

The Potential Impact of Large-Scale Voucher Programs

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This review assesses the potential impact of large-scale voucher programs, drawing on empirical literature in economics, education, and sociology. The discussion is guided by three research questions, grounded within an economic framework. First, are private schools more efficient than public schools? Second, does the increasingly competitive schooling market promoted by vouchers cause public schools to become more efficient? And third, do vouchers result in increased student sorting across public and private schools—perhaps increasing segregation by socioeconomic status—and what does sorting portend for student outcomes? For some questions, there is a paucity of credible evidence. For others, evidence from non-voucher systems is used inappropriately to forecast the impact of vouchers. The review concludes that empirical evidence is not sufficiently compelling to justify either strong advocacy or opposition to large-scale voucher programs.

The introduction of large-scale voucher programs will have manifold effects on schools and students. Many assert that vouchers will result in a more efficient production of academic achievement and attainment, without skewing the distribution of benefits among social groups (Chubb & Moe, 1990; Friedman, 1955; Hoxby, 1998; West, 1997). Towards assessing these conclusions, this paper specifies a full range of potential effects. It then evaluates whether current empirical research in economics, education, and sociology provides guidance on the existence and magnitude of these effects. Ultimately, it concludes that empirical evidence is not sufficiently compelling to justify either strong advocacy or opposition to large-scale voucher programs. In some cases, there is simply a paucity of credible evidence. In others, evidence from non-voucher systems is used inappropriately to forecast the impact of large-scale voucher programs.

The review proceeds in several steps. First, it describes the intellectual history and practice of vouchers. Second, it delineates an economic framework for understanding the effects that a voucher plan could have on schools and students. This discussion yields three research questions: (1) Are private schools more efficient than public schools? (2) Does the increasingly competitive schooling market promoted by vouchers cause public schools to become more effi-

cient? (3) Do vouchers result in increased student sorting across public and private schools—perhaps increasing segregation by socioeconomic status—and what does sorting portend for student outcomes? Third, it provides a general description of research methods that have been utilized to answer these questions, and some pitfalls in their application. Fourth, it assesses the empirical research in light of the three research questions. Finally, the review summarizes the main findings, gauges whether firm conclusions can be drawn about the potential impact of voucher programs, and suggests some new directions for research.

The Policy of School Vouchers

School vouchers are government-funded tuition coupons, redeemable by parents at the public or private school of their choice. The idea of school vouchers has a long lineage, beginning with a proposal by Thomas Paine in *The Rights of Man* (West, 1967), and another in France towards the end of the nineteenth century (Van Vliet & Smyth, 1982). The first modern calls for vouchers were issued by Milton Friedman (1955; 1962), and again during the War on Poverty (Jencks, 1966; Levin, 1968). During the 1990's, they again leapt to the forefront of policy debates (Chubb & Moe, 1990; Cookson, 1994; Fuller, Elmore, & Orfield, 1996; Ladd, 1996; Moe, 1995a; Peterson & Hassel, 1998; Wells, 1993).¹

As authors are quick to point out, there is not a single voucher policy but many. Plans differ in scope and in the provisions made for the financing and regulation of schools (Levin, 1991). Friedman's (1962) proposal would place few restrictions on either parents or schools. Parents would receive a voucher redeemable at any approved school, either public or private; they would be free to supplement the voucher with "add-on" payments if desired. Regulation would be designed to ensure compliance with minimal safety or curricular standards. In contrast, the proposal of Chubb and Moe (1990) would make some students — particularly those from disadvantaged backgrounds — eligible to receive larger vouchers and would restrict the ability of schools to collect "add-on" payments. In other respects, schools would have latitude to determine admissions policies, textbooks and curriculum standards, and other aspects of school organization. There has also been advocacy of "targeted" voucher plans that restrict eligibility to a certain group of students, typically those from lower-income families (Becker, 1995).

Practical experiences with voucher plans are limited. In the United States, small-scale public programs have been implemented in Milwaukee and Cleveland. The Milwaukee Parental Choice Program, established in 1990, initially provided scholarships for 1,500 students to attend secular private schools (Witte, 1998). The number of participants was expanded to 15,000 in 1995 and restrictions on the participation of religious schools were lifted. Nevertheless, religious schools did not participate until 1998. The Cleveland Scholarship and Tutoring Program, begun in 1996, awarded 3,000 scholarships in the 1997-98 school year for attendance at secular or religious private schools (Metcalf, 1999; Peterson, Howell, & Greene, 1999a). Participation in both the Milwaukee and Cleveland programs was restricted to lower-income families. Besides these publicly-funded experiments, there are privately-funded programs in several U.S. cities that award scholarships for private school attendance (Moe, 1995a).

Several countries outside the United States have experimented with vouchers. Since 1992 Colombia has awarded over 100,000 publicly-funded scholarships for secondary school attendance (Calderon, 1996; King, Rawlings, Gutierrez, Pardo, & Torres, 1997). Despite the larger scale of the Colombia program, vouchers were still restricted to poorer students. Sweden's voucher plan, instituted in 1991, required every municipality in the country to substantially fund local private enrollments (Carnoy, 1998; Miron, 1996). Most parents still opted for local public schools. Even those choosing a school outside their attendance area mostly chose public rather than private schools.

The 1980 reform implemented by Chile's military government bears a striking resemblance to Milton Friedman's original proposal, which is unsurprising given the influence of University of Chicago-trained economists in the design of Chilean education policy (Gauri, 1998; Valdes, 1995). Public and private voucher schools began to receive payments based on monthly enrollments multiplied by a per-pupil voucher. Private schools were free to enter the schooling market and regulations were lifted on curriculum and the teacher labor market. In the years immediately following the reform, enrollments grew sharply in private, mostly for-profit schools and declined in public schools (McEwan & Carnoy, 2000).

A Framework for Assessing Vouchers

Demand and Supply in the Schooling Market

Parents derive satisfaction from their children's schooling outcomes, such as academic achievement, a higher probability of college attendance, and religious training. Parents also obtain satisfaction from the consumption of non-school goods and services. Since both are costly to obtain and family incomes are limited, parents choose the school that appears to provide the best compromise. In addition to income, parents face other constraints. For instance, some reside in catchment areas that restrict public school choices, and some are denied admission to private schools.

A family's choice of school is based on the estimation of the utility expected from sending their child to a particular school.² All else being equal, higher tuition reduces the expected utility of a school by reducing the ability of the family to consume non-school goods and services. Greater amounts of schooling outcomes valued by parents increase the expected utility of schooling options. The outcomes produced by each school depend on two factors, which families do not always observe. First, outcomes depend on the level of school-related inputs, which may be broadly conceived to include resources such as class size and teacher characteristics, as well as characteristics of a child's potential peer group. Second, outcomes depend on the "effort" of schools. If personnel exhibit low effort, then the outcomes obtained from a given input mix will be less than optimal. Acting within their constraints, families make decisions based on available knowledge of input and effort levels.³

On the supply side, public and private schools maximize a given set of objectives. It bears emphasis that researchers do not have *a priori* knowledge of either public or private objectives. Public school administrators are sometimes characterized as maximizing on-the-job consumption of "rents," by diverting resources from student-valued expenditures to those valued by school person-

nel (Manski, 1992). Some private schools are organized as non-profit, religious organizations, which may maximize "membership or faith rather than pecuniary profits" (James, 1993, p. 577). Some private schools are explicitly allowed to maximize profits, and even non-profit schools may maximize "hidden" profits via increases in the salaries and perks of administrators.

Schools maximize their objective functions by choosing an input mix and an effort level. Various combinations of inputs and effort will attract different groups of applicants. Schools that are oversubscribed are able to select their students from among applicants. Often, the presumption is that schools engage in cream-skimming by selecting applicants of a higher socioeconomic status. This might endow schools with another attractive "input": the characteristics of its student body. Alternatively, external regulations might require schools to use a lottery to select students at random.

Schools are constrained in their decisions by several factors. First, all schools face a set of input prices (e.g., teacher wages). Second, schools are constrained by legislation that regulates school management. For example, private school teachers may be subject to labor codes that place fewer restrictions on labor conditions and salaries than the code for public teachers. Third, schools operate within a budget constraint. In the centralized systems of many developing countries, public school budgets are determined by the number of school personnel. Private school budgets depend on student enrollments, tuition levels, and private donations. In a pure voucher system, the budgets of public and private schools will mostly be a function of total student enrollments and an administratively-determined voucher.

The interaction of demand and supply will lead to an equilibrium "price" of attending private school, as well as equilibrium enrollments in public and private schools. Furthermore, each public and private school will possess a unique input mix, effort level, and student body composition. Critics of public education have argued that this equilibrium, dominated by public school enrollments, is marked by public school inefficiency (Hanushek, 1986; 1996).

Vouchers and the New Market Equilibrium

How would the introduction of school vouchers alter the market equilibrium?⁴ Vouchers represent additional family income that is earmarked for educational expenditures. By accepting a voucher, families face a lower price of sending their child to private school. One immediate effect is to induce some families to choose private rather than public schools, leading to declining revenues in some schools and increasing revenues in others. The exact revenue effects will depend on the voucher legislation; for example, the voucher may be equal to average per-pupil costs or some reduced percentage of costs.

Which kinds of students are most likely to exit public schools? Low-income families that were constrained to attend low-quality public schools may be among the first to exit. Or families of relatively higher socioeconomic status may be the first if they have greater access to information on school quality or a greater preference for school quality. The latter type of sorting (cream-skimming) is often presumed to dominate, although this is an empirical question. In either instance, vouchers encourage a process of student sorting across schools, which may lead to increased or decreased segregation along any number of student characteristics such as socioeconomic status.

In the short term, sorting could affect student outcomes in two ways (see Figure 1 for a schematic). First, students who transfer to private schools may have different outcomes, perhaps higher if private schools are relatively more effective than public schools. Second, students in either public or private schools will perform differently if they are exposed to new peer groups, presuming that peer-group effects are an important feature of educational production.

In the long term, new private schools may find it attractive to enter the market, while others, public or private, could shut their doors.⁵ Depending on the context, new schools could be of many types, including for-profit, non-profit, religious, or non-religious. Thus, the long-term effects of vouchers will further depend upon the relative effectiveness of newly-created private schools, and evolving patterns of student sorting. There is yet another effect in the long-term. If public schools lose enrollments and revenues in the increasingly competitive schooling market, they may face pressures to improve student outcomes or lower costs.

Given the previous discussion, a full assessment of the short- and long-term impact of a voucher plan should address at least three issues. First, it should compare the relative efficiency of public and private schools—both existing and newly-created. Second, it should assess whether public schools are spurred by increasing competition to improve their efficiency. Third, it should describe the patterns of student sorting encouraged by vouchers, and analyze whether sorting has affected student outcomes because of peer-group effects.

A Primer on Methods

The credibility of empirical evidence often hinges upon research methods that purport to establish a causal relationship between selected independent variables (e.g., private school attendance, private school competition, peer-group characteristics) and a dependent variable (e.g., student achievement and attainment). To briefly illustrate these methods, I shall focus on a single topic, although the issues raised are widely applicable.

A large body of research explores whether private schools are relatively more effective than public schools in raising student achievement. To explore this issue, one might compare the average achievement of students who are observed to attend either private or public schools. In all likelihood, the private outcomes would be higher. But is the difference caused by schools or by pre-existing differences among the students who happen to attend private and public schools? Families of higher socioeconomic status (SES) may be more inclined to enroll their children in private schools. If high-SES families also endow their children with higher outcomes, then a simple comparison of average outcomes will confuse the dual influences of schools and families.

Broadly speaking, there are two approaches to disentangling the unique contribution of private school attendance to student outcomes: experimental and non-experimental. Both involve comparing the outcomes of public students to private students. A key difference is the degree of control exercised by the researcher over which students attend private or public schools. This, in turn, has important consequences for our ability to infer a causal link between private school attendance and student outcomes.

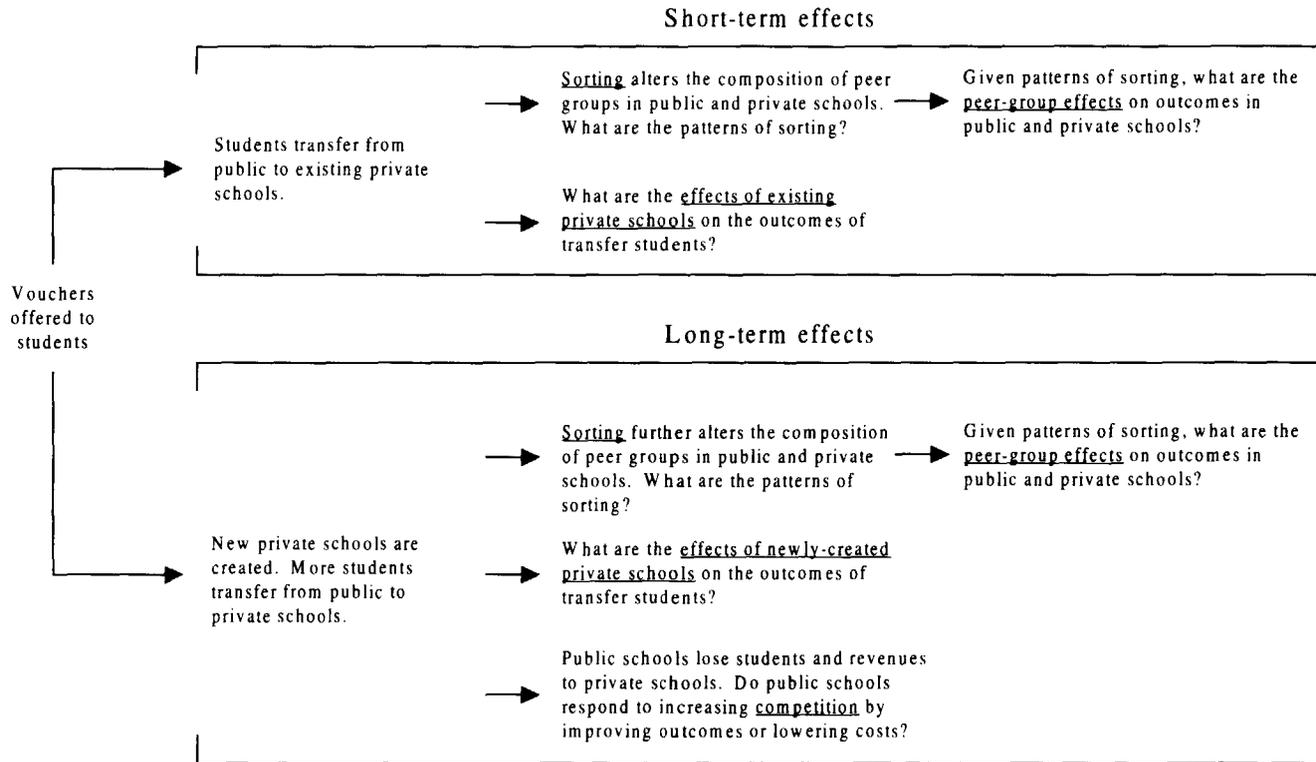


FIGURE 1. A schematic of the potential impact of vouchers

Experiments

In the experimental approach, subjects are assigned to a treatment group (e.g., private schools) or a control group (e.g., public schools).⁶ The defining feature of an experiment is that each individual has the same probability of being assigned to either group, regardless of socioeconomic status, motivation, or other characteristics.⁷ The use of randomized assignment implies that there are minimal pre-existing differences between students in each group. This confers an important strength on the evaluation design. After students have participated in the treatment and control groups for a specified period of time, we can be fairly confident that differences in their outcomes are the exclusive result of differences between private and public schools.

Non-Experiments

In non-experimental research, researchers exercise no control over who attends private and public schools. Instead, they collect data on the outcomes and background characteristics of students who are currently observed to attend each type of school. Researchers then employ statistical methods such as multiple regression analysis that control for the background of families and students.

By doing so, they attempt to identify the unique contribution of private schools to student outcomes. In principle, this should produce results comparable to those of a randomized experiment, *if all relevant family and student determinants of outcomes have been measured and controlled for in the statistical analysis*. The standard control variables include parental education and income, gender, race and ethnicity, and so forth. In practice, it is difficult to ensure that important control variables have not been omitted. For example, parents that send their children to private schools may be especially motivated. Even in the absence of good schools, highly motivated parents may engender higher outcomes among their children. If motivation is not controlled for, then its effects on outcomes will be confused with the effects of private schools. This is just one example of a common malady referred to as selection bias (Goldberger & Cain, 1982; Murnane, Newstead, & Olsen, 1985). The preceding example suggests that selection bias will lead to overestimates of private school effects, although we have no definitive means of predicting the direction or magnitude of bias.

The researcher's first line of defense against selection bias is to control for a wide variety of student and family characteristics. Ironically, this remedy is sometimes overlooked by researchers who make minimal controls for student background, even when using rich sources of data. The second line of defense is the use of sophisticated statistical methods.⁸ In the early 1980s, there was an unfortunate tendency to view these methods as a silver bullet that would magically correct for bias. More recently, there has been a recognition that the methods are founded upon strong assumptions. If these assumptions are reasonable, then the corrections can inspire a fair degree of confidence. If they are patently unreasonable, then the cure for selection bias may be worse than the disease.

To apply these methods, researchers must identify one or more variables—commonly referred to as “instrumental variables” or “instruments”—that fulfill two conditions. First, the instruments must be strongly correlated with the prob-

ability of choosing a private or public school. Second, they must be uncorrelated with student outcomes—specifically, with variance in outcomes that is not already explained by observed measures of student and family background. In the language of economists, the instrumental variables must identify “exogenous” variation in the probability of attending private schools. The violation of these conditions can lead to biases in the estimates of private school effects (Bound, Jaeger, & Baker, 1995). As one might imagine, it is usually difficult to identify instrumental variables that fulfill both conditions. In light of these difficulties, I shall argue that a great deal of non-experimental research must be interpreted with caution.

The Relative Efficiency of Private and Public Schools

This section summarizes and evaluates research that compares the relative efficiency of private and public schools. It distinguishes between studies of private school effectiveness and costs. A school type is deemed more effective than another if it produces greater outcomes among a similar group of students. If it also produces outcomes at a lower cost, it is more efficient. In addition to summarizing the evidence, I assess whether the findings are helpful in forecasting the relative efficiency of private and public schools under a large-scale voucher plan.

The Evidence on Effectiveness

In the early 1980s, Coleman, Hoffer, and Kilgore published an analysis of private secondary schools, using non-experimental data from the High School and Beyond (HSB) survey (Coleman, Hoffer, & Kilgore, 1982). They contended that students attending Catholic schools tended to achieve higher academically than their counterparts who attended public school. Their conclusions were immediately challenged, and the HSB achievement data were subjected to extensive re-analysis. The resulting studies have been reviewed by many authors and a fairly robust conclusion has emerged (Haertel, James, & Levin, 1987; Levin, 1998; Neal, 1998; Witte, 1992). After controlling for prior achievement and socioeconomic status in the HSB survey, the academic achievement of students in Catholic schools is, at best, about 0.1 standard deviation higher than that of public students. At worst, Catholic and public school achievement is not statistically different. Even so, most authors recognized that selection bias could be distorting the conclusions (for a careful discussion, see Goldberger & Cain, 1982). Despite attempts by some authors to correct for selection bias (Coleman et al., 1982; Murnane et al., 1985; Noell, 1982), a convincing resolution to the debate has yet to emerge.

The past five years have witnessed a flood of new research on private and public schooling. Emerging research is characterized by several features. First, it has made extensive use of NELS:88, a more recent longitudinal data set. Second, it has focused on a wider range of student outcomes, including attainment as well as achievement. Third, non-experimental research has devoted special attention to statistical corrections for selection bias. In general, these corrections have been applied with greater sophistication than in the past, and a more cautious recognition of their inherent pitfalls. Fourth, research now includes experimental and quasi-experimental evaluations of small-scale voucher pro-

grams in New York City, Dayton, Washington, DC, and Milwaukee.⁹

The recent empirical research is summarized in Tables 1-3.¹⁰ A discussion of the evidence is divided into four sections. The first two review experimental and non-experimental evidence, respectively. The third assesses whether the typical corrections for selection bias in non-experimental research inspire much confidence. The fourth section briefly summarizes evaluations of the Milwaukee voucher plan.¹¹

Experiments

The limited experimental evidence is described in Table 1, consisting of pilot programs in three cities: New York City, Dayton, Ohio, and Washington, DC (Howell & Peterson, 2000; Peterson, Myers, & Howell, 1998; Wolf, Howell, & Peterson, 2000).¹² In each case, families applied for scholarships to attend private schools. The pool of applicants was generally restricted to lower-income families (although any race or ethnicity was able to apply). Moreover, the scholarships were only available for study in the elementary grades.

A group of applicants was randomly selected to receive scholarships—generally between \$1000 and \$2000 annually—and another group was randomly selected to serve as a control group. The evaluators could not force the awardees to utilize the scholarships. Thus, about one-fourth of scholarship recipients in New York and almost half in Dayton and Washington did not use the scholarship. The recipients could attend any type of private school, including Catholic, other religious, and non-religious schools. The large majority, however, attended Catholic schools, and the estimates of private school effects are dominated by that category.

In each study, the authors conducted two achievement comparisons. In the first, they compared students in the control group to students who were offered a scholarship (even if they did not accept it). In the second, they compared students in the control group to those who actually attended a private school. There is a good reason to prefer the first comparison. It gauges results of the only policy tool available to policy-makers, who are unable to compel students to attend private schools. Even so, Table 1 reports results of the second comparison. The immediate reason for doing so is that it more closely parallels the private school effects that are estimated in non-experimental studies—that is, the relative effectiveness of attending a private instead of a public school.

The New York results suggest that private school attendance may raise the achievement of students in the upper-elementary grades (fifth-grade, in the case of reading achievement, and fourth-grade in math). There is no obvious explanation for why the finding is limited to these grades. Among these students, effects are around one-quarter of a standard deviation. When effects are estimated for the entire group of elementary students, they become statistically insignificant in mathematics. In reading, the results are statistically significant, but small in magnitude (0.1 standard deviations).

In Washington, there were statistically significant math effects of around one-fifth of a standard deviation for black elementary students. However, there were no statistically significant effects among non-black students, among students in grades 6-8, or on the reading test. The Dayton results are surprisingly consistent with the Washington results. There was a math effect of around one-

TABLE 1

A summary of recent experimental research on achievement in private and public schools

Study	Data	Grade level(s) at post-test	Method	Instrumental variable	Type of private school	Dependent variable(s)	Effect on dependent variable of attending private school
Howell & Peterson, 2000 ^a	Dayton, Ohio	2-8	Instrumental variables	Selection into program	Religious or non-religious	Reading	NS (blacks) NS (non-blacks)
						Math	0.17 (blacks) NS (non-blacks)
Wolf et al., 2000 ^b	Washington, DC	2-8	Instrumental variables	Selection into program	Religious or non-religious	Reading	NS (2-5, blacks) NS (2-5, non-blacks) NS (6-8, blacks) NS (6-8, non-blacks)
						Math	0.17 (2-5, blacks) NS (2-5, non-blacks) NS (6-8, blacks) NS (6-8, non-blacks)
Peterson et al., 1998 ^c	New York City	2-5	Instrumental variables	Selection into program	Religious or non-religious	Reading	0.10 (2-5) NS (2) NS (3) NS (4) 0.27 (5)
						Math	NS (2-5) NS (2) NS (3) 0.27 (4) NS (5)

Notes: NS indicates that the difference is not statistically significant at 5%. All studies use the group of rejected voucher applicants as a control group.

^a Table A3 (lower panel). The percentile gain was converted to an effect size using the standard normal curve.

^b Table A3 (lower panel). The authors do not report full results for non-blacks, except to mention that results were not statistically significant. The percentile gain was converted to an effect size using the standard normal curve.

^c Tables 18 and 19 (effects of receiving treatment). Also see Peterson, Myers, Howell, and Mayer (1999).

fifth of a standard deviation for black students. In reading, and for non-black students, there are no statistically significant effects.

The combined results suggest that attendance at private, mainly Catholic schools may improve the mathematics—and, to a lesser extent, reading achievement—of poor, black students in elementary schools. However, it bears emphasis that the effects are not consistently observed for all grades and the effects do not appear to exist for poor, non-black students.

Non-Experiments

Table 2 describes a series of non-experimental studies that include academic achievement as an outcome measure. In five of these, the authors examine secondary school achievement using the NELS:88 data set. Given that they use the same data, their conclusions are perhaps less consistent than one might have desired. Grogger and Neal (in press) and Altonji, Elder, and Taber (2000) find positive effects of Catholic school attendance on 12th grade math and reading achievement among white students (but not for minorities). However, neither identifies a Catholic effect when 10th grade achievement is used as the outcome measure. Also assessing 10th grade achievement, Figlio and Stone (1999) find no evidence that religious or non-religious schools have widespread effects on math achievement, although there is some evidence that urban blacks reap benefits. Gamoran (1996) finds a small effect of Catholic school attendance on 10th grade math achievement—less than 0.1 standard deviation—and none for reading, while Goldhaber (1996) finds no private school effects.

There are few non-experimental studies that assess private school achievement in elementary or middle schools. Although Sander (1996) uses the HSB data set on secondary students, he attempts to discern the effects of an elementary and middle school Catholic education. He finds that that 1-7 years of Catholic school have no effects on any of the 10th grade achievement measures. However, 8 years of Catholic school appear to produce large effects of more than half a standard deviation on the reading and vocabulary tests (but not mathematics). However, one is hard-pressed to explain why private school effectiveness is dormant for most of the elementary school career, and suddenly blooms in the eighth grade. The results are difficult to accept at face value without a plausible explanation of how Catholic schools function in these grades.

Jepsen (1999a) provides more credible evidence on the effectiveness of elementary schooling because his data include measured outcomes on cohorts of 1st and 4th graders. In his sample of low-income schools, he finds that Catholic schools produce no achievement gains among 1st graders. Among 4th graders, they produce modest gains in reading and math (around 1/5 of a standard deviation), but only for white students in urban schools. In Toma's (1996) analysis of eighth grade IEA data, a combined group of religious and non-religious private students has a small advantage of 0.06 standard deviation in math.

Table 3 describes six non-experimental studies that have explored the effects of private school attendance on high school completion and college attendance. The findings are quite striking for their consistency, especially when compared to the mixed findings on academic achievement.¹³ Using several data sets, including HSB and NELS:88, most authors find that attending a Catholic school increases the probability of completing high school or attending col-

TABLE 2

A summary of recent non-experimental research on achievement in private and public schools

Study	Data	Grade level(s) at post-test	Method	Instrumental variable(s)	Type of private school	Dependent variable(s)	Effect on outcome of attending private school (percentage of standard deviation)
Grogger & Neal, in press ^a	NELS:88	12 th	Ordinary least squares	n/a	Catholic	Math	NS (urban minorities) 0.13 (urban whites) NS (suburban minorities) 0.14 (suburban whites)
Altonji et al., 2000 ^b	NELS:88	10 th & 12 th	Ordinary least squares	n/a	Catholic	Reading	NS (10 th grade, full sample) NS (10 th grade, urban minorities) NS (10 th grade, urban whites) Positive ⁱ (12 th grade, full sample) NS (12 th grade, urban minorities) Positive ⁱ (12 th grade, urban whites)
						Math	NS (10 th grade, full sample) NS (10 th grade, urban minorities) NS (10 th grade, urban whites) 0.07 (12 th grade, full sample) NS (12 th grade, urban minorities) 0.12 (12 th grade, urban whites)
Figlio & 1999 ^c	NELS:88	10 th	Instrumental variables	Presence of duty-to-bargain or right-to-work laws; interactions of these variables with SES and income	Religious Non-Religious	Math	NS (religious, full sample) Stone, NS (non-religious, full sample) NS (religious, blacks) Positive ⁱ (religious, urban blacks)
Jepsen, 1999a ^d	Prospects	1 st & 4 th	Ordinary least squares	n/a	Catholic	Reading	NS (1 st grade, full sample) NS (1 st grade, urban blacks) NS (1 st grade, urban Hispanics) NS (1 st grade, urban whites) NS (4 th grade, full sample) NS (4 th grade, urban blacks) NS (4 th grade, urban Hispanics) 0.18 (4 th grade, urban whites)

TABLE 2 (cont.)

Study	Data	Grade level(s) at	Method variable(s) post-test	Instrumental private school	Type of	Dependent variable(s)	Effect on outcome of attending private school (percentage of standard deviation)
Jepsen, 1999a ^d (cont.)						Math	NS (1 st grade, full sample) NS (1 st grade, urban blacks) NS (1 st grade, urban hispanics) NS (1 st grade, urban whites) NS (4 th grade, full sample) NS (4 th grade, urban blacks) NS (4 th grade, urban Hispanics) 0.29 (4 th grade, urban whites)
Gamoran, 1996 ^e	NELS:88	10 th (urban schools)	Two-step selection	Catholic religious status, region, other	Catholic	Reading	0.00 (Catholic) -0.10 (non-religious)
					Non-religious	Math	0.09 (Catholic) -0.08 (non-religious)
						Science	-0.04 (Catholic) 0.01 (non-religious)
						Social Studies	0.13 (Catholic) -0.40 (non-religious)
Goldhaber, 1996 ^f	NELS:88	10 th	Two-step selection	Money set aside for educational needs, region,	Catholic	Reading	-0.16 (Catholic) -2.8 (non-religious)
					Non-religious	Math	-0.41 (Catholic) -0.59 (non-religious)

TABLE 2 (cont.)

Study	Data	Grade level(s) at post-test	Method	Instrumental variable(s)	Type of private school	Dependent variable(s)	Effect on outcome of attending private school (percentage of standard deviation)
Sander, 1996 ^e	HSB (non-Hispanic white students)	10 th	Two-step selection model	Interactions between Catholic status and region	Catholic	Reading	NS (1-7 years of Catholic school) 0.51 (8 years of Catholic school)
						Math	NS (1-7 years of Catholic school) NS (8 years of Catholic school)
						Vocabulary	NS (1-7 years of Catholic school) 0.65 (8 years of Catholic school)
						Science	NS (1-7 years of Catholic school) NS (8 years of Catholic school)
Toma, 1996 ^h	IEA	8 th	Ordinary least squares	n/a	Religious and non-religious	Math	0.06

Notes: NS indicates that the difference is not statistically significant at 5%. With one exception, all studies use public school students as the comparison group.

Gamoran (1996) uses public students in non-magnet schools as the comparison group. Abbreviations are as follows: HSB (High School and Beyond); IEA (International Association for the Evaluation of Educational Achievement); and NELS:88 (National Education Longitudinal Study of 1988).

^aTable 3B (specification B). Unstandardized regression coefficients were divided by the standard deviation of the dependent variable in the public school sample (see Table 3B).

^bTable 4 (full sample, specification 3, OLS) and Table 6 (columns 3 and 4, OLS). Unstandardized regression coefficients for mathematics were divided by the standard deviation of the dependent variable in the public school sample, taken from Table 3B (Grogger & Neil, in press). Standard deviations for reading were not available.

^cTable 6 (full sample) and Table 7.

^dTable 5 (full sample) and Table 8 (1992 test scores model). Unstandardized regression coefficients were divided by the standard deviation of the dependent variable in the public school sample (see Table 1).

^eTable 6. Unstandardized regression coefficients were divided by the standard deviation of the dependent variables (see Appendix A).

^fTable 1.

^gTable 2. Unstandardized regression coefficients were divided by the standard deviation of the dependent variables (obtained from the author). The results for 1-7 years of Catholic schooling are taken from OLS models that do not correct for selection bias. In these specifications, the author found that the instrumental variables were not good predictors of Catholic school attendance.

^hTable 4. The unstandardized regression coefficient was divided by the standard deviation of the dependent variable (see Table 3).

ⁱBecause standard deviations were not available for the dependent variable, the coefficients could not be expressed as percentages of a standard deviation.

lege. In general, the magnitudes of these effects are relatively larger in urban areas and for minority students. There is, however, a notable exception to this pattern of findings. Figlio and Stone (1999) find that religious schools only influence the probability of attending two years of a selective college. For other measures of attainment (including high school graduation), the effects are not statistically significant. This contradiction is troubling because the authors use the same NELS:88 data as other authors. In the next section, I forward a possible explanation for this finding.

Bias in Non-Experiments

Among the studies in Tables 2 and 3, many employ statistical corrections for selection bias. These corrections are unnecessary if the statistical models contain perfect controls for the background characteristics of students and families, although it is likely that some determinants of achievement are not measured. If these variables are also associated with the likelihood of attending private school, then results are biased.

To apply the corrections, the authors must identify “instruments” that are correlated with private school attendance, but uncorrelated with unexplained student outcomes. A quick scan of Tables 2 and 3 reveals that many authors employ Catholic religious status or a variation on the theme (e.g., the density of Catholic populations in local communities). These authors posit that an individual’s religious status (or local population densities of Catholics) are related to the likelihood of choosing a Catholic school. In fact, most of their analyses bear out this assertion. However, the instruments must fulfill a second condition: They cannot be correlated with unexplained student outcomes. In this respect, the empirical approach finds extremely weak support in the literature.

Murnane et al. (1985) report that Catholic religious status does not pass a statistical test of exogeneity (which would have bolstered its use as an instrumental variable). Sander (1992, 1995) and Sander and Krautman (1995) find that Catholic religious status is correlated with outcomes, even after controlling for a wide range of socioeconomic background variables. In analyses of NLSY and NELS:88 data, Neal (1997) and Grogger and Neal (in press) find that many of their instruments are correlated with attainment, and thus inappropriate. In the case of the urban minority subsample, Grogger and Neal (in press) report that *none* of the instruments are appropriate, in that they are correlated with student outcomes.¹⁴ Using NELS:88 data, Figlio and Stone (1999) conduct statistical tests that allow them to soundly reject the use of religious status or religious population densities as instruments. As the same authors note, the effect of using a poor set of instrumental variables is far from benign. In fact, doing so generally leads to *increases* in the estimated effects of private schools.¹⁵ Thus, the application of “corrections” for selection bias has the potential to exacerbate biases in private school effects.

Short of randomized experiments, are there alternative remedies for selection bias? Figlio and Stone (1999) implement statistical corrections using a different set of instrumental variables, including indicators of whether states have “duty to bargain” or “right-to-work” laws. Their results turn out to be less optimistic than other studies with NELS:88 data. They find that religious schools only

TABLE 3

A summary of recent non-experimental research on attainment in private and public schools

Study	Data	Method	Instrumental variable(s)	Type of private school	Dependent variable(s)	Effect on outcome of attending private school (increase in probability)
Grogger & Neal, in press ^a	NELS:88	Maximum likelihood selection model	Catholic religious status, population share, school density; interactions	Catholic	High school graduation	0.18 (urban minorities) 0.07 (urban whites) 0.05 (suburban minorities) 0.06 (suburban whites)
					College attendance	0.27 (urban minorities) 0.06 (urban whites) 0.20 (suburban minorities) 0.02 (suburban whites)
Altonji et al., 2000 ^b	NELS:88	Probit	n/a	Catholic	High school graduation	0.05 (full sample) 0.19 (urban minorities) 0.09 (urban whites)
					College attendance	0.07 (full sample) 0.14 (urban minorities) 0.11 (urban whites)
Figlio & Stone, 1999 ^c	NELS:88	Instrumental variables	Presence of duty-to-bargain or right-to-work laws; interactions of these variables with SES and income	Religious	High school graduation	NS (religious) NS (non-religious)
				Non-religious	Two years of college	NS (religious) 0.13 (non-religious)
					Two years of selective college	0.27 (religious) 0.48 (non-religious)
				Two years of college; major in math, science, or engineering	NS (religious) NS (non-religious)	

TABLE 3 (cont.)

Study	Data	Method	Instrumental variable(s)	Type of private school	Dependent variable(s)	Effect on outcome of attending private school (increase in probability)
Neal, 1997 ^d	NLSY	Maximum likelihood selection model	Catholic religious status, population share, school density	Catholic	High school graduation	0.10 (white urban) 0.26 (minority urban) NS (white non-urban) NS (minority non-urban)
Evans & Schwab, 1995 ^e	HSB	Maximum likelihood selection model	Catholic religious status and population share; interactions	Catholic	College graduation	0.12 (white urban) 0.16 (minority urban) NS (white non-urban) NS (minority non-urban)
Sander & Krautmann, 1995 ^f	HSB	Two-step selection model	Interactions between Catholic status and region	Catholic	High school graduation College attendance High school drop-out Total years of schooling	0.12 0.11 -0.10 NS

Notes: NS indicates that the difference is not statistically significant at 5%. All studies use public school students as the comparison group. Abbreviations are as follows: HSB (High School and Beyond); NELS:88 (National Education Longitudinal Study of 1988); and NLSY (National Longitudinal Survey of Youth).

^a Tables 1B and 2B (specification C, marginal effects). Given results from the selection model, the authors could not reject the null hypothesis of no selection bias. Thus, the results in the final column are from the single-equation estimates that are uncorrected for selection bias.

^b Table 3 (full sample, specification 3, probit, marginal effects) and Table 5 (specification 3, probit, marginal effects).

^c Table 6 (full sample).

^d Tables 4 and 8 (the marginal effects implied by the probit coefficients are taken from the text). Given results from the selection model, the author could not reject the null hypothesis of no selection bias. Thus, the results in the final column are from the single-equation estimates that are uncorrected for selection bias.

^e Table 4 (model 8, average treatment effect). Given results from the selection model, the authors could not reject the null hypothesis of no selection bias. Thus, the results in the final column are from the single-equation estimates that are uncorrected for selection bias.

^f Tables 4 and 5 (the marginal effect implied by the probit coefficient is taken from the text).

have positive effects on the achievement of urban black students (but not for other students), and on the likelihood of attending a selective college (but not the likelihood of attending *any* college or graduating from high school).

The Milwaukee Voucher Plan

In the early 1990s, the Milwaukee Parental Choice Program awarded scholarships to a limited number of low-income students who wished to attend private, non-religious schools. In subsequent years, the program was expanded to a larger number of students, and students were able to choose religious schools. However, the three main evaluations are restricted to the initial phase of the Milwaukee plan (Greene, Peterson, & Du, 1998; Rouse, 1998a; Witte, 1998). The findings of these evaluations are often in disagreement. However, much of the disagreement can be traced to differences in data and methods.¹⁶ For example, authors make different decisions regarding which group of students to compare to choice students. They also use different techniques to control for the pre-existing differences among choice students and the comparison group.

If applications to participating choice schools were over-subscribed, then schools were required to select students at random. In theory, this created a “mini-experiment” at the level of each school. Unsuccessful applicants can be used as a control group, and their outcomes can be compared to choice students (presuming that adequate controls are made for the application lottery of each school). Greene et al. (1998) pursued this strategy and found that attending a choice school tended to improve math scores after four years.¹⁷ They further estimated reading effects that were smaller in magnitude, and not statistically significant at a level of 5 percent.

In practice, the empirical strategy has at least two shortcomings (Rouse, 1998b). First, the actual school to which each student applied was not directly observed. Thus, application lotteries were imputed, which injects a measure of uncertainty to the analysis. Second, a number of unsuccessful applicants to the choice program were sufficiently motivated to attend another private school. Therefore, they do not appear in the control group. This could potentially bias the results—perhaps towards over-stating program effectiveness—if attrition from the control group is non-random.

Another evaluation employed a completely different strategy, best described as non-experimental. As a comparison group, Witte (1998) used a random sample of students in Milwaukee public schools. Upon controlling for prior achievement and student background characteristics, Witte finds no differences in reading or math achievement between the choice students and the comparison group.¹⁸ These results are subject to the same caveats regarding selection bias that were forwarded in the previous discussion of non-experimental evidence. In other work, Witte (2000, pp. 152-156) discusses attempts to apply statistical corrections for selection bias. However, the procedure is hampered by the lack of compelling instrumental variables that are correlated with selection into the program, and uncorrelated with outcomes.

In a third evaluation, Rouse (1998a) conducts multiple analyses that employ both comparison groups: the group of unsuccessful applicants and the random sample of Milwaukee public students. Unlike previous authors, she makes further attempts to control for the background of students in comparison and treat-

ment groups. She does so by including individual “fixed effects,” which control for unobserved student characteristics that do not vary across time. Her analyses suggest that attending a private school leads to annual math gains of around 0.13 standard deviations.¹⁹ These findings are robust to several different statistical specifications, although there are no statistically significant gains in reading scores.

The preponderance of evidence from evaluations of the Milwaukee plan suggests that attending a choice school may have produced small annual gains in mathematics scores among a group of low-income children in the elementary or middle school grades. However, it did not improve reading scores. Even so, the findings may be of limited applicability. Choice students were exclusively enrolled in non-religious private schools. Moreover, about three private schools accounted for 80 percent of the private enrollments (Moe, 1995a; Rouse, 1998a). The Milwaukee experience is best understood as an evaluation of three private schools, rather than a comprehensive evaluation of private schooling.²⁰ Viewed in this light, the amount of attention devoted to the Milwaukee plan seems wildly out of proportion to the general policy lessons that it might yield.

Interpreting the Evidence on Effectiveness

For the moment, let us assume that the previous research yields unbiased estimates of private school effects. What mechanisms are responsible for the observed effectiveness of private schools? And does this evidence—mainly from non-voucher systems—aid in forecasting the relative effectiveness of private and public schools under an expansion of private schooling, as might occur in a large-scale voucher program?

Inside the “Black Box”

There are several plausible explanations of what lies within the “black box” of private school effectiveness.²¹ First, Catholic high schools may promote a more challenging academic climate than public schools (Witte, 1992; 1996a). Many authors document that Catholic students take more academic courses than public students and are more likely to participate in academic rather than vocational tracks (Bryk, Lee, & Holland, 1993; Coleman & Hoffer, 1987; Coleman et al., 1982; Gamoran, 1996). Bryk et al. (1993) argue that Catholic high schools directly encourage all students to pursue a common academic core, regardless of background or college plans.

Second, the effects of Catholic schools may be rooted in their distinctive “communal organization” (Bryk et al., 1993). Compared to their public counterparts, Catholic schools provide more opportunities for face-to-face interactions among adults and students and give teachers greater responsibility for working with students outside the classroom. Moreover, they promote “a set of shared beliefs about what students should learn, about proper norms of instruction, and about how people should relate to one another” (Bryk et al., 1993, p. 299).

Third, private schools may permit a more decentralized and autonomous organization that allows for greater effectiveness. Chubb and Moe (1990) argue that effective schools possess organizational properties such as clearer goals, better leadership, and more emphasis on academic courses. They contend that such characteristics only proliferate where greater autonomy is given to organi-

zations, and autonomy, they argue, flourishes mainly in the private sector.²² In their view, the effectiveness of Catholic schools stems mainly from the fact that the schools operate in the private sector and not from their religious orientation. While Bryk et al. (1993, p. 299) also note the benefits of decentralized governance, they are less sanguine about attributing the entirety of the Catholic school effect to the benefits of operating in a free market.

Fourth, the effect may indicate that students in private schools are exposed to more privileged peer groups that positively influence student outcomes (in a later section, I examine the empirical evidence on peer-group effects). This effect might exist independently of other private school effects that are due to academic policies, the communal organization of schools, or school autonomy.

In most studies—experimental and non-experimental—it is simply not possible to determine the relative importance of each explanation. In recent experiments, students were randomly awarded or denied scholarships to attend private schools (e.g., Peterson et al., 1998). No statistical controls were made for characteristics of peer groups or schools. Thus, the overall effect could encompass all of the previous explanations.

Similarly, many non-experimental studies employ a parsimonious set of control variables, limited to student and family SES, when comparing private and public achievement. Among these studies, it is common to find positive effects of Catholic school attendance on attainment (Altonji et al., 2000; Grogger & Neal, in press; Neal, 1997; Sander & Krautmann, 1995). Among studies that make extensive controls for peer, neighborhood, and school characteristics there is a marked tendency to find small or statistically insignificant private school effects (Figlio & Stone, 1999; Gamoran, 1996; Goldhaber, 1996; Toma, 1996). This suggests that an overall private school effect may bundle together a diverse set of peer-group or school-resource effects. Without further evidence, however, interpretations beyond this are entirely speculative.

Private School Effects Under Vouchers

There are at least two reasons why it is problematic to use existing evidence to predict the relative effectiveness of private and public schools under a large-scale voucher plan. Both stem from our ignorance of the “black box.”

First, a large-scale voucher plan may encourage new private schools to enter the market. In all likelihood, these schools will bear little resemblance to existing Catholic (and other private) schools. They may be non-religious, and they may operate as for-profits rather than non-profits.²³ Would these private schools duplicate the currently observed Catholic effects? Insofar as such schools would reap the same benefits from operating in the private sector, Chubb and Moe (1990) would argue in the affirmative. Bryk et al. (1993) may be skeptical about the ability of non-religious and profit-maximizing schools to duplicate certain elements of effective Catholic schools, particularly their communal organization. Ultimately, this is an empirical question, although researchers know very little about how “new” private schools will affect student outcomes.²⁴ The existing evidence is almost entirely limited to Catholic schools, or categories of religious schools that are Catholic-dominated. When evidence refers to non-religious schools, as in Milwaukee, it is unclear whether or not it is representative of emerging categories of for-profit schools.

Second, a large-scale voucher program would lead to a massive sorting of students across schools, which could alter the composition of student peer groups in both public and private schools. For example, existing private schools might absorb larger numbers of lower-SES students from public schools. A corollary is that peer-group effects would not remain static. In private schools, they may decline in lockstep with declining peer-group SES. Now let us imagine that current estimates of the private school effect are largely (or entirely) reflective of peer effects. In this scenario, the “black box” estimates of private effects provides a very poor indicator of the potential effectiveness of private schools under a large-scale voucher plan, if only because peer-group composition and peer-group effects will not remain static.

The Evidence on Costs

There is no definitive comparison of private and public costs in the United States, a conclusion echoed by Rouse (1998b). Hoxby (1998) asserts that private schools cost around 50 to 60 percent less than public schools, but presents no data. Using principal-reported data from HSB, Coleman and Hoffer (1987) also conclude that per-pupil expenditures in Catholic schools are roughly 50 percent less than in public schools.²⁵ The same data show that “other private” and “high-performance private” schools are, respectively, 38 and 131 percent more expensive than public schools. In response to claims that Milwaukee choice schools operated at half the cost of public schools, Levin (1998) presents rough calculations suggesting that choice schools have no cost advantage. Several cost comparisons in developing countries purport to show lower private costs, but these give few details on data or methodology (Jimenez & Lockheed, 1995; Kingdon, 1996; Lockheed & Jimenez, 1996).

The accurate measurement of private and public costs faces challenging methodological issues. Using either tuition payments or money expenditures as a proxy of private costs is unlikely to provide a full accounting.²⁶ Private schools receive additional resources from several sources, such as parents who pay special fees, donate time, purchase school materials or uniforms, participate in fund-raising events, or provide direct donations.²⁷ Tsang and Taoklam’s (1992) careful cost accounting in Thailand shows that private school tuition accounts for only 40 percent of direct and indirect family costs. Even the public schools of many developing countries depend in large part on private contributions.²⁸

Besides tuition and other private contributions, religious schools receive support in the form of church subsidies, the services of clergy working at below-market wages, and the donated use of land and buildings. An early study by Bartell (1968), still unparalleled in its depth of analysis, shows that non-tuition receipts accounted for around 40 percent of elementary Catholic school revenues in the 1960’s, and around 20 percent of secondary revenues. Estimates from the 1990’s, based on a random sample of U.S. Catholic elementary schools, show that the average school receives 28 percent of its revenue from parish subsidies (Kealey, 1996). Besides monetary subsidies, many personnel in private schools are members of religious orders and their salaries understate their true market value. In 1995, for example, 47 percent of Catholic elementary principals were priests or members of a religious community (Kealey, 1996). Their average salary was \$20,274 per year, compared with an average salary

\$34,520 for principals who were laypersons. Nine percent of all private school teachers and a somewhat larger percentage in Catholic schools work on a “contributed services” basis (Chambers & Bobbit, 1996). Salaries of these teachers are an average of 19 percent lower than others in private schools.²⁹ Finally, churches may use donated land and buildings. Bartell’s study makes careful estimates of the value of contributed services of physical facilities as well as personnel, finding that cash operating costs of Catholic schools in his sample were between 36 and 45 percent of total resource costs, the rest accounted for by contributed services.³⁰

Even when costs are measured correctly, a different service mix between public and private schools complicates a straightforward comparison of per-pupil costs. Public schools receive greater numbers of children requiring special education or vocational education, both of which are substantially more costly than standard instruction (Levin, 1998). Furthermore, public schools often serve greater numbers of students who come from families with lower incomes and parental education. Empirical studies in the U.S. find that it is more costly to educate these students, after controlling for levels of school outcomes such as achievement (Downes & Pogue, 1994; Duncombe, Ruggiero, & Yinger, 1996).

Other research suggests that teachers reveal a willingness to trade off wages against non-monetary features of jobs such as class size, student ethnicity and socioeconomic status, and the incidence of violent behaviors (Antos & Rosen, 1975; Chambers & Bobbit, 1996; Chambers & Fowler, 1995; Levinson, 1988). The implication is that schools with lower quantities of “desirable” job characteristics, either public or private, must pay higher salaries to attract good teachers, holding all else equal. Chambers and Bobbit (1996) decompose the absolute difference between public and private teacher salaries into three components: the difference attributable to teacher characteristics, to school characteristics, and to the structure of the wage model. They find that differences in school characteristics—including the types of students in the school—account for between 8 and 34 percent of the total salary gap between teachers in public and various types of Catholic schools.³¹

An alternative method of making cost comparisons is within the framework of an educational cost function. Cost functions estimate the determinants of school costs, while holding constant student attributes (such as socioeconomic status), public/private status, the local prices of schooling inputs, and outcomes such as achievement.³² Although many studies estimate public school cost functions, only a few compare private and public schools.³³

The author is aware of three such studies, all in developing countries. Estimating cost functions for samples of Bolivian and Paraguayan schools, Jimenez (1986) finds that public schools may have a small cost advantage, all else equal.³⁴ Another study in Indonesia finds that private schools produce at lower cost than public schools, holding other variables constant such as student background (James, King, & Suryadi, 1996). Finally, Tsang and Taoklam (1992) estimate cost functions suggesting that recurrent costs of public schools are somewhat lower than private schools, but that capital costs are higher.

Interpreting the Evidence on Costs

Let us assume that accurate comparisons have been made of costs in private

and public schools. Do these comparisons provide an adequate means of predicting the relative costs of private and public schools under a large-scale expansion of private schooling? For several reasons, we might expect that cost differences would not remain static.

First, the service mix of private schools would be altered if schools were required to serve different types of students. Additional public funding could bring added regulation and the requirement to accept higher-cost students with special educational needs. Moreover, the expansion of private schooling might occur largely through the absorption of lower-SES students. If there is a labor market premium paid to teachers in such jobs, as some evidence indicates, then the cost structure of private (and public) schools will be altered over time (Chambers & Fowler, 1995; Chambers, 1987).

Second, there are limited numbers of individuals willing to provide contributed services and work at below-market wages in private schools, as do many clergy (Bartell, 1968; Kealey, 1996). New or expanding private schools—particularly non-religious and for-profit, but perhaps even Catholic schools—may need to pay higher wages in order to attract the requisite numbers of personnel.

Third, vouchers modify the political economy of education through the creation of new interest groups. Large numbers of private school teachers, less likely to possess “altruistic” preferences, would be more inclined towards and capable of unionization (Chambers, 1987). Increased unionization may lead to increases in the teacher wage bill (Hoxby, 1996b). Another plausible alternative is that increasing numbers of private school owners would effectively lobby for increases in voucher amounts. In Chile, for example, influential private school associations have proven adept at lobbying for increases in the size of the voucher.

Fourth, the implementation and operation of a voucher system is not without its own costs (Levin & Driver, 1997). Though a voucher system might bring about some cost savings at the central level, other costs would arise in the following categories: (1) transportation, as students attend schools outside their immediate neighborhoods;³⁵ (2) information provision, as central governments collect and distribute the indicators of quality necessary for a perfectly competitive educational market to function; (3) record-keeping and monitoring of school attendance; and (4) adjudication of parental disputes, particularly if the voucher is means-tested or limited to certain social groups.

Competition and Public School Efficiency

Milton Friedman contended that a voucher system would “permit competition to develop,” thus leading to the “development and improvement of all schools” (Friedman, 1962, p. 93). If so, vouchers might benefit those public school students who choose not to utilize a voucher. There are a growing number of attempts to explore his assertion.

Theories of Competition and Public School Efficiency

Manski (1992) was the first to formalize the intuitions of many economists by constructing a computational model that simulates the response of inefficient public schools to the introduction of vouchers. He assumes that public schools maximize “rents,” defined as the difference between total educational

expenditures and expenditures valued by students (the former include “wasteful” inputs which do not contribute to valued educational outcomes). Private schools, in contrast, operate in a perfectly competitive market. Students, who care about the amount of student-valued expenditures, choose the private or public option which maximizes utility.³⁶ Upon introduction of a voucher, inefficient public schools uniformly increase student-valued expenditures.³⁷ Note that monetary expenditures do not rise. Rather, existing ones are simply made more efficient in the eyes of students. The underlying logic is simple. Vouchers reduce the amount of income that families sacrifice to attend private schools, making exit from public schools less costly. Public schools are willing to sacrifice a certain amount of rent on each student, in order to avoid incurring the greater financial loss from a mass exodus.

However, recent work by economists has demonstrated conditions under which the effects of competition on public schools are ambiguous.³⁸ In McMillan (1997), for example, parents have the option of exit from public schools and voice, which encompasses parental monitoring of school officials. In a simple exit model—not unlike Manski’s—vouchers unambiguously improve the effort levels in public schools. Likewise, parental monitoring improves public schools, if taken alone. But when competition and parental monitoring are both incorporated, the introduction of vouchers may have the unintended consequence of reducing monitoring levels. In a sense, this is a different variety of cream-skimming than the one usually considered. Rather than their children having beneficial effects on the achievement of other students via peer effects, it is certain parents who affect public educational quality through effective pressure brought to bear on administrators and teachers.³⁹ The loss of these parents and the pressure they put on schools could nullify competitive improvements.

Evidence on Competition and Public School Efficiency

The existence and magnitude of competitive effects is an empirical question. A growing literature, summarized in Table 4, explores the links between increasing competition from private schools and public school quality (Arum, 1996; Couch, Shughart, & Williams, 1993; Dee, 1998; Hoxby, 1994a; Jepsen, 1999b; McMillan, 1998; Sander, 1999).⁴⁰ These papers share several features. First, they use the local percentage of enrollments in private schools as a proxy for the degree of competition in schooling markets. Second, they use multiple regression analysis to correlate this proxy with a variety of public school outcomes, conditional on student and family SES. Third, they recognize the inherent challenges to estimating the effects of competition in a regression framework with non-experimental data. Partial correlations between private enrollment shares and outcomes, even controlling for a wide range of student and family variables, are likely to yield biased estimates of the effects of competition.

The bias stems from two features of the analysis.⁴¹ First, the number of private schools in an area will depend on characteristics of the community, which are also likely to affect outcomes (perhaps schools are more likely to operate in relatively better-off communities). When correlating private enrollments and public school quality, researchers generally include a rich set of community controls in their models. Nevertheless, it is likely that controls will be imperfect.

If there are unobserved features of communities that are correlated with outcomes as well as private enrollments, then “competition” effects could simply reflect unobserved heterogeneity of communities.

Second, there may be greater numbers of private schools where public schools are of particularly low quality. Put another way, private enrollments may cause improvements in public school quality, but causality could also flow in the opposite direction. If the research framework does not account for this, then one risks the erroneous conclusion that greater competition leads to lower public school quality.

To address these biases, most authors use additional statistical techniques. They attempt to identify instrumental variables that are correlated with the key independent variable (the local percentage of private enrollments), but uncorrelated with variance in student outcomes that is unexplained by the other independent variables. If these conditions are violated, then we have less confidence that bias is ameliorated.

Using an instrumental variables approach, Couch et al. (1993) find positive effects of competition. However, their set of instrumental variables includes several socioeconomic characteristics that are probably correlated with student outcomes, thus invalidating their use as instruments. In another paper, Newmark (1995) finds that their results are not robust to alternative model specifications. Arum (1996) finds positive results that are small in magnitude, but these are suspect for two reasons. First, the analysis makes no attempt to address the previous biases. Second, the measure of private enrollments is aggregated to the state level, making the implausible assumption that every public school in a given state faces the same amount of competition from private schools.

Two authors find that competition improves measures of student outcomes when they use the local percentage of Catholics as instruments for private enrollment shares (Dee, 1998; Hoxby, 1994a). Despite their statistical significance, the magnitude of the estimates is modest. For example, a 10 percentage point increase in local private enrollments produces small gains in public school outcomes when gauged in standard deviation units (see Table 4).⁴² Furthermore, both authors assume that private enrollments increase as the number of Catholic adherents increases, but that the Catholic population share is uncorrelated with unexplained variance in outcomes. There is suggestive evidence, cited in the previous section, that the latter assumption is not tenable.

Other authors have been unable to identify competitive effects using the same instrumental variables strategy and additional data sets (Jepsen, 1999b; McMillan, 1998). In particular, Jepsen (1999b) uses two data sets, a variety of student outcome measures, and multiple measures of competition. Most of his estimates are not statistically different from zero. Given the inconsistent pattern of findings in the literature, there are two possible conclusions: (1) the effects of competition are small or zero, and (2) the current empirical strategies, particularly the use of Catholic densities as instrumental variables, are not appropriate for estimating the effects of competition in non-experimental data. Both are plausible, and they suggest that a fair amount of caution is warranted in extracting conclusions from these studies. In his own review, Jepsen (1999b, p. 20) feels that “the conclusions that can be drawn from the private school competition literature are limited.”

TABLE 4

A summary of research on private school competition and public school quality

Study	Data	Level of aggregation of data	Measure(s) of competition	Method	Instruments	Student outcomes	Effect on outcomes of a 10 percentage point increase in private enrollments
Jepsen (1999) ^a	NLS72 and NELS:88	Individual	% total private enrollment in zip code, county, and MSA; distance to nearest private school	Instrumental variables	Catholic population share and densities; Catholic church densities	Math, attainment, wages, high school graduation, college attendance	Mostly statistically insignificant
Sander (1999) ^b	Illinois	School	% total private enrollment in district	Instrumental variables	Catholic population densities	Math, graduation rates, % of seniors taking ACT	NS
McMillan (1998) ^c	NELS:88	School	% total private enrollment in district	Instrumental variables	Population share of Catholic, blacks, and college-educated; median county income	Reading scores	NS
Dee (1998) ^d	Common Core of Data	School district	% total private enrollment in county	Instrumental variables	Catholic population share	High school graduation rate	2.3 percentage points
Arum (1996) ^e	HSB	Individual	% total private enrollment in state	Ordinary least squares	n/a	Combined test scores	0.04 standard deviation

TABLE 4 (cont.)

Study	Data	Level of aggregation of data	Measure(s) of competition	Method	Instruments	Student outcomes	Effect on outcomes of a 10 percentage point increase in private enrollments
Hoxby (1994a) ^f	NLSY	Individual	% secondary private enrollment in county and MSA	Instrumental variables	Catholic population share and densities; Catholic church densities	Attainment, wages, Armed Forces Qualifying Test (AFQT)	0.33 years (attainment) 0.04 standard deviation (wages) 0.07 standard deviation (AFQT)
Couch et al. (1993) ^g	North Carolina counties	County	% total private enrollment in county	Instrumental variables	% college educated in county, per-capita county income	Math	0.90 standard deviation

Notes: NS indicates that the difference is not statistically significant at 5%. Abbreviations are as follows: HSB (High School and Beyond); NELS:88 (National Education Longitudinal Study of 1988); NLS72 (National Longitudinal Study of the High School Class of 1972); and NLSY (National Longitudinal Survey of Youth).

^aTable 6.

^bTable 3.

^cTable 7 (model 2).

^dTable 3 (model 3, 2SLS).

^eTable 4 (model 2). The unstandardized regression coefficient was divided by the standard deviation of the dependent variable (Table 2).

^fTables 8 and 9. The unstandardized regression coefficients were divided by the standard deviations of the dependent variables.

^gTable 2.

Note that prior evidence has focused exclusively on the links between competition and public school outcomes. Little has been said about the relation between competition and the costs of public schools, which is the ultimate concern. In a strictly-implemented voucher system, total spending is directly linked to student attendance and will decline by the amount of the voucher with the exit of each student. But vouchers need not be the sole source of financing. In Chile's national voucher system, for example, local municipalities can increase or decrease local contributions to schools, independently of voucher revenues. Under this regime it is not clear that total or per-pupil spending will either rise or fall.

In the United States, Hoxby (1996a) attempts to link the existence of higher private school tuition subsidies (intended to proxy vouchers) to per-pupil and total public school spending. In neither case does she find a statistically significant effect. This she attempts to explain as the "canceling out" of two forces (Hoxby, 1998). First, spending might decline because increased private enrollments lead to decreased voter support for public school funding (the typical source of efficiency gains posited by voucher proponents). Second, per-pupil public spending might increase because the exit of many students to the private sector has left additional resources for remaining public students. But her estimates could just as easily imply no effect at all. It is an open empirical question as to how private school competition could affect the costs of public schools.

Student Sorting and Peer Effects

Because vouchers induce some families to exit public schools, they encourage a process of student sorting. Many critics of vouchers presume that exiting students will be of higher socioeconomic status, a phenomenon typically referred to as cream-skimming. The following section assesses whether that is indeed the case. Sorting of any kind could affect student outcomes if students are influenced by the composition of their peer groups. Thus, the final section will explore the empirical evidence on peer-group effects.

The Effects of Vouchers on Sorting

Which families are most likely to exercise choice under vouchers? The following paragraphs call upon the limited evidence from the small-scale public voucher experiments in Milwaukee and Cleveland, as well as privately-funded scholarship programs. Indirect evidence is drawn from open enrollment plans that lift restrictions on public school attendance and a growing number of computational studies that forecast the impact of voucher programs on student sorting.

Since 1990 the Milwaukee Parental Choice Program (MPCP) has awarded vouchers for attendance at non-religious private schools to a small number of low-income children.⁴³ As might be expected, average incomes of applicants were lower than a random sample of all Milwaukee public students. Moreover, students were lower-achieving and more likely to be African-American than a sample of low-income students in public schools. However, the education of applicants' mothers was comparable or even a bit higher (Rouse, 1998a; Witte, 1996b; Witte & Thorn, 1996). The same studies suggest that choice parents were more involved in their children's education (as proxied, for example, by

parental involvement in school activities). A similar pattern was found in the Cleveland voucher program. Voucher recipients were more likely to be African-American than a random sample of Cleveland public school students, although the mothers of recipients had more formal education (Peterson, Howell, & Greene, 1999). Thus, research yields a mixed bag of results: These programs are clearly benefiting poor, black students, but these students may be slightly more advantaged in some respects than others who are eligible.

Another Milwaukee scholarship program, this one privately funded, was also begun in the 1990's. Partners Advancing Values in Education (PAVE) was limited to low-income students. Unlike the public program, there were no restrictions on attending religious private schools and the scholarship covered only a portion of tuition. The parental education of PAVE participants was strikingly similar to that of MPCP participants, and both were higher than a random sample of low-income Milwaukee public school students (Beales & Wahl, 1995). Moreover, PAVE families were less likely to be African-American. Yet another Milwaukee choice program allowed some inner-city children to attend suburban public schools. Witte and Thorn (1996) report that poor students are less likely to participate, despite state assistance with transportation costs.⁴⁴

Several cities have privately-funded scholarship programs similar to Milwaukee's PAVE. A widely publicized New York program offered students the opportunity to apply for private school scholarships. As in Milwaukee, the program was targeted at low-income families. In 1997 over 1,110 students took advantage of the scholarships. Compared to the eligible population, the parents of applicants were more highly educated (Peterson et al., 1998). While applicants were more likely to be African-American, they were less likely to speak Spanish in the home. Another private program in Indianapolis has yielded similar findings. Choice parents are low-income, largely because the program is means-tested, but they are also better-educated than the average Indianapolis public school parents (Heise, Colburn, & Lamberti, 1995).

Two separate programs in San Antonio have offered increased school choices to families. The first is similar to privately-funded program in other cities, offering private school scholarships to low-income students (Martinez, Godwin, & Kemerer, 1995). The second expands public school options by allowing some students to opt into special multilingual enrichment programs (Martinez, Godwin, & Kemerer, 1996). In the second program transportation is provided for, and no fees are required. In both choice programs, the individual probabilities of participating were positively related to parental education and educational expectations for their children (Martinez et al., 1996).

Since the early 1980's Scotland has allowed parents to apply for openings in public schools outside a student's attendance zone. The educational attainment of parents who chose new schools in Scotland was higher than that of parents who elected to remain in their schools, and school segregation by social class tended to increase over time (Willms, 1996; Willms & Echols, 1992). In 1991, New Zealand instituted a nationwide reform that eliminated neighborhood attendance zones for public schools. Fiske and Ladd (2000) found that, after the reform, families were most likely to gravitate from schools in the lowest deciles of socioeconomic status to higher-decile schools. Over time, they also found that ethnic minorities became increasingly concentrated in low-decile schools.

Further evidence on sorting is provided by computational models that simulate the behavior of parents and schools in schooling markets (Epple, Newlon, & Romano, 1997; Epple & Romano, 1998; Manski, 1992; Nechyba, 1996).⁴⁵ Both Manski (1992) and Epple and Romano (1998) find that sorting increases under vouchers, and that it most resembles cream-skimming. Their model results are driven by the assumption that peers-effects are important, explored more carefully in the following section. Manski's results show a declining fraction of "high motivation" children in public schools as vouchers increase in value.⁴⁶ In Epple and Romano's model, vouchers lead to increasing stratification by student ability and income.

Epple et al. (1997) are among the few authors to analyze sorting within schools, such as often occurs through tracking of students into high- and low-ability groups. They show that additional tracking in public schools tends to increase the size of the public relative to the private sector under a voucher plan. It does so by enticing high-ability students who would normally exit to avoid attending school with a less desirable peer group in the lower track, to remain in public schools. Strictly speaking, their model does not analyze how a voucher plan would alter patterns of tracking, because the degree of tracking is pre-established in their model. Nevertheless, it is reasonable to assume that public schools might strike a Faustian bargain with parents of high-ability students. That is, they might intensify tracking so as to prevent the exit of such parents under choice plans, so that even if vouchers do not increase sorting across schools, they may intensify stratification within schools.

In addition to sorting across schools, the introduction of vouchers could alter the residential decisions of families (Nechyba, 1996). He shows that vouchers, while perhaps increasing school-based stratification, might decrease residential stratification. The effect is driven by better-off families who move to poor neighborhoods. Because they are no longer constrained by attendance boundaries, they are able to take advantage of lower home prices while sending their children to private schools.

The Effects of Sorting on Outcomes

Let us assume that vouchers do encourage sorting by socioeconomic status or another variable. The final impact of sorting on student outcomes will depend on the existence and size of peer effects. Whether these effects are indeed the results of peer interactions is a matter of some debate. As Levin (1998, p. 382) notes, "it is not clear whether this effect comes from the influence of peers, school climate, teaching conditions, or differences in teacher expectations and curriculum."⁴⁷ Rather than resolve this debate, the following paragraphs will examine the current evidence on the existence of "peer-type" effects.⁴⁸

To estimate peer effects, researchers use non-experimental data to estimate the marginal effect of peer-group characteristics on individual outcomes, holding constant variables such as family and student socioeconomic background. For several reasons, this is fraught with methodological difficulties. First, the choice of peer measures is often ad-hoc and lacking in strong theoretical or empirical rationale (Jencks & Mayer, 1990). Typical choices include the average socioeconomic status and achievement of other students.

Second, the appropriate level of aggregation of the peer-group characteristic is not obvious. Researchers usually extract mean values of school-wide SES. Whether, in fact, this constitutes a “peer” group is little explored. To the degree that the peer group extends outside the school or includes a smaller subset of the school population, estimates of peer-group effects are potentially subject to bias from measurement error.

Third, the appropriate summary measure of peer characteristics is uncertain. It is usually assumed to be the mean SES of a student’s peers, although the relevant measure might be the 10th percentile, the 90th percentile, or some other measure. Glewwe (1997) cautions against simply considering the mean of peer characteristics. If children are disproportionately influenced by students at either end of the distribution, average peer characteristics will be misleading. In fact, Glewwe demonstrates that vastly different results emerge when alternative distributional assumptions are made.

Fourth, non-experimental estimates of peer effects are subject to omitted variables bias of several kinds. Evans, Oates, & Schwab (1992) suggest that typical estimates of peer effects capture variance in outcomes that is due to unobservable characteristics of individual students. They argue that some families choose schools based on desirable characteristics of potential peer groups, and that the same families possess unobserved characteristics that positively influence achievement. In this case, estimates of peer effects are biased because peer variables are correlated with unobserved individual determinants of achievement.

A similar bias occurs if superior teachers are rewarded with classes composed of privileged students (Argys, Rees, & Brewer, 1996). Peer quality reflects unmeasured teacher attributes that improve achievement. Conversely, school administrators may assign better teachers to the least privileged students, biasing effects in the opposite direction. *A priori*, it is difficult to predict the net bias in estimates of peer effects. To resolve these biases, researchers can apply additional statistical methods, such as instrumental variables. In this case, they must identify instruments that are correlated with peer-group characteristics, but uncorrelated with unexplained variance in student outcomes.

With these limitations in mind, we turn to the empirical literature. One of the first, and still largest-scale efforts to assess the magnitude of peer effects was that of James Coleman and his colleagues (Coleman et al., 1966). In carefully reviewing the Coleman results 25 years later, Jencks and Mayer (1990) conclude that school-level SES has limited effects on the academic achievement of white students and stronger effects on black students, after measures of individual SES are controlled. Since the Coleman report, a wide variety of studies have used essentially the same methodology. The majority have sought to link peer attributes to measures of achievement, and a few have analyzed other outcomes such as overall attainment. Several studies find that high levels of mean SES or achievement are associated with higher individual achievement (Henderson, Mieszkowski, & Sauvageau, 1978; Link & Mulligan, 1991; Robertson & Symons, 1996; Summers & Wolfe, 1977; Willms, 1986; Zimmer & Toma, 2000).

Other studies yield some positive results that are, nonetheless, inconsistent enough to give pause. Caldas and Bankston (1997) find that mean SES in-

creases achievement, but that the mean family incomes (proxied by the percentage eligible for free-and-reduced lunch) are negatively associated with outcomes. Bryk and Driscoll (1988) find a rather strong effect of mean SES on achievement that is counter-balanced by a negative effect of increasing mean achievement. Finally, Winkler (1975) finds that the percentage of low-SES students tends to lower the achievement of white students, but not that of black students.

There is only limited evidence on the effects of school-wide SES and achievement on other outcomes such as attainment. Both Mayer (1991) and Gaviria and Raphael (1997) find that advantaged peer groups tend to lower the probability of dropping out, while Bryk and Driscoll (1988) do not find the predicted influences of mean SES and achievement on attainment.

The majority of this research assumes that estimates of peer-group effects are not subject to biases of selection or omitted variables. However, Evans et al. (1992) demonstrate that typical estimates of peer-group effects may be subject to upward biases. In two instances of previously-cited work, the authors applied the familiar strategy of instrumental variables (Gaviria & Raphael, 1997; Robertson & Symons, 1996). In neither case, however, do the authors make a compelling case that their instrumental variables are uncorrelated with student outcomes.⁴⁹ This is clearly a priority area for additional empirical work.

A related literature explores the effects of sorting within the school, or tracking. These studies regress student outcomes on discrete measures of track placement, rather than the continuous measures of peer characteristics. Several papers using U.S. data find that moving students from low-ability tracks to high-ability tracks increases achievement, all else being equal. Gamoran and Mare (1989), analyzing HSB data, find that students in "college" tracks have higher mathematics achievement and overall attainment, *ceteris paribus*. For a representative sample of middle-school students, Hoffer (1992) establishes that students in high-ability classes have higher mathematics achievement than students in heterogeneous classes; the opposite holds for students in low-ability classes (findings are weaker for science achievement). Argys et al. (1996) find similar results with NELS:88 data.

Though authors correct for student selection into tracks based on unobservable characteristics of students, these corrections are only as good as the instrumental variables that are posited to affect track selection (but not achievement). Instruments include students' self-reported grade point averages (Hoffer), school-average characteristics (Argys et al.; Gamoran & Mare), and regional location (Argys et al.). Selection bias is ameliorated to the extent that variables identifying track assignment are not correlated with unexplained achievement. In each case, however, selection variables probably belong in achievement regressions. Only the analysis of Argys et al. (1996) controls for measured teacher characteristics. Thus, we have little guarantee that track effects do not simply reflect unobserved characteristics of the teachers assigned to those tracks.

A Summary of Findings

To assess the potential impact of vouchers, this review has explored three related issues. First, are private schools more efficient than public schools? Second, does competition from private schools lead to improvements in public

school efficiency? Third, do vouchers alter patterns of student sorting, and what are the effects of sorting on outcomes?

Private School Efficiency

There is mixed evidence that private (but mainly Catholic) schools in the United States improve student outcomes. The experimental results, which do the best job of accounting for selection bias, suggest that attending private elementary schools leads to modest mathematics gains for poor, minority students. The results appear to be driven by the upper elementary grades and by Catholic schools. The current evidence is limited to a single year, so we have no means of evaluating whether the results are cumulative over several years. In contrast, the evidence on reading is weak and inconsistent. The quasi-experimental evidence from Milwaukee, though based on a small subset of non-religious schools, is surprisingly consistent with these findings: There are math gains for some students, but no reading gains.

The evidence on secondary achievement is limited to non-experimental research. In general, this evidence does not suggest that private schools have strong effects. Numerous effects are statistically insignificant, and the positive effects are often small in magnitude. There are inconsistent patterns of effects for different social groups. In one case, Figlio and Stone (1999) find that religious schools have positive effects on the math achievement of urban minorities (similar to the elementary school research). In other cases, the effects are observed among different social groups and are statistically insignificant for minorities. In sharp contrast, the evidence on attainment suggests that Catholic secondary schools have consistent effects on improving rates of high school graduation and college attendance, especially for minority students in urban areas.

However, the usual approaches to correcting for selection bias in non-experimental research—relying upon instrumental variables associated with religious status—inspire little confidence and may exacerbate biases in favor of private schools. Alternative strategies to correcting for bias imply much less favorable results for private schools (Figlio & Stone, 1999). In light of this, the findings on secondary schools must be viewed with greater caution.

An efficiency analysis is incomplete without evidence on relative costs. On this point the literature provides few guides. Rough cost estimates generally favor private schools, but these are fraught with methodological problems. The best available evidence suggests that private cost estimates could be substantially biased by excluding contributed services of personnel, among other cost categories.

A more germane question, but one rarely posed, is whether or not the available evidence on effectiveness and costs provides any useful guide to the relative efficiency of private and public schools under a voucher plan. The current effectiveness of Catholic schools could stem from higher levels of academic course-taking, their communal organization, private sector autonomy, or peer effects. Despite assertions by advocates and critics, the best empirical evidence does not provide clear guides as to which is most relevant. Because of this, it difficult to predict whether newly created private schools, perhaps non-religious and for-profit, would produce effects similar to those of Catholic schools.

Competition and Public School Efficiency

Evidence on the effects of competition on public school efficiency is sparse. The usefulness of small-scale experiments is limited because only small numbers of students exit public schools, effectively muting any competitive pressures (a point often lost in the debate over programs such as Milwaukee).⁵⁰ Only a few studies have sought to examine how variation in the competitiveness of local schooling markets affects public school quality. Two find some evidence of competitive effects, but the magnitude of such effects is small, limiting their practical significance (Dee, 1998; Hoxby, 1994a). Several other studies, using similar data and methods, find no evidence of statistically significant links between competition and quality. The conflicting findings and difficult empirical issues involved should warrant a great deal of caution in extracting conclusions. There is almost no evidence on the potential effects of competition on public schools costs and, hence, efficiency.

Sorting and Peer Effects

On student sorting there is somewhat more consistent evidence. In several institutional contexts the first parents to take advantage of diminished constraints on choice are generally those with higher levels of parental education and involvement. This lends support to a cream-skimming account. Yet in a few suggestive instances, students who leave public for private schools are more likely to belong to minority groups or more likely to be lower-achieving. It is also worth noting that almost every study ignores the issue of residential sorting and how vouchers may affect home location decisions in addition to school choice decisions. (The work of Nechyba, 1996 is a notable exception.)

Several policy alternatives might affect sorting patterns, including vouchers that are restricted to low-income students, better information systems for eligible parents, or higher vouchers for poor students (e.g., Moe, 1995b). However, the evidence suggests that means-tested vouchers could still lead to cream-skimming, if only from a smaller and more disadvantaged pool of applicants. And by restricting the group of applicants, we may also limit the potential benefits of competition (because fewer students are empowered to exit public schools). Information systems or larger vouchers would imply greater costs to the education system, further altering the relative efficiency of schooling options, on which there is already little evidence (Levin, 1998).

How will sorting affect student outcomes? A simple reading of the evidence suggests that student achievement usually increases when the average socioeconomic status of peer groups increases, holding constant student and family SES. This is suggestive that the achievement of students who remain in public schools under a voucher plan might decline due to sorting, and the exit of higher-SES students. Nevertheless, there are serious shortcomings in these studies that should restrain generalization. First, complicated issues of student selection and omitted variables bias have received limited attention. When econometric corrections are made, they do not inspire confidence. Second, even less attention has been devoted to rationalizing the measurement and definition of peer characteristics.

Uncertain Effects

In sum, our ability to forecast the potential impact of large-scale voucher programs is not as impressive as many would claim. There is substantial uncertainty about the existence and magnitude of most effects. To be sure, we possess more knowledge in some areas than others. Advocates and opponents of vouchers have a tendency to focus on these pieces of the debate—to the neglect of all others—and lay claim to victory. In support of vouchers, advocates will often point to the positive effects of existing Catholic schools. But are they also less costly? And how valid is the analytical leap required to predict the future efficiency of private schools? On these points, evidence is lacking. Opponents of vouchers are quick to observe that cream-skimming is common in choice plans, in that children with better-educated parents are more likely to exit public schools. But while vouchers may lead to the exit of higher-SES students from public schools, we require better evidence that sorting will lower the achievement of remaining students (or that losses are not outweighed by competitive efficiency gains).

Future Research Directions

A complete assessment of large-scale voucher programs requires answers to the three questions posed. How might these answers be obtained? In some cases, further empirical study could be accomplished without great difficulty. There is a resounding lack of private and public cost studies, notwithstanding frequent—and tendentious—claims that private schools are less costly. We also have much to learn about the relationship between peer attributes and student outcomes, perhaps from more careful analysis of non-experimental data sets such as NELS:88, or even from unique natural experiments.⁵¹

However, the most convincing evidence will probably be drawn from an education system where vouchers have been (or will be) implemented on a large scale, such as that in Chile or in the United States. In Chile, ongoing research is directed at answering some of the questions already posed in this paper. Two papers compare the relative effectiveness and costs of public and private schools in the wake of 15 years of reform (McEwan, in press; McEwan & Carnoy, 2000). Another attempts to determine whether increasing local concentrations of private school enrollments evoked competitive improvements in public school efficiency, using longitudinal data on public schools (McEwan & Carnoy, 1999).

In the United States, it seems inevitable, whatever the opinions of academia, that a voucher program will eventually be implemented on a scale larger than Milwaukee or Cleveland. Large-scale programs might evoke systemic responses that others, by virtue of their smaller scales, simply cannot, such as competitive efficiency gains in public schools or extensive student sorting. The design of careful evaluations, perhaps with experimental components, should be a priority.

In Florida, a statewide plan awards vouchers to students in persistently failing public schools. An evaluation of the plan is being conducted by David Figlio, Dan Goldhaber, Jane Hannaway, and Cecilia Rouse. Charter school laws, particularly in Arizona and Michigan, have encouraged the rapid growth of privately managed and publicly funded schools. In many cases, schools are

managed by for-profit businesses, and these schools might be expected to resemble the private schools established under vouchers (certainly more than existing Catholic schools). However, the potential for empirical research in this area is barely tapped (for an exception, see Bettinger, 1999).

Notes

1. Levin (1992) and Lamdin and Mintrom (1997) provide additional discussion of the history of the voucher literature.

2. This model of parental demand is similar to that of Lankford and Wyckoff (1992).

3. Unless regulatory agencies provide information such as test scores, some authors have expressed concern that parents will have insufficient information to make decisions, or that information will be more accessible to parents of a higher socioeconomic status (Levin, 1991; Manski, 1992).

4. The following discussion relies on Hoxby (1996a, pp. 178-182).

5. In fact, there is very little research on the determinants of private school supply, and whether vouchers would elicit a large supply response. Downes and Greenstein (1996) find that private school location decisions in California are sensitive to the characteristics of local populations. In Chile, vouchers produced a large supply response, although new private schools were mainly non-religious and for-profit, and they avoided rural areas and the poorest urban areas (McEwan & Carnoy, in press).

6. In practice, it is difficult to randomly assign students to attend a particular school. Instead, recent experimental evaluations have accepted applications for private school scholarships (e.g., Peterson, Myers, & Howell, 1998). From the initial pool of applicants, some students are randomly awarded scholarships, and others are randomly denied scholarships. Although a large portion of scholarship recipients eventually choose to attend a private school, evaluators cannot force this decision.

7. For general discussions of social experiments, see Boruch (1997) and Orr (1999).

8. See Heckman (1979). For a general review of methods, see Vella (1998). For a discussion of selection bias corrections in the context of public/private comparisons, see Murnane et al. (1985).

9. There are evaluations of the Cleveland voucher program (Metcalf, 1999; Peterson, Howell, & Greene 1999), but I have chosen not to review this evidence. Both evaluations are hampered by the lack of adequate controls for student background. Thus, it is difficult to assess whether test score differences among voucher recipients and other students stem from private school effects or pre-existing differences.

10. I employ a level of statistical significance of five percent as a criterion for the reporting of private school effects. Otherwise, effects are reported as "NS", or not statistically significant at five percent. In some cases, this criterion leads to interpretations that differ from those of authors who utilize a less stringent standard (e.g., 10 percent). Furthermore, I consider the magnitudes of effects, in addition to whether they are statistically different from zero. When possible, I followed a common practice in the social science literature by expressing test scores as percentages of a standard deviation (and I note when it was not possible to do so). I expressed attainment as a change in the probability that an individual graduates from high school or attends college.

11. The discussion is placed apart for two reasons. First, the Milwaukee plan was limited to a quite small number of non-religious private schools, which may

limit its overall applicability. Second, it was the subject of a contentious debate, in which three evaluators used different data and methods to arrive at different conclusions. I shall attempt to gauge whether there is any consistency or logic to this pattern of findings.

12. All three studies are available as working papers on the website of Harvard University's Program on Education Policy and Governance (<http://www.ksg.harvard.edu/pepg/papers.htm>). As of this writing, only the New York study has been published (Peterson, Myers, Howell, & Mayer, 1999). However, there are some minor discrepancies in sample sizes and results between the published version and the original first-year report (Peterson et al., 1998). In the absence of clarification, I have relied upon the original report. In the Dayton and Washington evaluations, there is an unresolved empirical issue. The authors chose to exclude students from the analysis with test score gains of greater than two standard deviations or losses of greater than 1.5 standard deviations. By itself, excluding outliers is not controversial, although the evaluations provide no rationale for the *asymmetrical* exclusion of outliers. This could potentially alter the results, although I have no means of investigating this further. After going to press, second-year results for the three experiments became available. However, these could not be included in the discussion.

13. Another reviewer makes a similar point, using a more limited set of attainment studies (Neal, 1998).

14. Even so, they still estimate a bivariate selection model, because private school attendance is a non-linear function of the variables. However, allowing identification of the selection model to rest purely on functional form has little basis in theory, and generally produces inflated standard errors.

15. Using similar instruments, Evans and Schwab (1995) and Neal (1997) find the same pattern.

16. Also see Rouse (1998b) for an excellent comparison of the three approaches.

17. See their Table 4.

18. See his Table 5.

19. See her Table 7; the percentile gain is divided by the standard deviation of the dependent variable, reported on p. 584.

20. As Terry Moe (1995a, p. 19) observes "...any assessments of performance, attrition, parent satisfaction, and the like turn almost entirely on how those three schools are doing. This is hardly a solid basis for evaluating the effects of vouchers. In fact, it verges on the ridiculous."

21. I do not wish to suggest that these explanations are exhaustive. For example, Rouse (1998b) suggests an explanation that is specific to the Milwaukee voucher program. She provides suggestive evidence that better math achievement in Milwaukee choice schools might be due to their lower pupil-teacher ratios. An anonymous reviewer of this paper notes that private school teachers may spend less time on classroom management and discipline, and more time on student instruction. In part, this focus may stem from the different socioeconomic characteristics of students in private schools. In a sense, therefore, it is another form of selection bias.

22. For some evidence on this, see Hannaway (1991).

23. There is evidence of this type of supply response in several contexts. For-profit educational management organizations now operate a large portion of publicly-funded charter schools in states like Arizona and Michigan. In Chile, a large-scale voucher plan has existed for two decades. Chile is a staunchly Catholic country, and one might have expected that the Church would be a primary engine for the growth of new private schools. It turned out, however, that non-

religious, for-profit schools were the most active participants in the emerging educational market (McEwan & Carnoy, 2000).

24. Recent work by Bettinger (1999) suggests that test scores of charter students in Michigan did not improve, and may have declined relative to those of public school students. In Chile's voucher plan, the evidence suggests that Catholic voucher schools are slightly more effective than public schools. In contrast, non-religious schools that emerged under the voucher plan are similarly effective, or somewhat less effective (McEwan, in press; McEwan & Carnoy, 2000).

25. See their Table 2.5.

26. See Levin (1998) for further discussion on this point.

27. See Tsang (1988; 1995) for a general discussion.

28. Bray (1996) surveys educational cost studies in nine East Asian countries. He finds that direct private costs as a percentage of total costs in public primary schools range from less than 10 percent in Lao PDR to over 70 percent in Cambodia. Most hover around 20 percent. Evidence in McEwan (1999) suggests that direct private costs account for around 44 percent of total costs in Honduran primary schools. Case studies of several African and Asian countries show that families assume between 40 and 81 percent of public school costs (Mehrotra & Delamonica, 1998).

29. See Chambers and Bobbit (1996). Whether teachers work on a "contributed services" basis is self-reported, and the salary comparison holds constant a large number of personal and job characteristics of teachers.

30. See Bartell (1968), Table III-12.

31. See their Table 3.1.

32. See Duncombe et al. (1996) for a general exposition of educational cost functions.

33. Other studies have used cost functions to compare the relative efficiency of other public and private organizations, such as child-care facilities (e.g. Mocan, 1997).

34. He notes that "preliminary findings imply that it may be inappropriate to compare private and public sector average costs and infer that any differentials are due to 'inefficiencies'" (Jimenez, 1986, p. 35).

35. One implication of this, as noted by an anonymous reviewer, is that vouchers may be prohibitively expensive in rural areas where extensive transportation services would be required to support family choice.

36. No monitoring problems are assumed to exist. Further, student utility functions incorporate the "motivational" level of other students in the school, reflecting the importance of peer effects in student decisions.

37. In their model, student utility is also affected by the changing composition of the student body. Moe and Shotts (1995), in a reanalysis of Manski's model, find that utility losses in this respect are somewhat outweighed by benefits due to public school quality improvements. These results, as Manski's, are dependent on the particular functional forms of utility and objective functions, as well as assumed parameter values.

38. Also see Rangazas (1997).

39. Kane (1996) makes a similar observation.

40. An additional strand of empirical literature explores the efficiency-enhancing effects of competition among public schools, although I shall not review that literature (Blair & Staley, 1995; Borland & Howsen, 1992; Hoxby, 1994b; Zanzig, 1997).

41. See Dee (1998) or Hoxby (1998) for further explanation.

42. While arbitrary, the gain of 10 percentage points is not inconsistent with enrollment gains in one of the only large-scale voucher plans such as Chile (McEwan & Carnoy, 2000). Even if the enrollment share increases by 20 percentage points, the overall gains in public school outcomes are still modest.

43. Private schools received payments equivalent to the per-member state aid of Milwaukee Public schools (Witte, 1998).

44. As in the MPCP, students with bilingual or special educational needs could be excluded.

45. Authors utilize similar approaches. Public schools, private schools, and parents are assumed to maximize a set of objectives under various constraints. Parents, for example, choose the school type that maximizes their overall utility, constrained by income. In part, utility is determined by a school's peer quality. Models are formalized in a set of behavioral equations, and further assumptions are made regarding parameters such as the relative size of peer effects. Models derive their assumptions about peer effects from the empirical literature discussed in the following section.

46. The effects on income stratification are more ambiguous. Under his assumption of public sector efficiency, the fraction of low-income children is strictly increasing in public schools; the result is not robust when public sector inefficiency is assumed.

47. See Levin (1998) and citations therein.

48. In this literature there is a somewhat artificial barrier between "tracking" and "peer effect" studies (many of the former appear in sociology journals and the latter in economics journals). Though methodological approaches are sometimes different, both attempt to gauge the influence of the mix of a student's school-based peer groups on outcomes. A distinct set of studies, reviewed elsewhere, searches for "neighborhood" effects (Brooks-Gunn, Duncan, & Aber, 1997; Jencks & Mayer, 1990). These studies correlate measures of neighborhood-wide socioeconomic status with student outcomes.

49. As an instrument for school characteristics, Robertson and Symons (1996) use the characteristics of the region in the United Kingdom the student was born in. Unfortunately, they do not provide a clear rationale why the region of birth should be correlated with peer group characteristics, but uncorrelated with outcomes. Gaviria and Raphael (1997) estimate the effect of other students' drop-out behavior on individual drop-out decisions. They instrument this peer measure with variables that reflect the aggregate characteristics of other students, such as the proportion of each student's classmates that live in single-parent families. However, it seems quite plausible that the chosen instrumental variables also influence achievement.

50. Hoxby (1996a) makes a similar point.

51. For example, recent papers in economics have explored peer-effects among college students, relying upon the random assignment of college roommates (e.g., Sacerdote, 2000).

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