Evaluation in Practice

A Methodological Approach

edited by

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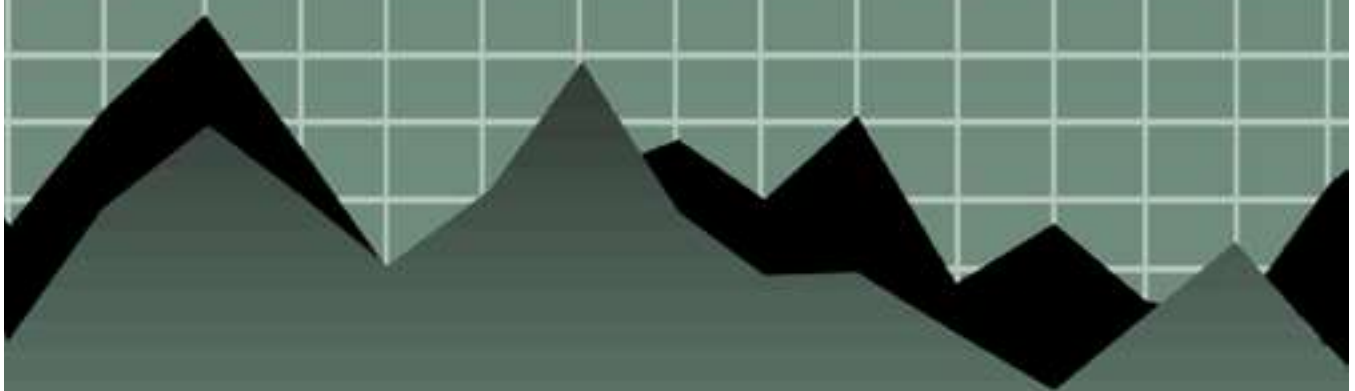

SECOND EDITION

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PART VIII

Dilemmas of Evaluation

We close the book with two articles and a critique. They are two of the most interesting articles to come down the pike in a long time, and they illustrate one of the real dilemmas of evaluation. As evaluators, we must always be sure that we are asking the right question, and this is the dilemma presented here. Two different research groups did two separate evaluations of the school choice program in Milwaukee, Wisconsin, *using the same data set* and came to different conclusions. One evaluation concluded that the program was ineffective—that it did not lead to improved academic achievement for participants. The other evaluation looked for the same outcomes—improved academic achievement—but in a slightly different way. They came to the conclusion that the program did indeed produce improved academic achievement for participants.

Read these evaluations and see what you think. Then turn to the critique. We have asked one of our colleagues, research methodologist Chieh-Chen Bowen, what she thinks.

CHAPTER 20

Fifth-Year Report

Milwaukee Parental Choice Program

John F. Witte
Troy D. Sterr
Christopher A. Thorn

Department of Political Science and
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Executive Summary

This report consists of four sections: (1) a description of the Milwaukee Parental Choice Program and the data being collected; (2) a description of the choice families and students; (3) a five-year report on outcomes; and (4) a brief response to some of the criticisms of our previous evaluations. For the most part, this report updates the *Fourth Year Report* (December 1994) and should be read in conjunction with that report. Most of the findings are consistent with that very detailed report.

The Original Program. The Milwaukee Parental Choice Program, enacted in spring 1990, provides an opportunity for students meeting specific criteria to attend private, nonsectarian schools in Milwaukee. A payment from public funds equivalent to the MPS per member state aid ($3,209 in 1994–95) is paid to the private schools in lieu of tuition and fees for the student. Students must come from families with incomes not exceeding 1.75 times the national poverty line. New choice students must not have been in private schools in the prior year or in public schools in districts other than MPS. The total number of choice students in any year was limited to 1% of the MPS membership in the first four years, but was increased to 1.5% beginning with the 1994–95 school year.

Schools initially had to limit choice students to 49% of their total enrollment. The legislature increased that to 65% beginning in 1994–95. Schools must also admit choice students without discrimination (as specified in s. 118.13, Wisconsin Stats.). Both the statute and administrative rules specify that pupils must be accepted "on a random basis." This has been

interpreted to mean that if a school was oversubscribed in a grade, random selection is required in that grade. In addition, in situations in which one child from a family was admitted to the program, a sibling is exempt from random selection even if random selection is required in the child's grade.

The New Program. The legislation was amended as part of the biennial state budget in June 1995. The principal changes were: (1) to allow religious schools to enter the program: (2) to allow students in grades kindergarten through three who were already attending private schools to be eligible for the program: (3) to eliminate all funding for data collection and evaluations (the Audit Bureau is required simply to file a report by the year 2000). Thus unless the legislation changes, data of the type collected for this and previous reports will not be available for the report to be submitted in the year 2000.

Choice Families and Students. Enrollment in the Choice Program has increased steadily but slowly, never reaching the maximum number allowed by the law. September enrollments have been 341, 521, 620, 742, and 830 from 1990–91 to 1994–95. The number of schools participating was: 7 in 1990–91, 6 in 1991–92, 11 in 1992–93, and 12 in the last two years. The leading reasons given for participating in the Choice Program [were] the perceived *educational quality* and the *teaching approach and style* in the private schools. That is followed by the *disciplinary environment and the general atmosphere* that parents associate with those schools. Frustration with prior public schools, although not unimportant, was not as important a reason for applying to the Choice Program as the attributes of the private schools.

The Choice Program was established, and the statute written, explicitly to provide an opportunity for relatively poor families to attend private schools. The program has clearly accomplished this goal. In terms of reported family income (Table 5a), the average income was $11,630 in the first five years. Incomes of 1994 choice families increased considerably to an average of $14,210. In racial terms, the program has had the greatest impact on African-American students, who comprised 74% of those applying to choice schools and 72% of those enrolled in the first five years of the program (Table 5b). Hispanics accounted for 19% of the choice applicants and 21% of those enrolled. In terms of marital status (Table 5c), choice families were much more likely to be headed by a non-married parent (75%) than the average MPS family (49%), and somewhat more likely than the low-income MPS parent (65%). The percentage was almost identical for the five separate years. A unique characteristic of choice parents was that despite their economic status, they reported higher education levels than either low-income or average MPS parents (Table 5e). Over half of the choice mothers reported some college education (56%), compared with 40% for the entire MPS sample and 30% of the low-income MPS respondents. Consistent with the education levels of parents, educational expectations for their children were also somewhat higher for choice parents than MPS and low-income MPS parents.

Finally, the experiences of choice families with prior MPS schools differed from non-choice, MPS families in three important ways: (1) students enrolling in the choice program were not achieving as well as the average MPS student; (2) choice parents were considerably less satisfied than other MPS parents; and (3) parental involvement in their prior schools and in educational activities at home was greater for choice parents.

Outcomes. Outcomes after five years of the Choice Program remain mixed. Achievement change scores have varied considerably in the first five years of the program. Choice stu-

dents' reading scores increased the first year, fell substantially in the second year, and have remained approximately the same in the next three years. Because the sample size was very small in the first year, the gain in reading was not statistically significant, but the decline in the second year was. In math, choice students were essentially the same in the first two years, recorded a significant increase in the third year, and then significantly declined this last year.

MPS students as a whole gained in reading in the first two years, with a relatively small gain in the first year being statistically significant. There were small and not significant declines in the last two years. Low-income MPS students followed approximately the same pattern, with none of the changes approaching significance. Math scores for MPS students were extremely varied. In the first year there were significant gains for both the total MPS group and the low-income subgroup. In the second year, the scores were essentially flat, but in the third year they declined significantly. Again, in the fourth year there was essentially no change in either the total MPS or low-income MPS groups.

Regression results, using a wide range of modeling approaches, including yearly models and a combined four-year model, generally indicated that choice and public school students were not much different. If there was a difference, MPS students did somewhat better in reading.

Parental attitudes toward choice schools, opinions of the Choice Program, and parental involvement were very positive over the first five years. Parents' attitudes toward choice schools and the education of their children were much more positive than their evaluations of their prior public schools. This shift occurred in every category (teachers, principals, instruction, discipline, etc.) for each of the five years. Similarly, parental involvement, which was more frequent than for the average MPS parent in prior schools, was even greater for most activities in the choice schools. In all years, parents expressed approval of the program and overwhelmingly believed the program should continue.

Attrition (not counting students in alternative choice schools) has been 44%, 32%, 28%, 23%, and 24% in the five years of the program. Estimates of attrition in MPS are uncertain, but in the last two years, attrition from the Choice Program was comparable to the range of mobility between schools in MPS. The reasons given for leaving included complaints about the Choice Program, especially application and fee problems, transportation difficulties and the limitation on religious instruction. They also included complaints about staff, general educational quality and the lack of specialized programs in the private schools.

Conclusions. We ended the *Fourth Year Report* by summarizing the positive and negative consequences of the program. We then wrote:

> Honorable people can disagree on the importance of each of these factors. One way to think about it is whether the majority of the students and families involved are better off because of this program. The answer of the parents involved, at least those who respond to our surveys, was clearly yes. This is despite the fact that achievement, as measured by standardized tests, was no different than the achievement of MPS students. Obviously the attrition rate and the factors affecting attrition indicate that not all students will succeed in these schools, but the majority remain and applaud the program.

Although achievement test results may be somewhat more bleak for choice in this analysis, the differences are not very large in terms of their impact, and the negative esti-

mates are based on less stable models and smaller sample sizes. In addition test scores are only one indication of educational achievement. Thus we see no reason to change last year's conclusion.

Acknowledgments

This research was funded initially by a grant from the Robert La Follette Institute of Public Affairs of the University of Wisconsin-Madison and subsequently by a substantial grant from the Spencer Foundation in Chicago. We gratefully acknowledge this support.

Availability of Data, Codebooks, Reports and Papers on the Internet

A World Wide Web site has been created for choice data and some reports and papers. Included are all data modules used for all reports, with electronic codebooks; the 1994 report; this report; and a paper by John Witte and Chris Thorn comparing the characteristics of public and private school families in the State of Wisconsin and in the City of Milwaukee. Subsequent analyses and publications will be added to the site.

The Internet address of the site is:
http://dpls.dacc.wisc.edu/choice/choice_index.html

I. Introduction

This report consists of four sections: (1) a description of the Milwaukee Parental Choice Program and the data being collected; (2) a description of the choice families and students; (3) a five-year report on outcomes; and (4) a brief response to some of the criticisms of our previous evaluations.

This report is an abbreviated version of earlier reports, for three reasons. First, adequate resources to do this research have not been available for the past two years. Second, program legislation has been changed to include parochial schools and allow students already in those schools to enter the program. The new program will not be comparable and will not be evaluated except for an audit in the year 2000. Third, comparable Milwaukee Public School (MPS) system achievement test score data were not available for the fifth year because the MPS has essentially dropped the Iowa Test of Basic Skills (ITBS) as state mandated tests have been introduced. The private schools continue to use the ITBS and are not required to take the state tests. This report does, however, more than fulfill the statutory requirements (section 119.23 (5) (d)). This report does not contain any information on the 1995–96 school year.

For the most part, this report updates the *Fourth Year Report* (December 1994) and should be read in conjunction with that report. Most of the findings are consistent with that very detailed report. This report includes three new items: (1) a brief description of the new program legislation; (2) new multivariate estimates of achievement differences between choice and MPS schools, combining four years of data; and (3) a brief response to some of the criticisms of previous reports.[1] Some of these critiques were inaccurate in the extreme and should not have been used as policy-making tools. The Peterson critique, for example, circulated when the legislature was considering major changes in the legislation, and dramatic changes did occur—particularly an increase in the scope of the program and appropriations for students to attend religious schools.

II. The Milwaukee Parental Choice Program

The Original Program. The Milwaukee Parental Choice Program, enacted in spring 1990, provides an opportunity for students

meeting specific criteria to attend private, nonsectarian schools in Milwaukee. A payment from public funds equivalent to the MPS per-member state aid ($3,209 in 1994–95) is paid to the private schools in lieu of tuition and fees for the student. Students must come from families with incomes not exceeding 1.75 times the national poverty line. New choice students must not have been in private schools in the prior year or in public schools in districts other than MPS. The total number of choice students in any year was limited to 1% of the MPS membership in the first four years, but was increased to 1.5% beginning with the 1994–95 school year.

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The New Program. The legislation was amended as part of the biennial state budget in June 1995. The principal changes were: (1) to allow religious schools to enter the program; (2) to allow students in grades kindergarten through grade three who were already attending private schools to be eligible for the program;[2] (3) to eliminate all funding for data collection and evaluations (the Audit Bureau is required simply to file a report by the year 2000). Thus unless the legislation changes, data of the type collected for this and previous reports will not be available for the report to be submitted in the year 2000.

Research and Data. The study on which this report is based employs a number of methodological approaches. Surveys were mailed in the fall of each year from 1990 to 1994 to all parents who applied for enrollment in one of the choice schools. Similar surveys were sent in May and June of 1991 to a random sample of 5,475 parents of students in the Milwaukee Public Schools. Among other purposes, the surveys were intended to assess parent knowledge of and evaluation of the Choice Program, educational experiences in prior public schools, the extent of parental involvement in prior MPS schools, and the importance of education and the expectations parents hold for their children. We also obtained demographic information on family members. A follow-up survey of choice parents assessing attitudes relating to their year in private schools was mailed in June of each year.

In addition, detailed case studies were completed in April 1991 in the four private schools that enrolled the majority of the choice students. An additional study was completed in 1992, and six more case studies in the spring of 1993. Case studies of the K-8 schools involved approximately 30 person-days in the schools, including 56 hours of classroom observation and interviews with nearly all of the teachers and administrators in the schools. Smaller schools required less time. Researchers also attended and observed parent and community group meetings, and Board of Director meetings for several schools. Results of these case studies were included in the December 1994 report.

Finally, beginning in the fall of 1992 and continuing through the fall of 1994, brief mail and phone surveys were completed with as many parents as we could find who chose

not to have their children continue in the program.

In accordance with normal research protocol, and with agreement of the private schools, to maintain student confidentiality, reported results are aggregated and schools are not individually identified. Thus these findings should not be construed as an audit or an assessment of the effectiveness of the educational environment in any specific school.

Readers of earlier reports may notice slight differences in a few of the statistics reported for earlier years. These differences are attributable to improvements in collecting data (such as search for student names in MPS records), errors in reporting, survey results that come in after the reports are prepared, and data entry and analysis errors that are subsequently corrected. Unless noted in the text, these differences are minor.

III. Choice Families and Students

Over the years we have reported on a number of important questions concerning the students and families who applied to the choice program. How many families apply? How many seats are available in the private schools? How do families learn about the program? Why do they want to participate? What are the demographic characteristics of students and families? What are parental attitudes toward education? What are the prior experiences of applicants in public schools? How well were the students doing in their prior public schools?

Findings are very consistent over the five years of the program. For economy, and because five years of data provide a better picture than a single year, most tables contain combined data from 1990 to 1994. The most appropriate comparison group to the choice families, on most measures, is the low-income MPS sample. That group, which includes about two-thirds of Milwaukee students, is defined as qualifying for free or reduced-priced lunch. The income level for reduced-priced lunch is 1.85 times the poverty line, free lunch is 1.35 times the poverty line. Almost all low-income students qualify for full free lunch.

Enrollment in the Choice Program. Enrollment statistics for the Choice Program are provided in Table 1. Enrollment in the Choice Program has increased steadily but slowly, never reaching the maximum number allowed by the law. September enrollments have been 341, 521, 620, 742, and 830 from 1990–91 to 1994–95. The number of schools participating was: 7 in 1990–91, 6 in 1991–92, 11 in 1992–93, and 12 in the last two years. The number of applications has also increased, with again the largest increase in 1992–93. In the last two years, however, applications have leveled off at a little over 1,000 per year. Applications exceeded the number of available seats (as determined by the private schools) by 171, 143, 307, 238 and 64 from 1990–91 through 1994–95. Some of these students eventually fill seats of students who are accepted but do not actually enroll. The number of seats available exceed the number of students enrolled because of a mismatch between applicant grades and seats available by grade. It is difficult to determine how many more applications would be made if more schools participated and more seats were available. In 1992–93, when the number of participating schools increased from 6 to 11, applications rose by 45%. In the last two years, however, seats available increased by 22% and 21%, but applications increased only by 5% from 1992–93 to 1993–94 and declined this past year.

Learning About Choice and the Adequacy of Information on Choice. Table 2 indicates how survey respondents learned about the Choice Program. The results are fairly similar

for the first four years. The most prevalent source of information on choice remains friends and relatives, which essentially means word-of-mouth information. That informal communication is almost double the frequency of other sources. It also increased in 1994.

Parental satisfaction with the amount and accuracy of that information, and with the assistance they received, is presented in Table 3. Satisfaction with the amount of information on the program in general is high in all years and even higher in 1994. There is, however, a difference in satisfaction of parents selected and not selected into the choice schools. Looking ahead to Table 6, when we create additive scales for these questions measuring satisfaction with administration of the Choice Program, we see a large difference between those applicants who enroll in the Choice Program and those not selected.[3] As indicated in the rows for Scale T3 (top panel, Table 6), choice enrollees for 1990–94 have a mean dissatisfaction of 11.6, while non-selected parents have a much higher average dissatisfaction score of 14.4.

Why Choice Parents Participated in the Choice Program. Table 4 provides the responses to survey questions rating the importance of various factors in parents' decisions to participate in the Choice Program. The results are consistent across the years. The leading reasons given for participating in the Choice Program [were] the perceived *educational quality* and the *teaching approach and style* in the private schools. That is followed by the *disciplinary environment and the general atmosphere* that parents associate with those schools. Frustration with prior public schools, although not unimportant, was not as important a reason for applying to the Choice Program as the attributes of the private schools. At the bottom of the list are siblings in the school and the location of the school.

Demographic Characteristics of Choice Families and Students. The Choice Program was established, and the statute written, explicitly to provide an opportunity for relatively poor families to attend private schools. The program has clearly accomplished this goal. Relevant demographic statistics are presented in Table 5, which, unless otherwise noted, are based on our surveys. For comparison purposes seven groups are identified, including applicants in the most recent year (1994), and the combined year samples for choice applicants, choice students actually enrolled, students not selected, students who left the program (and did not graduate), and the MPS control groups.

In terms of reported family income (Table 5a), the average income was $11,630 in the first five years. Incomes of 1994 choice families increased considerably to an average of $14,210. Low-income MPS parents reported a slightly higher family income, which averaged $12,100. The average in the full MPS control group was $22,000.

In racial terms, the program has had the greatest impact on African-American students, who comprised 74% of those applying to choice schools and 72% of those enrolled in the first five years of the program (Table 5b). Hispanics accounted for 19% of the choice applicants and 21% of those enrolled. Both of these groups were disproportionately higher than the MPS sample. Compared with the low-income MPS sample, however, Hispanics were the most overrepresented, with Asians and White students the most underrepresented. The overrepresentation of Hispanics was due to a new building and considerable expansion in capacity of Bruce-Guadalupe Bilingual School, which has an emphasis on Spanish culture and is located in a neighborhood with an increasing Hispanic population.

In terms of marital status (Table 5c), choice families were much more likely to be headed by a non-married parent (75%) than

the average MPS family (49%), and somewhat more likely than the low-income MPS parent (65%). The percentage was almost identical for the five separate years.

One important difference between MPS and choice applicants was in family size (Table 5d). For the combined years, only 42% of the choice families reported having more than two children. The average number of children in families applying to the choice program was 2.54. This compared with 54% of the MPS families having more than 2 children (2.95 children per family) and 65% of the low-income MPS families (3.24 on average).

A unique characteristic of choice parents was that despite their economic status, they reported higher education levels than either low-income or average MPS parents (Table 5e). Over half of the choice mothers reported some college education (56%), compared with 40% for the entire MPS sample and 30% of the low-income MPS respondents. That number was even higher in 1994, with 64% of the mothers new to the program reporting some college. That was consistent with the higher incomes reported for the last year (Table 5a, column 2). The biggest difference in education appears in the category titled "some college." Although fathers more closely match the MPS control groups, they were also somewhat more educated.

The Importance of Education and Educational Expectations. Based on our measures, choice and MPS parents were similar in terms of the importance they place on education. We measured the importance of education relative to other important family values. Scale descriptive statistics are in Table 6.

Educational expectations were high for all groups, with choice parents somewhat higher than MPS and low-income MPS parents. Eighty-six percent of choice parents in the first four years indicated that they expected their child to go to college or do post-graduate work. This compared with 76% of the MPS parents and 72% of the low-income MPS parents. Because sample sizes were large, these proportions were significantly different.

Experience of Choice Parents in Prior Public Schools. A more complete picture of choice parents includes the level of parental involvement, attitudes toward, and student success in their child's prior public school. Our surveys measured the degree of parental involvement in the school, the amount of parental help for children at home, and parent satisfaction with prior schools. The results are presented in Table 6. Prior achievement test results are provided in Table 8.

Based on five years of highly consistent data, the conclusions we draw are as follows: (1) that choice parents were significantly more involved in the prior education of their children than MPS parents (Table 6, Scales A3–A5); (2) that they expressed much higher levels of dissatisfaction with their prior schools than MPS parents (Table 6, Scale A6); and, (3) that their children had lower prior achievement test results than the average MPS student (Table 8).[4]

Choice students had prior test scores at or, in some years below, the low-income MPS students. The test scores in Table 8 are Iowa Test of Basic Skills (ITBS) which are given in grades 1–8. The tests used are the Reading Comprehension Test and the Math Total Test. The latter consists of three subtests: Math Concepts, Math Problem Solving, and Math Computations.[5] The results reported are for the last test taken while the student was in MPS. The reason for this is that we are trying to get as complete a picture as possible of a relatively small number of choice students. The majority of those tests were taken in the spring of the year of application.[6]

The prior test scores for the last year, 1994, were the lowest of any cohort, especially in math. This may have been due to the small number of students who completed all portions of the math test (because of changes in MPS testing requirements). In any event, the pattern remained consistent.

The absolute level of the scores indicates the difficulty these students were having prior to entering the program. The median national percentile for choice students ranged from 25.5 to 31, compared with the national median of 50. The Normal Curve Equivalent (NCE), which is standardized to a national mean of 50, ranged from 35.5 to 39.8, which is about two-thirds of a standard deviation below the national average. *In short, the choice students were already very low in terms of academic achievement when they entered this program.*

IV. Outcomes

We discuss five outcome measures in this section: (1) achievement test results; (2) attendance data; (3) parent attitudes; (4) parental involvement; and 5) attrition from the program. The legislation specified that suspension, expulsion, and dropping out also be monitored. Those measures, however, would be meaningful only at the high-school level.[7] The high-school-level choice schools consisted of alternative education programs for seriously at-risk students. Therefore it is unclear what the relevant MPS comparisons would have been.

Achievement Test Results

Cohort Test Results. Table 9 provides the aggregate test results for 1991 to 1995 for choice students and for 1991 to 1994 for students taking tests in MPS in the respective years. Tests were administered in April or May of each year. The results may be compared only crudely with those in Table 8, which indicated prior test scores for students accepted into the Choice Program. The prior test data in Table 8 were mostly from the previous spring, but were based on the last prior test in the student's file. As stated above, those data indicated the choice applicants were clearly behind the average MPS student, and also behind a large random sample of low-income MPS students.

As noted in the introduction, test scores from the MPS sample were not available for 1995. MPS is only requiring the ITBS of fifth-grade students due to state-mandated tests, which have been substituted for the ITBS. In addition, the ITBS version given to fifth graders is a new and very different test which is not substantively comparable to the older versions used in the choice school. Choice schools were not required to take the state tests. Thus we report on only choice 1995 tests, and change scores are analyzed using only the first four years of data.

In reading, Table 9 indicates that the choice students tested in 1991 did better than the MPS low-income group, but in math they did somewhat worse. Tests taken in the choice schools in the second year—spring 1992—were considerably lower. They were lower than the scores in both the full MPS sample and among the low-income MPS students. Comparing the more relevant low-income MPS and choice students, the choice students mean math NCE was more than five points lower; the reading score was two points lower.

For 1993, the scores of choice students were approximately the same in reading as in the prior year, but were considerably higher in mathematics. Although the reading scores were lower than the low-income MPS sample, the math scores had the same median National Percentile Ranking, but were 2.3 NCE points higher.

The 1994 spring scores for all choice stu-

dents were slightly higher than the 1993 choice cohort, and very similar to the MPS low-income group on both reading and mathematics. In contrast to the first two years, for both 1993 and 1994, choice students did better in math than in reading (which is the pattern for MPS students in all years).

Choice scores in 1995 were very similar to the scores in 1994. In terms of NCEs, they were almost identical. In terms of median National Percentile Rankings, the 1995 scores of choice students were 2% lower than in 1994 for both reading and math.

Because the cohort scores do not report on the same students from year to year, and because we hope that schools are able to add to the educational achievement of students, the most accurate measure of achievement are "value-added," change scores.

Change Scores. Analysis of year-to-year change scores for individual students produced somewhat different results.[8] By comparing students' scores with national samples, we can estimate how individual students changed relative to the national norms over the year. Descriptive change scores are depicted in Tables 10a to 10e for the respective years. We caution the reader, as we have in previous reports, that sample sizes in some years are quite small for choice students. The results in the table are based on differences in NCEs, subtracting the first-year score from the second.[9]

In the first year, choice students gained in reading, but math scores stayed essentially the same. MPS students improved considerably in math, with smaller gains in reading. Both low-income and non-low-income MPS students gained in math. Three of these gains were statistically significant (due in part to larger sample sizes), while the gains for choice students were not.

There were quite different effects in the second year (Table 10b). Change scores for choice students in math, and for MPS students in both reading and math, were not appreciable. None of these differences approached standard levels of statistical significance. In contrast to the first year, however, reading scores dropped for choice students. The decline was 3.9 NCE points for all students between 1991 and 1992. Because NCE scores are based on a national norm, this means that choice students scored considerably below the national sample in the prior year. The decline was statistically significant at the .001 level.

The results shifted again in the third year. Choice students declined slightly in reading, which was not significant. On the other hand, for the first time, math scores for choice students improved. The mean math NCE went from 38.3 to 42.7 for a 4.4 NCE gain, which was statistically significant. Scores for MPS students, on the other hand, declined for both tests and for both groups. Because of relatively large sample sizes for the MPS control group, the decline in math scores was significant and estimated to be 1.2 NCEs for both the total MPS control group and the low-income sample.[10]

The results for 1994 again shifted for both groups. For all groups, reading scores effectively did not change. The same was true of math scores for the MPS groups, although the low-income MPS scores decline[d] slightly more than the non-low-income group. For the choice students, after a large math increase in 1993, there was a decline of 2 NCE points in 1994.

In 1995, reading scores for choice students essentially remained the same, but again there was a 1.5 drop in NCE math scores.

Regression Analysis. Because test score differences could be based on a number of factors, and the factors could be distributed differen-

tially between choice and MPS students, it is necessary to control statistically for factors other than if students were in MPS or choice schools. Those controls are provided by multivariate regression analysis of the combined MPS and choice samples. There are a number of ways to model achievement gains. The most straightforward is to estimate the second-year test score, controlling for prior achievement and background characteristics, and then include an indicator (dichotomous) variable to measure the effect of being in a choice or MPS school. A series of dichotomous variables indicating the number of years in the choice program can be substituted for the single choice indicator variable.[11]

In previous year reports regressions were provided for each yearly change. The conclusion of those analyses, which were comprehensively described in the *Fourth Year Report* (Tables 12–16), was that there was no consistent difference between choice and MPS students, controlling for a wide range of variables.

For annual reporting purposes, and to test for consistency of results between years, previous reports included yearly regressions. For those analyses, small sample sizes precluded including survey variables. In this report, we "stack" four years of change scores and thus we have included survey variables in the regressions reported in Tables 12a and 12b. Critics of earlier reports surmised that exclusion of these variables biased the results against choice. Unfortunately, as Witte predicted in response to critics, *the results of including these variables do not favor the choice schools.*[12]

The *B* columns in Tables 11 and 12 contain the coefficients that determine the effects of the variables on predicting the dependent variable (the relevant test). In both tables, a critical coefficient will be the "Choice" (dichotomous) indicator variable. Dichotomous variables have a value of 1 if the student has the characteristic, and 0 if they do not. The effects of the coefficient can be read as a straight difference in the NCE score on the respective test being estimated. Thus, for example, in Table 11a, being a low-income student has an estimated negative effect of 2.510 NCE points on the reading score of students taking two tests a year apart from 1990 to 1994. Similarly, the effect of being in a private school in the Choice Program (as compared to being in MPS) produced an estimated decrease in Reading of .568 Normal Curve Equivalent points. Because of the size of the coefficient relative to its standard error, the negative impact of being in a low-income family is considered statistically significant, but not the effect of being in a choice school. These predicted effects occurred after simultaneously controlling, or in essence keeping constant, all other variables in the model.

The variables in the models that are not dichotomous variables are also easy to interpret. They can be read as the effect that a *one point difference in the variable* has on the predicted test. Thus, for reading in Table 11a, if a student is one NCE point better on the prior reading test than another student, we would predict he or she would do .495 points better on post-test. For "Test Grade" for each additional grade students are in, we would estimate a lower reading score of .176 NCE points—lower because the sign of the coefficient is negative.

The probabilities indicated in the tables are a statistical measure of how likely the coefficients are to differ from 0. Traditionally, those coefficients that have a probability of .05 or less are considered "significant."

Table 11 differs from Table 12 in that the latter includes a full set of survey variables, with a more accurate measure of income than free-lunch, mother's education, marital status, and educational expectations for their children. There are also four scales of paren-

tal involvement, paralleling scales A2–A5 in Table 6. Also notice that the sample sizes decline from 4716 and 4653 (in Table 11) to 1385 and 1372 (in Table 12).

There are several consistent patterns across all the models. Prior tests were always good predictors of post-tests, and the coefficients were relatively stable no matter how the results were modeled. Income was also always significant, measured either as a low-income dichotomous variable (qualifying for free lunch) or as family income in thousands of dollars. Higher income was always associated with higher achievement gains. Gender was significantly related to greater achievement in three of the models, with girls scoring higher than boys. Test grade was only significant on math models (tables 11b and 12b). The higher the grade of the student, the lower they scored.

Racial effects varied, although African American students always did less well than white students. For Hispanics and other minority students, the coefficients relative to whites were usually negative, but only significantly so for reading as shown in Table 11a.

As we anticipated in an earlier response to our critics, most of the other survey variables included in Tables 12a and 12b were not statistically significant. Mother's education was significant for reading, and two parental involvement variables were barely significant for math. The parental involvement variables, however, went in the wrong direction: greater involvement predicted *lower* test scores. The reason these variables were generally insignificant, and probably the reason the sign of the parental involvement coefficients were negative, was that these were correlated with other variables in the equations. This correlation, and the small sample sizes, produce unstable results and large standard errors for the estimates of the Bs.

The effects of being in a choice school were somewhat more discouraging. With the large samples, excluding the survey variables, none of the choice variables were even close to significant. In model 1, including the simple indicator variable of being in a choice school or not (Tables 11a and 11b), the coefficients were very close to zero, with large standard errors. The same was generally true when, rather than simply indicating whether a student was in a choice school, we included a set of indicator variables for the years they had been in choice (thus the "Choice Year 2" variable is 1 for a second-year choice student and 0 for everyone else). The negative 1.2 on reading for two-year students was the only one of these variables that was close to significant (Table 11a, model 2).

The one exception to the result that there were no differences between public school students and choice students was for reading scores including the survey variables (Table 12a). In that model, choice students' reading scores were estimated to be 2.15 NCE points *lower* than MPS students—controlling for all the other variables. The estimate was significant at the .01 level of probability. When analyzed one year at a time in the Choice Program, the effect was mostly due to second-year students. This result may well be an artifact of the poor performance of choice students in the second year of the program (1991–92). In earlier reports it was estimated that the difference in reading in that year was -3.35 points (significant at the .001 level). It was also previously reported that after that year, many of the poorest students left the choice schools (see "*Second Year Milwaukee Parental Choice Report*," pp. 16–17, Tables 20 and 21).

The reason choice estimates were more negative in this model than in other models in Table 11a and 11b, was probably due to the inclusion of mother's education. The variable was positively related to higher achievement and was significant in the model. Choice mothers were more educated than MPS mothers, which

should have produced higher scores for choice students, but did not. Thus when we controlled for education of mothers, the estimated impact of choice schools was more negative than when we did not. As noted above, this result was produced with a much smaller sample size, and with high standard errors on a number of variables. Thus it should be interpreted with caution. What clearly cannot be concluded, however, is that the inclusion of survey variables improves the results for choice students relative to public school students. At best they did as well as MPS students, and they may have done worse.[13]

Attendance

Attendance is not a very discriminating measure of educational performance at this level because there is little school-to-school variation. For example, in the last three years, average attendance in MPS elementary schools has been 92% in each year. Middle school attendance for the same years averaged 89, 88 and 89%. Attendance of choice students in the private schools (excluding alternative schools) averaged 94% in 1990–91 and 92% in 1991–92, which puts them slightly above MPS, but the differences were obviously slight. In 1992–93, excluding SEA Jobs and Learning Enterprises, attendance at the other schools was 92.5%. For 1993–94, attendance in the nonalternative schools (thus excluding Exito and Learning Enterprises) was 93%. For 1994–95 attendance dropped in the choice schools to 92%. It can be concluded that overall attendance was satisfactory and on average not a problem in choice schools.

Parental Involvement

Parental involvement was stressed in most of the choice schools and, in fact, was required in the contracts signed by parents in several of the schools. Involvement took several forms: (1) organized activities that range from working on committees and boards to helping with teas and fund raising events; (2) involvement in educational activities such as chaperoning field trips, and helping out in the classroom or with special events.

Table 6 provides five-year data on parental involvement scales for choice parents both in their prior public schools and in the private schools their students attended under the Choice Program. Table 6 provides the means and standard deviations of the scales for the applicants from 1990 to 1994 and the comparable scale scores for choice parents from 1991 to 1995 June surveys.

Of the four types of parental involvement we measured, already very high levels of involvement significantly increased in the private choice schools in three areas (Table 6, Scales A2, A3, and A4). These include parents contacting schools, schools contacting parents, and parental involvement in organized school activities. The increases were significant at the .001 level. The one exception was parent involvement in educational activities at home (Scale A5). For that scale, involvement increased but the increase was not statistically significant. These results have been confirmed independently in each of the five years of the program.

Parental Attitudes

Parental Satisfaction with the Choice Schools. In all five years, parental satisfaction with choice schools increased significantly over satisfaction with prior public schools. School satisfaction is indicated by scale A6 in Table 6. As described above, satisfaction with their prior schools was significantly lower than satisfaction of the average or low-income MPS parent. Reported satisfaction with the choice schools is indicated in these tables as "Choice Private School 1991–95" and was measured on the spring surveys in each year.

Another indication of parent satisfaction was the grade parents gave for their children's school. On the follow-up survey in June of each year, we asked parents to grade the private school their children attended. The grades, which indicate substantial differences with the grades they gave their prior MPS schools, are given in Table 7. For the five-year period, the average prior grade (on a scale where an A is 4.0) improved from 2.4 for prior MPS schools to 3.0 for current private schools. The overall grades were relatively consistent for each year and were always above the grades given to prior MPS schools.

Attitudes Toward the Choice Program. Follow-up surveys in June of each year asked parents of choice students if they wanted the Choice Program to continue. Respondents almost unanimously agreed the program should continue. Over the five years, 98% of the respondents felt the program should continue.

Attrition From the Program

Attrition Rates. One of the issues concerning the Choice Program is the rate of attrition from the program. Attrition rates, calculated with and without alternative high school programs are presented in the last two rows of Table 1. Attrition from the program was not inconsequential, although there appears to be a downward trend that leveled off in the last year. Overall attrition, defined as the percentage of students who did not graduate and who could have returned to the choice schools, was 46%, 35%, 31%, 27%, and 28% over the five years. Excluding students in alternative programs, the rates were 44%, 32%, 28%, 23%, and 24%. Whether these attrition rates are high or not has been discussed at length in prior reports. A Legislative Audit Bureau report on the program last year concluded it was a problem. We interpreted it as a problem for both public and private schools in that the public school student mobility rate was similar in magnitude to the attrition rate from choice schools.

Why students did not return to the choice schools? Because analysis of the causes of attrition was not part of the original study design, by the time we realized how many students were not returning after the first year, we were unable to follow up with non-returning families. That was rectified in the following years by using very brief mail surveys or telephone interviews. The surveys and interviews simply asked where the students were currently enrolled in school (if they were), and (open ended) why they left the choice school. The response rate to our inquiries has been 39%. This rate of return was slightly lower than for the other surveys. The normal problems of mailed surveys were compounded by the fact that we did not know who would return until enrollment actually occurred in September. Thus in many cases addresses and phones numbers were not accurate. The largest bias in our responses was undoubtedly families who moved out of the Milwaukee area and did not leave forwarding addresses. Telephone searches were impossible for that group. Our results should thus be treated with some caution.

The reasons parents gave for leaving are presented in Table 13. Approximately 18% o the responses indicated child or family specific reasons—including moving. We suspect that this category is underestimated since we undoubtedly were not able to locate as high a proportion of families who moved out of the area.

Almost all of the remaining respondents were critical of some aspect of the Choice Program or the private schools. The leading problems with the program were transportation problems, difficulties in reapplying to the program, problems with extra fees charged by some schools, and the lack of religious train-

ing, which was not allowed by the statute. Within-school problems most often cited were unhappiness with the staff, usually teachers, dissatisfaction with the general quality of education, and perceptions that discipline was too strict. The lack of special programs, which might have been available elsewhere, was also cited in 6% of the responses.

Outcome Summary

Outcomes after five years of the Choice Program remain mixed. Achievement change scores have varied considerably in the first five years of the program. Choice students' reading scores increased the first year; fell substantially in the second year, and have remained approximately the same in the next three years. Because the sample size was very small in the first year, the gain in reading was not statistically significant, but the decline in the second year was. In math, choice students were essentially the same in the first two years, recorded a significant increase in the third year, and then significantly declined this last year.

MPS students as a whole gained in reading in the first two years, with a relatively small gain in the first year being statistically significant. There were small and not significant declines in the last two years. Low-income MPS students followed approximately the same pattern, with none of the changes approaching significance. Math scores for MPS students were extremely varied. In the first year there were significant gains for both the total MPS group and the low-income subgroup. In the second year, the scores were essentially flat, but in the third year they declined significantly. Again, in the fourth year there was essentially no change in either the total MPS or low-income MPS groups.

Regression results, using a wide range of modeling approaches, including yearly models and a combined four-year model, generally indicated that choice and public school students were not much different. If there was a difference, MPS students did somewhat better in reading.

Parental attitudes toward choice schools, opinions of the Choice Program, and parental involvement were very positive over the first five years. Attitudes toward choice schools and the education of their children were much more positive than their evaluations of their prior public schools. This shift occurred in every category (teachers, principals, instruction, discipline, etc.) for each of the five years. Similarly, parental involvement, which was more frequent than for the average MPS parent in prior schools, was even greater for most activities in the choice schools. In all years, parents expressed approval of the program and overwhelmingly believed the program should continue.

Attrition (not counting students in alternative choice schools) has been 44%, 32%, 28%, 23%, and 24% in the five years of the program. Estimates of attrition in MPS are uncertain, but in the last two years, attrition from the Choice Program was comparable to the range of mobility between schools in MPS. The reasons given for leaving included complaints about the Choice Program, especially application and fee problems, transportation difficulties and the limitation on religious instruction. They also included complaints about staff, general educational quality and the lack of specialized programs in the private schools.

V. A Brief Response to Some of Our Critics

Several choice supporters criticized our previous reports in terms of outcomes they interpreted as negative for choice. Positive results were usually lauded and accepted as gospel.[14] They focused on two sets of results: high attrition from the program, and test

score results. Our attrition numbers are straightforward and accurate. We counted students who could have returned to choice schools, not subject to random selection, but chose not to return. These statistics were highlighted in an independent report by the Wisconsin Legislative Audit Bureau. Many students did not stay in these schools. It is that simple.

In terms of test scores, three points require a response. The first is the belief that inclusion of the survey variables would aid the results for choice students, and thus omitting them from the analysis biased the results against choice. In general, the excluded variables were correlated with variables already in the earlier equations and thus did not have significant effects. For the most part that is what happened in the regressions depicted in Tables 12a and 12b.

If the survey variables would have an effect, however, the *a priori* prediction would be that inclusion would favor the public schools, not the reverse. The reason is that on most of the omitted variables choice families were higher, and we would expect the variables to be positively correlated with greater achievement. For example, we would predict that the greater a mother's education, the more her involvement in her child's education, and the higher her educational expectation for her child, the higher her child's achievement.[15] *Because those variables were higher for choice families, holding those factors constant should lower, not increase, the relative scores for choice families. And that is precisely what happened on reading scores (Table 12a).*

Second, several commentators and newspaper reports have picked up on claims that because choice students started with low test scores, they should be expected to do worse than other students. This is undoubtedly true in absolute terms, but not likely in terms of *change scores.* That is why, in both regressions and descriptive statistics, we emphasized the changes in test scores, not the cohort scores. In fact, because of regression to the mean effects, those scoring near the bottom of a distribution on a prior test are likely to improve more than those scoring initially higher.[16] That means that in terms of change scores, choice students would be advantaged, not disadvantaged by initially lower cores.

Finally, and the most absurd of all, is the charge that we did not control for poor English speakers, and that biased the results against choice because of the disproportionate number of Hispanics in the program. Unfortunately, there is no measure of this variable available in any of the data sets. Hispanic origin (which has always been included in the analyses) probably is a good proxy, but certainly not a perfect one.

However, again, when considering *changes scores*, the exact opposite result is likely. Regression to the mean will also occur for these students and thus favor students with poor English on a second test. These students, however, will also be getting a year of instruction in English between the tests. Think of a child coming into a lower grade knowing little English. The student will obviously not do well on either the initial reading or math tests (because math tests also require reading). The student is then immersed in English for the first time. Relative to English speakers, what will the *change score* look alike after one year?

The analysis of the critics is so blatantly erroneous that it seems as though either they have little statistical or research training, or they simply do not care and are motivated by other factors, or both.

VI. Conclusions and Recommendations

We ended the *Fourth Year Report* by summarizing the positive and negative consequences of the program. We then wrote:

TABLE 1

Participation and Attrition from the Choice Program, 1990–1995

	1990–91	*1991–92*	*1992–93*	*1993–94*	*1994–95*
Number of students allowed in the Choice Program (limited to 1% of MPS enrollment)/ 1.5% 1994–95	931	946	950	968	1450
Number of private non-sectarian schools in Milwaukee	22	22	23	23	23
Number of schools participating	7	6	11	12	12
Number of applicants	577	689	998	1049	1046
Number of available seats	406	546	691	811	982
Number of students participating					
September count	341	521	620	742	830
January count	259	526	586	701	—
June count	249	477	570	671	—
Graduating students	8	28	32	42	45
Number of returning Choice students	NA	178	323	405	491
Attrition rate[1]	.46	.35	.31	.27	.28
Attrition rate without alternative schools	.44[2]	.32	.28	.23	.24

1. The attrition rate for year *t* is defined as: 1.0 – [the number of returning students in year *t* + 1/ (the September count in year *t* - graduating students in year *t*)]

2. If Juanita Virgil Academy is excluded, the attrition rate is .29.

> Honorable people can disagree on the importance of each of these factors. One way to think about it is whether the majority of the students and families involved are better off because of this program. The answer of the parents involved, at least those who respond to our surveys, was clearly yes. This is despite the fact that achievement, as measured by standardized tests, was no different than the achievement of MPS students. Obviously the attrition rate and the factors affecting attrition indicate that not all students will succeed in these schools, but the majority remain and applaud the program.

Although achievement test results may be somewhat more bleak for choice in this analysis, the differences are not very large in terms of their impact, and the negative estimates are based on less stable models and smaller sample size. In addition test scores are only one indication of educational achievement. Thus we see no reason to change last year's conclusion.

Our recommendations are no different from those offered in previous years. For those interested, they are stated in their entirety in the *Fourth Year Report.*

TABLE 2

How Choice Applicants Learn about the Program, 1990–1994
(Percent indicating source used)

	1990–94	*1994*
Friends or relatives	50.9	58.3
Television or radio	21.4	8.6
Newspapers	24.2	5
Private schools	17.7	20.1
Churches	3.7	2.9
Community centers	6.7	10.1
(N)	(1,060)	(139)

Question: "How did you learn about the Private School Choice Program?"

TABLE 3

Satisfaction with Information and Assistance on the Choice Program, Applicants 1990–94 (Percent satisfied or very satisfied)

	1990–94	*1994*
Amount of information on the Choice Program	76.4	79.4
Accuracy of information on the Choice Program	75.3	80.8
Amount of information on the Private Schools	66.3	65.9
Accuracy of information on the Private Schools	70.9	76.1
Assistance from school you applied to	76.8	75.2
Assistance from Dept. of Public Instruction in Madison	64.8	78.2
(N)	(1,060)	(139)

Question: "How satisfied were you with the following?"

TABLE 4

Factors Affecting Decisions to Participate in Choice Program, Applicants 1990–94 (Percentages)

	1990–1994				*1994*			
	Very Import.	*Import.*	*Some Import.*	*Not Import.*	*Very Import.*	*Import.*	*Some Import.*	*Not Import.*
Educational Quality in Chosen School	88.6	10.5	0.7	0.30	90.4	9.6	0.00	0.00
Teaching Approach or Style[3]	85.7	13.2	0.7	0.4	86.8	12.5	0.7	0
Discipline in Chosen School	74.6	22.1	2.9	0.40	68.4	26.5	4.4	0.7
General Atmosphere in Chosen School	74.3	22.5	2.7	0.50	73.4	21.9	4.4	0.00
Class Size[3]	72	22.4	4.9	0.7	66.7	25.2	6.7	1.5
Financial Considerations	69.8	23.3	4.8	2.1	74.6	18.1	3.6	3.6
Special Programs in Chosen School	68.4	25.7	3.6	2.3	68.1	23.7	6.7	1.5
Location of Chosen School	60.5	22.2	11.7	5.6	64.2	24.1	7.3	4.4
Frustration with Public Schools	59.5	23.9	10.7	5.9	61.3	20.4	14.6	3.6
Other Children in Chosen School	36.6	29.2	14.8	19.4	36.1	30.1	3.5	20.3
(N)	(964)				(139)			

Question: "Please rate all of the following issues and their importance in your decision to participate in the Choice program."

3. This question was not asked in the first two years of the study. The N for these responses is 581 in the 1990–94 responses.

TABLE 5 DEMOGRAPHICS

TABLE 5A

Household Income (Percentages)

Thousands	***1*** *Choice Applied 1990–94*	***2*** *New Choice 1994*	***3*** *Choice Enrolled 1990–94*	***4*** *Choice Non-Select 1990–94*	***5*** *Attrition 1990–93*	***6*** *Low-Inc MPS 1991*	***7*** *MPS Control 1991*
$0–$5	17.7	12.8	16	19.4	18	19	13
$5–$10	37.2	33	39.6	33.2	40	34	23
$10–$20	28.7	24.1	29.6	28.9	27	29	21
$20–$35	15.8	29.3	14.7	16.9	15	14	24
$35–$50	.6	0	0.20	1.2	1	3	13
$50 & over	0.10	0.80	0.00	0.30	0	0	8
(N)	(1,020)	(133)	(627)	(325)	(293)	(880)	(1,513)
Mean Income	11.63	14.21	11.34	12.24	11.77	12.13	22.00

TABLE 5B

Race[4] (Percentages)

	1 *Choice Applied 1990–94*	***2*** *New Choice 1994*	***3*** *Choice Enrolled 1990–94*	***4*** *Choice Non-Select 1990–94*	***5*** *Attrition 1990–93*	***6*** *Low-Inc MPS 1991*	***7*** *MPS Control 1991*
African American	74.5	71.9	73.2	78.0	75	67	55
Asian	0.40	0	0.20	0.20	0	5	4
Hispanic	18.7	19.4	21.3	14.2	16	11	10
Native American	0.4	0.00	0.1	0.7	1	1	1
White	4.9	7.9	4.9	4.3	7	15	29
Other	1	0.70	0.3	2.6	1	1	1
(N)	(2,673)	(139)	(1,490)	(886)	(676)	(3,179)	(5,365)

[4] Based on MPS Student Record Data Base (SRDB). In cases where Choice Students could not be found in the SRDB, race data were taken from the school enrollment sheets.

TABLE 5C

Parent Marital Status (Percentages)

	1 *Choice Applied 1990–94*	***2*** *New Choice 1994*	***3*** *Choice Enrolled 1990–94*	***4*** *Choice Non-Select 1990–94*	***5*** *Attrition 1990–93*	***6*** *Low-Inc MPS 1991*	***7*** *MPS Control 1991*
Married	24.9	23	23.5	29.6	24	35	51
Single	38.9	42.2	39.8	35	42	32	22
Separated	12.4	17.8	13	11.5	9	11	8
Divorced	15.1	9.6	15.6	13.9	15	14	13
Widowed	3.2	.7	3.5	2.7	5	2	2
Living Together	5.5	6.7	4.6	7.3	5	6	4
(N)	(1,032)	(135)	(633)	(331)	(294)	(924)	(1,637)

TABLE 5D

Family Size (Percentages)

Children per Family	***1*** *Choice Applied 1990–94*	***2*** *New Choice 1994*	***3*** *Choice Enrolled 1990–94*	***4*** *Choice Non-Select 1990–94*	***5*** *Attrition 1990–93*	***6*** *Low-Inc MPS 1991*	***7*** *MPS Control 1991*
1	27.8	31.7	25.9	29.7	19	9	13
2	30.2	30.9	31.9	28	31	26	33
3	19.2	18.7	22.3	14	24	27	26
4	12.5	8.6	11.5	13.4	16	18	151
5 or more	10.3	10	8.3	14.9	10	20	1,415
(N)	(1,060)	(139)	(645)	(343)	(287)	(908)	(1,611)
Avg. number of children per family	2.54	2.4	2.5	2.66	2.72	3.24	2.95

TABLE 5E

Parents' Education (Percentages)

	8th Grade	***Some HS***	***GED***	***H.S.***	***Some College***	***College Grad***	***Some Post Grad***	***(N)***
Choice Applied 1990–94								
Mother/Female Guardian	4.3	11.6	9.2	20.3	46.4	6.5	1.8	(1,027)
Father/Male Guardian	10.4	20.4	6.9	25.8	28.8	5	2.7	(624)
New Choice 1994								
Mother/Female Guardian	.8	9.8	9.8	15.9	53	6.8	3.8	(132)
Father/Male Guardian	6.2	16	6.2	23.5	33.3	4.9	9.9	(81)
Choice Enrolled 1990–94								
Mother/Female Guardian	3	12	9.3	21.2	45.5	7.5	1.4	(626)
Father/Male Guardian	7.6	19.3	7	28.2	28.5	6.3	3.1	(383)
Choice Non-Select 1990–94								
Mother/Female Guardian	6.9	10.8	9	19.2	46.2	5.7	2.1	(333)
Father/Male Guardian	15.6	22.1	6.5	20.6	30.2	3.5	1.5	(199)
Attrition 1990–93								
Mother/Female Guardian	4	9	11	19	48	8	1	(292)
Father/Male Guardian	6	18	8	32	30	4	2	(175)
Low-Inc MPS 1991								
Mother/Female Guardian	12	25	9	25	26	3	1	(881)
Father/Male Guardian	15	22	9	25	21	6	2	(535)
MPS Control 1991								
Mother/Female Guardian	8	18	7	28	29	6	5	(1,525)
Father/Male Guardian	9	16	8	26	27	9	6	(1,127)

TABLE 6

Scale Data — Means, Standard Deviations, Reliability, and Sample Size

Source of Scale, Range, and Direction by Group	*Mean*	*Standard Deviation*	*Alpha*	*(N)*
Dissatisfaction with the administration of the choice application process				
T3 – DisChAdm – Range = 6–24 (High = More Dissatisfied)				
Choice Applied 1990–94 (Fall)	12.6	4.4	.91	609
New Choice 1994 (Fall)	12.0	3.8	.88	86
Choice Enrolled 1990–94 (Fall)	11.6	3.6	.88	364
Non-selected Choice 1990–94 (Fall)	14.4	5.0	.92	206
Importance of education compared to other goals				
A1 – EdImport – Range = 7–15 (High = More Important)				
Choice Applied 1990–94 (Fall)	11.6	1.9	.72	996
New Choice 1994 (Fall)	11.4	1.8	.72	129
Choice Enrolled 1990–94 (Fall)	11.7	1.8	.70	627
Non-Selected Choice 1990–94 (Fall)	11.4	1.9	.73	301
Choice Private School 1991–95 (Spring)	11.5	1.9	.77	732
Non-Selected Choice 1991–95 (Spring)	11.3	1.9	.76	210
Low-Income MPS 1991	11.8	2.0	.74	811
MPS Control 1991	11.7	2.0	.71	1,554
Frequency of parent contacting school				
A2 – PiParScl – Range = 0–21 (High = More)				
Choice Applied 1990–94 (Fall)	8.8	4.9	.79	775
New Choice 1994 (Fall)	9.3	5.1	.78	88
Choice Enrolled 1990–94 (Fall)	8.6	5.1	.81	466
Non-Selected Choice 1990–94 (Fall)	9.0	4.6	.74	269
Choice Private School 1991–95 (Spring)	9.4	4.8	.78	722
Non-Selected Choice 1991–95 (Spring)	8.0	5.0	.83	212
Low-Income MPS 1991	5.8	4.4	.79	807
MPS Control 1991	6.0	4.3	.78	1,529
Frequency of school contacting parent				
A3 – PiSclPar – Range = 0–12 (High = More)				
Choice Applied 1990–94 (Fall)	3.6	2.9	.67	811
New Choice 1994 (Fall)	4.1	2.9	.67	94
Choice Enrolled 1990–94 (Fall)	3.7	2.9	.67	495
Non-Selected Choice 1990–94 (Fall)	3.5	2.7	.63	276
Choice Private School 1991–95 (Spring)	4.4	2.9	.70	740
Non-Selected Choice 1991–95 (Spring)	3.8	3.0	.71	223
Low-Income MPS 1991	2.7	2.5	.67	834
MPS Control 1991	2.7	2.5	.65	1,594
Parental involvement in school organizations				
A4 – PiSclOrg – Range = 0–5 (High = More)				
Choice Applied 1990–94 (Fall)	2.4	1.5	.71	788
New Choice 1994 (Fall)	2.6	1.4	.71	90
Choice Enrolled 1990–94 (Fall)	2.4	1.5	.72	481
Non-Selected Choice 1990–94 (Fall)	2.4	1.4	.69	268
Choice Private School 1991–95 (Spring)	3.0	1.3	.54	731
Non-Selected Choice 1991–95 (Spring)	2.3	1.4	.66	218
Low-Income MPS 1991	1.7	1.3	.67	831
MPS Control 1991	1.9	1.4	.67	1,586

TABLE 6 *(CONTINUED)*

Source of Scale, Range, and Direction by Group	*Mean*	*Standard Deviation*	*Alpha*	*(N)*
Parental involvement in educational activities with child				
A5 – PiChild – Range = 0–15 (High = More)				
Choice Applied 1990–94 (Fall)	8.7	3.5	.76	987
New Choice 1994 (Fall)	8.7	3.5	.76	129
Choice Enrolled 1990–94 (Fall)	8.8	3.5	.76	619
Non-Selected Choice 1990–94 (Fall)	8.7	3.5	.78	302
Choice Private School 1991–95 (Spring)	8.9	3.8	.81	737
Non-Selected Choice 1991–95 (Spring)	8.8	3.9	.82	218
Low-Income MPS 1991	7.5	4.3	.85	833
MPS Control 1991	6.9	4.2	.83	1,575
Dissatisfaction with prior school				
A6 – DisPrScl – Range = 8–32 (High = More Dissatisfied)				
Choice Applied 1990–94 (Fall)	16.5	5.5	.89	646
New Choice 1994 (Fall)	16.0	5.2	.88	69
Choice Enrolled 1990–94 (Fall)	16.4	5.7	.89	406
Non-Selected Choice 1990–94 (Fall)	16.7	5.1	.88	209
Choice Private School 1991–95 (Spring)	13.6	4.9	.90	604
Non-Selected Choice 1991–95 (Spring)	15.4	5.6	.92	178
Low-Income MPS 1991	14.4	4.2	.85	636
MPS Control 1991	14.5	4.2	.85	1,224

TABLE 7

Grade Given to Choice School Experience 1991–95 (Percentages)

	1 Choice Private 1991	*2 Choice Private 1992*	*3 Choice Private 1993*	*4 Choice Private 1994*	*5 Choice Private 1995*
A	40.1	33.0	29.5	42.0	39.7
B	37.4	39.4	35.9	32.7	37.5
C	14.3	22.0	21.1	17.5	18.0
D	2.7	3.2	9.7	6.3	4.1
F	4.5	2.3	3.8	1.5	0.7
GPA	3.04	2.89	2.78	3.07	3.11
(N)	(147)	(218)	(237)	(269)	(461)

TABLE 8
Prior Test Scores

	Applied Choice		*Low-Income MPS*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1990						
% At or Above 50% of NPR	23.3	31.1	27.2	.36.2	34.8	42.8
Median NPR	29	31	32	37	37	42
Mean NCE	39.1	39.7	40.1	42	43.6	45.8
Std. Dev. Of NCE	15.9	18.9	17	19.2	18.5	20.2
(N)	(262)	(257)	(2,136)	(117)	(3,157)	(3,130)
1991						
% At or Above 50% of NPR	27.1	22.6	28.2	36.2	36.1	43.4
Median NPR	26	30	32	38	38	43
Mean NCE	37.5	37.9	40.2	42.9	43.7	46.3
Std. Dev. Of NCE	16.8	17.7	17	19	18.6	20.2
(N)	(199)	(204)	(2,470)	(2,447)	(3,668)	(3,643)
1992						
% At or Above 50% of NPR	28.2	31.4	28.2	35.3	36.6	43.2
Median NPR	29	33	32	38	38	43
Mean NCE	40.0	40.3	40.2	42.4	43.9	46.0
Std. Dev. Of NCE	17.6	19.5	17.7	19.5	19.0	20.7
(N)	(234)	(226)	(2,839)	(2,801)	(4,024)	(3,991)
1993						
% At or Above 50% of NPR	25.7	26.7	28.2	34.7	37.5	42.1
Median NPR	29	28	32	37	37	37.5
Mean NCE	38.0	40.0	40.2	42.0	43.3	45.2
Std. Dev. Of NCE	19.1	18.6	17.7	19.3	19.0	20.6
(N)	(179)	(175)	(3,069)	(3,049)	(3,980)	(3,962)
1994						
% At or Above 50% of NPR	27.4	23.3	25.9	34.4	36.3	43.8
Median NPR	26.0	25.5	32.0	36.0	40.0	44.0
Mean NCE	37.3	35.8	38.7	41.4	42.9	46.0
Std. Dev. Of NCE	17.4	19.0	17.5	20.0	19.0	20.9
(N)	(146)	(86)	(1,940)	(1,208)	(4,127)	(3,204)

R = Reading Scores
M = Math Scores
NPR = National Percentile Ranking
NCE = Normal Curve Equivalent

TABLE 9
Achievement Test Scores, 1991–95

	Enrolled Choice		*MPS Low-Income*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1991						
% At or Above 50% of NPR	29.3	29.2	24.9	33.4	32.2	40.0
Median NPR	34	32	31	35	35	40
Mean NCE	42.6	40.2	39.1	42.0	42.2	45.2
Std. Dev. Of NCE	16.4	19.9	16.1	18.2	17.9	20.0
(N)	(177)	(185)	(1,433)	(1,419)	(1,697)	(1,957)
1992						
% At or Above 50% of NPR	21.2	22.8	25.2	33.5	32.4	39.5
Median NPR	27	28	29	35	34	40
Mean NCE	37.3	36.7	39.3	41.8	42.3	44.6
Std. Dev. Of NCE	16.2	18.0	17.2	19.1	18.8	20.5
(N)	(349)	(369)	(1,397)	(1,338)	(1,919)	(1,896)
1993						
% At or Above 50% of NPR	16.7	28.3	24.9	29.5	29.9	35.0
Median NPR	26	32	30	32	32	36
Mean NCE	37.2	42.2	38.8	39.9	40.9	42.9
Std. Dev. Of NCE	15.6	17.6	16.9	18.9	18.0	19.6
(N)	(398)	(395)	(1,212)	(1,189)	(1,443)	(1,370)
1994						
% At or Above 50% of NPR	28.8	31.3	25.7	42.4	30.5	48.4
Median NPR	30	38	30	39	32	47
Mean NCE	38.2	42.2	38.4	42.0	40.5	44.0
Std. Dev. Of NCE	15.4	18.8	17.0	19.1	18.1	20.2
(N)	(440)	(471)	(1,019)	(996)	(1,168)	(1,141)
1995[6]						
% At or Above 50% of NPR	14.6	26.1				
Median NPR	28.0	36.0				
Mean NCE	38.8	42.8				
Std. Dev. Of NCE	16.3	19.3				
(N)	(421)	(462)				

R = Reading Scores
M = Math Scores
NPR = National Percentile Ranking
NCE Normal Curve Equivalent

6. Only one MPS grade took the ITBS in 1995. Since it was a new version of the ITBS, comparable data do not exist for 1995.

TABLE 10A

Achievement Test Change Scores (NCEs), 1990 to 1991[7]

	Enrolled Choice		*MPS Low-Income*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1990 Mean NCE	40.0	39.2	37.5	39.5	39.5	41.6
Std. Dev.	13.9	19.6	15.2	17.8	16.6	18.7
1991 Mean NCE	41.8	39.1	38.2	42.2	40.5	44.2
Std. Dev.	14.4	15.3	15.3	17.7	16.6	18.6
Mean Difference	+1.8	-0.1	+0.7	+2.7	+1.0	+2.6
Std. Dev.	13.1	16.0	14.7	14.7	14.3	14.4
Probability Mean Difference = 0	.193	.935	.144	.000	.022	.000
(N)	(84)	(88)	(812)	(792)	(1,048)	(1,029)

7. Only students who were tested in the Spring of 1990 and the Spring of 1991 are included. For Choice students only those who completed a full year in the Choice schools are included. Sample sizes are small relative to the total students in the Choice Program.

TABLE 10B

Achievement Test Change Scores (NCEs), 1991 to 1992

	Enrolled Choice		*MPS Low-Income*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1991 Mean NCE	39.8	39.0	38.0	41.5	40.0	43.4
Std. Dev.	17.0	18.6	15.1	17.3	16.6	18.4
1992 Mean NCE	35.9	38.4	38.4	41.3	40.5	43.1
Std. Dev.	14.4	15.3	15.3	17.7	16.6	18.6
Mean Difference	-3.9	-0.6	+0.4	-0.3	+0.5	-0.3
Std. Dev.	16.0	16.3	14.6	15.3	14.2	14.6
Probability Mean Difference = 0	.001	.586	.484	.605	.286	.574
(N)	(192)	(198)	(911)	(895)	(1,173)	(1,148)

TABLE 10C

Achievement Test Change Scores (NCEs), 1992 to 1993

	Enrolled Choice		*MPS Low-Income*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1992 Mean NCE	38.7	38.3	39.5	42.2	40.8	43.2
Std. Dev.	16.2	19.0	17.3	19.0	17.9	19.4
1993 Mean NCE	38.3	42.7	38.8	41.0	40.1	42.0
Std. Dev.	14.2	17.2	16.7	18.1	17.4	18.7
Mean Difference	-0.4	+4.4	-0.7	-1.2	-0.8	-1.2
Std. Dev.	14.6	16.8	14.0	15.5	14.0	15.1
Probability Mean Difference = 0	.928	.000	.131	.002	.091	.019
(N)	(282)	(288)	(873)	(842)	(973)	(938)

TABLE 10D

Achievement Test Change Scores (NCEs), 1993 to 1994

	Enrolled Choice		*MPS Low-Income*		*MPS Control*	
	R	*M*	*R*	*M*	*R*	*M*
1993 Mean NCE	37.6	44.0	28.8	41.8	40.1	42.
Std. Dev.	15.7	17.1	16.7	18.58	17.6	19.4
1994 Mean NCE	37.5	42.0	35.6	41.5	39.9	42.7
Std. Dev.	15.8	17.8	16.9	19.1	18.6	19.8
Mean Difference	-0.1	-2.0	-0.2	-0.3	-0.2	-0.1
Std. Dev.	14.7	14.7	14.0	16.5	14.0	16.3
Probable Mean Difference = 0	.928	.002	.564	.631	.620	.904
(N)	(289)	(281)	(688)	(678)	(766)	(755)

TABLE 11A

Estimated Reading Iowa Test of Basic Skills, 1990–94, with Choice Indicator and Yearly Choice Variables

	Model 1 *Choice Indicator*		***Model 2*** *Choice Years*	
Variable	*B*	*SE B*	*B*	*SE B*
Constant	18.131***	.971	18.123***	.971
Prior Reading	.495***	.014	.495***	.014
Prior Math	.170***	.012	.170***	.013
Test Grade	-.176	.091	-.175	.091
Gender (1 = female)	1.559***	.366	1.562***	.366
African American	-3.911***	.546	-3.915***	.546
Hispanic	-1.599*	.725	-1.617*	.726
Other Minority	-2.435*	1.047	-2.439*	1.047
Low Income	-2.510***	.577	-2.506***	.577
Choice Indicator	-.568	.484	—	—
Choice Year 1	—	—	.100	.818
Choice Year 2	—	—	-1.200	.734
Choice Year 3	—	—	-.093	.875
Choice Year 4	—	—	-1.394	1.411
Adj. R^2	.46		.46	
F Statistic	446.32		441.23	
Probability F = 0	.000		.000	
Dependent Mean	40.09		40.09	
Dependent St. Dev.	16.98		16.98	
(N)	(4,716)		(4,716)	

*** probability that B=0 < .001

** probability that B=0 < .01

* probability that B=0 < .05

TABLE 10E

Achievement Test Change Scores (NCEs), 1994 to 1995[8]

	Enrolled Choice	
	R	*M*
1994 Mean NCE	38.4	43.1
Std. Dev.	15.1	18.8
1995 Mean NCE	38.6	41.7
Std. Dev.	16.0	17.6
Mean Difference	0.2	-1.5
Std. Dev.	14.8	15.8
Probable Mean Difference = 0	.783	.102
(N)	(285)	(306)

8. See footnote 6.

TABLE 11B

Estimated Math Iowa Test of Basic Skills, 1990–94, with Choice Indicator and Yearly Choice Variables

	Model 1 *Choice Indicator*		***Model 2*** *Choice Years*	
Variable	*B*	*SE B*	*B*	*SE B*
Constant	22.260***	1.060	22.249***	1.061
Prior Math	.546***	.014	.546***	.014
Prior Reading	.170***	.015	.170***	.015
Test Grade	-1.089***	.100	-1.086***	.100
Gender (1 = female)	-.065	.400	-.065	.400
African American	-4.182***	.599	-4.182***	.599
Hispanic	-1.546	.795	-1.568*	.796
Other Minority	.288	1.151	.284	1.152
Low Income	-1.431	.633	-1.429*	.633
Choice Indicator	-.109	.527	—	—
Choice Year 1	—	—	-.682	.894
Choice Year 2	—	—	.059	.801
Choice Year 3	—	—	.564	.946
Choice Year 4	—	—	-.823	1.541
Adj. R^2	.46		.49	
F Statistic	498.60		487.23	
Probability F = 0	.000		.000	
Dependent Mean	42.48		42.48	
Dependent St. Dev.	18.83		18.83	
(N)	(4,653)		(4,653)	

*** probability that B=0 < .001
** probability that B=0 < .01
* probability that B=0 < .05

TABLE 12A

Estimated Reading Iowa Test of Basic Skills, 1990–94, With Choice Indicator, Yearly Choice, and Survey Variables

	Model 1 *Choice Indicator*		***Model 2*** *Choice Years*	
Variable	*B*	*SE B*	*B*	*SE B*
Constant	14.947***	2.168	14.968***	2.168
Prior Reading	.433***	.025	.436***	.025
Prior Math	.177***	.021	.174***	.021
Test Grade	-.020	.167	-.036	.168
Gender (1 = female)	2.508***	.657	2.517***	.657
African American	-3.737***	.930	-3.762***	.929
Hispanic	-1.144	1.173	-1.029	1.174
Other Minority	-3.216	2.343	-3.194	2.340
Income ($1,000)	.092**	.032	.092**	.032
Mother's Education	.491**	.260	.499*	.260
Married (1-=yes)	-.271	.792	-.304	.792
PI-Schl. Cont.	-.115	.090	-.112	.090
PI-Par. Cont.	.081	.154	.080	.154
PI-Schl. Organ.	.477	.267	.469	.267
PI-Child	-.093	.097	-.094	.097
Edu. Expectations	.167	.349	.180	.349
Choice Indicator	-2.154**	.749	—	—
Choice Year 1	—	—	-1.382	1.092
Choice Year 2	—	—	-3.849***	1.024
Choice Year 3	—	—	-1.256	1.225
Choice Year 4	—	—	-.410	1.852
Adj. R^2	.50		.50	
F Statistic	86.15		73.03	
Probability F = 0	.000		.000	
Dependent Mean	42.95		42.95	
Dependent St. Dev.	16.47		16.47	
(N)	(1,385)		(1,385)	

*** probability that B=0 < .001
** probability that B=0 < .01
* probability that B=0 < .05

TABLE 12B

Estimated Math Iowa Test of Basic Skills, 1990–94, with Choice Indicator, Yearly Choice, and Survey Variables

	Model 1 *Choice Indicator*		***Model 2*** *Choice Years*	
Variable	*B*	*SE B*	*B*	*SE B*
Constant	22.558***	2.501	22.574***	2.509
Prior Math	.535***	.024	.533***	.024
Prior Reading	.150***	.028	.151***	.029
Test Grade	-1.285***	.192	-1.296***	.194
Gender (1 = female)	1.486*	.755	1.488*	.756
African American	-5.112***	1.081	-5.13***	1.083
Hispanic	-2.449	1.357	-2.389	1.361
Other Minority	-3.384	2.691	-3.373	2.693
Income ($1,000)	-.112**	.037	.112**	.037
Mother's Education	.007	.301	.012	.302
Married (1 = yes)	-.520	.911	-.532	.913
PI-Schl. Cont.	-.217*	.103	-.215*	.103
PI-Par. Cont.	.304	.179	.303	.179
PI-Schl. Organ.	-.202	.309	-.204	.310
PI-Child	-.264*	.112	-.265*	.112
Edu. Expectations	.740	.403	.749	.404
Choice Indicator	.093	.864	—	—
Choice Year 1	—	—	.282	1.261
Choice Year 2	—	—	-.464	1.187
Choice Year 3	—	—	.190	1.410
Choice Year 4	—	—	1.297	2.131
Adj. R^2	.53		.53	
F Statistic	97.02		81.60	
Probability F = 0	.000		.000	
Dependent Mean	45.37		45.37	
Dependent St. Dev.	19.53		19.53	
(N)	(1,372)		(1,372)	

*** probability that B=0 < .001
** probability that B=0 < .01
* probability that B=0 < .05

TABLE 13

Why Choice Students Left the Choice Program, 1991–1994[9]

Responses	*(N)*	*%*
Quality of Program	68	30.5
Lack of religious training	13	5.8
Lack of transportation	17	7.6
Income	9	4.0
Application problems	14	6.3
Fee changes	13	5.8
Selection problems	2	0.9
Quality of the Choice School	96	43.0
Poor education	20	9.0
Too disciplinarian	13	5.8
Unhappy with staff	31	13.9
Lack of programs for special needs students	14	6.3
Lack of programs for talented students	2	0.9
Too segregated	1	0.4
Child terminated	3	1.3
Lack of teaching materials	7	3.1
Child/Family Specific	43	19.3
Transportation – too far away	12	5.4
Moved	15	6.7
Pregnancy	3	1.3
Quit school	4	1.8
Child custody change	2	0.9
Miscellaneous	9	4.0
Total	223	

Question: "What were the major reasons your child did not continue in last year's school?" (Respondents could give up to three answers)

[9] Response rate for years 1992 to 1994 was 38%.

TABLE 14

Parents Participation In Educational Activities, 1991, Including Parents of Students No Longer In Choice Schools (Percentages)

	Times/Week				
	0	*1–2*	*3–4*	*5 or more*	*(N)*
Help with child's homework	3	17	31	48	(137)
Read with or to your child	9	25	29	37	(137)
Work on arithmetic or math	8	26	27	39	(135)
Work on penmanship or writing	16	31	24	30	(135)
Watch educational program on T.V. with your child	15	46	24	15	(136)
Participate together in sports activities	23	43	15	18	(136)

Question: "How many times in a normal week did you participate in the following activities with your child?"

Notes

1. Daniel McGroarty, "School Choice Slandered," *The Public Interest* (No. 17), Fall, 1994; Paul E. Peterson, "A Critique of the Witte Evaluation of Milwaukee's School Choice Program," Center for American Political Studies, Harvard University, February 1995, unpublished occasional paper.
2. This change is extremely important because most students have been admitted to the Choice Program in those grades. Private schools in general prefer to limit lateral entry at higher grades and therefore most have a grade structure which has many more students in the lower grades than the upper grades.
3. Table 6 contains information on "scales" which are sets of questions measuring an underlying concept. The exact questions for the scales appear in the *Fourth Year Report* (Tables A1–A6). We created simple scale scores by adding together responses to each item. Table 6 contains statistics on the scales, defines the scale direction, and reports the Cronbach Alpha statistic for the scale. Cronbach Alpha is a measure of how well the items form a scale. It has a maximum value of 1.0 when all items are perfectly correlated with each other.
4. Test scores were not available for all students in either group because tests were not given every year in MPS. Therefore, there were no tests for 4- and 5-year old kindergarten, and few for first-grade students. Lateral entry into higher grades could also have missed some students because primary testing was in grades 2, 5, 7, and 10. For the few high school students in the Choice Program, the 10th grade test was excluded because very few of these students were tested and because students were entering alternative schools (schools for students contemplating dropping out of school or pregnant teenage students).
5. A number of the tests taken in MPS were dictated by rules for the federal Chapter I program which required testing in every grade in reading and math using a standardized achievement test. In 1993, the federal regulations changed from requiring total math, consisting of three subtests, to just "problem solving." With that change, MPS dropped Chapter I testing using all three subtests for some students. Fortunately, the correlation between the Problem Solving Component and the Total Math score is .88. We were able to use an estimated regression model with Problem Solving estimating Total Math for students taking just the Problem Solving portion. The details of this procedure were described in Technical Appendix F of the *Fourth Year Report.*
6. Because sample sizes were relatively small for choice students, the most reliable statistic in these tables is the mean Normal Curve Equivalent (NCE). Median and percent at or above 50% National Percentile Rankings are included because these statistics are routinely reported by MPS. Because a number of students may be bunched together with small samples, both of these numbers are volatile.
7. There is almost no dropping out at the elementary level. Drop-out rates are also extremely low in middle schools. In MPS suspensions are also rare in these grades and the policies and reporting vary considerably from school to school. For example, student fighting, which leads to a suspension for up to three days in most of the private schools, may result in a student being sent home in MPS. Whether that becomes an official suspension or not may depend on the principal and the reactions of the child or parents. The numbers of official expulsions are even smaller than dropouts or suspensions. See John F. Witte, "Metropolitan Milwaukee Dropout Report," Report of the Commission on the Quality and Equity of the Milwaukee Metropolitan Public Schools, 1985.
8. Please note that the cohort population described in the last section is not identical with students for whom we have change data from one year to the next. Thus tables 9 and 10 are not directly comparable.
9. Normal Curve Equivalents are used because National Percentile Rankings are not interval level data. One of the problems with the transformation from NPRs to NCEs is that the very lowest and highest ends of the distribution are compressed. This tends to inflate very low-end scores and deflate very high-end scores. The lower end inflation may affect this population, which has quite a few test scores below the 10th National Percentile. For later analysis, if sample sizes become large enough, we will also analyze scores by grade using the Iowa Test Standard Scores, which are interval level but do not have this compression effect. NCEs are, however, the national standard for reporting results across populations and grades for Chapter I and other programs.
10. All MPS results in this and last year's report use the estimated math score. In the third year we reported the estimated scores in footnotes and appendices. We changed because we feel the more accurate method is to include the estimated scores to prevent an income bias associated with Chapter I eligibility.
11. These regressions are the equivalent of a change score with prior tests weighted proportionate to their B values (rather than constrained to 1.0). This can be determined by simply rewriting the regression equations subtracting the prior tests from each side of the equation.

12. See John F. Witte, "A Reply to Paul Peterson," unpublished paper, February 1995.
13. A final set of regressions, correcting for selection into and out of the choice and MPS samples, is currently being done. Those corrections require quite robust statistical assumptions and will be interpreted with caution. However, preliminary results indicate that taking into account primarily students not taking tests in a given year (because they left the respective systems or were not tested if present), both MPS and choice scores are corrected upward, but MPS scores are corrected upward considerably more. The reason is that non-Chapter I, MPS students were not as likely to be tested each year and White, non-low-income students were more likely to leave the system.
14. The McGroarty article may be the most notable for this. After spending pages demonstrating how choice opponents use our reports to support their positions, and criticizing test scores and attrition along the lines indicated below, he ends with an unqualified litany of the positive results we "demonstrated" (positive parental attitudes, parental involvement, etc.). See McGroarty, 1994.
15. The one exception to this is the income variable. Choice families have lower income than MPS families and we assume income is related to higher achievement. That variable, however, was already in the earlier equations as an indicator variable defined as qualifying for free lunch. the survey variable may add accuracy, but essentially that control was already in place.
16. Regression to the mean, taught in first semester statistics courses, occurs because of measurement error in tests. The easiest way to think about this is to consider that a person's true score varies randomly around the score the person actually records. For the person near the bottom of the distribution of test scores, the distribution of true scores is limited by not being able to go below 0, and thus the probability is that they will go up, not down. The same is true for people near the top of the distribution, because they cannot go above 100. If they are at 98 on the first test, the likelihood is that they will go down on the second try. Thus those near the bottom are likely to move upward, toward the mean, and those at the top the reverse.

CHAPTER 21

School Choice in Milwaukee

A Randomized Experiment

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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 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 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, nonselected. . . 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 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

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 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 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.

Controlling for Family Background

The results in the main analysis in table 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 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 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 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

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

TABLE 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

those selected for the choice 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 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 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 estimates 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

TABLE 5

The Effects of Attending a Choice School on Test Scores, Controlling for Gender and Test Scores 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

TABLE 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 7, columns 1–4)	89	72
Total number who applied, 1990–93	908	363
Percent with test scores three of four years after application	63	48
Total number who applied in 1990 or 1991, making it possible to have scores three or hour years after application	592	166

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 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? The answer to this question is given in table 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.

TABLE 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

All results but one are statistically significant at the .1 level; fourth-year results are significant at the .05 level.

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 Milwaukee 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 coproduction 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

1. Kenneth Arrow, "An Extension of the Classical Theorems of Welfare Economics," in *Proceedings of the Second Berkeley Symposium on Mathematical Statistics* (Berkeley: University of California Press, 1951); Jacob Schmookler, *Invention and Economic Growth* (Harvard University Press, 1966); and James Dearden, Barry W. Ickes, 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).
3. Elinor Parks, Roger B. Parks, and Gordon P. Whitaker, *Patterns of Metropolitan Policing* (Cambridge, Mass.: Ballinger Publishing, 1978).
4. Cash-transfer programs are larger but do not involve direct service provision. Although publicly funded medical services are more costly than publicly-funded schools, most medical services are provided by private vendors. In recent years the cost of defense has fallen below the cost of state-provided educational services.
5. Paul E. Peterson, *The Price of Federalism* (Brookings, 1995).
6. *Public Perspective,* Roper Center Review of Public Opinion and Polling (November-December 1993), p. 13.
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.
9. Larry V. Hedges and Rob Greenwald, "Have Times Changed? The Relation between School Resources and Student Performance," in Gary Burtless, ed., *Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success* (Brookings, 1996), pp. 74–92; information cited on p. 78.
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.
12. Pam Belluck, "Learning Gap Tied to Time in the System: As School Stay Grows, Scores on Tests Worsen," *New York Times,* January 5, 1997, p. B1; George A. Mitchell, *The Milwaukee Parental Choice Plan* (Milwaukee: Wisconsin Policy Research Institute, 1992); and Paul E. Peterson, "Are Big City Schools Holding Their Own?" in John Rury, ed., *Seeds of Crisis* (University of Wisconsin Press, 1993).

13. Richard J. Herrnstein and Charles Murray, *The Bell Curve: Intelligence and Class Structure in American Life* (Free Press, 1994).
14. David Card and Alan B. Krueger, "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy* 100 (February), pp. 1–40; Hedges and Greenwald, "Have Times Changed?"; Christopher Jencks and Meredith Phillips, "Does Learning Pay Off in the Job Market?" in Mayer and Peterson, eds., *When Schools Make a Difference;* Jay Girotto and Paul E. Peterson, "Do Hard Courses and Good Grades Enhance Cognitive Skills?" in Mayer and Peterson, eds., *When Schools Make a Difference;* Susan Mayer and David Knutson, "Early Education Versus More Education: Does the Timing of Education Matter?" in Mayer and Peterson, eds., *When Schools Make a Difference;* Robert Meyer, "Applied versus Traditional Mathematics: New Evidence on the Production of High School Mathmatics Skills," in Mayer and Peterson, eds., *When Schools Make a Difference.*
15. John E. Chubb and Terry M. Moe, *Politics, Markets, and American Schools* (Brookings, 1990).
16. Anthony Bryk, Valerie E. Lee, and Peter B. Holland, *Catholic Schools and the Common Good* (Harvard University Press, 1993).
17. Kevin B. Smith and Kenneth J. Meier, *The Case Against School Choice* (Armonk, N.Y.: M. E. Sharpe, 1995), p. 126; but also see Hoxby, "When Parents Can Choose, What Do They Choose?"
18. Richard F. Elmore, "Choice As an Instrument of Public Policy: Evidence from Education and Health Care," in Clune and Witte, eds., *Choice and Control in American Education,* vol. 1; Carnegie Foundation for the Advancement of Teaching, *School Choice* (Princeton, N.J., 1992); and Amy Gutmann, *Democratic Education* (Princeton University Press, 1987).
19. James S. Coleman, Thomas Hoffer, and Sally Kilgore, *High School Achievement* (New York: Basic Books, 1982); James S. Coleman and Thomas Hoffer, *Public and Private Schools: The Impact of Communities* (Basic Books, 1987); Chubb and Moe, *Politics, Markets, and American Schools;* Bryk, Lee, and Holland, *Catholic Schools and the Common Good;* William N. Evans and Robert M. Schwab, "Who Benefits from Private Education? Evidence from Quantile Regressions," Department of Economics, University of Maryland, 1993; and Christopher Jencks, "How Much Do High School Students Learn?" *Sociology of Education* 58 (April 1985), pp. 128–35. But for different results, see Douglas J. Wilms, "School Effectiveness within the Public and Private Sectors: An Evaluation," *Evaluation Review* 8 (1984), pp. 113–35; Douglas J. Wilms, "Catholic School Effects on Academic Achievement: New Evidence from the High School and Beyond Follow-up Study," *Sociology of Education* 58 (1985), pp. 98–114.
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22. John F. Witte, "First Year Report: Milwaukee Parental Choice Program," Department of Political Science and the Robert M. La Follette Institute of Public Affairs, University of Wisconsin-Madison, November 1991; John F. Witte, Andrea B. Bailey, and Christopher A. Thorn, "Second Year Report: Milwaukee Parental Choice Program," Department of Political Science and the Robert M. La Follette Institute of Public Affairs, University of Wisconsin-Madison, December 1992; John F. Witte, Andrea B. Bailey, and Christopher A. Thorn, "Third Year Report: Milwaukee Parental Choice Program," Department of Political Science and the Robert M. La Follette Institute of Public Affairs, University of Wisconsin-Madison, December 1993; John F. Witte and others, "Fourth Year Report: Milwaukee Parental Choice Program," Department of Political Science and the Robert M. La Follette Institute of Public Affairs, University of Wisconsin-Madison, December 1994; John F. Witte, Troy D. Sterr, and Christopher A. Thorn, "Fifth Year Report: Milwaukee Parental Choice Program," Department of Political Science and the Robert M. La Follette Institute of Public Affairs, University of Wisconsin-Madison, December 1995; and John F. Witte, "Achievement Effects of the Milwaukee Voucher Program," paper presented at the 1997 annual meeting of the American Economics Association.
23. Jay P. Greene, Paul E. Peterson, and Jiangtao Du, with Leesa Berger and Curtis L. Frazier, "The Ef-

fectiveness of School Choice in Milwaukee: A Secondary Analysis of Data from the Program's Evaluation," Occasional Paper, Program in Education Policy and Governance, Harvard University, 1996.

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 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 of-

fered 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 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 2.