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9. Anthony Bryk, Valerie E. Lee, and Peter B. Holland, *Catholic Schools and the Common Good* (Harvard University Press, 1993).

10. Jonathan Kozol, *Savage Inequalities: Children in America's Schools* (New York: Crown, 1991).

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12. Carnegie Foundation for the Advancement of Teaching, *School Choice: A Special Report* (Princeton, N.J.: The Carnegie Foundation for the Advancement of Teaching, 1992); Henry M. Levin, "The Economics of Educational Choice," *Economics of Education Review* 10 (1988), pp. 137–58.

13. Carnegie Foundation for the Advancement of Teaching, *School Choice: A Special Report*.

14. Carnegie Foundation for the Advancement of Teaching, *School Choice: A Special Report,* p. 1.

15. Actually, the analysis includes results from two different control groups, both defined in the same way but at different points in time. The first control group completed surveys in the fall of 1994, describing their characteristics and reasons for joining the program; these results are discussed later in this chapter. We were unable, however, to obtain test scores for this group of students; therefore, a second control group was established in the spring of 1995. This second control group was used in the comparisons of standardized test scores presented in the section on student achievement.

16. Our measure of income is static. It might be that persons reporting financial considerations as the reasons for withdrawing experienced a reduction in their incomes. This change could have been the operational factor leading to a decision to leave regardless of the average level of income.

17. Percentile rankings and grade equivalents suffer from a mathematical problem: They do not represent an "equal interval" scale. Simply put, this means that a 5 percent gain in the middle of the distribution represents a different interval of growth from those in outer portions of the distribution. Therefore, it is not possible to "add up" gains from students at different points in the distribution to obtain an overall average.

THIRTEEN

School Choice in Milwaukee: A Randomized Experiment

Jay P. Greene, Paul E. Peterson, and Jiangtao Du

SCHOOL CHOICE OR VOUCHER plans in which parents could use public funds to select the public or private schools that their children would attend have been receiving serious consideration as a means of improving the quality and efficiency of educational services. Although there are theoretical reasons to believe that choice and competition in schooling might be beneficial, evidence from a randomized experiment has not been available to substantiate or refute those theories. The evidence presented in this chapter from the school choice program in Milwaukee provides an opportunity to learn more about the effects of voucher programs from experimental data.

The Milwaukee experiment is unique in that it is the first publicly funded voucher program in the country and the only one with several years of results. The Milwaukee experiment is also unique in that vouchers were assigned by lottery when classes were oversubscribed. Analysis of data from a randomized experiment avoids the selection bias of comparing choosers to nonchoosers that has plagued other studies of choice in education. The Milwaukee experiment is of further interest in that it offers a hard test of choice theories because of the numerous constraints under which the program has operated.

Scholars have suggested that privatization may enhance efficiency in education in three different ways. First, competition among providers may reduce the cost and improve the quality of services.[1] Second, government-financed services may more closely match consumer preferences if the latter are given opportunities to sort themselves among an array of options.[2] Third, private producers may more easily enlist the participation of consumers in the co-production of the services, thereby enhancing service quality and effectiveness.[3]

If school choice could significantly improve the quality of education, the political and social benefits would be more than trivial. Apart from cash-transfer services, education is the largest part of the gross national product (GNP) of any publicly provided service.[4] In 1990 the cost of publicly financed education services constituted $305.6 billion, or 5.6 percent of the GNP.[5] Yet public confidence in public schools remains very low. In 1993 only 19 percent of the population was willing to give schools a grade of A or B, a fall of 8 percentage points since a decade earlier.[6]

Weak confidence in our public schools may be due to their failure to keep pace with rising public expectations. Estimated real costs within the educational sector, adjusted for inflation, rose by 29 percent or at an annual rate of 1.5 percent between 1974 and 1991.[7] Meanwhile, students' performance as measured by test scores, an important educational outcome, remained fairly constant.[8] Between 1970 and 1992 elementary and secondary students averaged no more than a gain of .1 of a standard deviation in mathematics and reading on the National Assessment of Educational Progress, generally thought to be the best available measure of student achievement. Meanwhile, their scores in science fell by .24 of standard deviation.[9] Increasing costs with at best slight gains in student achievement suggest that the public school system has become less efficient.

Opportunities for efficiency gains are particularly large in central cities. Whereas competition among small school districts exists in suburban parts of many metropolitan areas,[10] most city schools are governed by a single school board that does not ordinarily allow schools to compete for students.[11] Schools in rural areas often function as community institutions, facilitating co-production of educational services, but city schools have more limited ties to their immediate neighborhoods. Perhaps for these reasons, educational outcomes in the city lag those outside the central city.[12]

It has been argued that any efficiency gains are unlikely to result in higher levels of student achievement, because cognitive skills are either inherited or set in place at an early age, making them hardly susceptible to manipulation by educational processes.[13] But the weight of the evidence is

in the opposite direction; numerous studies have found that school characteristics affect student achievement.[14] If these findings are correct, it may be hypothesized that if government grants are made available to families so they can purchase educational services for their children, efficiency gains accompanying privatization will result in enhanced student achievement.[15] Under such arrangements, competition among producers increases. Inasmuch as consumers' educational preferences vary and entry into the educational market is not prohibitively large, many producers will attempt to meet a demand for a range and variety of services. Co-production by consumers and providers (families and the schools) is more likely if families have a choice of schools.[16]

Yet efficiency gains that facilitate academic achievement may not be as great as these considerations suggest. Consumers may not have the information necessary to discern schools' academic quality.[17] Or consumers may choose schools on the basis of the schools' nonacademic characteristics, such as proximity, religiosity, sports facilities, or social segregation.[18]

Potential gains in student achievement as a result of privatization are much disputed, in part because empirical research has left the issue unresolved. Although two different research traditions have sought to estimate the comparative efficiency of private and public schools, neither has provided a definitive answer. The first research tradition has relied on data from national samples (High School and Beyond, the National Longitudinal Study of Youth, and the National Education Longitudinal Study) to estimate the achievement effects of attending public and private schools. Most of these studies have found that students who attend private schools score higher on achievement tests or are more likely to attend college than those who attend public schools.[19]

Because private schools are generally less expensive than public schools, these studies suggest greater efficiency in the private sector. But these findings may be contaminated by selection bias: Students in private schools, who come from tuition-paying families, may have unobserved characteristics that increase the likelihood of their scoring higher on achievement tests, regardless of the schools they attend.[20]

The second research tradition consists of studies that evaluate the test performance of students from low-income or at-risk backgrounds who have received scholarships that give them the opportunity to move from public to private schools.[21] Although these evaluations also have reported that private schools produce higher levels of student achievement with less expenditure per pupil, their findings may also be contaminated by unobserved background characteristics of scholarship recipients. In almost all the programs studied, scholarships have been distributed on a first-come, first-served basis. They also require additional tuition payments by families, increasing the likelihood that scholarship recipients have unobserved characteristics (such as motivation) correlated with higher test scores.

A previous evaluation of the Milwaukee choice program reports no systematic achievement effects of enrollment in private schools.[22] But this evaluation compared students from low-income families with public school students from more advantaged backgrounds, leaving open the possibility that unobserved background characteristics could account for the lack of positive findings.[23] In sum, with the exception of the Milwaukee evaluation, most studies have found efficiency gains from the privatization of educational services. Yet all studies have suffered from potential selection bias, because they have relied on nonexperimental data that have included unobserved but possibly relevant background characteristics that could account for reported findings.

One way to improve on previous research is to conduct an experiment that avoids selection bias by randomly assigning students to treatment and control groups. With random assignment the members of the two groups can be assumed to be similar, on average, in all respects other than the treatment they receive. Differences in average outcomes can be reasonably attributed to the experimental condition. Only a few studies of school effectiveness have been able to draw upon data from randomized experiments, probably because it is difficult to justify random denial of access to apparently desirable educational conditions.[24] The results from the Milwaukee choice program reported here are, to the best of our knowledge, the first to estimate from a randomized experiment the comparative achievement effects of public and private schools.

Some results from the randomized experiment in Milwaukee were reported by Witte and associates in 1994,[25] but that study concentrated on a comparison of students in choice schools with a cross-section of students attending public schools. Data from the randomized experiment were underanalyzed and discussed only in passing.[26] In addition to our initial report,[27] two other studies have reported results from the randomized experiment in Milwaukee,[28] but all three studies relied on inaccurate test score data.

Subsequent to issuing our report in 1996, we discovered that the Milwaukee test score data available on the world wide web did not adjust for the fact that some students were not promoted from one grade to the next. For example, students in both test and control groups who were held back for a year at the end of third grade were scored as third graders when they

otherwise would have been scored as fourth graders. When this happens, a student can receive a much higher percentile score than is appropriate. Other students are allowed to skip a grade, and if this promotion is not taken into account, it produces an error of the opposite kind. We were able to eliminate both types of error by adjusting test scores to the correct grade level by means of conversion tables.[29]

A Hard Case

The Milwaukee choice program, initiated in 1990, provided vouchers to a limited number of students from low-income families to be used to pay tuition at their choice of secular private schools in Milwaukee. The program was a hard case for testing the hypothesis that efficiency gains can be achieved through privatization, because it allowed only a very limited amount of competition among producers and choice among consumers.[30]

The number of producers was restricted by the requirement that no more than half of a school's enrollment could receive vouchers. Because this rule discouraged the formation of new schools, no new elementary school came into being in response to the establishment of the voucher program. Consumer choice was further limited by excluding the participation of religious schools (thereby precluding use of approximately 90 percent of the private school capacity within the city of Milwaukee). Co-production was also discouraged by prohibiting families from supplementing the vouchers with tuition payments of their own. (But schools did ask families to pay school fees and make voluntary contributions.) Other restrictions also limited program size. Only 1 percent of the Milwaukee public schools could participate, and students could not receive a voucher unless they had been attending public schools or were not of school age at the time of application.

These restrictions significantly limited the amount of school choice that was made available. Most choice students attended fiscally constrained institutions with limited facilities and poorly paid teachers.[31] One school, Juanita Virgil Academy, closed a few months after the program began.[32] Although the school had existed as a private school for a number of years, it was eager to admit sixty-three choice students in order to alleviate its enrollment and financial difficulties. Even with the addition of the choice students, the school's problems persisted. To comply with the requirement that schools offer secular curricula, the school had to drop its Bible classes. Parents complained about the food service, overcrowded classrooms, a shortage of books and materials, and a lack of cleanliness and discipline. The executive director had hired a new principal away from the public schools, but she had to be relieved of her responsibilities two months into the school year. The school withdrew from the choice program the next semester, giving as its reason the desire to "reinstate religious training in the school." A few weeks later the school closed altogether.[33]

Given the design of the Milwaukee choice program, more school failures might have been expected. The three schools that together with Juanita Virgil Academy admitted 84 percent of the choice students in 1990 had modest facilities and low teacher salaries. Bruce Guadalupe Community School was in particular difficulty. Established in 1969, it sought to preserve Latino culture and teach children respect for both English and Spanish. Many teachers had once taught in Central American schools. Instruction was bilingual, often more in Spanish than English. Despite its distinctive educational mission, the school had difficulty making ends meet. Even finding an adequate school building seemed a never-ending problem; the school moved from one location to another on several occasions during its first two decades. By January 1990 things had become so desperate that the school was on "the verge of closing." But enactment of the choice program gave the school "new hope for the future," a hope that "otherwise had been snuffed out."[34] A tuition voucher of more than $2,500 per student was a boon to a school that had had trouble collecting $650 from each participating family.

Despite the arrival of choice students in the fall of 1990, the school, still in financial distress, was forced to cut its teaching staff by a third. The school's difficulties were fully reported in the *Milwaukee Journal:* "Two staff aides were fired, the seventh and eighth grades were combined, the second grade was eliminated with children put into the first or third grade, and the bilingual Spanish program was cut. . . . Two teachers were transferred. . . . The former eighth grade teacher [was] teaching fourth grade. . . . Overall, the teaching staff was reduced from 14 to 9."[35] The school's principal described staff morale as "low."

The two other community schools with large choice enrollments, Harambee Community School and Urban Day School, had better reputations, but still suffered from serious financial difficulties.[36] Like Bruce Guadalupe, they catered almost exclusively to a low-income minority population. Established in the 1960s in former Catholic parish schools, they tried to survive as secular institutions after the archdiocese closed the parochial schools. Named for the Swahili word meaning "pulling together," Harambee presented itself as "an African American–owned school emphasizing the basics through creative instructional programs, coupled with a strong cultural foundation."[37] Urban Day was said to place "a heavy emphasis on African history and culture."[38]

Like Bruce Guadalupe, these schools could ask families to pay only a very modest tuition. Though they set their annual rates at somewhere between $650 and $800, only a few families whose children were attending the schools actually paid full tuition. Tuition scholarships were the norm, not an exceptional privilege. But parents were expected to participate in fund-raising activities. Teacher salaries were much lower than those paid by the Milwaukee public schools. As one principal observed, "The teachers who stay here for a long time are either very dedicated or can afford to stay on what we pay."[39]

The quality of the physical plant provided a visible sign of the school's modest financial resources: "Recess and physical education facilities were relatively poor in the schools. One school had easy access to a city park for recess, one relied on a blocked off street, two others asphalt playgrounds with some wood chips and playground equipment. All the schools had some indoor space for physical education, but it often served multiple purposes."[40] One of its hardest-working supporters was asked what she would most wish for the school. She said, "I'd like to see the school financially self-sufficient."[41]

To repeat, the Milwaukee choice program is a hard case to test the hypothesis that privatization can result in efficiency gains. If one finds efficiency gains under considerably less than ideal circumstances, one is likely to find gains under more opportune conditions.

School Costs

The relative costs of the public and private schools in Milwaukee remained approximately the same throughout the four years of the experiment. In the 1991–92 school year payments per pupil to schools participating in the choice program schools were $2,729. Based on interviews with school administrators, it is estimated that schools received an additional $500 per student through fees and fund-raising activities. Therefore, the total costs per pupil are estimated to have been $3,229. Per-pupil costs for the Milwaukee public schools at this time averaged $6,656, somewhat higher than the $5,748 cost of educating the average public school student in the United States as a whole.[42]

Although it appears that the cost of educating a pupil in a choice school was only 48 percent of the cost of educating a student in the Milwaukee public schools, the actual difference was not this large. Choice school students were provided transportation by the Milwaukee public school system if they needed it. In addition, the reported per-pupil expenditures for the Milwaukee public schools included the costs of educating secondary school students (which may be more expensive than elementary education) as well as students receiving special services. But even after taking these considerations into account, the per-pupil costs of the private schools were lower.

The Milwaukee Randomized Experiment

The Milwaukee school choice program was a randomized experiment. To ensure equal access to the choice program among eligible applicants, the legislature required choice schools, if oversubscribed, to admit applicants at random. In the words of the original evaluation team, "Students not selected into the Choice Program in the random selection process represent a unique research opportunity. . . . If there are any unmeasured characteristics of families seeking private education, they should on average be similar between those in and not in the program."[43] The legislature asked the state's Department of Public Instruction to evaluate the Milwaukee choice experiment. Data were collected on family background characteristics and student performance on the Iowa Test of Basic Skills in reading and mathematics. These data were made available on the world wide web in February 1996.

Students did not apply to the choice program as a whole; instead, they applied each year for a seat in a specific grade in a particular school. They were selected or not selected randomly by school and by grade. Because the random assignment policy was implemented in this way, in our analysis we used a fixed effects model that took into account the grade to which a student applied and the year of application.[44] Our analysis was unable to ascertain the particular school to which a student applied,[45] but it took this factor partially into account by adjusting for the ethnicity of the applicant. More than 80 percent of the choice students attended one of three schools, and of these three schools virtually all students applying to one school were Hispanic, and almost all students applying to the two others were African American. Though the analysis took the two predominantly African-American schools as a block, it otherwise distinguished among schools by adjusting for whether the applicant was Hispanic or African American. Because the number of white students and other minority students for

whom information was available was so sparse that no reliable results could be obtained, these students were removed from the analysis.

By using a fixed effects model that took into account each point at which randomization occurred, together with a control for gender, it was possible to estimate the effects of enrollment on test scores in choice schools.[46] This procedure treated each point at which randomization occurred as a dummy variable. The measures of test score performance were the students' normal curve equivalent (NCE) scores for math and reading on the Iowa Test of Basic Skills. The NCE is a transformation of the national percentile rankings that arranges the scores around the fiftieth percentile in a manner that can be described by a normal curve. A standard deviation for NCE is 21 percentile points.

Separate ordinary least squares regressions produced an estimate of the effect of one, two, three, and four years of treatment on math and reading scores. The analysis of the Milwaukee randomized experiment conducted by Rouse constrained the effects of treatment to be linear in order to estimate the effect of each year in a single regression for math and reading, respectively.[47] Because the effects of treatment do not seem to have been linear, our approach of estimating each amount of treatment separately avoided this source of potential bias.

Coefficients for each dummy representing the points of randomization and the constant are too cumbersome to present in a book of this nature, but are available from the authors upon request, as are the coefficients for all substantive variables employed in the models. We controlled for gender in every regression, because it was available for virtually all students and produced a more precise estimate of the effect of treatment.

Our data are limited by the fact that test data were available for only 78 percent of those assigned to the treatment group and 72 percent assigned to the control group. The percentage of test scores available decreased to 40 percent of the treatment group and 48 percent of the control group by the third or fourth year following application to the program (see table 13-1).

Our results depend on the assumption that the missing cases did not differ appreciably from those remaining in the sample.[48] One way of estimating whether this assumption is reasonable is to examine the observed characteristics of students in the treatment and control groups. As can be seen in table 13-2, the background characteristics of the two groups do not differ in important respects. In the words of the original evaluation team, "In terms of demographic characteristics, non-selected . . . students came from very similar homes as choice [students did]. They were also similar in terms of prior achievement scores and parental involvement.[49]

Table 13-1. *Students for Whom Data Are Available*

Student category	*Choice students*	*Control students*
Percent with test scores available (table 13-3, columns 1–4)	79	72
Total number who applied, 1990–93	908	363
Percent with test scores three or four years after application (table 13-3, column 5)	40	48
Total number who applied in 1990 or 1991, making it possible to have scores three or four years after application	592	166

Table 13-2. *Background Characteristics of Students in Treatment and Control Groups*
Total numbers of cases in parentheses

	All students in the study			*All students with scores three or four years after application*		
Characteristic	*Choice students*	*Control students*	*p value*[a]	*Choice students*	*Control students*	*p value*[a]
Math scores before application	39.7 (264)	39.3 (173)	.81	40.0 (61)	40.6 (33)	.86
Reading scores before application	38.9 (266)	39.4 (176)	.74	42.1 (60)	39.2 (33)	.35
Family income	10,860 (423)	12,010 (127)	.14	10,850 (143)	11,170 (25)	.84
Mothers' education 3 = some college 4 = college degree	4.2 (423)	3.9 (127)	.04	4.1 (144)	3.8 (29)	.15
Percent married parents	24 (424)	30 (132)	.17	23 (145)	38 (29)	.11
Parents' time with children 1 = 1–2 hours/week 2 = 3–4 hours/week 3 = 5 or more	1.9 (420)	1.8 (130)	.37	1.9 (140)	1.7 (27)	.26
Parents' education expectations of children 4 = college 5 = graduate school	4.2 (422)	4.2 (129)	.85	4.2 (142)	3.7 (27)	.01

a. The tests of significance are suggestive of the equivalence of the two groups. Technically, tests of significance should be done at each point of random assignment, but the number of cases at each point is too few for such tests to be meaningful.

Results

Using the analytical procedures discussed above, we estimated the effects of choice schools on students' performance after one, two, three, and four years of attending choice schools.[50] Table 13-3 reports the results of our main analysis, in which we estimated the difference in test scores between students attending choice schools and those in the control group after controlling for gender using a fixed effects model that takes into account the points of randomization in the experiment.

The estimated effects of choice schools on mathematics achievement were slight for the first two years students were in the program. But after three years of enrollment students scored 5 percentile points higher than the control group; after four years they scored 10.7 points higher. These differences between the two groups three and four years after their application to choice schools are .24 and .51 standard deviation of the national distribution of math test scores, respectively. They are statistically significant at accepted confidence levels.[51] Differences on the reading test were between 2 and 3 percentile points for the first three years and increased to 5.8 percentile points in the fourth. The results for the third and fourth years are statistically significant when the two are jointly estimated.[52]

Table 13-3. *The Effect of Attending a Choice School on Test Scores, Controlling for Gender, Using Fixed Effects Model*
NCE Percentile points[a]

Effect and subject	*1 year of treatment*	*2 years of treatment*	*3 years of treatment*	*4 years of treatment*	*3 or 4 years jointly estimated*
Differences in mathematics scores between choice students and control group					
Effect on math scores	1.31	1.89	5.02**	10.65**	6.81**
Standard error	1.98	2.05	3.07	4.92	2.97
N	772	584	300	112	316
Differences in reading scores					
Effect on reading scores	2.22*	2.26	2.73	5.84*	4.85**
Standard error	1.74	1.78	2.63	4.22	2.57
N	734	604	301	112	318

* = p < .10 in one-tailed t-test
** = p < .05 in one-tailed t-test
a. Normal curve equivalent

Controlling for Family Background

The results in the main analysis in table 13-3 provide the best estimate of the achievement effects of attendance in private schools, because this analysis had the fewest missing cases. But because these results do not take into account family background characteristics, they depend on the assumption that students were assigned at random to the test and control groups. Inasmuch as even the main analysis had many missing cases, it was possible that the two groups were no longer similar in relevant respects, despite their similar demographics (see table 13-2). To explore whether this possibility contaminated our results, We performed a fixed effects analysis that took into account gender, mother's education, and parents' marital status, income, education expectations, and time spent with the child. Table 13-4 reports the results.

This analysis depended on information provided in response to a written questionnaire which, unfortunately, many parents did not complete. Background information was available for only 47 percent of the selected students and 36 percent of the control group. The number of cases available for analysis was therefore considerably reduced, and the point estimates are less reliable. Nevertheless, all point estimates are positive, and six of the eight are actually larger than those reported in the main analysis.

Table 13-4. *The Effect of Attending a Choice School on Test Scores, Controlling for Gender, Education Expectations, Income, Marital Status, Mother's Education, and Time Spent with Child, Using Fixed Effects Model*
NCE percentile points[a]

Effect and subject	*1 year of treatment*	*2 years of treatment*	*3 years of treatment*	*4 years of treatment*
Differences in mathematics scores between choice students and control group				
Effect on math scores	6.01**	5.36*	8.16*	7.97
Standard error	3.39	3.39	5.82	9.85
N	378	289	149	57
Differences in reading scores				
Effect on reading scores	4.72**	1.17	8.87**	15.00*
Standard error	2.88	2.99	5.27	9.45
N	358	293	150	55

a. Normal curve equivalent.
* = p < .10 in one-tailed t-test
** = p < .05 in one-tailed t-test

Controlling for Prior Test Scores

The main analysis did not control for students' test scores before entering into the choice program. It is not necessary to control for pre-experimental test scores when comparing a treatment group and a control group in an experimental situation, because the two groups, if randomly assigned to each category, can be assumed to be similar. But because of the sizable number of missing cases it is possible that the two groups we compared had different pretest scores before the experiment began. This potential source of bias did not appear, however. The average pretest scores at the time of application for the two groups were essentially the same. The average math and reading pretest scores for those selected choice for the program were the NCE equivalents of 39 and 38 percentile rankings, respectively; for those not selected they were the NCE equivalents of a 39 percentile ranking for reading and a 40 percentile ranking for math (see table 13-2).

Inasmuch as the students' pretest scores at the time of application were essentially the same, it is unlikely that controls for this variable would alter the result. We nonetheless tested for the possibility, and the results are reported in table 13-5. Because pretest scores at the time of application were available for only 29 percent of the selected students and 49 percent of the control group, the sample size for this analysis is smaller and the results are generally not statistically significant. Yet five of the eight point es-

Table 13-5. *The Effect of Attending a Choice School on Test Scores, Controlling for Gender and Test Score before Application, Using Fixed Effects Model*
NCE percentile points[a]

Effect and subject	*1 year of treatment*	*2 years of treatment*	*3 years of treatment*	*4 years of treatment*
Differences in mathematics scores between choice students and control group				
Effect on math scores	2.34	3.46*	7.40**	4.98
Standard error	2.32	2.71	4.08	9.16
N	286	185	83	31
Differences in reading scores				
Effect on reading scores	1.50	3.24*	5.28*	–3.29
Standard error	2.07	2.46	3.74	7.46
N	303	189	84	31

a. Normal curve equivalent.
* = p < .10 in one-tailed t-test
** = p < .05 in one-tailed t-test

timates are larger than those in the main analysis, and all but one have positive signs.

Effects on All Students Accepted into the Choice Program

The results reported so far compare students who attended private schools with students who had applied for choice but were assigned to the control group. Some students, however, were accepted into the program but chose not to participate for the full four years. Some students immediately turned down the opportunity, but others left sometime during the four-year period.

To see the effect of the choice program on all those admitted, regardless of their subsequent enrollment decisions, we conducted an analysis identical to the main analysis, except that analysis compared all students initially assigned to treatment and control groups, regardless of the schools they chose to attend. This type of analysis is known in medical research as an intention-to-treat analysis. In many medical experiments subjects may be more or less faithful in complying with the treatment. For example, some forget to take their pills three times a day as instructed. An intention-to-treat analysis answers this question: Is the treatment effective even when compliance is less than 100 percent? Those who refused enrollment in the private schools or left before the end of the experiment can be thought of as not having complied with the treatment.

This approach had the important disadvantage of including in the treatment group many students who either did not attend the private schools or attended the private schools for less than the full period under study. But it had two advantages. First, departure from an ideal randomized experiment was less in this case than in the main analysis. All cases were preserved except instances in which test data were not collected. The percentage of intention-to-treat cases in the analysis was 89 percent; sixty-three percent of the intention-to-treat cases three or four years after application remained in this analysis (see table 13-6).[53] (There were fewer missing cases because the students who left private schools but were tested in the Milwaukee public schools were not excluded from the intention-to-treat analysis.) Second, this analysis may have better captured what might happen if choice between public and private schools were generalized; students can be expected to migrate back and forth between the two systems.

Are there efficiency gains when comparisons are made between all those randomly assigned to the intention-to-treat group and the control group?

Table 13-6. *Percentage of Students in Intention-to-Treat Analysis for Whom Data Are Available*

Student category	*Selected students*	*Control students*
Percent with test scores available (table 13-7, columns 1–4)	89	72
Total number who applied, 1990–93	908	363
Percent with test scores three or four years after application	63	48
Total number who applied in 1990 or 1991, making it possible to have scores three or four years after application	592	166

The answer to this question is given in table 13-7. The effects do not differ in any significant way from those reported in the main analysis. Slight positive effects are found for the first three years after application to the program, and moderately large effects are found after four years. Students who were given a choice of schools performed better than did the control group, regardless of the public or private schools they attended. All results but one are statistically significant at the .1 level; fourth-year results are significant at the .05 level.

Table 13-7. *The Effect of Being Selected for a Choice School (Intention to Treat) on Test Scores, Controlling for Gender, Using Fixed Effects Model*
NCE percentile points[a]

Effect and subject	*1 year of treatment*	*2 years of treatment*	*3 years of treatment*	*4 years of treatment*
Differences in mathematics scores between choice students and control group				
Effect on math scores	2.68*	2.59*	3.83*	11.00**
Standard error	1.89	1.94	2.87	4.14
N	854	728	435	175
Differences in reading scores				
Effect on reading scores	2.46*	2.57*	2.10	6.26**
Standard error	1.71	1.68	2.48	3.65
N	816	738	441	175

a. Normal curve equivalent.
* = $p < .10$ in one-tailed t-test
** = $p < .05$ in one-tailed t-test

These results suggest that some of the achievement effects produced by choice may be due to a closer match between school qualities and student needs. When families are given a choice between public and private schools, they may be choosing the options best suited to their children. It is possible that public schools induced some families with students in the treatment group to return to the public schools by providing them with better public school alternatives. The Milwuakee public school system had the ability to respond in this manner because it had a number of magnet schools. It also had the incentive to react, because the system could regain funds equivalent to the size of the voucher if a student returned to the public school system.

Conclusions

The Milwaukee choice experiment suggests that privatization in education may result in efficiency gains. This finding emerges from a randomized experiment less likely to suffer from selection bias than studies dependent on nonrandomized data. The consistency of the results is noteworthy. Positive results were found for all years and for all comparisons except one. The results reported in the main analysis for both math and reading are statistically significant for students remaining in the program for three to four years when these are jointly estimated.

These results after three and four years are moderately large, ranging from .1 of a standard deviation to as much as .5 of a standard deviation. Studies of educational effects interpret effects of .1 standard deviation as slight, effects of .2 and .3 standard deviation as moderate, and effects of .5 standard deviation as large.[54] Even effects of .1 standard deviation are potentially large if they accumulate over time.[55] The average difference in test performances of whites and minorities in the United States is one standard deviation.[56] If the results from Milwaukee can be generalized and extrapolated to twelve years, a large part of between-group reading differences and all of between-group math differences could be erased.

Without data beyond the Milwaukee program's first four years, one can only speculate as to whether such generalization and extrapolation are warranted. But if they are, the effectiveness of government-financed education could be greatly enhanced. These moderately large effects on student achievement were observed even though the Milwaukee plan offered students and families only a slightly enlarged set of educational choices. These achievement effects were produced at lower per-pupil cost than that of a Milwaukee public school education.

One must be cautious concerning the universe to which these results are generalized. Efficiency gains may be greater in Milwaukee and other central cities than in suburban areas where competition among school districts is greater. They may also be greater in cities than in rural communities where opportunities for co-production in public education may be more prevalent. The magnitude of the gains reported here may not be generalizable beyond central cities.

In addition, the study was limited to students from low-income families. Other studies suggest that private schools have a larger positive effect on the achievement of disadvantaged students.[57] Perhaps the results found in Milwaukee are restricted to low-income minority populations. Finally, the results are for families who applied for vouchers. It may be that the benefits of privatization are greater for those families who desire alternatives to the public schools serving them. Their children may have been particularly at risk in public schools, and they may be more willing to engage in co-production than all other families.

The conclusions that can be drawn from our study are further restricted by limitations of the data made available on the world wide web. Many cases are missing from this data set. The percentage of missing cases is especially large when one introduces controls for background characteristics and preexperimental test scores. But given the consistency and magnitude of the findings as well as their compelling policy implications, they suggest the desirability of further randomized experiments capable of reaching more precise estimates of efficiency gains through privatization.

Randomized experiment are under way in New York City, Dayton, and Washington, D.C.[58] If the evaluations of these randomized experiments minimize the number of missing cases and collect preexperimental data for all subjects in both treatment and control groups, they could, in a few years' time, provide more precise estimates of potential efficiency gains from privatizing the delivery of educational services to low-income students. Similar experiments should be conducted in a variety of contexts, but especially in large central cities, where potential efficiency gains seem particularly likely.

Notes

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and Larry Samuelson, "To Innovate or Not to Innovate: Incentives and Innovation in Hierarchies," *American Economic Review* (December 1990), pp. 1105–06.

2. Charles M. Tiebout, "A Pure Theory of Local Expenditures," *Journal of Political Economy* 64 (October 1956), pp. 416–24; Robert L. Bish, *The Public Economy of Metropolitan Areas* (Chicago: Markham, 1971).

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5. Paul E. Peterson, *The Price of Federalism* (Brookings, 1995).

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7. Some of these increased school costs are due to improved services for students with disabilities and those who are otherwise disadvantaged.

8. Helen F. Ladd, ed., "Introduction," in Ladd, ed., *Holding Schools Accountable: Performance-based Reform in Education* (Brookings, 1996), p. 3; Richard Rothstein and Karen Hawley Miles, *Where's the Money Gone? Changes in the Level and Composition of Education Spending* (Washington: Economic Policy Institute, 1995), p. 7.

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10. Caroline Minter Hoxby, "When Parents Can Choose, What Do They Choose? The Effects of School Choice on Curriculum," in Susan Mayer and Paul E. Peterson, eds., *When Schools Make a Difference* (forthcoming).

11. Paul E. Peterson, "Monopoly and Competition in American Education," in William H. Clune and John F. Witte, eds., *Choice and Control in American Education,* vol. 1 (New York: Falmer, 1990), pp. 47–78.

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16. Anthony Bryk, Valerie E. Lee, and Peter B. Holland, *Catholic Schools and the Common Good* (Harvard University Press, 1993).

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24. But see the evaluation of the Tennessee randomized experiment in Frederick Moesteller, "The Tennessee Study of Class Size in the Early School Grades," *The Future of Children* 5 (1995), pp. 113–27. This study found that class size has a positive affect on student achievement, contrary to many econometric studies. For the latter, see Eric Hanushek, "The Economics of Schooling: Production and Efficiency in Public Schools," *Journal of Economic Literature* 24 (September 1986), pp. 1141–77.

25. Witte and others, "Fourth Year Reports."

26. Paul E. Peterson, "A Critique of the Witte Evaluation of Milwaukee's School Choice Program," Occasional Paper, Center for American Political Studies, Harvard University, February 1995.

27. Greene and others, "Effectiveness of School Choice."

28. Witte, "Achievement Effects of the Milwaukee Voucher Program"; Cecilia Rouse, "Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program," Department of Economics and the National Bureau of Economic Research, Princeton University, 1997.

29. A. N. Hieronymous, *Teacher's Guide Multilevel Battery Level 9–14: Iowa Test of Basic Skills Forms G/H* (Chicago: Riverside Publishing, 1986); H. D. Hoover and others, *Iowa Tests of Basic Skills: Norms and Score Conversions, Form K, Complete and Core Batteries, Levels 5–14* (Chicago: Riverside, 1993).

30. The Milwaukee choice program is described as it was in its initial years, because the data on student achievement are available for only the first four years. In subsequent years the program was expanded somewhat, but the important expansion in 1995 to include religious schools has yet to be implemented due to court challenges. For a fuller discussion of the program, see Paul E. Peterson, Jay P. Greene, and Chad Noyes, "School Choice in Milwaukee," *Public Interest* (Fall 1996), pp. 38–56; and Paul E. Peterson and Chad Noyes, "Under Extreme Duress, School Choice Success," in Diane Ravitch and Joseph Viteritti, eds., *New Schools for a New Century: The Redesign of Urban Education* (Yale University Press, 1997.)

31. The number of students attending each school was made available by the State Department of Public Instruction and reported in the *Milwaukee Journal* ("Court Time on Choice Extended," October 3, 1991) and a report by the Wisconsin Legislative Audit Bureau (*An Evaluation of Milwaukee Parental Choice Program,* February 1995, table 2, p. 22, table 3, p. 23). In addition to the schools discussed in the text, a Montessori school serving a middle-class constituency admitted three students the first year and four the next. Woodland School, formerly a laboratory school for a local Catholic college, enrolled between twenty and forty choice students each year. After the first year three other private elementary schools admitted a small number of students. Test performances of a small number of students attending the high schools participating in the program were not analyzed because no appropriate control group was available. These schools were initially established to serve at-risk students referred to them by the Milwaukee public schools.

32. The students attending this school are not included in the main analysis because they were not in a choice school at the end of the first year; nevertheless, they are included in the intention-to-treat analysis in table 13-7.

33. Witte, "First Year Report: Milwaukee Parental Choice Program."

34. Amy Stuart Wells, "Milwaukee Parents Get More Choice on Schools," *New York Times,* March 28, 1990, p. B9.

35. Barbara Miner, "'Choice' School in Turmoil Because of Staff Cuts, Changes," *Milwaukee Journal,* November 23, 1990, p. B5.

36. Four of the original choice schools were said to be in "serious financial difficulty" and, in addition to Juanita Virgil, two more were said to be "on the verge of closing in the Spring of 1990" (Witte, Bailey, and Thorn, "Third Year Report: Milwaukee Parental Choice Program").

37. *Milwaukee Community Journal,* Special Supplement, May 4, 1994.

38. Paul Taylor, "Milwaukee's Controversial Private School Choice Plan Off to Shaky Start," *Washington Post,* May 23, 1991, p. A3.

39. Witte, "First Year Report: Milwaukee Parental Choice Program," p. 12.

40. Witte, "First Year Report: Milwaukee Parental Choice Program," p. 13.

41. *Milwaukee Community Journal,* Special Supplement.

42. U.S. Department of Education, *Digest of Education Statistics* (Washington, Office of Educational Research and Improvement, 1991), table 158.

43. Witte and others, "Fourth Year Report."

44. Siblings were exempt from the random assignment rule. We were unable to identify siblings from the information made available on the world wide web.

45. To protect the confidentiality of students, the data on the world wide web do not identify the schools they attended. To obtain this information we offered to protect students' confidentiality, but we were unable to obtain access to these data.

46. Inasmuch as there were nine grades, two racial groups, and four years in which students applied, analyses could potentially include seventy-two dummy variables representing all possible points of randomization. In practice, the number of dummy variables or "blocks" included in the analyses reported in table 13-3 varied between eleven and sixty-five, the precise number depending on the number of grades for which students applied in particular years. See W. G. Cochran, "The Planning of Observational Studies of Human Populations," *Journal of the Royal Statistical Society,* Series A, 128 (1965), pp. 234–65, and D. B. Rubin, "William Cochran's Contributions to the Design, Analysis, and Evaluation of Observational Studies," in Podurl S. R. S. Rao, ed., *W. G. Cochran's Impact on Statistics* (New York: John Wiley, 1984).

47. Rouse, "Private School Vouchers and Student Achievement."

48. Many factors contributed to the large number of missing cases: Milwaukee public schools administered tests intermittently; students were absent on the days the tests were administered; students left the city, left the choice program, or were excluded from testing; test scores were lost; and so forth. One can speculate that the large number of missing cases may bias results in one direction or another. Low performers may be more likely to be tested (because of federal requirements) or may be less likely to be tested (designated as special students); they may be more likely to have moved (live in mobile homes) or less likely to have moved (do not have many options). If the initial assignment to test and control groups was random, one may reasonably assume that all extraneous factors operate with equal effect on both treatment and control groups. The fact that most observable characteristics of the treatment and control groups do not differ significantly is consistent with such an assumption.

49. Witte and others, "Fourth Year Report."

50. These data are from the first four years of the choice school experiment. Test score information on the control group was not available on the world wide web for subsequent years.

51. We prefer the one-tailed t-test to estimate the statistical significance of the findings, because theory and prior research both suggest that students should perform better in private schools.

52. Results for three and four years after application were jointly estimated by averaging scores for students who were tested in both years and by using the single score available for each of the remaining students. Dummy variables were included for those who had only third-year or fourth-year scores.

53. The background characteristics of students who are included in the intention-to-treat category are virtually identical to those who actually enrolled, as reported in table 13-2.

FOURTEEN

Lessons from the Cleveland Scholarship Program

Jay P. Greene, William G. Howell, and Paul E. Peterson

DOES SCHOOL CHOICE WORK? If so, who benefits? Choice critics say private schools do not appear to serve students' academic needs any better than public schools. They further argue that the few detectable benefits of school choice accrue mainly to students who need the least assistance. Parents who are already involved with their children's education will capitalize on choice. What is more, private schools are disinclined to accept students with special needs. And after choice students gain admission to private schools, the argument goes, the weakest will be weeded out.

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