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School Choice and Student Performance: Are Private Schools Really Better?

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Abstract

Are private schools really better than public schools, or is it simply that better students attend private schools? Although a number of recent studies find that students perform better in private schools (more specifically, Catholic schools), others do not. Typically, however, the instruments used to adjust for nonrandom selection are weak. This study employs uniquely detailed local instruments and jointly models selection into religious and nonreligious private high schools, relative to public high schools—improving instrument power in predicting private sector attendance to roughly three times that of prior studies. Failing to correct adequately for selection leads to a systematic upward bias in the estimated treatment effect for religious schools, but a downward bias for nonreligious private schools. With adequate correction, religious schools are modestly inferior in mathematics and science, while nonreligious schools are substantially superior. However, minority students, particularly in urban areas, benefit from religious schools. Other factors that may make both religious and nonreligious private schools attractive include possibly better retention rates, increased security and discipline, and greater opportunities for a variety of specialized school-day and extracurricular activities.

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1. INTRODUCTION

Religion, politics, and schools are a volatile mixture in the national debate over various proposals for school choice, proposals in which students would no longer be constrained to attend the public-school district in which they live, but could instead use government-supplied vouchers or tuition subsidies to offset the costs of attending a private school. The national debate has reached the level of presidential politics and led to well-known private-school choice experiments such as those currently underway in Milwaukee, Wisconsin, and Cleveland, Ohio.

Underlying the school-choice movement is the widely held belief that private schools respond to competition in ways public schools do not, and consequently are superior to public schools in providing educational services. Some basic empirical evidence seems to bear out this contention: Private-school students routinely perform at a higher level on standardized tests and are more likely to graduate from high school and attend college than their public-school counterparts, even with many other observed differences, such as family income, parental education levels, and school inputs, held constant. Indeed, despite substantial performance differentials between public- and private-school students, private schools, especially religious schools, generally spend considerably less per pupil than do public schools. If private-school students perform at higher levels at lower cost, proponents argue, clearly private schools are superior to public schools.

Our purpose here is to investigate the relative academic performance of public and private schools, relying upon uniquely detailed data and model specifications. A number of other studies have also taken up the question of whether private schools are truly better than public schools. Early studies (e.g., Coleman, Hoffer, and Kilgore 1982; Coleman and Hoffer 1987; and Chubb and Moe 1990) compare public and Catholic schools, often finding that Catholic schools outperform public schools, even with the inclusion of extensive controls.[1] Critics of these findings (e.g., Goldberger and Cain 1982) argue that they might be driven by selection bias: Students and parents, by the very act of seeking out a private school, may signal attributes, both observed and unobserved, that are conducive to higher educational achievement. If so, then models that do not control for this nonrandom selection will lead to upward-biased estimates of the treatment effect of private schools. In fact, this argument is the primary objection raised by many of the critics of school-voucher proposals. Opponents argue that public funding for private-school attendance will drain public schools of many of the best students, leaving public schools with a disproportionate share of the students most difficult to educate. Proponents counter that the largest gains for students in private schools appear to be for low-achieving, low-income, and minority students.

Recently several authors have sought to control for sector selection in modeling the treatment effect of private schools. For instance, Evans and Schwab (1995, 1996), Sander and Krautmann (1995), Sander (1996, 1997), Goldhaber (1996), and Neal (1997) compare the effects of public and Catholic schools (or all private schools, in the case of Goldhaber) on standardized test scores, high-school dropout probabilities, and other outcomes. These authors use a variety of instruments to identify the selection into Catholic (or private) schools, including average tuition levels, religious affiliation, percentage of Catholics in the county of residence, the density of Catholic schools in the area, and interactions between religion and the region of the country and urbanicity, depending upon the study. Results of these efforts are mixed. Evans and Schwab (1995, 1996) and Neal (1997) find strong evidence that Catholic schools increase student achievement, especially for minorities and initial low achievers, but Sander (1996) finds

[1]Related studies in sociology include Noell (1982) and Bryk, Lee, and Holland (1993). Witte (1996) provides a critical summary of recent comparisons of public and private schools.

no significant effect. Goldhaber (1996) finds little evidence of either a positive treatment effect for private schools (religious and nonreligious combined) or of significant selection bias.

The mixed evidence on the effects of private schools (and particularly, Catholic schools) may be due to differences in dependent variables or particular samples, but may also be due to differences in the instruments used by the varying authors to identify sector selection. In particular, the differences may be due to the presence of weakly correlated instruments. Bound, Jaeger, and Baker (1995) illustrate how weak instruments may lead to biased estimates of treatment effects. Indeed, in our data, previous authors' specifications rarely explain a substantial portion of the selection into the relevant private-school sector, raising the possibility that the weak-instrument critique may hold for this literature. This point is corroborated in work by Ludwig (1997), who also finds compelling evidence that prior authors' instruments are weak.

We use data from the National Educational Longitudinal Survey (NELS) administered by the U.S. Department of Education's National Center for Education Statistics (NCES) to measure public- versus private-school differences in student mathematics and science performance.[2] In doing so, we offer two principal innovations to the literature comparing public and private schools. First, we permit greater heterogeneity in treatment effects by distinguishing selection into and effects of religious and nonreligious private schools, not just religious schools (or private schools as a group, in the case of Goldhaber 1996). To estimate a multisector model that jointly models selection and student performance, we use a two-stage polychotomous choice method suggested by Lee (1982) and described by Maddala (1986). We first estimate a multinomial logit model in which public schools, as well as religious and nonreligious private schools, are separate sectors, and then use a polychotomous choice equivalent to an

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[2]Researchers commonly focus on mathematics (and science, when available), because schools appear to be relatively more important for these areas than to, say, reading, where raw differentials are typically much smaller (Madaus et al. 1979), and because achievement in these areas is often linked most strongly to post-schooling earnings (Murnane et al. 1995).

inverse Mills' ratio to correct for selection in switching regressions representing the achievement function for the school sector the student actually attends.

The second innovation is that our instrument set for selection into private-school sectors has roughly *three times* the incremental predictive power of the set of *all* instruments used in previous studies. The improved power in accurately predicting religious and nonreligious private-sector choice is attributable to our unique data. The restricted-access version of the NELS identifies individual public schools, but not private schools. Even so, we are able to use school-specific information reported in the NELS to match private schools in the NELS to those reported in a veritable census of private schools maintained by Dun and Bradstreet. Once the locations of the private schools are identified, one can then use detailed community-specific variables from a variety of supplemental sources to identify the selection across public schools and both religious and nonreligious private schools.

Based upon our findings, the large, positive differences in test scores observed for students in religious schools appear to be due in large measure to differential selection into that sector. With no correction for nonrandom selection, large, significantly positive treatment effects in mathematics and science achievement persist, even after we control for a standard set of student, family, and other variables. However, for religious schools positive treatment effects do not remain after selection correction with our detailed instruments. In fact, estimated religious school treatment effects are significantly *negative,* though only modestly so, in both mathematics and science. Estimates for a variety of student subgroups also indicate that failing to correct for selection leads to measurably overstated estimates of the religious-school treatment effect. The treatment effect does remain significantly positive for black and Hispanic students, particularly those in urban areas. Estimated treatment effects for nonreligious private schools, however, become even more positive in both mathematics and science after we control for nonrandom selection, suggesting that nonrandom selection arising from unobserved attributes is negative in this sector. In a final exercise, we reconcile these results with those of other authors by demonstrating how discrepancies are due to either the choice of instrument sets or the failure to distinguish between religious and nonreligious private schools, not to choice of sample data or school-outcome variable.

2. SCHOOL CHOICE AND SAMPLE-SELECTION BIAS

Table 1 illustrates the large performance differentials for students in public and private schools. It presents unadjusted logarithmic differences in 12th-grade test scores in mathematics (and science) for religious and nonreligious private schools relative to public schools based upon data from the NELS. In mathematics, for example, the 11.5 (approximate) percentage-point difference in scores for religious schools is large, roughly equivalent to the full two-year average gain in test scores between the 10th and 12th grades for all students. Similarly large gaps are observed for science scores. The public-private differentials persist regardless of the way in which we split the sample, as shown in Table 1.

These comparisons, however, are unlikely to yield an accurate representation of differences in school quality. Students who attend public and private schools differ in systematic ways. Some of these differences are accounted for by the standard control variables used to predict student achievement (e.g., prior achievement level, family background variables, and school characteristics), but these observable characteristics are unlikely to fully capture the nonrandom differences in sector selection. Unless they do, the estimated treatment effects of private schools will be biased. As noted earlier, several authors have recently jointly modeled sector selection and student achievement. In all cases but one, Goldhaber (1996), these authors have restricted their comparison to public and Catholic schools.[3] Goldhaber compares public schools to both Catholic and nonreligious private schools, but assumes that selection

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[3]We exclude, here, studies of the ongoing Milwaukee school-voucher program (e.g., Witte 1994; Rouse 1996), which excludes participation of religious private schools.

TABLE 1

Logarithmic Differences in Average 12th-Grade Mathematics Scores (Comparison Group: Public Schools)

Group	Religious Private vs. Public Schools	Nonreligious Private vs. Public Schools
Full sample	0.127**	0.258**
	(0.013)	(0.017)
Black and Hispanic students	0.132**	n/a
	(0.033)	
Income > $50,000	0.038	0.112**
	(0.024)	(0.019)
Income < $25,000	0.129**	0.275**
	(0.023)	(0.046)
Top third, 8th grade	0.027**	0.037**
	(0.009)	(0.009)
Bottom third, 8th grade	0.095**	0.258**
	(0.024)	(0.057)
12th-grade science score,	0.073**	0.196**
full sample	(0.008)	(0.011)

**Significant at the 5 percent level.

Notes: Standard errors are in parentheses beneath estimated logarithmic differences. An entry of 0.115 signifies an approximate 11.5 percent difference between the two sectors. Entries of n/a indicate that the estimate is not available due to excessively small cell size. Sample includes students who attend the same school in both the 10th and 12th grades.

into Catholic and nonreligious private schools is identical (i.e., he uses the inverse Mills' ratio derived from a selection equation that pools Catholic and nonreligious private schools as the selection term in separate achievement equations for Catholic and nonreligious schools).

Unlike prior studies, we estimate a model in which selection and achievement equations differ explicitly for three sectors: public schools and religious and nonreligious private schools. Distinguishing the three sectors avoids confounding potential differences among the sectors in either selection or achievement. We experiment with other distinctions, such as separating Catholic schools from other religious schools, but the latter sector is very small. One cannot reject the null that selection into Catholic and other religious schools is the same. One can, however, reject the null that selection into religious and nonreligious schools is the same. Hence, given the available evidence, the three-sector approach seems the appropriate one to employ.

Selection-Correction Procedure

Maddala (1986) describes an estimator for a model with polychotomous choice, first suggested by Lee (1982). In the current application, school selection s takes a value j={0, 1, 2}, representing the three possible school sectors in our model. The selection equations should be viewed as behavioral reduced-forms, reflecting the influence of both individual choice and potentially endogenous or nonrandom sector supply. Thus, school selection is determined by

$$Prob[s_i = j] = \frac{\exp(\alpha_j \ s_i)}{1 + \sum_{k=1}^{3} \exp(\alpha_k \ s_i)}$$

Denote as P_j the derived probability that individual i={1,...,N} selects school sector j. The second-stage achievement equation in sector j is

$$y_j = \beta_j'x + \gamma_j \lambda_j + \eta_j$$

where individual subscripts are suppressed, y is a school outcome, x is a vector of control variables, β and γ are parameters, η is an error term, and

$$\lambda_j = \frac{\phi(\Phi^{-1}[P_j])}{P_j}$$

The terms ϕ and Φ, respectively, are the standard normal pdf and cdf. Denoting by R_j the second-stage regressor matrix (including λ_j), one can express the asymptotic covariance matrix as

$$C = (R'_j R_j)^{-1} \; [\sigma_j^2 R'_j (I - \rho_j^2 \Delta_j) R_j \; + \; \gamma_j^2 F_j \Sigma F'_j] \; (R'_j R_j)^{-1}$$

where $\Delta_j = \text{diag}(\delta_{1j}, ..., \delta_{Nj})$, $\delta_{ij} = \lambda^2_{ij} + \Phi^{-1}(P_{ij})$, Σ is the asymptotic covariance matrix of estimated $\alpha = [\alpha_1, ..., \alpha_3]$, and $F_j = R'_j L_j$, where L_j is a matrix of the derivatives of the λ's with respect to the logit parameters. Though this approach is straightforward, and has been used extensively in the urban economics literature in particular, we also experiment with more restrictive instrumental-variables specifications suggested by Dubin and McFadden (1984). These IV specifications yield comparable results.

Empirical Specifications

Two sets of variables are required to implement the empirical model: variables that appear in both the second-stage achievement functions and the polychotomous-choice model, and those excluded from the second stage but used to explain sector selection in the first stage. Each instrument for sector choice satisfies two criteria. It has significant *incremental* power (at the 5 percent level or better) in explaining sector selection in the multinomial logit model, but no significant independent explanatory power (even at the 10 percent level) in an academic achievement function that controls for private-school selection.[4] Therefore, all the identifying instruments are significantly correlated with sector selection, but any significant effect on student achievement in mathematics or science is transmitted only through sector selection.

We adopt a standard set of variables for inclusion in the second-stage achievement regressions, one roughly comparable to the sets used in other recent studies of public and private schools. As with other studies, these equations yield predictions of the achievement gain in each sector for a student with specific characteristics, without school-based inputs held constant. Hence, they are not solely measures of “technical” efficiency. The conceptual experiment is to place otherwise equivalent students in different school environments. Appendix 1 offers descriptive statistics and data sources for all variables included in either the first or second stages. The dependent variable in the second-stage regressions is the natural logarithm of the NELS 12th-grade item response theory (IRT) mathematics or science test.[5] To ensure that each student attends the same school for the period of measured academic achievement, the sample includes all NELS students who attended the same public or private school in both 10th and 12th grades. Sample attrition and dropouts are examined in Section 3.

The control variables in the second-stage achievement equations are dummy variables for gender, race, Hispanic ethnicity; interactions between gender and, in turn, race and ethnicity; the family’s socioeconomic status (a continuous variable constructed by NCES from parent-reported data on family income, parental education, and parental occupation); a dummy variable for a two-parent household; a dummy variable for whether the parent(s) attends religious services “often” or “frequently”;[6] seven

[4]Tests of this criterion are possible because of overidentification, i.e., because the rank of the instrument set exceeds the rank of the required sample-selection terms (in addition to nonlinearity).

[5]The IRT tests, where students get modified examinations depending upon their previous examination scores, are used in the NELS to reduce the problems associated with ceiling and floor effects.

[6]Sander (1997) shows that differentiating between Catholic and non-Catholic religiosity is important when using Catholic religion as an instrument for sector selection. This distinction, while significant in the student achievement equations, does not change our estimated private school treatment effects.

religion variables (Catholic, Baptist, fundamentalist Christian denominations, Jewish, Muslim, Eastern religions, and no religious affiliation, with other religious affiliations, mostly Protestant, as the omitted category), the natural logs of the student's relevant 8th- and 10th-grade test scores, and nine Census-subregion dummies reflecting regional variations in achievement.[7]

The sector-selection equations include all variables in the second stage, as well as other variables that should influence sector selection. These identifying instruments are suggested by Constitutional and other limitations on public schools (e.g., religious instruction), heterogeneity in the service population for the locally provided public education, as well as variables likely to magnify the importance of local heterogeneity. Some of these variables are motivated by work in industrial organization by Downes and Greenstein (1996) on the entry of private schools into local markets, and by other recent work on public-private school choice (e.g., Lankford and Wyckoff 1992; Lankford, Lee, and Wyckoff 1995). Hence, the instrument set includes the urbanicity of the school's county (variables reflecting central or suburban counties in a Metropolitan Statistical Area), three school-availability variables (separate variables for whether the county has a Catholic private school, a nonreligious private school, or a private school affiliated with the student's stated religion),[8] and variables reflecting the demographic and economic characteristics of the county and characteristics of the schools in the county.[9] In addition, the instrument set includes variables reflecting the state's percentage of private manufacturing workers who are unionized, and the degree to which the state constitution or legislation permits teacher unionization.

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[7]Evans and Schwab (1995, 1996) use Catholic religion as an instrument. However, in our data, the instrument validity tests suggest that religion belongs in both the first- and second-stage equations. Murnane, Newstead, and Olsen (1985) and Sander (1996) report a similar finding.

[8]We also estimate specifications in which we only consider students with the relevant choices in their counties. Our results do not change.

[9]In specifying the relevant geographic area, we assume that students go to school in the county where they live—on the presumption that most parents look at private schools beyond their immediate neighborhood but, at the other extreme, do not consider all schools in a large multicounty metropolitan area. We know where public-school students live, but must impute the county of residence for private-school students. Hence, we exclude military and other boarding schools to ensure reliable imputations.

These last variables are included to account for the possibility for differential public-private selection depending on the union status of the local public schools.[10]

County demographic and economic characteristics used as instruments in the first-stage selection model include the county population, median household income, dummy variables denoting whether the student's family income exceeds $50,000 or $100,000, percentage of residents in poverty, percentage of residents who are nonwhite, percentage of adults with a bachelor's degree, and the violent crime rate.[11] To account for interactions between various measures of heterogeneity in the local population and individual family characteristics, we also include interactions between median household income and dummy variables for whether the student's family income exceeds $50,000 or $100,000 (in 1988); the percentage nonwhite and the student's race; the percentage of adults with bachelor's degrees and a dummy variable reflecting whether both of the student's parents have bachelor's degrees (or, in the case of a single-parent household, whether the custodial parent has a bachelor's degree). County school characteristics include a Herfindahl index of the concentration of public-school districts in the county,[12] and the average student-teacher ratios for the public- and private-school sectors in the county, weighted by enrollment. In addition, we control for the dispersion of 1990 house prices in the county by including the coefficient of variation.[13]

Again, the final set of instruments satisfy two criteria: Each instrument offers significant incremental explanatory power in determining sector choice, but does not directly enter the second-stage

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[10]In a later section, we also explore the potential nonrandom selection or endogeneity of union status, as well as differences in treatment effects by union status.

[11]Family income is reported in the NELS only as a categorical variable.

[12]Hoxby (1994) finds that concentration affects student achievement levels. While we also find this result in a model that does not correct for selection, this result is no longer present when we correct for selection. Hence, it appears that school concentration affects student achievement, at least in our data, primarily via sector selection.

[13]We exclude mean or median housing price, however, because school quality is likely to be endogenously capitalized into local housing prices.

achievement equations at a significant level. Consequently, the estimates are relatively insensitive to the inclusion or exclusion of individual instruments.

Predictive Power for Sector Selection

County-level variables can be included in the sector-selection equations because we identify not only the public schools in the NELS, but also, with virtual certainty, the locations of the private schools, which are not otherwise identified in the NELS. To identify locations of private schools, we use reported characteristics common to both the NELS school survey and those in a veritable census of private schools produced by Dun and Bradstreet.[14]

The payoff to using these detailed geographic-specific characteristics, rather than the instruments used by previous authors, is substantial, as can be seen in Table 2. The first row of Table 2 presents the percentage of Catholic-school students (or all private-school students) that a model including all of the control variables used in the second-stage achievement function regressions would predict to select into the Catholic- (or all private-) school sector, in *two-sector models* akin to those used by prior authors.[15] The control variables correctly predict 5 percent of the Catholic-school students and 21 percent of all private-school students. The next five rows of Table 2 present these "percent correct" calculations, and the incremental predictive improvement based upon the instrument sets, in turn, of Evans and Schwab (1995, 1996), Sander and Krautmann (1995), Sander (1996), Neal (1997), and Goldhaber (1996), for the private-school sectors they consider.[16] The incremental predictive power of these instrument sets ranges

[14]Due to data confidentiality requirements, we are not permitted to reveal herein the algorithm used to identify private schools. Those with authorized access to the NELS may request details.

[15]Results are essentially identical if we expand the Catholic-school sector to include all religious private schools. The first column of Table 2 reports Catholic school results to permit direct comparisons for studies that considered only Catholic schools.

[16]In addition to the percentage of Catholics in the county, Evans and Schwab (1995, 1996) also use Catholic religious affiliation of the family as an instrument. Table 2 counts Catholic religion as a control variable, not an instrument, consistent with our instrument validity tests. See footnote 6.

TABLE 2

Performance of Instrument Sets Used to Identify Selection (Percentage Correctly Predicted Based upon NELS-88 Data)

Variables Used	Catholic Schools	Incremental Improvement Over Base Controls	All Private Schools	Incremental Improvement Over Base Controls
Base controls (includes religion)	5	—	21	—
Evans/Schwab (1995,1996) instruments	8	3	n/a	n/a
Sander/Krautmann (1995) instruments	5	0	n/a	n/a
Sander (1996) instruments	11	6	n/a	n/a
Neal (1997) instruments	10	5	n/a	n/a
Goldhaber (1996) instruments	7	2	29	8
Pooled set of *all instruments* above	18	13	29	8
Instruments used in this paper	38	33	49	28

Note: An entry of n/a indicates that the study did not apply to that sector. Neal (1997) includes some variables as regressors that are included in our instrument list.

from 0 to 8 percentage points, depending on the instrument set and private-school sector in question.[17] Since previous authors use different sets of instruments, one can also gauge the incremental predictive power of the set of *all* prior instruments used by previous authors. This pooled set of instruments yields a 13 percentage-point improvement over the control variables in predicting Catholic-school enrollment and an 8 percentage-point improvement in predicting private-school enrollment. Ludwig (1997) uses a different approach, but arrives at a similar conclusion—that prior authors' instruments may not be particularly strong indicators of sector selection.

Contrast these incremental predictive improvements with those obtained from our set of instruments: 33 percentage points for the Catholic-school sector (2.9 times the incremental power of the set of all previously used instruments) and 28 percentage points for all private schools together (3.9 times the incremental power of Goldhaber's (1996) instruments). If, instead, we were to compare the incremental amount of the variance explained, we would find that while previous authors' instrument sets improve R^2 by between 0.005 and 0.04 (the controls explain about 21 percent of the variance in the Catholic-public choice and 16 percent of the variance in the overall private-public choice), our instrument set improves the pseudo-R^2 by about 0.14 in the Catholic-public choice and by about 0.19 in the private-public choice. Hence, our instruments substantially improve sector prediction over those used by previous authors.

Multinomial-logit estimates require the assumption of independence of irrelevant alternatives (IIA). Given the nature of school-sector selection, it is questionable as to whether a four-sector selection model necessarily satisfies this criterion. The null hypothesis of independence of irrelevant alternatives is rejected at the 4.9 percent level, which in practice is not an egregious violation of the IIA assumption. Also, one can assess the logit estimates by examining how many of the students predicted to choose one

[17] This characterization of Neal's (1997) instrument set is not quite correct, since several of our instruments, most notably the urban status variables, are also used by Neal as second-stage variables. Neal's variable set, including instruments, correctly explains about 15 percent of selection into Catholic schools in our data.

sector are then predicted to switch to another sector when a third sector is omitted as a choice. This group of "IIA violators" is quite small. Omitting private nonreligious schools as a choice, for example, results in a sector switch for less than one-half of 1 percent of the students (12 of the 4,442 initially assigned to the public sector, and 10 of the 383 assigned to the religious school sector). Consequently, second-stage estimates, too, are essentially unchanged.

First-Stage Results: Determinants of Sector Selection

While the purpose of this paper is to evaluate the relative performance of public and private schools, the determinants of sector selection are interesting in their own right. Therefore, while we do not present the full set of parameter estimates from the multinomial logit sector selection model in this paper (interested readers may request them from the authors), a brief discussion of some of the first-stage results is in order.

Unsurprisingly, given the large improvements in sector-selection explanatory power reported in Table 2, our community-level variables are significantly related to sector choice. The signs on the coefficients in the sector-selection equations are consistent with our priors. For instance, the higher the crime rate in the county, the more likely families are to send their children to private schools in either sector, all else equal. The more concentrated are the public schools in the county, or the higher the *public* school student to teacher ratios, the more likely parents are to send their children to *private* schools, and particularly, private nonreligious schools. The higher the *private sector* student-teacher ratios in the county, the more likely parents are to send their children to *public* schools. Students who have a local religious-school option in their religion are more likely to attend a religious private school and are less likely to attend a nonreligious private school, all else equal.

Also important are the *interactions* between community characteristics and the student's own characteristics. For instance, while minorities and whites are equally likely to attend private school, all else equal, their responses to increases in the community's minority population apparently differ substantially: the larger the minority fraction in the population, the more likely that white students will attend a private school, and particularly a nonreligious private school. Parents with bachelor's degrees are typically more likely to send their children to private school, but this tendency is diminished (particularly for religious school selection) as the fraction of adults in the community with bachelor's degrees increases. Similarly, while high-income parents are more likely to send their children to private schools, their tendency to do so (for nonreligious schools, at least) decreases with the median income in the community. Therefore, we find strong evidence that parents are responsive to community characteristics, and particularly to interactions between community characteristics and their own characteristics, when choosing their children's schooling sector.

3. ESTIMATED PRIVATE-SCHOOL TREATMENT EFFECTS

We are interested in addressing the question, How much will public-school students with a particular set of individual and family characteristics improve his or her mathematics or science test performance if the student moves to either a religious or nonreligious private school in the relevant sector? Here, we assume that the student only takes his or her own characteristics to the private school, but takes along no school or peer-group characteristics. Again, our conceptual experiment is to place otherwise equivalent students in different school environments. The calculated treatment effect of a particular sector k is $\sum_c(\beta_{ck} - \beta_{c0})\cdot x_{c0}$, for all second-stage regressors c, where β_{ck} and β_{c0} are regression coefficients (including the constant terms) in sectors k and 0 and the x's represent mean values of the public sector.[18]

[18] Results are virtually unchanged if the x's are taken from the relevant private sector.

Table 3 presents estimated treatment effects (and asymptotic standard errors) in mathematics and science of religious and nonreligious private schools. The first column reports the raw differences in test scores, repeated from Table 1. The second column presents the estimated treatment effects with controls for observed differences in characteristics but without correction for selection. The third column reports the estimated treatment effects in a model that corrects for selection. The fourth and fifth columns repeat columns two and three, with 10th-grade test scores excluded from the model, so that the achievement reflects the full high school gain from the 8th to 10th grade. Appendix 2 reports sample sizes for these and other specifications below.

When one fails to correct for sector selection, the religious-school treatment effect is positive and statistically significant for both mathematics and science test scores. All else equal, these results suggest that public school students will on average perform 2.4 percent better in mathematics and 1.4 percent better in science if they move to a religious private school. However, estimated treatment effects for nonreligious private schools are small and statistically insignificant. After selection correction, however, the positive religious-school treatment effects go away, due to significant positive selection into the sector. In fact, results in column 3 suggest that students in public schools perform somewhat *worse* in mathematics and science if they move to a religious private school, -7.0 and -2.4 percent, respectively, in mathematics and science, measured relative to the public sector.[19] On the other hand, there is significantly negative selection into nonreligious private schools, and estimated treatment effects for increase relative to those in column 2. They are now significantly positive for both mathematics and science (2.2 and 2.7 percent, respectively, relative to public schools).

[19]Much of the policy debate has centered on mathematics and science, but we also estimate differences in reading, correcting for sector selection. These auxiliary estimates are insignificantly positive for nonreligious schools and negative for religious private schools, in comparison to public schools.

TABLE 3

Estimated Treatment Effects by School Sector, With and Without Sector-Selection Correction (Comparison Group: Public Schools)

	Raw Score Difference (1)	Estimated Treatment Effects (10th to 12th Grades) No Correction (2)	Estimated Treatment Effects (10th to 12th Grades) Correction (3)	Estimated Treatment Effects (8th to 12th Grades) No Correction (4)	Estimated Treatment Effects (8th to 12th Grades) Correction (5)
I. Math Test Scores					
(1) Religious vs. public	0.127**	0.019**	-0.070**	0.011*	-0.089**
	(0.013)	(0.005)	(0.005)	(0.006)	(0.006)
(2) Nonreligious private vs. public	0.258**	0.005	0.022**	0.053**	0.073**
	(0.017)	(0.006)	(0.005)	(0.008)	(0.008)
II. Science Test Scores					
(1) Religious vs. public	0.073**	0.016**	-0.024**	0.031**	-0.059**
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
(2) Nonreligious private vs. public	0.196**	-0.006	0.027**	0.045**	0.114**
	(0.011)	(0.009)	(0.007)	(0.010)	(0.010)

*Significant at the 10 percent level; **significant at the 5 percent level.

Notes: Treatment effects are the estimated (logarithmic) percentage improvement in 12th-grade test scores predicted for an average student in the unionized public sector who switches enrollment to the relevant other sector. An entry of 0.019, for example, signifies an approximate 1.9 percent difference between the sectors. Asymptotic, robust standard errors are in parentheses beneath the estimated treatment effects. See text for details, Appendix 1 for variable definitions,, and Appendix 2 for sample sizes.

Eighth- to Twelfth-Grade Achievement

The preceding estimates control for the student's 10th-grade test scores, but suppose that most of the value added of private schools occurs in the first two years of high school. This might occur for many reasons, perhaps because for many students the transition from middle school (or junior high school) is a critical period, or because the peer effects of private schools are strongest early on. If so, then controlling for the 10th-grade score would understate the true effect of private high schools. Alternatively, much of the benefit of high schools might accrue in the latter two years for other students. High-achieving students, for example, might be more challenged in the latter two years, as they have the opportunity to study at an advanced level. In addition, the peer-group effects for students who complete the latter two years may be stronger because some disruptive or low-achieving students may have dropped out, since the second year of high school often coincides with the end of mandatory attendance requirements.

To address these and other possibilities, one can repeat all analyses, this time omitting 10th-grade test scores from both the selection and second-stage regressions, so that achievement reflects the full high-school period. In this case, the selection-corrected estimates, presented in column 5 of Table 3, are slightly more negative than before for religious schools in both mathematics and science, but are much more strongly positive for nonreligious schools. The latter result suggests that the first two years in a nonreligious private school are more important than the last two, as compared to public schools—perhaps due to differential patterns in peer-group effects or to greater specialization available in the last two years of public high schools. Dropouts from public high schools, for example, are particularly low achievers, in comparison both to continuing students in public high schools and even to dropouts from private schools, based upon initial 8th-grade test scores.

Based on these results, we can draw several tentative conclusions. First, the private-public raw score differences are much larger for both religious and nonreligious private school sectors than are the estimated treatment effects without corrections for nonrandom selection. That is, most of the raw difference in test scores is due to differences in observable characteristics between the public and private sectors. However, religious-school treatment effects, in either mathematics or science, are even lower after we correct for selection, while nonreligious private-school treatment effects are higher. Hence, failure to correct for selection leads to an overstatement of the religious-school treatment effects in both mathematics and science, but to an understatement of treatment effects for nonreligious private schools, especially for gains from the 8th to 10th grades. Significantly, these estimated treatment effects are qualitatively the same if one restricts the sample to exclude public-school students who live in counties where no private schools are present, or if one substitutes the 10th-grade test score as the dependent variable to avoid potential selection problems with dropouts in the last two years of high school (since mandatory attendance laws typically apply through the 10th grade).

Urban Differences in Treatment Effects

Although urban status is a valid exclusion restriction in our data, in that urban variables are not significantly associated with student achievement after correcting for selection, we experiment with specifications that allow public-private treatment effects to differ by urban status.[20] One might speculate, for example, that treatment effect differentials are larger in densely populated urban areas, i.e., that private schools are relatively better than public schools in populous urban areas than elsewhere. Many critics of public schools suggest, for example, that urban public schools, particularly in the most densely populated areas, are particularly ineffective, for a variety of reasons.

Estimated treatment-effect differentials are virtually identical, however, whether or not we control for urban status directly in the second stage, or estimate entirely separate models looking only at

[20]Neal (1997) suggests that religious treatment effects may vary substantially across different types of urban areas.

students residing in either large metropolitan areas or large central cities.[21] After correction for selection bias, then, private-school treatment effects do vary considerably depending upon the urban status of the community in question. If urban public schools are particularly ineffective, the private schools in the same area appear to be affected too, so that treatment differentials are unaffected.[22]

Because our instruments rely on the county as the geographical unit of variation, our results might be sensitive to the potential for endogenous household location choice within a multicounty metropolitan area.[23] Ideally, we could jointly model household location choice and school sector selection, but the instruments needed to do this are not apparent. Instead, we attempt two methods of "aggregating over" the potential problem of endogenous location choice. First, we replace all county-level instruments with metropolitan area-level instruments. Second, we replace all county-level instruments with instruments based on the county and all of its contiguous urban neighbor counties (for urban counties only). In both cases, the estimated treatment effects of religious and nonreligious schools are extremely similar to those where we use county-level geographic variation for identification. The only difference is that while the "replacement" specifications still yield strongly statistically significant results, they are somewhat less precisely estimated than before (the standard errors are, on average, 15 percent higher). Hence, it is very unlikely that our results are being driven by intraurban household location choice.

Union-Nonunion Differences in Treatment Effects

Until now, we have assumed that differences in unionization patterns of public schools are not important in measuring the difference in treatment effects of public and private schools. Work by Eberts

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[21]Private nonreligious school treatment effects are slightly—but statistically significantly—higher in large central cities than in the general population.

[22]The invariance of estimated treatment differentials with respect to urban status also suggests, at least indirectly, that intraurban household location decisions are unlikely to affect treatment differentials.

[23]Thanks to Dennis Epple for pointing out this possibility to us.

and Stone (1987) and more recently Hoxby (1996) and Peltzman (1996), however, suggests that union and nonunion schools differ systematically. If so, then public-private achievement differences may also depend on the union status of public schools.

To address this possibility, we explore several specifications to gauge the union-nonunion difference in the private-public treatment effect: (1) introducing a union-status dummy variable to the second-stage achievement regression for public schools; (2) estimating a four-sector model, similar in all respects to the three-sector model above, except that we distinguish the union and nonunion public sectors in both the selection and achievement equations; and (3) estimating three-sector models, as above, but in which we consider selection and achievement separately in states with schools dominated by teacher unions versus states with very few teacher unions.[24]

In all three cases, we find that the private-public school treatment effects differ significantly in statistical terms, depending on whether the public sector is unionized or not, but that the magnitude of the differences is small. Private-school treatment effects are generally between one and two percentage points larger (or less negative) in union public schools, as compared to nonunion public schools, significantly so in each case at the 10 percent level or better.[25] Therefore, the estimated religious-school treatment effect remains significantly negative relative to both union and nonunion schools, but is modestly less negative when the relevant comparison is to the nonunion sector.

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[24]A state is designated as “union-dominated” if the state has a duty-to-bargain law; among schools in our sample, over 95 percent of public school students in these states attend unionized schools. A state is “not union-dominated” if it does not have such a law; fewer than one-third of sampled students in these states attend unionized schools. As an even more restrictive threshold, we consider whether the state has no duty-to-bargain law, but does have a right-to-work law. In these states, fewer than 15 percent of our sampled students, almost exclusively in cities, attend unionized public schools.

[25]While this result is consistent with the findings of Hoxby (1996), who estimates a negative union effect on achievement for public schools, it is not directly comparable, since we do not estimate differences in *technical* efficiency (i.e., achievement differentials with school input levels held constant).

Student-Level Heterogeneity in Treatment Effects

Average treatment effects may not be representative for all population subgroups. Therefore, we explore heterogeneity in treatment effects by repeating the entire analysis for various subgroups. Table 4 reports estimated treatment effects in mathematics for five distinct subgroups: black and Hispanic students; students with family incomes over $50,000 and below $25,000; and students in the bottom and top thirds of the 8th-grade mathematics test score distribution. The entire analysis is repeated for each subgroup, i.e., we estimate a new two-stage model using only the observations in the relevant subgroup. Sample sizes for each subgroup are reported by sector in Appendix 2.

Religious-school treatment effects, corrected for selection, remain significantly negative for both low-achieving and low-income students. High-achieving and high-income students appear not to fare any better or worse in religious schools, relative to public schools. For black and Hispanic students, however, estimated treatment effects are significantly *positive*, and particularly strong for science achievement. Although only 8th- to 12th-grade treatment effects are presented in Table 4, results for 10th- to 12th-grade treatment effects generally follow similar patterns.

The positive effects of religious schools for black and Hispanic students are consistent with Neal (1997), who finds that Catholic schools have a positive, significant effect on black and Hispanic student performance (measured as a reduced risk of dropping out)—but primarily in big cities—and no substantial effect for the general student population. Evidence presented by Neal suggests that the positive gains to black and Hispanic students who attend religious schools arise from the poor quality of public-school alternatives in large cities. Most of the minorities in our sample come from urban areas; however, given the importance of Neal’s result, we also investigated whether the minority treatment effect is different in urban areas, or in large cities. We find that the religious school treatment effect is even stronger for blacks and Hispanics in urban areas, and particularly in large central cities, though these results are based on very small sample sizes. Hence, while there is apparently no difference in the

TABLE 4

Alternative Specifications of Private-School Treatment Effects (8th to 12th Grades)
(Comparison Group: Public Schools)

	Estimated Treatment Effects (Mathematics)		Estimated Treatment Effects (Science)	
Specification	Religious Private (1)	Nonreligious Private (2)	Religious Private (3)	Nonreligious Private (4)
(1) Black, and Hispanic students	0.139**	n/a	0.110**	n/a
	(0.020)		(0.021)	
(2) Income > $50,000	-0.012	0.007	-0.018	0.054**
	(0.013)	(0.012)	(0.018)	(0.014)
(3) Income < $25,000	-0.074**	0.057**	-0.058**	0.131**
	(0.013)	(0.019)	(0.015)	(0.026)
(4) Top third, 8th-grade scores	-0.066**	0.003	-0.083**	-0.032**
	(0.006)	(0.008)	(0.011)	(0.010)
(5) Bottom third, 8th-grade scores	-0.078**	0.118**	-0.034**	0.114**
	(0.020)	(0.027)	(0.017)	(0.021)

* significant at the 10 percent level, * indicates significant at the 5 percent level.
n/a = Model not estimated because 10th-grade scores are excluded.

Notes: The treatment effects are the estimated (logarithmic percentage improvement in 12th-grade test scores predicted for the mean student (in the relevant group) in the public sector who switches enrollment to the religious sector. An entry of 0.139, for example, indicates an approximate 13.9 percent difference between the sectors. Asymptotic, robust standard errors are in parentheses below the estimated treatment effects. See text for details, Appendix 1 for definitions of variables, and Appendix 2 for sample sizes.

general population between the religious school treatment effects across urban status, we find that, consistent with Neal (1997), for blacks and Hispanics there do seem to be real differences across urban status.

The relatively positive effects of religious schools for students from high-income families appear to stem from the fact that these students tend to attend higher-quality religious schools, as measured by a number of factors. Relative to the religious schools attended by students with parental incomes below $50,000, for example, the religious schools attended by high-income students pay teachers 14 percent more (throughout the salary schedule), have 16 percent smaller classes, and send 15 percent more of their students on to four-year colleges and universities. In addition, among Catholic-school students, high-income students are much more likely to attend a religious-order school, rather than a diocesan school. (Differences are significant at the 1 percent level.)

Nonreligious private-school treatment effects, corrected for selection, remain significantly positive for 8th- to 12th-grade achievement for both low-income and initially low-achieving students, but are substantially lower for high-income or initially high-achieving students. (High-achieving students may actually fare worse in science.) These differential treatment effects by income and achievement level could arise from strong peer-group effects in nonreligious private schools, or perhaps from more specialized courses available to advanced students in public high schools. We should emphasize, however, that the private nonreligious school sample sizes for the low-income and low-achievement groups are very small (47 and 39, respectively). The positive effects for low-income and low-achieving students, however, are also consistent with positive treatment effects estimated by Rouse (1996) for elementary school participants in the Milwaukee school-choice program.

In sum, the estimates provide strong, consistent evidence that failure to correct for selection leads to a substantial overstatement of the religious-school treatment effect. If anything, religious private schools are less productive than public schools in either mathematics or science. However, religious schools do seem to have positive treatment effects for black and Hispanic students. Our results, therefore, call into question previous findings of a positive overall treatment effect for religious schools, but are in partial agreement with the findings of Neal (1997). Private nonreligious schools, on the other hand, appear to have positive treatment effects for most students, primarily for low-income and initially low-achieving students.

Reconciliation with Prior Results

Our results differ substantially from those of a number of prior studies, perhaps most notably Evans and Schwab (1995, 1996), who find significantly positive treatment effects for Catholic schools, and Goldhaber (1996), who finds no significant treatment effect for private schools. However, the data differ (we use NELS, while most others, except Goldhaber, use either the High School and Beyond or the National Longitudinal Survey of Youth, both from earlier periods); the dependent variables differ (Neal 1997, and Evans and Schwab 1995, use the probability of dropping out of high school as their dependent variable); and the specification and instrumentation of school sectors differ. Results here would be bolstered by additional evidence that differences are due to more precise specification and estimation of sector selection, rather than to idiosyncratic differences in data or dependent variable.

One possible reconciliation with Goldhaber's results is straightforward, because he pools the selection equations for religious and nonreligious private schools, finding no significant selection in the second-stage achievement equations. Here, we specify separate selection equations for religious and nonreligious private schools, and find significantly positive selection into religious schools, but significantly negative selection into nonreligious schools. Hence, pooling the two private-sector selection equations appears to confound the opposing positive and negative selections into religious and nonreligious schools, respectively.

To reconcile our results with those of Evans and Schwab and others, we reestimate the models, applying to our data and dependent variable the instruments used by previous authors whose results are at odds with ours. Consider an approximation to the Evans and Schwab specification, in which one uses Catholic religion and Catholic concentration in the county as the instruments.[26] When one adopts this identification strategy, the estimated religious- (or Catholic-) school mathematics and science treatment effects are 2.1 and 2.8 percent, respectively, relative to public schools, both statistically significant at conventional levels. These results are consistent with the Evans and Schwab finding of a positive treatment effect for Catholic schools, as well as negative selection into Catholic schools. With Sander's (1996) instruments (i.e., interactions between Catholic religion, urbanicity, and region), the estimated treatment effects in mathematics relative to public schools are 3.2 percent. While Sander's reported positive treatment effect for Catholic schools is statistically insignificant, his estimated treatment effect increases when correcting for selection, which is also what we find when his instruments are applied to our data and dependent variable. Therefore, a substantial part of the differences between the findings here and those of previous authors are attributable to differences in the variables used to instrument for sector selection.

Another major difference between the approach taken here and that taken by Neal (1997) or Evans and Schwab (1995) is that they estimate the probability of dropping out of high school, rather than using test scores, as the performance measure for schools. Evans and Schwab correctly note that high school graduation is strongly related to future labor market success, but there is also substantial evidence that cognitive test scores are related to labor market success (e.g., Bishop 1989; Murnane, Willett, and Levy 1995). Furthermore, Neal and Johnson (1996) show that black-white differences in test scores explain a substantial portion of the black-white wage gap. That said, the NELS at this time does not

[26]For consistency with Evans and Schwab, we exclude religion from the second-stage in this exercise.

permit one to measure with certainty whether students eventually drop out of high school. Definitive evidence of high school completion will come with release of the 1994 wave of the NELS (two years after on-time graduation), but the 1994 NELS wave was not available at the time of this writing. Therefore, to date, NELS users can only observe dropouts before the 12th-grade survey was conducted.

Despite the fact that dropout probabilities through graduation are not available in the NELS, one can observe whether or not students drop out before the 12th-grade survey. Thus, one can reestimate the full model, using a second-stage linear probability model with heteroskedasticity-corrected errors, where the dependent variable is the probability of dropping out before the 12th-grade survey and the control and sample-selection variables are as before.[27] In this specification, religious-school students are 1.15 percentage points less likely to drop out by the 12th-grade survey than public-school students. While this result is not statistically significant (the standard error of the estimated treatment effect is 1.140), it provides at least some suggestive evidence that religious schools may be modestly more likely to keep students enrolled in school than are public schools. However, our point that Evans and Schwab (1995) may overstate the religious school treatment effect is apparently valid here too: the estimated treatment effect of religious schools is more than twice as large in magnitude (and is statistically significant) if we use Evans and Schwab's instrumenting strategy in this application. Once the 1994 wave of the NELS is released, it will be possible to gauge with more reliability whether private schools are associated with an increased probability of high school completion, and one can explore this possibility.

<u>More on the Validity of Catholic Religion as an Instrument</u>

Estimated treatment effects of religious schools are significantly positive when Catholic religion is used as an instrument and excluded from the achievement equation, but negative when it is not

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[27]The use of a linear probability model in the second stage permits an expedient correction for sample selection for the binary dropout variable; in this case, correction for heteroskedastic errors is appropriate.

excluded from the achievement equation. What might cause this divergent result? Suppose that different types of Catholics select into Catholic schools than into public schools. If Catholics with stronger unobserved abilities or motivation tend to select into Catholic schools rather than into public schools, then it becomes easier to reconcile our results to those of Evans and Schwab.

Catholic students as a group perform at a significantly lower level on eighth-grade standardized tests than non-Catholic students, even after we control for the wide range of observable demographic and economic characteristics used in our achievement equations. This result squares with the finding of negative selection into Catholic schools reported by Evans and Schwab and others. But are the one-quarter of Catholics who go to Catholic school equivalent to the three-quarters of Catholics who go to public school in terms of ability or motivation (after one partials out observable demographic characteristics)? The answer in our data is no. Catholic students who attend Catholic schools initially perform at a 3 percent higher level (statistically significant) in the 8th grade than Catholic students who attend public schools, with other observable characteristics held constant. If one fails to control for these other characteristics, the initial test-performance differential is even more stark—11 percent, and significant at any reasonable threshold. So among Catholic students, there is evidence of positive selection into Catholic schools.

One can certainly heap criticism on a test of this sort. It is likely that children attending Catholic school in the 8th grade are also more likely to have gone to Catholic elementary schools. (We have no way of gauging this in our data.) If this were the case, then one could still possibly interpret the results from the preceding paragraph as evidence that Catholic schools make a positive difference, but earlier in the schooling process—not in high schools. Catholic children who are high achievers by the 8th grade may have gotten there because they attended Catholic schools. If so, one would expect that the initial performance differential among Catholics to persist even for students who select into Catholic schools for reasons that are plausibly exogenous.

Instead, we find that the differential narrows when we examine specific, plausibly exogenous motivations for enrolling in Catholic schools, suggesting that there is unobserved positive selection into Catholic schools. We observe, for example, parental responses to questions about the importance of a religious college environment for their child. While this exercise is also not perfect, it may be instructive. The positive gap in initial achievement between Catholics attending Catholic schools and those attending public schools is only half as large among students whose parents state that a religious college environment is very important, as among those whose parents say it is not. An even sharper distinction is evident if one splits the Catholic sample by those parents who say that a low-crime environment is very important, versus those who do not. Indeed, there is no difference in initial achievement between Catholics attending Catholic and public schools who say that a low-crime environment is very important for their children, while the performance difference is over 3 percent, all else equal, for those who say that a low-crime environment is not very important. Hence, arguments that these 8th-grade differences are due primarily to Catholic elementary school attendance seem unlikely.

Another way of gauging whether Catholics who attend Catholic school are more motivated than Catholics who attend public school may be to observe usage of libraries and museums. Catholic students who attend Catholic schools have parents who are 14 percent more likely to borrow books from libraries and 37 percent more likely to visit art museums than are Catholic students who attend public schools. Since both parental library usage and museum visitation are strongly positively correlated with initial student achievement, this pattern provides additional evidence that Catholics who attend Catholic schools are of higher ability than are Catholics who select into public schools. Moreover, we find that, regardless of quartile of 8th-grade achievement (with observed characteristics partialed out, as before), Catholic students who attend Catholic schools are still significantly more likely than other Catholic students to have parents who borrow library books or visit art museums.

In sum, while our conclusions regarding the role of Catholic religion in sector selection and achievement are not ironclad, we contend that the suggestive evidence presented above points in our favor. The fact that in our data Catholics have lower initial test performance, holding constant observable characteristics, provides a potential explanation for why studies that use Catholic religion or percentage Catholic in the county as instruments might find evidence of negative selection into Catholic schools. But there exists considerable heterogeneity among Catholics, and we find evidence that Catholics who send their children to Catholic school may be more motivated, or more academically oriented, than Catholics who send their children to public schools. Hence, we find positive selection into the religious sector.

4. SOME QUESTIONS—AND POTENTIAL ANSWERS

Our key results raise at least two questions worthy of further pursuit. First, why do public schools compare so favorably, or at least as well, to religious schools in mathematics and science achievement? Second, why do parents choose to send their children to private schools even when there are no positive differences in academic performance between public and private schools for their children, i.e., for most students attending religious schools and high-income and initially high-achieving student subgroups in nonreligious private schools?

One can address the first question by exploring some of the ways in which public and religious schools differ. Evidence on science instruction may be particularly informative. In the NELS data, public and religious schools are equally likely to have a science major teaching science courses, but in other dimensions there are considerable differences. With standard controls for observable demographic characteristics, for example, students in religious schools in our sample take significantly fewer high school science courses, according to transcript data. Moreover, when they *do* take science courses, the courses they take appear to fall short of science courses in public schools along a number of dimensions. Compared to science classes in religious schools, for instance, science classes in public schools average 11.3 percent more class time per week and 11.6 percent more lab time, and are 41.4 percent more likely to have experiments at least twice per week. All of these results are statistically significant at the 1 percent level.

In addition, Betts (1996) reports that student academic achievement is strongly positively related to the amount of homework assigned, provided that some of the work is graded and returned. Although science teachers in public and religious schools in the NELS assign about the same amount of homework per week, science teachers in public schools are 9.6 percent more likely to grade and return homework than their religious-school counterparts. Therefore, based on this evidence, public-school students may have higher-quality science instruction than that received by religious-school students.

While there are not as many objective measures of differences in mathematics quality between public and religious schools as for science quality, we find similar differences with regard to mathematics quality. As in the case of science, after controlling for observable demographic characteristics, we find that students in religious schools take significantly fewer college-preparatory mathematics courses, have 8 percent less weekly mathematics class time, and are less likely to have homework graded and returned than are students in public schools. Hence, the public-religious school differences apparent with science instruction are visible with mathematics as well.

The next question involves why parents might send their children to private schools, even if public school productivity is as high as or higher than that of private schools for their children. One answer is that student academic achievement is but one of many outcomes of schooling about which parents might care. For instance, parents might seek a more disciplined environment for their children, might desire for their children to have a religious education, or might desire a higher probability that their children will be able to participate substantively in extracurricular activities. In addition, parents might wish for their children to interact with a certain peer group. Peer group differences are clear: Religious schools, and nonreligious private schools in particular, have students with significantly higher than average socioeconomic status levels and who are significantly more likely to have plans for college. Religious preferences are also apparent: Parents of students who attend religious schools are substantially more likely to be religiously active than their public-school counterparts. Presumably, religious instruction or at least a religious environment makes religious schools more attractive to such parents.

Other suspected differences are apparently present as well, in both religious and nonreligious private schools. Private schools tend to have substantially higher discipline levels than public schools: Private schools in our sample, for instance, are 245 percent more likely than public schools to expel students for possession of alcohol, 170 percent more likely to expel students for injuring others, and 62 percent more likely to expel students who bring weapons to school. These heightened discipline policies likely lead to an increased sense of security that most parents desire for their children. Beyond these disciplinary and security issues, we find that private-school students are 28 percent more likely than public-school students to participate in extracurricular sports, cheerleading, school government, school yearbook or newspaper, or school-sponsored musical or dramatic activities, and are 48 percent more likely to spend at least five hours per week on extracurricular activities (both significant at the 1 percent level). Hence, private-school students apparently have more opportunities to participate meaningfully in a broader range of other school-day and extracurricular activities.

Therefore, even when private schools are not more beneficial academically in terms of mathematics and science achievement, they appear to offer other advantages (e.g., perceived peer-group benefits, religious education, opportunities for extracurricular participation, and increased discipline and security) that are also important to many parents. Thus, it is not surprising that parents may still choose to send their children to private school, even if there is no advantage to their particular children in terms of standard academic achievement.

5. CONCLUDING REMARKS

Private schools may have a number of advantages over public schools. They offer, for parents who desire it, considerably more religious education than would be available in public schools. Private-schools offer a potentially different peer group than do public schools: Private-school students are more likely to have college ambitions and come from high-socioeconomic status families. Private schools offer a more disciplined (and probably more secure) learning environment, and offer more opportunities for meaningful participation in extracurricular activities for the students who desire to do so. It is also widely believed that private schools are generally academically superior to public schools.

We find evidence to support this last belief, but only if the private school is nonreligious. Only for minorities do we find that religious private schools outperform public schools, after we correct for sector selection. We find a positive religious-school treatment effect for black and Hispanic students for gains from the 8th to 12th grades, a result consistent with Neal’s (1997) finding that Catholic schools particularly benefit urban black and Hispanic students. We also find evidence that religious schools may benefit students from families with high incomes, in part because high-income families select higher-quality religious schools. However, in all other population groups that we investigated, we found negative, or at best statistically zero, estimated effects of religious schools. Therefore, we have little reason to believe that religious schools lead in general to higher academic achievement than do public schools.

Evidence that nonreligious private schools outperform public schools, however, is substantially stronger. In the general population, the estimated nonreligious private-school treatment effect is positive, particularly in the early high-school grades. Consistent with recent work on the Milwaukee school-choice program, the positive effects are most substantial for low-income and initially low-achieving students. The beneficial effects of nonreligious private schools for these groups in the early high school grades suggest that peer-group effects disproportionately benefit these students, and also perhaps that public high schools serve advanced students relatively well in the latter high-school years.

But even a negative religious-school treatment effect may not be inconsistent with religious schools being better than public schools in another academic dimension. Private schools—and particularly, religious schools—tend to spend far less per student than do their public-school counterparts. If religious schools yield nearly comparable performance at lower cost, then one might still conclude that they are more cost-efficient than public schools. Our results seem to indicate that the results of religious-school competition, if present, may be found predominantly in the cost, rather than the quality, dimension of education provision, at least in terms of standard academic achievement.

Finally, our results should be used with caution if applied to the voucher debate. The estimated treatment effects only simulate the effect of moving a *marginal student* from the public sector to the private sector (or vice versa). Thus, characteristics of each school (including peer characteristics associated with that school) are unchanged. A voucher system, however, would likely change substantially the composition of public and private schools, e.g., peer-group effects on achievement might deteriorate in both sectors. In particular, the strong positive treatment effects of nonreligious private schools, which appear to be concentrated among low-income and initially low-achieving students, may be especially sensitive to composition changes if the benefits arise from strong peer-group effects.

APPENDIX 1
Summary Statistics and Data Sources for Explanatory Variables

Variable Definition	Data Source	Mean	Standard Deviation	Mean (Nonunion)	Mean (Union)	Mean (Religious)	Mean (Nonreligious)
Female	Student	0.51	0.50	0.51	0.52	0.47	0.45
Black	Student	0.09	0.29	0.17	0.07	0.08	0.03
Hispanic	Student	0.10	0.31	0.13	0.11	0.09	0.02
Log 8th-grade math	NELS	3.63	0.33	3.56	3.61	3.71	3.91
Log 10th-grade math	NELS	3.82	0.32	3.76	3.80	3.91	4.07
Black * female	Student	0.05	0.22	0.09	0.04	0.05	0.01
Hispanic * female	Student	0.05	0.22	0.07	0.05	0.04	0.00
Two-parent household	Student	0.88	0.32	0.87	0.88	0.91	0.92
Parent often attends religious services	Parent	0.45	0.50	0.52	0.40	0.57	0.36
Socioeconomic status	Parent data	0.23	0.80	0.05	0.13	0.54	1.14
Catholic religion	Student	0.36	0.48	0.30	0.35	0.66	0.17
Baptist religion	Student	0.16	0.36	0.29	0.13	0.06	0.10
Fundamentalist religion	Student	0.07	0.26	0.07	0.08	0.04	0.03
Jewish religion	Student	0.03	0.17	0.01	0.02	0.05	0.14
Muslim religion	Student	0.003	0.05	0.00	0.004	0.00	0.01
Eastern religion	Student	0.02	0.13	0.01	0.02	0.01	0.01
No rel. affiliation	Student	0.02	0.13	0.02	0.02	0.00	0.01
Central county in MSA	Census	0.66	0.47	0.66	0.64	0.69	0.83
Suburb. county in MSA	Census	0.21	0.41	0.16	0.22	0.30	0.11
Presence of Catholic school in county	D&B	0.75	0.43	0.56	0.79	1.00	0.80
Presence of nonreligious school in county	D&B	0.75	0.43	0.77	0.70	0.83	1.00
Presence of school in student's religion in county	D&B/student	0.54	0.50	0.58	0.55	0.31	0.71
Herfindahl index of public school concentration	CCD	0.41	0.35	0.52	0.32	0.48	0.54

(table continues)

APPENDIX 1, continued

Variable Definition	Data Source	Mean	Standard Deviation	Mean (Nonunion)	Mean (Union)	Mean (Religious)	Mean (Nonreligious)
County population (in millions)	Census	0.98	1.78	0.66	0.96	1.54	1.35
Median household income in county ($10,000's)	Census	3.21	0.86	2.81	3.35	3.34	3.26
Percentage poverty in county	Census	17.46	9.19	22.02	14.80	18.07	20.48
Percentage nonwhite in county	Census	24.69	18.34	28.74	19.39	32.91	37.65
Percentage in county with bachelor’s degree	Census	22.97	8.33	21.45	22.79	24.93	26.71
Percentage with bachelor’s in county * parents have bachelor’s degree	Census, parents	6.68	12.21	4.85	5.63	24.93	18.59
Percentage nonwhite in county * student is white	Census, student	0.80	0.40	17.87	13.62	26.47	36.24
Median income in county * family income > $50,000	Census, parents	0.50	1.29	0.27	0.35	0.81	2.01
Median income in county * family income > $100,000	Census, parents	0.09	0.57	0.04	0.03	0.18	0.54
Family income > $50,000	Parents	0.14	0.35	0.09	0.09	0.22	0.60
Family income > $100,000	Parents	0.02	0.16	0.01	0.01	0.05	0.16
Violent crime rate in county (per 100 residents)	CCDB	0.52	0.43	0.50	0.45	0.68	0.90
Mean student-teacher ratio in county, public schools	CCD	17.82	2.59	17.41	17.84	18.59	17.79
Mean student-teacher ratio in county, private schools	D&B	18.00	2.99	16.28	18.87	18.22	17.12
Coefficient of variation of county house prices	CCD	0.16	0.13	0.15	0.18	0.15	0.18

(table continues)

APPENDIX 1, continued

Variable Definition	Data Source	Mean	Standard Deviation	Mean (Nonunion)	Mean (Union)	Mean (Religious)	Mean (Nonreligious)
Percentage union, manufacturing workers in state	BLS	22.85	13.30	16.47	26.08	22.87	21.01
State right-to-work law—teachers	NBER	0.28	0.45	0.64	0.14	0.23	0.08
State duty-to-bargain law—teachers	NBER	0.58	0.49	0.10	0.78	0.67	0.60

Notes: "Student" refers to the NELS 10th-grade student survey; "Parent" to the NELS 10th-grade parent survey; "NELS" to the NELS test-score file; "Census" to the 1990 Census of Population, and Housing, county-level extract; "D&B" to the Dun and Bradstreet data set, matched to the NELS by the authors; "CCD" to the U.S. Department of Education's Common Core of Data; "CCDB" to the *City-County Data Book*; "BLS" to 1988 BLS union coverage data; "NBER" to the NBER Public-Sector Collective-Bargaining Law Data Set.

APPENDIX 2

Sample Sizes for Full and Subgroup Specifications
(number of students)

Group	Nonunion Public	Union Public	Religious Private	Nonreligious Private
Full sample	1325	2810	613	345
Black and Hispanic students	396	492	99	16*
Income > $50,000	113	257	133	210
Income < $25,000	702	1360	195	47
Top third, 8th-grade test scores	314	860	236	255
Bottom third, 8th-grade test scores	555	971	133	39
Number of schools	176	374	99	36

* Model not estimated for this subgroup due to small number of observations.

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