 140 students or over 25 percent—from an average of 525 students to 385 students (Table 10).<sup>43</sup>

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<sup>42</sup> Data taken from the U. S. Department of Education, Office of Educational Research and Improvement. National Center for Education Statistics, *Common Core of Data, School Years 1993-94 through 1997-98*. (Washington, D. C.: 2000). Comparable data estimate excludes public-school expenditure for student transportation, food services, enterprise operations, non-elementary/secondary programs, adult education, capital outlay, payments to other school systems, payments to state governments, interest on school system debt, central support for planning research and management services, and unspecified support services.

<sup>43</sup> Provided with large differences in school size and other characteristics of schools for members of the treatment and control groups, we plan in the future reports to assess the extent to which the differences may indicate impacts of vouchers on other outcomes.

In a few instances, private-school parents in New York reported more resources. They were somewhat more likely to say their school had a computer laboratory, a music program, and individual tutors. In other cases such as programs in arts, programs for advanced learners, a gymnasium, and after-school programs, no differences between the two groups were evident.

Despite the more limited financial resources of the private school, parents reported that their children had been in classes that had on average two fewer students.<sup>44</sup> However, the reduction in class size was only two students, considerably less than the amount generally thought to be necessary to achieve significant gains from class size reduction.<sup>45</sup> It is thus unlikely that the positive effects of attending a private school on African American test scores were due to the slightly smaller instructional settings within which students were being taught.

### **School Climate**

In John Chubb and Terry Moe's study of public and private schools, they found the educational environment of private schools was more conducive to learning than that of public schools.<sup>46</sup> They pointed out that public schools are governed by state laws, federal regulations, school board requirements, and union-contract obligations that impose multiple and not always consistent rules on teachers and principals. Because they must respond to numerous legal and contractual requirements, school administrators and teachers focus more on rule-compliance than on educational mission, undermining the morale of educators whose original objective was to help children learn.

The problem, Chubb and Moe say, is particularly prevalent in big city schools, a viewpoint shared by a Dayton focus-group parent, who tried to explain the Catholic-school advantage in these terms:

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<sup>44</sup> Peterson, Myers, and Howell, 1998, Table 5.

<sup>45</sup> The reduction in class size in the Tennessee experiment was an average of approximately eight students. Mosteller.

<sup>46</sup> John E. Chubb and Terry M. Moe, *Politics, Markets and America's Schools* (Washington, D. C.: Brookings Institution Press, 1990).

The other advantage to Catholic schools that I've seen is.... they're not governed by the Board of Education....And there's no bureaucracy. If your child needs this service and the school's providing it and it's working, there's one little tiny group that you go to..... You try fighting the Board of Education. I've done it. It can be done, but most of us don't have the energy.<sup>47</sup>

Private schools, operating with greater autonomy, focus more directly on their educational mission and, as a result, achieve a higher degree of internal cohesion, Chubb and Moe say. To do otherwise would jeopardize their survival as a fragile institution dependent upon the annual recruitment of new students. As a result, principals and teachers in the private sector enjoy higher morale. Their interactions with one another and with their students are more positive, fostering a more effective learning environment.

Chubb and Moe's findings were based on interviews with teachers and administrators. To see whether parents confirmed these reports, focus group parents were asked about order and discipline in public and private schools. One District of Columbia mother compared the public and private schools in terms of structure, "I like the structure [in the private school], which is why I took him out of public school. Where he was attending.... there was no structure."<sup>48</sup> Said another public-school parent:

The discipline is just not up to par. I mean, one day I ... just popped in and it was like, kids just running all over the class. I mean, the class size is enormous. I think it's too many kids for one teacher to handle.<sup>49</sup>

Another mother described an experience she had one day when she was monitoring the Stanford 9 test at her son's public school.

My job was to walk around and to make sure everyone wasn't cheating. But I tell you this, when a teacher went to a kid's table, and I thought it was real wrong, and that kid put down the wrong answer, it was like [made a sound like clearing her throat. Those teachers wanted their kids to be like on top. I thought that was so wrong.<sup>50</sup>

<sup>47</sup> Focus Group Afternoon Session B, March 20, 1999, Dayton, Ohio.

<sup>48</sup> Focus group session, Washington, D.C., March 6, 1999.

<sup>49</sup> Focus group session, Washington, D.C., March 27, 1999, afternoon session.

<sup>50</sup> Focus group session, Washington, D.C., April 18, 1999.

On the other hand, one D. C. mother felt her public school was so excellent it was just like a private school:

**Mother:** My daughter is in private school, [name of school given] and...

**Another Mother:** That's public school.

**Mother:** Oh, excuse me, public school. And I am very... and I have said probably because it's very much like a private school, I am very satisfied with ----- . . . , one of the best elementary schools in D.C..

**Focus Group Leader:** When you say it's like a private school, how is it like a private school?

**Mother:** Well, because they are well-structured . . .--a lot of parent participation. . . . The academics are great. The children are well disciplined. An example, we went to ---Auditorium on Thursday. . . . her class was the best disciplined in the entire auditorium.<sup>51</sup>

Open classrooms and multiple grades seem to add to the discipline problems in D.C. public schools, at least in the view of some parents. Said one mother, in a focus group session:

I have two boys. Both of them are in the first grade. . . . Last year they were fine because they had a closed classroom. This year, my youngest son, he's not doing too well. . . . The classroom is too big. Discipline is terrible. . . . I've seen the teachers snatch the kids around there, they running all in the hallways and you can hear what's going on in that classroom all the way over there. I don't think the teachers are dedicated to their work. . . . And there's no after-school tutoring, nothing.<sup>52</sup>

If parent reports are accurate, the scholarship program had a major impact on the daily life of students at school. New York public-school parents were more likely to report that the following were a serious problem at their school: students destroying property, tardiness, missing classes, fighting, cheating, and racial conflict. For example, 33 percent of the private-school parents thought that fighting was a serious problem at their school versus 70 percent of the control group (Table 11). The percents perceiving tardiness as a problem were 33 for the scholarship users and 62 for the control group. Less than 30 percent of private-school parents but 45 percent of the control group said that destruction of property was a serious problem at their school. The results are similar to those reported in the first-year evaluation.<sup>53</sup>

<sup>51</sup> Focus group session, Washington, D.C., March 27, 1999, afternoon session.

<sup>52</sup> Focus group session, Washington, D.C., March 27, 1999, afternoon session.

<sup>53</sup> Peterson, Myers, and Howell, 1998, table 8.

### Homework and Parental Communication

Hoffer, Greeley, and Coleman, have attributed the higher level of student performance in private schools in part both to the amount of homework expected of the students and to the amount of communication between schools and parents.<sup>54</sup> The reports by New York parents are consistent with their interpretation.<sup>55</sup> Sixty-four percent of private-school parents reported that their child had at least an hour of homework a day, whereas only 41 percent of the control-group parents reported a similar volume of homework (Table 12). Private-school parents were also less likely to say the homework was too easy. Twenty percent of the control-group parents gave this response, as compared with 4 percent of private-school parents.

Compared with control-group parents, parents of students in private schools also said that they received more communication from their school about their child. The results presented in Table 13 indicate that a higher percent of private-school parents versus control-group parents reported

- being more informed about student grades halfway through the grading period;
- being notified when their child is sent to the office the first time for disruptive behavior;
- parents speaking to classes about their jobs;
- regular parent-teacher conferences;
- parents participating in instruction;
- parents receiving notes about their child from the teacher;
- parents receiving a newsletter about what is going on in school; and
- regular parent-teacher conferences.

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<sup>54</sup> Thomas Hoffer, , Andrew Greeley, and James Coleman, 1985. "Achievement Growth in Public and Catholic Schools," *Sociology of Education*, April, 58: 74-97.

<sup>55</sup> For very similar first-year results, see Peterson, Myers, and Howell, 1998, table 9.

## Conclusions

Randomized field trials are the best available tool for detecting the effects of an educational intervention, because random assignment to test and control groups assures that all significant effects may be attributed to the intervention, not to the students' initial abilities or their family backgrounds. Nonetheless, when interpreting the findings from the evaluation of any one program in a particular city, generalizations to a larger universe are problematic. Conditions specific to that place or minor fluctuations in testing conditions might skew results in one direction or another. But when similar results emerge from evaluations of school voucher programs in three sites in different parts of the United States, they provide a stronger basis for drawing conclusions and generalizing to a larger context. Thus, the average impact across the three sites may provide a reasonable estimate of the likely initial impact of a school voucher initiative elsewhere.

In the three cases, taken together, we found effects of school vouchers only on the average test performance of students from African American backgrounds. Black students who switched from public to private schools in the three cities scored after two years, on average, approximately 6.3 percentile points higher on the Iowa Test of Basic Skills than comparable blacks who remained in public schools.

At this point we do not know why the gains from switching to a private school are evident for black students after two years, but not for students from other ethnic backgrounds. However, parents in New York reported that private schools are smaller in size, maintain a better disciplinary climate, ask students to do more homework, maintain closer communication with families, and have slightly smaller classes (about 2 fewer pupils). It remains to be seen whether any or all of these factors are especially associated with black test-score performance. Given the widespread concern about racial differences in academic performance, our research is particularly salient in that it suggests that school voucher programs may have the capacity to

shrink the black-white test-score gap for participating students. We plan to explore this topic further in future analyses.

One must qualify any generalizations from the results of this pilot program to a large-scale voucher program that would involve all children in a large urban school system. Only a small fraction of low-income students in these three cities' schools were offered vouchers, and these voucher students constituted only a small proportion of the students attending private schools in these cities. A much larger program could conceivably have quite different program outcomes.

Still, slightly larger voucher programs directed at low-income families initially would attract those families with the greatest interest in exploring an educational alternative, exactly the group that applied for a voucher in these three cities. Thus, positive consequences of school choice reported herein may prove encouraging to those who seek to extend and expand school choices for low-income, inner-city families, while negative findings may indicate problems that need to be addressed. It is hoped that additional careful research will accompany larger programs established by private philanthropists and/or public authorities.



**TABLE 1: The Impact in Three Cities of Being Offered a Voucher on Test Score Performances**

<b>Test Score Performance</b>	<b>Year 1 (Percentiles)</b>	<b>Year 2 (Percentiles)</b>
<b>African Americans</b>		
Overall	2.0	3.5**
Math	3.2**	3.4*
Reading	0.8	3.6**
<b>All Other Ethnic Groups</b>		
Overall	0.1	-0.6
Math	-0.2	-0.8
Reading	0.3	-0.4

Figures represent the average impact of being offered a voucher on test score performance scores in New York, D.C., and Dayton. Averages are based upon effects observed in the three cities weighted by the inverse of the standard errors of the point estimates. For African Americans, the unweighted average effects after one year are 2.0 overall, 2.9 in math, and 1.1 in reading; after two years, the unweighted average effect sizes are 3.5 overall, 3.3 in math, and 3.6 in reading.

**TABLE 2A: Impact in New York of Being Offered a Voucher  
on Test Score Performance**

Test Score Performance	Year 1	(N)	Year 2	(N)
	(Percentiles)		(Percentiles)	
<b>African Americans</b>				
Overall	4.5**	642	3.3**	497
Math	5.4***	642	3.1*	497
Reading	3.5**	642	3.4**	497
<b>All Other Ethnic Groups</b>				
Overall	-1.2	817	-1.0	699
Math	-1.5	817	-2.2	699
Reading	-0.9	817	0.1	699

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted OLS regressions performed. All models control for baseline test scores and lottery indicators. Impacts expressed in terms of national percentile rankings. When using bootstrapped standard errors, the year 2 math score is not statistically significant; When using bootstrapped standard errors, the year 2 math score is not statistically significant; the significance levels of all other estimates remain the same when significance levels are estimated using the bootstrap technique. See Robert Stine, 1990. "An Introduction to Bootstrap Methods: Examples and Ideas" in J. Fox and J. S. Long, eds., Modern Methods of Data Analysis. p. 325-373. Newbury Park, CA: Sage Publications. Bradley Efron, 1982. "The Jackknife, the Bootstrap and other Resampling Plans." Philadelphia, PA: Society for Industrial and Applied Mathematics.

**TABLE 2B: Impact in D.C. of Being Offered a Voucher on Test Score Performance**

Test Score Performance	Year 1 (Percentiles)	(N)	Year 2 (Percentiles)	(N)
<b>African Americans</b>				
Overall	-0.3	891	3.6***	700
Math	2.9**	891	4.0***	700
Reading	-3.6**	891	3.3**	700
<b>All Other Ethnic Groups</b>				
Overall	4.7	39	0.0	44
Math	5.5	39	3.2	44
Reading	4.0	39	-3.1	44
<b>African Americans, Grades 2-5</b>				
Overall	1.0	620	3.9***	490
Math	4.2***	620	4.2***	490
Reading	-2.2	620	3.6**	490
<b>African Americans, Grades 6-8</b>				
Overall	-3.0*	270	3.3*	210
Math	0.5	270	4.1*	210
Reading	-6.5***	270	2.5	210

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted OLS regressions performed. All models control for baseline test scores; in year 1, models also control for initial testing session. Impacts expressed in terms of national percentile rankings. Grade levels refer to 1998-1999 school year.

**TABLE 2C: Impact in Dayton of Being Offered a Voucher  
on Test Score Performance**

<b>Test Score Performance</b>	<b>Year 1 (Percentiles)</b>	<b>(N)</b>	<b>Year 2 (Percentiles)</b>	<b>(N)</b>
<b>African Americans</b>				
Overall	1.9	296	3.5*	273
Math	0.2	296	2.8	273
Reading	3.5	296	4.1*	273
<b>All Other Ethnic Groups</b>				
Overall	0.7	108	-0.1	96
Math	-0.5	108	0.0	96
Reading	1.8	108	-0.2	96

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted OLS regressions performed. All models control for baseline test scores. Impacts expressed in terms of national percentile rankings.

**TABLE 3: The Impact in Three Cities of Switching to a Private School on Test Score Performances**

Test Score Performance	Year 1 (Percentiles)	Year 2 (Percentiles)
<b>African Americans</b>		
Overall	3.3	6.3**
Math	5.5*	6.2*
Reading	1.3	6.3**
<b>All Other Ethnic Groups</b>		
Overall	0.2	-1.0
Math	-0.2	-1.2
Reading	0.4	-0.8

\*\* significant at the .05 level, two tailed test; \* .10 level. Figures represent the average impact of switching to a private school on test score performance scores in New York, D.C., and Dayton. Averages are based upon effects observed in the three cities weighted by the inverse of the standard errors of the point estimates. For African Americans, the unweighted average effects after one year are 2.7 overall, 4.8 in math, and 0.6 in reading; after two years, the unweighted average effect sizes are 6.6 overall, 6.5 in math, and 6.8 in reading.

**TABLE 4A: Impact in New York of Switching to a Private School  
on Test Score Performance**

Test Score Performance	Year 1 (Percentiles)	(N)	Year 2 (Percentiles)	(N)
<b>African Americans</b>				
Overall	5.8**	623	4.3**	497
Math	7.0***	623	4.1*	497
Reading	4.6**	623	4.5**	497
<b>All Other Ethnic Groups</b>				
Overall	-1.7	817	-1.5	699
Math	-2.1	817	-3.2	699
Reading	-1.3	817	0.2	699

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for baseline test scores and lottery indicators. Impacts expressed in terms of national percentile rankings. 2.8 percent of the African American control group in the year 2 models attended a private school for one of two years. When using bootstrapped standard errors, the year 2 math score is not statistically significant; the significance levels of all other estimates remain the same when significance levels are estimated using the bootstrap technique. See Robert Stine, 1990. "An Introduction to Bootstrap Methods: Examples and Ideas" in J. Fox and J. S. Long, eds., Modern Methods of Data Analysis. p. 325-373. Newbury Park, CA: Sage Publications. Bradley Efron, 1982. "The Jackknife, the Bootstrap and Other Resampling Plans." Philadelphia, PA: Society for Industrial and Applied Mathematics.

**TABLE 4B: Impact in D.C. of Switching to a Private School on Test Score Performance**

Test Score Performance	Year 1 (Percentiles)	(N)	Year 2 (Percentiles)	(N)
<b>African Americans</b>				
Overall	-0.9	891	9.0***	700
Math	7.3**	891	9.9***	700
Reading	-9.0**	891	8.1**	700
<b>All Other Ethnic Groups</b>				
Overall	7.4	39	0.1	44
Math	8.5	39	5.8	44
Reading	6.3	39	-5.6	44
<b>African Americans, Grades 2-5</b>				
Overall	2.4	620	9.3***	490
Math	9.8***	620	10.0***	490
Reading	-5.1	620	8.6**	490
<b>African Americans, Grades 6-8</b>				
Overall	-8.8*	270	10.3*	210
Math	1.5	270	12.8*	210
Reading	-19.0***	270	7.8	210

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for baseline test scores; in year 1, models also control for initial testing session. Impacts expressed in terms of national percentile rankings. Grade levels refer to 1998-1999 school year. 3.7 percent of the African American control group in the year 2 models attended a private school in the second year but not the first year.

**TABLE 4C: Impact in Dayton of Switching to a Private School on Test Score Performance**

Test Score Performance	Year 1 (Percentiles)	(N)	Year 2 (Percentiles)	(N)
<b>African Americans</b>				
Overall	3.3	296	6.5*	273
Math	0.4	296	5.3	273
Reading	6.1	296	7.6*	273
<b>All Other Ethnic Groups</b>				
Overall	1.0	108	-0.2	96
Math	-0.8	108	0.0	96
Reading	2.8	108	-0.4	96

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for baseline test scores. Impacts expressed in terms of national percentile rankings. 2.0 percent of the African American control group in the year 2 models attended a private school in the second but not the first year.



**Table 5: Size of the Effects of Switching to a Private School on African Americans' Overall Test Score Performances**

<b>Test Score Performance</b>	<b>Effect Size Year One (Standard Deviations)</b>	<b>Effect Size Year Two (Standard Deviations)</b>
Overall	0.17	0.33
Math	0.29	0.30
Reading	0.07	0.26

Figures represent the unweighted average impact of switching to a private school on test scores in New York, D.C., and Dayton expressed in standard deviations.

**Table 6. Estimated Effects after Two Years of Switching from a Public to a Private School on African Americans' Combined Test Scores, With and Without Controls for Family Background Characteristics**

	<b>Private-School Impact, Original Results</b>	<b>Private-School Impact, Controlling for Family Background</b>	<b>p-value</b>
Three-City Average Impact	6.3**	6.3**	[.012]
New York City	4.4*	4.2*	[.086]
Dayton, OH	6.5*	5.9	[.118]
Washington, D.C.	9.0***	9.1***	[.001]

\* significant at .10 level, 2-tailed test; \*\* .05 level; \*\*\* .01 level. P-values reported in brackets. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for baseline test scores, mother's education, employment status, whether or not the family receives welfare, and family size (missing case values for demographic variables estimated by imputation); NY model also includes lottery indicators. Impacts expressed in terms of national percentile rankings. Average three-city impact is based on effects observed in the three cities weighted by the inverse of the standard errors of the point estimates.

**TABLE 7**  
**PARENTAL AND STUDENT SATISFACTION WITH SCHOOL**  
**TWO YEARS AFTER BEGINNING OF VOUCHER PROGRAM**  
**(Percent "Very Satisfied")**

Parental Satisfaction with:	Switched to Private School (1)	Public School Control Group (2)	Programmatic Impact (3)	Difference in Impact between Year One and Year Two (4)
Ability to Observe religious traditions	47	5	42***	-9
Class size	34	7	27***	-6
Discipline	47	5	42***	-5
Academic quality	44	5	39***	-2
Student respect for teachers	50	11	39***	-2
Parental support	37	6	31***	-2
Teaching values	40	7	33***	-8
What taught in school	47	7	40***	-5
School safety	52	9	43***	5
Teaching	51	10	41***	-2
Teacher-parent communication	49	19	30***	-6
Clarity school goals	36	6	30***	-6
Staff teamwork	32	6	26***	-4
Sports program	23	6	17***	-1
School facility	33	5	28***	-2
Parental involvement	35	12	23***	0
Location	53	28	25***	2
Gave school an 'A'	38	9	29***	-10**

N = 1354-1436

<sup>d</sup>Those who were offered a scholarship and identified by SCSF staff as having used their scholarship to attend a private school.

<sup>e</sup>Those in the control group who would have used a scholarship had they been offered one as described in Appendix C.

<sup>f</sup>Estimated impact of participation in the program, using a two-stage least squares model, as described in Appendix C.

\* significant at .1 level, two-tailed test; \*\* significant at .05 level; \*\*\* significant at .01 level.

**Table 8.—Percentage of Non-recipient Parents 'Very Satisfied' with their Public School at Baseline and After One and Two Years**

*How satisfied are you with the following aspects of your child's current school?*

<i>% very satisfied with:</i>	<b>1997: Baseline</b>	<b>1998: Year One</b>	<b>1999: Year Two</b>
Teaching:	14	23	10
School Safety:	13	21	9
Parental Involvement:	11	19	12
Class Size:	7	12	7
School Facility:	9	14	5
Student Respect for Teachers:	18	21	11
Communication Regarding Student Progress	18	23	19
Freedom to Observe Religious Traditions	8	9	5
Location	25	34	28

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Note: Baseline satisfaction scores are for all families not offered a scholarship from which control group is drawn. Year One and Year Two satisfaction scores are scores for control group weighted so as to make equivalent to baseline applicant pool.

TABLE 9

## PARENTAL INVOLVEMENT IN CHILD'S EDUCATION

Parent Reports	Switch to Private School (1)	Public School Control Group (2)	Programmatic Impact (3)
<i>Average number of times parents did the following:<sup>a</sup></i>			
Helped child with homework	11	12	0
Helped child with reading, math	10	10	0
Talked with child about school	13	14	1
Attend school activity with child	5	5	0
Worked on school projects	6	5	1
N = 1399-1424			

See notes to Table 7.

<sup>a</sup>The index is scored 0 if a parent never did the activity, 3 for 1-5 times, 8 for 6-10 times, 13 for 11-15 times and 18 for 16 or more times.

**TABLE 10**  
**SIZE AND QUALITY OF SCHOOL FACILITIES**

(Percentages)

<b>Parental Reports:</b>	<b>Switched to Private School (1)</b>	<b>Public School Control Group (2)</b>	<b>Programmatic Impact (3)</b>
Average school size	385	525	-140***
Average class size	25	27	-2**
Percentage satisfied with school facilities	33	5	28***
<i>Percentage with the following resources</i>			
Special programs for non-English speakers	44	80	-36***
Special programs for learning disabled	58	74	-16***
Nurses' office	75	96	-21***
Child counselor	75	83	-8*
Library	88	93	-5**
Cafeteria	90	98	-8***
Special programs for advanced learners	48	55	-7
After-school program	92	90	2
Gym	91	89	2
Arts program	80	79	1
Computer lab	90	83	7**
Music program	84	75	9***
Individual tutors	57	45	12***
(N= 889-1399)			

NOTES: SEE TABLE 7.

**TABLE 11**  
**PARENT'S PERCEPTION OF SCHOOL CLIMATE**  
**(Percentages)**

	<b>Switched to Private School (1)</b>	<b>Public School Control Group (2)</b>	<b>Programmatic Impact (3)</b>
<i>Parents report as serious problem:</i>			
Fighting	33	70	-37***
Tardiness	33	62	-29***
Kids missing class	29	54	-25***
Kids destroying property	27	45	-18***
Cheating	29	42	-13***
Racial Conflict	26	41	-15***

See notes to Table 7.

**Table 12**  
**HOMEWORK**  
**(Percentages)**

	Switched to Private School (1)	Public School Control Group (2)	Programmatic Impact (3)	
<i>Parents report:</i>				
Child has more than one hour of homework	64	41	23***	4
Homework too easy	4	20	-16***	4 *
N = 1410-1431				1
See notes to table 7.				
		(		
		(		9

See notes to Table 7.



**TABLE 13**  
**SCHOOL COMMUNICATION WITH PARENTS**

(Percentages)

Parent Reports	Switched to Private School (1)	Public School Control Group (2)	Programmatic Impact (3)	
Parents regularly informed about student grades	94	84	10***	
Parents receive notes from teacher	89	76	13***	
Parents receive newsletter	85	62	23***	
Notified of disruptive behavior	91	77	14***	
Parents speak to classes about jobs	36	24	12***	
Parents participate in instruction	63	44	19***	
Parent night	92	88	4	4
Regular Parent-Teacher Conferences	95	91	4*	
Notified of disruptive behavior	91	77	14***	
N = 1137-1401				

See notes to table 7.

**Appendix A -1**  
**Baseline Characteristics of Respondents and Non-Respondents in**  
**Second-Year Follow-Up Testing Sessions**

**Washington D. C.**

	OFFERED VOUCHER (TREATMENT)		NOT OFFERED VOUCHER (CONTROL)	
	Attended Session	Didn't Attend Session	Attended Session	Didn't Attend Session
% African American	90.4	92.1	90.9	92.1
% Welfare Recipients	38.0	34.1	32.1	30.3
% Catholic	15.5	12.6	16.0	13.8
% Protestant	72.7	69.9	65.6	70.6
Test Scores (ave.)	26.5	26.4	26.9	26.7
Family Size (ave.)	3.1	3.1	3.3	3.0
Residential Mobility (ave.)	3.4	3.5	3.5	3.4
Church Attendance (ave.)	3.7	3.5	3.7	3.7
School Satisfaction (ave.)	2.5	2.7	2.7	2.6
Mother's Education (ave.)	5.4	5.0	5.3	5.2

Items used to generated year 2 weights in D.C. Averages refer to the mean score of scaled items on survey.

**Appendix A-2**  
**Baseline Characteristics of Respondents and Non-Respondents in**  
**Second-Year Follow-Up Testing Sessions**

**Dayton**

	OFFERED VOUCHER (TREATMENT)		NOT OFFERED VOUCHER (CONTROL)	
	Attended Session	Didn't Attend Session	Attended Session	Didn't Attend Session
% Catholic	7.1	13.7	13.2	17.6
% Protestant	65.5	60.1	64.6	59.1
% Employed Full Time	56.1	51.3	54.8	52.6
% Welfare Recipients	22.5	23.1	24.8	21.7
% Learning Disabled	12.3	6.2	4.0	9.4
% African American	73.0	66.7	70.4	66.7
Mother's Education (ave.)	6.0	5.9	5.7	5.8
Church Attendance (ave.)	3.4	3.3	3.4	3.6
Residential Mobility (ave.)	3.4	3.4	3.6	3.7
Parental Involvement (ave.)	4.4	4.5	4.4	4.4
Test Scores (ave.)	26.3	24.2	27.4	24.3

Items used to generated year 2 weights in Dayton. Averages refer to the mean score of scaled items on survey.

TABLE A-3

**BASELINE CHARACTERISTICS FOR TREATMENT AND CONTROL GROUPS  
(Mean Values Reported)**

**New York City**

Variable	Control group	Treatment group	Difference	t-stat	Sig.
<i>Grade of Student ('96-'97)</i>					
Kindergarten	0.17	0.16	0.01	0.62	—
First	0.18	0.20	-0.01	-0.76	—
Second	0.22	0.21	0.01	0.42	—
Third	0.23	0.23	0.00	0.25	—
Fourth	0.20	0.20	-0.01	-0.53	—
Years student attended this school	2.47	2.39	0.08	1.37	—
<i>Satisfaction with aspects of current school</i>					
Location	2.94	3.00	-0.06	-1.44	—
School Safety	2.75	2.79	-0.04	-1.01	—
Teaching	2.70	2.66	0.04	1.07	—
How much school involves parents	2.72	2.72	0.01	0.24	—
Class sizes	2.32	2.34	-0.01	-0.34	—
School Facilities	2.63	2.61	0.02	0.61	—
Student respect of teachers	2.86	2.89	-0.03	-0.77	—
Parent-teacher communication	2.81	2.81	0.01	0.14	—
Observation of religious traditions	2.31	2.24	0.07	1.71	—
Student in gifted classes	0.12	0.12	0.00	0.21	—
Student received help for disability	0.10	0.12	-0.02	-1.28	—
Mother's educational expectations for child (10=some HS, 12=HS grad, 14=some college, 16=college grad, 18=more than college)	16.67	16.76	-0.09	-1.07	—
<i>Education level of mother or female guardian</i>					
Some high school (did not graduate)	0.22	0.21	0.01	0.44	—
High school graduate or GED	0.27	0.25	0.02	0.78	—
Some college	0.40	0.41	-0.01	-0.34	—
Graduated from 4-year college	0.07	0.08	-0.01	-0.91	—
More than 4-year college degree	0.03	0.03	0.00	0.33	—
Don't know	0.01	0.02	-0.01	-1.45	—
<i>Race/ethnicity of mother/female guardian</i>					
White	0.05	0.06	-0.01	-1.08	—
Black	0.44	0.47	-0.02	-0.91	—
Puerto Rican	0.21	0.18	0.03	1.48	—
Hispanic other than Puerto Rican	0.27	0.25	0.02	0.83	—
Other	0.03	0.05	-0.01	-1.28	—

TABLE A-3 (continued)

Variable	Control group	Treatment group	Difference	t-stat	Sig.
<i>Birth place of mother/female guardian</i>					
Born in United States	0.61	0.59	0.02	0.84	—
Born in Puerto Rico	0.09	0.08	0.01	0.80	—
Born outside U.S. and Puerto Rico	0.30	0.33	-0.03	-1.37	—
Length of residence of mother in months	36.09	35.89	0.20	0.36	—
<i>Job status of mother/female guardian</i>					
Full-time job	0.22	0.22	0.00	0.00	—
Part-time job	0.15	0.16	-0.02	-0.96	—
Not working now but looking for work	0.46	0.47	-0.01	-0.32	—
Not working and not looking for work	0.15	0.14	0.02	0.91	—
Don't know	0.02	0.01	0.01	1.46	—
<i>Religious affiliation of female guardian</i>					
Catholic	0.52	0.51	0.01	0.56	—
Religion other than Catholic	0.42	0.44	-0.02	-0.91	—
None	0.06	0.05	0.01	0.76	—
Number of children in home	2.38	2.34	0.05	0.86	—
<i>In child's home (percent saying yes):</i>					
A daily newspaper	0.84	0.85	-0.01	-0.57	—
An encyclopedia	0.71	0.71	-0.01	-0.24	—
A dictionary	0.98	0.97	0.01	0.83	—
More than 50 books	0.85	0.85	0.00	-0.20	—
<i>Member of household receiving assistance:</i>					
Food stamps	0.67	0.66	0.01	0.62	—
Welfare	0.58	0.57	0.01	0.36	—
Social Security	0.11	0.11	0.00	-0.01	—
Medicaid	0.66	0.63	0.04	1.63	—
Supplemental Security Income	0.14	0.13	0.00	0.06	—
Family income	9450.23	9466.92	-16.69	-0.05	—
<i>Reading Achievement Scores</i>					
Overall	24.52	22.88	1.64	1.58	—
1 <sup>st</sup> grade cohort	27.29	22.02	5.27	2.39	**
2 <sup>nd</sup> grade cohort	25.05	25.10	-.05	-.02	—
3 <sup>rd</sup> grade cohort	21.03	20.35	.67	.40	—
4 <sup>th</sup> grade cohort	24.80	24.40	.40	.18	—
<i>Math Achievement Scores</i>					
Overall	17.12	17.06	0.05	0.06	—
1 <sup>st</sup> grade cohort	11.88	10.83	1.05	.74	—
2 <sup>nd</sup> grade cohort	19.14	19.51	.37	.23	—
3 <sup>rd</sup> grade cohort	17.16	16.66	.51	.26	—
4 <sup>th</sup> grade cohort	21.32	22.79	1.47	.57	—
English spoken at home	0.75	0.78	-0.03	-1.45	—

Source: fay30300.xls,fay30713.do

Table A-4

DIFFERENCES IN BASELINE CHARACTERISTICS FOR RESPONDENTS AND  
NON-RESPONDENTS IN SECOND FOLLOW-UP: TREATMENT GROUP  
(Mean Values Reported)

**New York City**

Variable	Respondents	Non- Respondent	Difference	t-stat	Sig.
<i>Grade of Student ('96-'97)</i>					
Kindergarten	0.16	0.12	0.05	1.86	—
First	0.18	0.21	-0.03	-1.10	—
Second	0.21	0.21	0.00	-0.07	—
Third	0.23	0.26	-0.02	-0.71	—
Fourth	0.21	0.21	0.01	0.20	—
Years student attended this school	2.46	2.37	0.09	0.97	—
<i>Satisfaction with aspects of current school</i>					
Location	3.03	2.93	0.10	1.57	—
School Safety	2.81	2.62	0.19	2.90	***
Teaching	2.66	2.58	0.08	1.34	—
How much school involves parents	2.72	2.57	0.15	2.54	**
Class sizes	2.35	2.26	0.08	1.27	—
School Facilities	2.61	2.52	0.09	1.56	—
Student respect of teachers	2.91	2.71	0.19	2.98	***
Parent-teacher communication	2.84	2.7	0.14	2.21	**
Observation of religious traditions	2.25	2.25	0.00	0.00	—
Student in gifted classes	0.11	0.13	-0.02	-1.00	—
Student received help for disability	0.11	0.15	-0.04	-1.58	—
Mother's educational expectations for child (10=some HS, 12=HS grad, 14=some college, 16=college grad, 18=more than college)	16.77	16.55	0.22	1.63	—
<i>Education level of mother or female guardian</i>					
Some high school (did not graduate)	0.24	0.21	0.03	1.05	—
High school graduate or GED	0.24	0.27	-0.04	-1.05	—
Some college	0.40	0.40	0.00	0.01	—
Graduated from 4-year college	0.07	0.07	0.00	0.22	—
More than 4-year college degree	0.03	0.03	-0.01	-0.57	—
Don't know	0.02	0.01	0.01	0.60	—
<i>Race/ethnicity of mother/female guardian</i>					
White	0.03	0.08	-0.05	-2.65	***
Black	0.45	0.53	-0.09	-2.30	**
Puerto Rican	0.18	0.18	0.00	-0.01	—
Hispanic other than Puerto Rican	0.29	0.18	0.11	3.76	***
Other	0.05	0.03	0.02	1.78	*

TABLE A-4 (continued)

Variable	Respondents	Non-Respondents	Difference	t-stat	Sig.
<i>Birth place of mother/female guardian</i>					
Born in United States	0.57	0.67	-0.10	-2.65	***
Born in Puerto Rico	0.08	0.08	0.00	-0.02	—
Born outside U.S. and Puerto Rico	0.35	0.25	0.10	2.80	***
Length of residence of mother in months	36.73	34.27	2.45	2.53	**
<i>Job status of mother/female guardian</i>					
Full-time job	0.22	0.19	0.03	1.02	—
Part-time job	0.16	0.15	0.00	0.16	—
Not working now but looking for work	0.44	0.52	-0.08	-2.13	**
Not working and not looking for work	0.16	0.12	0.04	1.71	*
Don't know	0.01	0.01	0.00	0.19	—
<i>Religious affiliation of female guardian</i>					
Catholic	0.53	0.43	0.10	2.63	***
Religion other than Catholic	0.43	0.49	-0.06	-1.57	—
None	0.04	0.08	-0.04	-2.08	**
Number of children in home	2.43	2.42	0.01	0.08	—
<i>In child's home (percent saying yes):</i>					
A daily newspaper	1.16	1.15	0.01	0.21	—
An encyclopedia	1.30	1.29	0.01	0.25	—
A dictionary	1.03	1.03	0.00	-0.12	—
More than 50 books	1.15	1.17	-0.01	-0.48	—
<i>Member of household receiving assistance:</i>					
Food stamps	0.65	0.72	-0.07	-2.14	**
Welfare	0.55	0.64	-0.09	-2.52	**
Social Security	0.12	0.11	0.01	0.37	—
Medicaid	0.63	0.68	-0.05	-1.47	—
Supplemental Security Income	0.15	0.15	0.00	-0.01	—
Family income	9875	8451.54	1423.46	2.91	***
<i>Reading Achievement Scores</i>					
Overall	22.34	21.92	0.42	0.23	—
1 <sup>st</sup> grade cohort	22.80	22.80	0.00	0.00	—
2 <sup>nd</sup> grade cohort	23.44	23.27	0.17	0.04	—
3 <sup>rd</sup> grade cohort	19.59	19.07	0.52	0.18	—
4 <sup>th</sup> grade cohort	24.04	23.26	0.78	0.21	—
<i>Math Achievement Scores</i>					
Overall	17.29	16.03	1.26	0.72	—
1 <sup>st</sup> grade cohort	9.68	10.17	-0.50	-0.18	—
2 <sup>nd</sup> grade cohort	20.54	17.25	3.29	1.02	—
3 <sup>rd</sup> grade cohort	17.41	13.77	3.63	1.30	—
4 <sup>th</sup> grade cohort	19.81	23.50	-3.69	-0.78	—
English spoken at home	0.75	0.84	-0.10	-3.26	***

Source: fay30903.xls,fay30903.do

TABLE A-5

**DIFFERENCES IN BASELINE CHARACTERISTICS FOR RESPONDENTS AND  
NON-RESPONDENTS IN SECOND FOLLOW-UP: CONTROL GROUP  
(Mean Values Reported)**

## New York City

Variable	Respondents	Non- Respondents	Difference	t-stat	Sig.
<i>Grade of Student ('96-'97)</i>					
Kindergarten	0.16	0.16	0.01	0.27	—
First	0.19	0.13	0.06	2.20	**
Second	0.21	0.23	-0.02	-0.74	—
Third	0.23	0.25	-0.02	-0.48	—
Fourth	0.19	0.22	-0.03	-0.87	—
Years student attended this school	2.47	2.55	-0.08	-0.88	—
<i>Satisfaction with aspects of current school</i>					
Location	2.92	2.86	0.06	0.85	—
School Safety	2.74	2.62	0.12	1.85	*
Teaching	2.70	2.59	0.11	1.76	*
How much school involves parents	2.75	2.60	0.15	2.56	***
Class sizes	2.35	2.22	0.13	1.93	*
School Facilities	2.62	2.55	0.07	1.07	—
Student respect of teachers	2.86	2.74	0.12	1.90	*
Parent-teacher communication	2.82	2.71	0.11	1.72	*
Observation of religious traditions	2.32	2.21	0.11	1.51	—
Student in gifted classes	0.11	0.12	-0.01	-0.56	—
Student received help for disability	0.12	0.08	0.04	1.80	*
Mother's educational expectations for child (10=some HS, 12=HS grad, 14=some college, 16=college grad, 18=more than college)	16.66	16.73	-0.07	-0.55	—
<i>Education level of mother or female guardian</i>					
Some high school (did not graduate)	0.23	0.23	0.00	-0.08	—
High school graduate or GED	0.28	0.26	0.02	0.68	—
Some college	0.39	0.39	0.01	0.17	—
Graduated from 4-year college	0.06	0.09	-0.03	-1.53	—
More than 4-year college degree	0.03	0.02	0.01	0.85	—
Don't know	0.01	0.02	-0.01	-0.58	—
<i>Race/ethnicity of mother/female guardian</i>					
White	0.03	0.03	0.00	0.38	—
Black	0.42	0.49	-0.08	-2.07	**
Puerto Rican	0.22	0.21	0.01	0.28	—
Hispanic other than Puerto Rican	0.31	0.21	0.09	3.01	***
Other	0.02	0.05	-0.03	-2.07	**



TABLE A-3 (continued)

Variable	Respondents	Non-Respondents	Difference	t-stat	Sig.
<i>Birth place of mother/female guardian</i>					
Born in United States	0.60	0.66	-0.06	-1.57	—
Born in Puerto Rico	0.10	0.07	0.03	1.49	—
Born outside U.S. and Puerto Rico	0.30	0.27	0.03	0.74	—
Length of residence of mother in months	36.67	36.03	0.64	0.74	—
<i>Job status of mother/female guardian</i>					
Full-time job	0.21	0.22	-0.02	-0.52	—
Part-time job	0.14	0.16	-0.02	-0.58	—
Not working now but looking for work	0.46	0.45	0.02	0.44	—
Not working and not looking for work	0.17	0.14	0.03	1.04	—
Don't know	0.02	0.03	-0.01	-0.97	—
<i>Religious affiliation of female guardian</i>					
Catholic	0.57	0.48	0.09	2.56	**
Religion other than Catholic	0.38	0.47	-0.09	-2.50	**
None	0.05	0.05	0.00	-0.16	—
Number of children in home	2.41	2.51	-0.11	-1.06	—
<i>In child's home (percent saying yes):</i>					
A daily newspaper	1.16	1.15	0.01	0.37	—
An encyclopedia	1.30	1.29	0.01	0.22	—
A dictionary	1.02	1.02	0.00	-0.44	—
More than 50 books	1.15	1.15	0.00	0.10	—
<i>Member of household receiving assistance:</i>					
Food stamps	0.67	0.71	-0.04	-1.13	—
Welfare	0.58	0.63	-0.05	-1.30	—
Social Security	0.12	0.09	0.03	1.28	—
Medicaid	0.67	0.67	0.01	0.19	—
Supplemental Security Income	0.16	0.09	0.07	2.63	***
Family income	9303.63	9330.68	-27.05	-0.05	—
<i>Reading Achievement Scores</i>					
Overall	23.83	23.83	0.00	0.00	—
1 <sup>st</sup> grade cohort	26.25	33.30	-7.05	-1.69	*
2 <sup>nd</sup> grade cohort	25.67	20.62	5.05	1.34	—
3 <sup>rd</sup> grade cohort	20.99	19.50	1.49	0.58	—
4 <sup>th</sup> grade cohort	22.98	25.56	-2.58	-0.74	—
<i>Math Achievement Scores</i>					
Overall	17.37	17.02	0.35	0.22	—
1 <sup>st</sup> grade cohort	10.47	17.51	-7.04	-2.43	**
2 <sup>nd</sup> grade cohort	19.95	16.22	3.72	1.28	—
3 <sup>rd</sup> grade cohort	17.87	13.35	4.52	1.57	—
4 <sup>th</sup> grade cohort	20.76	21.53	-0.76	-0.19	—
English spoken at home	0.73	0.81	-0.07	-2.42	**

Source: fay30903.xls,fay30903.do

## APPENDIX B

### Adjusting Sample Weights for Non-Response

Families within the sample had different probabilities of being offered a scholarship. To reflect these differences in the probability of selection and to reflect the composition of the population of eligible applicants, we weight the sample data. The weights were constructed by taking the inverse of the probability of being selected for a scholarship. Weights for scholarship families were multiplied by .217 and weights for control group families were multiplied by .783 to reflect the ratio of treatment to control group families in the initial pool of eligible applicants.

<sup>56</sup> All weights were divided by 2 to sum to the size of the population we are trying to represent, not twice the population. In this sample, the average weight was about 4.2. A family with a weight of 4.2 stands in for 3.2 other families in the pool of applicants as well as itself. The weights, which were adjusted for the same family applying multiple times, range in size from about .5 to 22.

About 18 percent of all families in the first year and 26 percent of families in the second year did not complete a survey. To adjust for this non-response, we computed the probability of responding based on a logit model. The independent variables in the logit model included family characteristics such as race/ethnicity, number of siblings, language spoken at home, mother's education, and family income, and other variables used to stratify the sample when we collected

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<sup>56</sup> The control group was reduced to 1,293 students from the initial eligible population of 5,658 and the treatment group was reduced from 1,558 to 1,374 students. The weights for the reduced sample were re-scaled to sum to the initial eligible population.

the baseline data. After computing the predicted probability of responding, we adjusted the baseline weight as follows:

$$W_i = 1/[f_i * p_i * pr_i]$$

where  $f_i$  includes the adjustment factors used for deriving the baseline weight<sup>57</sup>,  $p_i$  is the probability of being selected for a scholarship (control group),  $pr_i$  is the probability of responding for each follow-up survey, and  $W_i$  is the new weight variable. Families that did not respond to the follow-up survey were assigned a weight of zero.

For the second year student data, we found that 66 percent of the students responded to the survey and that we had test scores for 66 percent. During the previous round of data collection, 75 percent of students completed the survey and 78 percent took the achievement test. To adjust the weights for the student level data, we followed the same procedures that were used for the parent data.

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<sup>57</sup> The adjustment factors are as follows: 1) five discrete points at which families applied for scholarships; 2) whether a child attended a public school with below average achievement; 3) the number of eligible children within the family.

## APPENDIX C

### Analytic Approach

For some analysts and program operators, the important policy question is as follows: what happens when a voucher or scholarship program is put into effect? But other analysts also want an answer to a second question: what is the impact of attending a private school? Angrist, Imbens, and Rubin (1996) make some interesting distinctions between the two estimators used to address these two different questions. We refer to the first estimator as the intended to treat (ITT) estimator and the second as the complier average causal effect estimator (CACE). The most important issues concerning the ITT estimator, which compares all children randomly assigned to the scholarship group with all children randomly assigned to the control group, are that among the children who are assigned to the two groups are children who are induced by the offer of a scholarship to attend a private school, children who would have made the decision to attend a private school regardless of the scholarship offer, and children who would never attend a private school. The CACE estimator provides an estimate of the impact of the scholarship for only those who were or would have been induced by the offer of a scholarship to attend private school. Children who would have attended a private school regardless of the offer of a scholarship and those who would have opted to not attend irrespective of the scholarship do not play a direct role in the estimated impact with the CACE estimator.

### Computing Impacts of Being Offered a Scholarship (ITT)

To compute the impact of being offered a scholarship we use a simple statistical model that includes as independent variables an indicator for treatment status (offered a scholarship or in the control group) and a set of indicators that show the stratum from which a family was selected. The strata are based on (1) five discrete points at which families applied for scholarships, (2) whether a child attended a public school with below average achievement, and (3) the number of eligible children within the family. When computing the impact on student achievement test scores, we also included student baseline reading and math achievement. The basic form of the model is:

$$\begin{aligned}y_{1i} &= \mathbf{b}_0 + \mathbf{b}_1 T_i + \mathbf{b}_2 X_i + \mathbf{e}_{1i} \\y_{2i} &= \mathbf{b}_3 + \mathbf{b}_4 T_i + \mathbf{b}_5 X_i + \mathbf{e}_{2i}\end{aligned}$$

where  $T_i$  equals 1 if we offered a family a scholarship and 0 otherwise (families were randomly selected for the scholarship and control groups);  $X_i$  is a vector that includes indicator variables for each of the strata used in the random selection of scholarship families and baseline test scores when computing impacts on achievement. The outcomes of interest are  $y_{1i}$  and  $y_{2i}$ . The former is the first year test score and the latter is the second year test score. The random error terms,  $\epsilon_{1i}$  and  $\epsilon_{2i}$ , capture the effects of unobserved factors that influence the outcomes; and the  $\beta$  s are parameters or vectors of parameters to be estimated. The parameters of most interest are  $\beta_1$  and  $\mathbf{b}_4$  because they show the impact of being offered a scholarship on the outcome for year 1 and

year 2, respectively. We estimate the model parameters by using ordinary least squares for both categorical and continuous outcomes.

### Computing the Complier Average Causal Effect (CACE)

A simple comparison of an outcome for families in the scholarship group (those whom we offered a scholarship) and the control group shows the impact of *being offered a scholarship*, regardless of whether a family sent their child to a private/parochial school or not.

To compute the CACE estimator for the impact of attending a private school, we need to estimate a statistical model that focuses on (1) the relationship between being offered a scholarship and attending private school and (2) the relationship between attending private school and family and student outcomes. These relationships can be expressed as:

$$\begin{aligned}
 P_{1i} &= \mathbf{a}_0 + \mathbf{a}_1 T_i + \mathbf{a}_2 X_i + \mathbf{e}_{p1i} \\
 y_{1i} &= \mathbf{b}_0 + \mathbf{b}_1 P_{1i} + \mathbf{b}_2 X_i + \mathbf{e}_{y1i} \\
 P_{2i} &= \mathbf{a}_3 + \mathbf{a}_4 T_i + \mathbf{a}_5 X_i + \mathbf{e}_{p2i} \\
 y_{2i} &= \mathbf{b}_3 + \mathbf{b}_4 P_{2i} + \mathbf{b}_5 X_i + \mathbf{e}_{y2i}
 \end{aligned}$$

where  $T_i$  equals 1 if we offered a family a scholarship and 0 otherwise (families were randomly selected for the scholarship and control groups);  $X_i$  is a vector that includes indicator variables for each of the stratum used in the random selection of scholarship families and baseline test scores when computing impacts on achievement;  $P_{1i}$  if attended a private school in year one and  $P_{2i}$  equal 1 if a family attended a private school in year one and year two, and 0 otherwise;  $y_{1i}$  and  $y_{2i}$  are the outcomes of interest;  $\epsilon_{p1i}$ ,  $\epsilon_{y1i}$ ,  $\epsilon_{p2i}$  and  $\epsilon_{y2i}$  are random error terms that capture the effects of unobserved factors that influence both private school attendance and the outcome; and  $\alpha$  s and  $\beta$  s are parameters or vectors of parameters to be estimated.<sup>58</sup> We allow for across equation error correlation only within time periods. The parameters of most interest are  $\mathbf{b}_1$  and  $\mathbf{b}_4$  because they show the impact of attending a private school on the outcome.<sup>59</sup>

We estimate the model parameters by using the instrumental variables estimator. This technique allows us to compute asymptotically unbiased and efficient estimates of the parameters; which can be interpreted as the causal impact for compliers (students that were induced to attend a private school by the scholarship offer) using the framework developed by Angrist, Imbens, and

<sup>58</sup> For analyses of the parent and student survey data, we focused in attendance at a private school in year two only. In this case,  $P_{i2} = 1$  if attended a private school in year 2, and 0 otherwise.

<sup>59</sup> As already described in the report, we used two definitions of private school attendance when analyzing the test scores: (1) ever attend a private school, and (2) attended a private school for two years. We discuss the results of attending for two years in the report (Appendix E shows results for ever attending, which are quite close to the estimates of attending for two years). The impacts for ever attending are implemented by making a small adjustment to the analytic models described in this appendix.

Rubin, 1996. To implement the instrumental variables estimator, we use the two-stage least squares procedure.

In our tables we present (1) the impact of attending private school, (2) the average of each outcome for families or students in the scholarship group that attended private school (complied), and (3) the average of outcomes for families or students in the control group who would have attended a private school if offered a scholarship. The first quantity is obtained from the statistical model described previously. The average for compliers in the scholarship group is computed by adding the impact of attending private school to the average for members of the control group who would have complied. To compute the last quantity, we can use an alternative expression for computing the impacts of private school attendance (compliance):

$$E(y^T_t | P_t = 1) - E(y^C_t | P_t = 1) = [E(y^T_t) - E(y^C_t)] / Pr(P_t = 1)$$

$$E(y^C_t | P_t = 1) = E(y^T_t | P_t = 1) - [E(y^T_t) - E(y^C_t)] / Pr(P_t = 1)$$

where  $t$  is time and  $[E(y^T_1) - E(y^C_1)] / Pr(P_1 = 1) = \mathbf{b}_1$  for  $t = 1$  and

$[E(y^T_2) - E(y^C_2)] / Pr(P_2 = 1) = \mathbf{b}_4$  for  $t = 2$ .<sup>60</sup> The last expression tells us that the average of each outcome for controls, which is unobserved, can be computed from known quantities.

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<sup>60</sup> This expression for program impacts draws on Bloom's earlier work (1984).