Are Catholic Primary Schools More Effective Than Public Primary Schools?

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Abstract

This paper assesses the causal effects of Catholic primary schooling on student outcomes such as test scores, grade retention, and behavior. Catholic school students have substantially better average outcomes than do public school students throughout the primary years, but we present evidence that selection bias is entirely responsible for these advantages. Estimates based on several empirical strategies, including an approach developed by Altonji et al. (2005a) to use selection on observables to assess the bias arising from selection on unobservables, imply that Catholic schools do not appreciably boost test scores. All of the empirical strategies point to sizeable *negative* effects of Catholic schooling on mathematics achievement. Similarly, we find very little evidence that Catholic schooling improves behavioral and other non-cognitive outcomes once we account for selection on unobservables.

Keywords: Catholic schools, achievement, selection bias

JEL Codes: I21, J24

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I. Introduction

Critics of the nation's public education system have long suggested that public schools would benefit from being operated more like private schools. Advocates of vouchers take this reasoning a step further, arguing for the use of public funds to help students defray the costs of attending private schools. These arguments are based largely on research suggesting that private schools boost educational attainment and achievement. In particular, a long line of research has compared outcomes of students in Catholic versus public high schools, finding compelling evidence that attending Catholic high schools positively influences educational attainment. However, much less is known about the efficacy of Catholic primary schooling.

Using data from the Early Childhood Longitudinal Survey: Kindergarten Class of 1998-99 (ECLS-K), we study the effects of Catholic schooling on cognitive and non-cognitive outcomes measured between kindergarten and eighth grade. Average achievement test scores among Catholic school students are substantially higher than among public school students throughout this grade range, but these advantages may be driven by systematic differences in students across school sector rather than by the effectiveness of Catholic schools. In order to distinguish between the competing explanations for Catholic schooling advantages, we pursue several approaches for controlling for selection bias. Our preferred approach involves using propensity score matching to control for observable differences between Catholic and public school students. We also adopt the methods of Altonji et al. (2005a) to use selection on observed variables to quantify the importance of selection on unobserved variables, a novel approach in the context of the Catholic primary schooling literature.

Our analysis produces five central findings. First, we document that Catholic school attendees have large advantages in both mathematics and reading achievement before entering into formal schooling. Roughly half of the mathematics score gap disappears between kindergarten and eighth grade, while the reading score gap is roughly stable over time. Second, all of the approaches we pursue suggest that Catholic primary schooling *decreases* mathematics achievement, with estimated effects ranging from two to four percentile-point reductions in test scores. Third, estimates based on the Altonji et al. (2005a) approach suggest that selection bias drives the small positive OLS and propensity-score estimates of Catholic primary schooling on reading scores. Specifically, selection bias could account for the entire positive OLS estimate if the association between Catholic schooling and unobservable determinants of test scores is only 5 percent as strong as the association between Catholic schooling and the observable determinants of test scores. Fourth, we find little evidence of a positive Catholic primary schooling effect on a set of non-cognitive outcomes available in ECLS-K once we assume even modest amounts of selection on unobserved determinants of these outcomes. Fifth, we illustrate the importance of controls for achievement prior to school entry in analyzing the effects of Catholic primary schooling by comparing estimates based on ECLS-K and NELS data. Overall, the results suggest that the Catholic school advantages present in eighth grade are largely due to differences in the skills (and other attributes) of public and Catholic school students that existed prior to kindergarten.

II. Relationship to the Current Literature

The vast majority of the literature on Catholic schooling has been devoted to the effect of Catholic high school attendance on test scores and educational attainment. Early work finds large positive effects of attending a Catholic high school (e.g., Coleman et al., 1982; Evans and Schwab, 1995; and Neal, 1997), but Altonji et al. (2005b) argue that these estimates could be driven solely by selection bias. Altonji et al. (2005a) develop techniques to quantify the degree of bias in single-equation models, based on the idea that the relationship between Catholic schooling and observable determinants of outcomes can be informative about the relationship between Catholic schooling and unobservables. They find that Catholic high school attendance increases the likelihood of graduating from high school and enrolling in college, but that the positive effects on test scores implied by OLS models are likely driven by selection bias. Morgan (2001) instead uses propensity score models to nonparametrically control for observable differences between Catholic and public school students, finding positive effects of Catholic high school attendance on test scores.

In contrast to the large literature on Catholic high schools, only a handful of studies have addressed the effects of Catholic primary schooling. Lee and Stewart (1989), Jones (1997), and Lubienski and Lubienski (2006) study differences in National Assessment of Educational Progress (NAEP) test scores between Catholic and public primary schools. Lee and Stewart (1989) and Jones (1997) find higher test scores for Catholic school students, whereas Lubienski and Lubienski (2006) find slightly lower (insignificantly so) test scores for Catholic school students. However, it is difficult to draw causal inferences from NAEP data because they are cross-sectional and contain no controls for student ability.

Carbonaro (2006) uses the ECLS-K to estimate the Catholic schooling effect on test score gains from the fall to spring of kindergarten. He finds negative and often insignificant Catholic school effects in his preferred propensity score model. Lubienski, Crane, and Lubienski (2008) also find a negative, statistically insignificant Catholic school effect on fifth-grade mathematics test scores in the ECLS-K data, based on models that include controls for kindergarten test scores. Similarly, Reardon, Cheadle, and Robinson (2009) estimate negative and insignificant Catholic schooling effects on test scores for each wave of ECLS-K data from kindergarten through fifth grade. They use propensity score matching models, OLS models, and area-level fixed effects models to control for selection on observable variables.

Jepsen (2003) studies the effects of Catholic schooling for two cohorts of primary school students in the *Prospects* data on Title I programs. Using OLS models, he finds small, statistically-insignificant effects of Catholic schooling on test scores and some suggestive evidence that Catholic schools may positively affect attendance. Finally, Sander (1996) finds positive effects of Catholic primary school attendance on tenth-grade test scores using High School and Beyond data, but because this study does not directly control for Catholic high school attendance, the estimates may instead capture positive effects of Catholic high schools.

III. Data

We use data from the Early Childhood Longitudinal Study: Kindergarten Class of 1998-99 (ECLS-K), a longitudinal study of kindergarteners beginning in the 1998-1999 academic year. Follow-up surveys were administered in the spring of kindergarten (1999), the fall of first grade (1999), the spring of first grade (2000), the spring of third grade (2002), the spring of fifth grade (2004), and the spring of eighth grade (2007).

We focus on the set of students who participated in the fall kindergarten sample because the extensive set of control variables provides valuable information about children's experiences and aptitudes prior to kindergarten. We limit our estimation samples to students who attended Catholic or public primary schools. Although substantial numbers of students attend other private schools in the ECLS-K, the non-Catholic private schools are sufficiently diverse that measuring a mean effect for these schools is of little value, so we exclude students who attend non-Catholic private schools at any grade level. After excluding these cases, the eighth-grade sample contains approximately 7,000 students, and the fifth-grade sample contains approximately 9,000 students. Exact sample sizes vary across specifications and estimation techniques; Appendix Table 1 contains descriptive statistics for students in either the fifth- or eighth-grade sample.

As our measure of Catholic schooling, we define an indicator equal to one if a child initially enrolled in a Catholic kindergarten, and zero otherwise.[1] We also measure other demographic variables based on kindergarten-year survey responses. Our measures of cognitive skills are fifth- and eighth-grade mathematics and reading test scores in the ECLS-K. Psychometric evaluations have shown that these assessments provide reliable measures of children's mathematics and reading skills (see Reardon, Cheadle, and Robinson, 2009, for a discussion). As measures of non-cognitive skills, we use school-reported measures of number of days absent and the number of days tardy in the fifth-grade wave. From the eighth-grade wave, we use a binary measure of whether a parent reported that the student had ever been suspended, a binary measure of whether a student has fallen behind their cohort's grade advancement, equal to one if a student has not reached the eighth grade at the time of the eighth grade survey, and a student-reported "locus of control" scale, which measures student ratings of agreement to questions such as "I don't have enough control over the direction my life is taking" and "In my life, good luck is more important than hard work for success."

A particularly attractive feature of the ECLS-K lies in its breadth of included information about students, parents, teachers, and schools. We include extensive sets of control variables along each of these dimensions in order to minimize the role of unobserved characteristics in estimates of the effects of Catholic primary schooling. Student characteristics include sex, race/ethnicity, age, birth weight, and fall kindergarten test scores. Family background

[1] In all cases, our primary empirical results are robust to measuring Catholic schooling based on grade levels other than kindergarten.

characteristics include the marital status of the child's primary caregiver, log family income, parental education, and family structure. Appendix Table 1 lists descriptive statistics for all of these variables.

The ECLS-K is designed to be nationally representative in each survey wave through the inclusion of sample weights for each wave (such as spring 2003) as well as for a panel (such as the set of students who participated in all follow-up surveys). We conduct all empirical analyses both with and without the appropriate sample weights to assess the sensitivity of our results, but we report weighted estimates below. Because of the sample restrictions listed above, our analysis sample is a subset of the full ECLS-K dataset, so our use of ECLS-K weights may or may not produce representative samples.[2]

IV. Methodology

Because Catholic schools charge tuition and often require parental involvement, students who attend Catholic primary schools likely differ from public school students along many dimensions. Of particular concern is the likely correlation between unobservable determinants of school sector and outcomes of interest such as student achievement. We study multiple techniques to evaluate the extent of selection bias as well as to control for it.

The starting point of our analysis is OLS estimation of a linear model,

$$Y_i = \alpha + \beta CS_i + X_i'\gamma + \varepsilon_i, \tag{1}$$

where Y_i denotes the outcome of interest (e.g., eighth-grade mathematics test scores), CS_i denotes an indicator variable equal to one for students in Catholic primary school and zero

[2] The timing of the ECLS-K panel overlaps with the sex abuse scandal involving Catholic priests starting in 2002, when ECLS-K students would have been in second grade. If the scandal led to non-random attrition from Catholic schools, then the ECLS-K follow-ups may be non-representative of samples that would have been collected either long before the scandal or afterward. Dee and Jacob (2009) document a sizable decrease in enrollment in Catholic elementary schools starting in 2002 using NAEP data, compared to essentially flat enrollment in public elementary schools.

otherwise, X_i denotes the student characteristics and family background measures listed in Appendix Table 1, and ε_i denotes unobserved determinants of Y_i.

As noted above, the ECLS-K data include extensive controls for family background, student characteristics, and school characteristics. Most importantly, the data include test scores in mathematics and reading from the fall of 1998, taken shortly after students entered kindergarten. The test score variables from the fall of kindergarten are particularly valuable – because these tests are administered early in the school year, they primarily measure a student's ability and human capital accumulation prior to enrolling in school, rather than the child's kindergarten experience.[3] The inclusion of such extensive controls reduces, but likely does not eliminate, the influence of confounding factors on the outcomes of interest.

Propensity Score Matching

As in regression-based analyses, propensity score matching relies on the assumption of "selection on observables": conditional on observable characteristics, students in Catholic and public schools do not systematically differ along unobservable dimensions. The primary advantage of the propensity score approach is that it is robust to misspecification of the outcome model given by (1); this approach does not rely on linearity of the outcome model in order to generate consistent estimates of treatment effects.

For comparability with earlier studies of Catholic primary schooling, we follow the propensity score approaches of Reardon, Cheadle, and Robinson (2009) and Morgan (2001). We specify the propensity score as follows:

(2) $$CS_i = 1(f(X_i) + v_i > 0),$$

[3] All wave 1 surveys were collected between September and early December in the kindergarten year, with roughly 90 percent collected by the end of November. The ECLS-K website includes comprehensive information about the wave 1 sampling design: http://nces.ed.gov/ecls/Kindergarten.asp.

where $1(\cdot)$ denotes the indicator function that takes on the value 1 if its argument is true and zero otherwise, $f(X_i)$ denotes a flexible function of all of the elements of X_i, and υ_i denotes unobserved determinants of Catholic primary school attendance.

In the main analyses below, we estimate the propensity scores based on probit models, but we assess the sensitivity of the estimates to the assumed distribution of υ_i by also using logit models. Although propensity scores are widely used in the matching literature, no single method has dominated (see, for example, Frolich, 2004), so we employ three commonly-used matching methods: kernel density, nearest neighbor, and caliper. The kernel density estimator compares each student in the treatment group (in this case, students enrolled in Catholic primary schools) to a weighted average of all comparison group observations, with the weight for each observation in the comparison group inversely proportional to the difference between that observation's estimated propensity score and the propensity score of the treatment student.[4] In the nearest-neighbor approach, each treatment student is matched with the four students in public schooling who have the most similar propensity scores.[5] Finally, in the caliper method, we match each treatment student with all students in public schools who have propensity scores within a predetermined distance (or "radius"). Below we choose a radius of 0.0005, although results based on radii ranging from 0.0001 to 0.0010 produce qualitatively similar results. As we show below, our central conclusions are insensitive to not only the smoothing parameters we

[4] We use the Stata routine *psmatch2* (see Leuven and Sianesi, 2003) to calculate the propensity score estimators. For a recent review of matching estimators with an application to job training programs, see Mueser, Troske, and Gorislavsky (2007) and the references cited there. We use the default options in *psmatch2*: an Epanechnikov kernel (as recommended by Silverman, 1986) with a bandwidth of 0.08.

[5] We include four nearest neighbors because the simulations in Abadie and Imbens (2006) imply that the use of four neighbors minimizes mean-squared error in our sample sizes, although our results are largely insensitive to including between one and five nearest neighbors. We match with replacement, and we include ties, i.e., students with identical propensity scores, even if including them raises the number of closest neighbors above four. The results below are not sensitive to either of these choices.

choose for a given method, but to the method itself – estimates based on the kernel density, caliper, and nearest neighbor methods are similar in all cases.

In all of these propensity score methods, the selection on observables assumption boils down to assuming that, conditional on the propensity score, the choice of treatment is unrelated to potential outcomes, i.e., outcomes that would hold if Catholic or public schooling were chosen. If so, the effect of Catholic schooling can be consistently estimated by examining differences in mean outcomes between the Catholic schooling sample and the propensity-score-matched public school sample.

Selection on Observables and Unobservables

In order to evaluate whether selection bias drives the OLS and propensity-score estimates that dominate the literature on Catholic primary schooling, we adopt a technique developed in Altonji et al. (2005a). This approach is based on measuring the ratio of selection on unobservables to selection on observables needed to attribute the entire effect of Catholic school attendance to selection bias. For example, Altonji et al. (2005a) find that selection on unobservables would need to be 3.55 times stronger than selection on observables in order to "explain away" their large estimated effect of Catholic high school attendance on the likelihood of enrolling in college. We will use these methods to analyze the role of selection bias on estimates of the effects of Catholic primary schooling on student achievement.

Altonji et al. (2005a) provide a detailed explanation of their approach, which we briefly describe here in the context of Catholic primary schooling. Using the notation of equation (1), the condition that "selection on the observables equals selection on unobservables" implies that

$$\frac{cov(CS_i, X_i'\gamma)}{var(X_i'\gamma)} = \frac{cov(CS_i, \varepsilon_i)}{var(\varepsilon_i)}. \quad (3)$$

Under the null hypothesis of no Catholic schooling effect, we can obtain consistent estimates of γ via OLS estimation of a version of equation (1) that imposes the restriction that $\beta = 0$:

$$Y_i = \alpha + X_i'\gamma + \varepsilon_i, \tag{4}$$

which in turn yields estimates of $cov(CS_i, X_i'\gamma)$, $var(X_i'\gamma)$, and $var(\varepsilon_i)$, implying that $cov(CS_i, \varepsilon_i)$ is identified from (3). With an estimate of $cov(CS_i, \varepsilon_i)$ in hand, it is straightforward to estimate the implied bias in OLS estimates of (1).[6] The ratio of this implied bias to the estimate of β is then an estimate of how strong selection on unobserved variables would have to be relative to selection on observed variables to attribute the entire estimated effect to selection bias.[7]

V. The Effects of Catholic Schools on Test Scores

Figure 1 shows average mathematics and reading test score percentiles by survey in ECLS-K, based on a child's percentile rank among all children who took the same test in the same survey wave. The top panel of the figure shows that children enrolled in Catholic kindergartens scored at roughly the 62nd percentile on fall kindergarten mathematics tests, on average, compared to the 47th percentile among children in public kindergartens (the overall sample mean of the percentile scores is 50.5 by construction). This 15 percentile-point advantage for Catholic kindergarteners declines monotonically from the fall of kindergarten until

[6] To see how this condition yields an estimate of the bias in OLS models, note that this bias is given by $\frac{cov(\widetilde{CS_i}, \varepsilon_i)}{var(\widetilde{CS_i})} = \frac{cov(CS_i, \varepsilon_i)}{var(\widetilde{CS_i})}$, where $\widetilde{CS_i}$ is the residual from a regression of CS_i on X_i, and the equality follows if $cov(X_i, \varepsilon_i) = 0$. Because $var(\widetilde{CS_i})$ is straightforwardly identified from the data while $cov(CS_i, \varepsilon_i)$ is identified from condition (3), $\frac{cov(\widetilde{CS_i}, \varepsilon_i)}{var(\widetilde{CS_i})}$ is identified.

[7] Altonji et al. (2005b) use a similar approach to argue against the validity of several instrumental variables strategies used in the Catholic schooling context, including those based on a student's religious affiliation or proximity to Catholic school. However, Cohen-Zada and Elder (2009) suggest new instruments based on historical Catholic population shares, claiming that these measures are unrelated to present-day determinants of outcomes while being correlated with the decision to attend Catholic schools (because many Catholic schools were founded in the early 20th century). Unfortunately, these instruments were too weakly correlated with Catholic primary school attendance in ECLS-K to generate useful estimates of Catholic primary schooling effects, so we do not report results based on this strategy.

third grade, in which Catholic kindergarteners have only a 7 percentile-point advantage (the difference widens to roughly 8 percentile points in eighth grade). This decline provides suggestive evidence that much of the Catholic school students' advantage in mathematics scores in eighth grade is driven by differences already apparent at the beginning of kindergarten, not by higher rates of learning; if anything, the implied effects of Catholic schooling are negative.

Panel B of the figure shows the analogous temporal pattern for reading scores. Unlike in the case of math scores, the Catholic advantage only declines between kindergarten and first grade, widening thereafter. Overall, the reading differential increases slightly from 13.4 to 15.5 percentile points between kindergarten and eighth grade, suggesting that Catholic schools may have a modest positive effect on reading scores.

OLS Estimates

Table 1 presents OLS estimates of equation (1) for a variety of specifications. Column (1) shows estimated Catholic school effects from models that include no additional control variables. The top row shows that students who attend a Catholic school in kindergarten score 7.17 percentile points higher on eighth-grade mathematics tests than students attending public kindergartens, with a standard error of 1.69 (standard errors in all specifications are clustered to allow for correlation among students attending the same school). The second column presents estimates from models that include fall kindergarten reading and mathematics test scores as controls, with striking results – the positive Catholic school effect vanishes. The third column adds individual-level controls for race, ethnicity, family structure, parental marital status, education, income, employment, Census region, and urbanicity. The implied Catholic schooling effect in this column is negative, with Catholic kindergarteners scoring 5.96 percentile points lower than observationally similar public kindergarteners. The final column adds state indicators to control for correlations between Catholic school enrollment rates and underlying state-level student achievement, which does not substantially alter the results. The results shown in Table 1 are weighted using the relevant cross-sectional weight provided by NCES, e.g., the fifth-grade cross-sectional weight for fifth-grade test scores. For comparison, Appendix Table 2 contains results from unweighted regressions. In all cases, the estimates based on the weighted regressions are quite similar to those based on unweighted regressions.

The remaining rows of the table show results for eighth-grade reading scores, fifth-grade mathematics scores, and fifth grade reading scores. For all four outcomes, much of the large Catholic student advantage vanishes after conditioning on initial kindergarten achievement. The individual-level controls included in column (3) further reduce the magnitude of the estimates. Kindergarten test scores and the individual-level controls explain much of the between-student variation in fifth- and eighth-grade achievement, with the regression r^2 values ranging from 0.44 to 0.55 in column (3). Overall, the estimates suggest that Catholic primary schooling significantly lowers mathematics achievement in both fifth and eighth grades. Although there is no evidence for a negative Catholic primary school effect on reading scores, there is little evidence for a positive effect, either: the estimate of 0.93 (1.40) in eighth grade is modest and statistically indistinguishable from zero. Again, the results suggest that Catholic primary schools do not markedly increase student achievement, but this null result may be an artifact of the linearity imposed in (1). We turn next to relaxing this assumption by considering our preferred specifications based on propensity score matching.[8]

[8] As a complement to the OLS results, we also estimate student fixed effects models of math and reading scores, based on the roughly five percent of students in the ECLS-K who switch school sectors. Of the 460 students who switch, roughly 83 percent (= 380 / 460) start in Catholic school and later move to public schools; the remainder starts in public school and moves to Catholic school. These estimates are generally consistent with the OLS estimates, in that Catholic schooling is associated with large mathematics score reductions (2.25 percentile points, with a standard error of 1.01) and somewhat smaller and insignificant reading score reductions (1.41 percentile points, with a standard error of 1.05).

Propensity Score Matching

Table 2 contains the results from the propensity score analyses, based on the kernel matching, caliper matching, and nearest neighbor matching approaches described above. The reported results are the differences in means between the set of students who were enrolled in Catholic schools and the matched set of public school students. The propensity score is estimated using a probit model on the full set of kindergarten controls, which correspond to the same set of covariates used in column (3) of Table 1. Regression-adjusted estimates are similar to the simple averages and are therefore not reported.[9]

The point estimates in Table 2 are generally similar to the OLS estimates in columns (3) and (4) of Table 1. In all specifications, Catholic primary schooling is associated with significantly lower mathematics scores. The estimates indicate that Catholic primary schooling has an insignificant effect on eighth-grade reading scores, with point estimates of 0.67 to 1.02 percentile points. The estimates are negative for fifth-grade reading scores but are statistically insignificant in all three specifications. In all cases, standard errors are calculated from 200 bootstrapped samples drawn with replacement within school clusters, which allows for correlations among students attending the same school and accounts for estimation error in the propensity scores. However, Abadie and Imbens (2008) argue that the bootstrap is inappropriate for nearest-neighbor matching and possibly for caliper matching, so we focus hereafter on the kernel density matching estimates. Appendix Table 3 illustrates the effectiveness of the kernel density matching procedure by presenting summary statistics for the treatment, control, and

[9] The primary advantage of such "doubly robust" estimators is that they potentially control for misspecification of the propensity score. As Bang and Robins (2005) describe, as long as *either* the propensity score or the model of outcomes is specified correctly, the resulting estimates of β will be consistent. The similarity of the doubly robust estimates and the simple mean differences suggests that misspecification of the propensity score is not an important factor in the estimates in Table 2. We report the simple mean differences for simplicity and to be consistent with the previous literature on Catholic schooling.

matched control samples. As the table shows, the large differences between public and Catholic school students in the covariates are almost entirely eliminated by reweighting the sample of public school students.[10]

The fact that the estimated effects of Catholic schools are insensitive to controlling for observable characteristics via OLS or by using them to create propensity score-matched samples suggests that both methods would capture the causal effects of Catholic schooling if the "selection on observables" assumption holds. However, there is reason to doubt that this assumption holds, given the large observable differences between Catholic and public primary students implied by Tables 1 and 2. We turn next to assessing the sensitivity of the estimates to possible systematic unobservable differences between Catholic and public students.

Selection on Observables and Unobservables

Table 3 presents results from models based on the techniques developed in Altonji et al. (2005a). As discussed above, these techniques are based on asking how strong the relationship between Catholic primary schooling and unobservable determinants of outcomes would have to be, relative to the strength of the relationship between Catholic primary schooling and observable determinants of outcomes, in order to attribute the entire estimated effects of Catholic school attendance to selection bias. This approach relaxes the "selection on observables" assumption but, unlike the matching estimators, imposes linearity, so we view the two sets of estimates as complementary.

[10] Using pairwise *t* tests, we reject the hypothesis of no mean differences at the 5 percent level between the matched public and Catholic samples for 2 of the 42 included covariates, Midwest and West region. This is consistent with the notion that the samples represent balanced populations, as one would expect to reject mean equality in roughly 5 percent of cases (the rejection rate under a true null). We also ran a single *F*-test for joint significance of the difference in the covariate means between the treatment and matched control samples, obtaining an *F*-statistic of 0.61 (p = 0.94). This again suggests that the matched samples represent balanced populations.

Column (1) of the table replicates the OLS estimates from column (3) of Table 1, which are based on models that include the full set of controls but no state indicators. In column (2), we present the estimates of bias in these estimates, i.e., $\frac{cov(\widetilde{CS}_i,\varepsilon_i)}{var(\widetilde{CS}_i)}$, based on the condition that "selection on the observables equals selection on unobservables". In the top row, which refers to eighth grade mathematics scores, this estimated bias is 14.01, with a standard error of 1.30.[11] In other words, the strong positive correlation between Catholic primary schooling and observable determinants of outcomes (represented by the index $X_i'\gamma$) implies that Catholic schooling is also strongly positively related to ε_i, resulting in *positive* bias in the OLS estimates of β.

Column (3) of the table, labeled "Implied Ratio", shows the ratio of the OLS estimate to the estimated bias in column (2). This ratio is an estimate of how strong selection on unobserved variables would have to be relative to selection on observed variables to produce the OLS estimate if the true Catholic schooling effect were zero. In the top row, this implied ratio is negative, meaning that selection on observables and unobservables would have to be of opposite signs to be consistent with a true effect of zero. Although the "selection on the observables equals selection on unobservables" condition is unlikely to hold exactly, it is likely that selection on observables and unobservables at least be of the same sign. Based on this logic, the negative OLS point estimate in column (1) can be viewed as an upper bound – Catholic primary schooling likely lowers math test scores by at least 5.96 percentile points.

In the second row, which presents results for eighth-grade reading scores, the OLS estimate implies a small, positive effect of Catholic schooling on reading achievement. However, the estimated bias is 16.91, implying that the OLS estimate would be solely driven by selection bias if selection on unobservables were only 5 percent as strong as selection on

[11] Standard errors in columns (2) and (3) are obtained via 200 bootstrapped samples drawn with replacement within school clusters.

observables. If selection on unobservables were instead equal to selection on observables, the implied treatment effect is negative 15.98 (= 0.93 – 16.91) percentile points.

The remaining rows of the table show estimates for fifth-grade mathematics and reading test scores. These results imply that the negative OLS and matching estimates may actually understate the negative effects of Catholic schooling, casting substantial doubt on the existence of a positive effect of Catholic primary schooling on achievement. In both fifth and eighth grades, the estimates are consistent with a large negative effect of Catholic primary schooling on mathematics skills.

VI. Non-cognitive Outcomes

Although the estimates presented thus far imply that Catholic primary schools do not boost test scores, they may affect other outcomes valued by both parents and students. This scenario is especially plausible given the findings of Altonji et al. (2005a), who provide compelling evidence that Catholic high schools influence outcomes such as high school completion and college attendance while having no discernible effects on test scores. Recent authors such as Heckman, Stixrud, and Urzua (2006) and Heckman, Pinto, and Savelyev (2012) have argued that noncognitive skills, such as those captured by measures of suspension from school and absences, are often more powerful predictors of adult outcomes (such as wages) than are test scores. In light of the potential importance of non-cognitive skills and the previous evidence that Catholic schooling influences non-cognitive skills, we turn next to analyses of these outcomes.

Our measures of non-cognitive skills include three measures taken from the fifth grade survey: the number of days absent and the number of days tardy as reported in school records, and the “locus of control” scale reported by students. We also analyze two measures taken from the eighth grade survey: an indicator for whether the student had repeated a grade at any time up to that point, and an indicator for whether the student was suspended (either in- or out-of-school) in the past school year. Coleman and DeLeire (2003) argue that locus of control is a determinant of eventual educational attainment, whereas Cebi (2007) finds that locus of control is positively associated with wages conditional on test scores.[12]

Table 4 contains the estimated effects of Catholic primary schooling on all of these outcomes. The first three columns present OLS results analogous to the specifications in Table 1, column (4) present estimates based on kernel density matching using the full set of controls, and column (5) shows estimates of implied bias based on the Altonji et al. (2005a) assumptions. The first set of results in the table show that Catholic schooling is associated with a reduction in absences of approximately 1.1 days in columns (3) and (4). These estimates are nearly identical to those found by Jepsen (2003) based on *Prospects* data. Conversely, Catholic schooling is associated with an increase in tardiness of roughly 1 day (the OLS results in column (3) and matching results in column (4) are strikingly similar across all five outcomes in the table). However, both absenteeism and tardiness are strongly associated with observable covariates that are in turn associated with Catholic schooling. As a result, the estimates in column (5) imply that if selection on observables and unobservables were identical, the OLS and matching estimates are strongly negatively biased. Using the OLS estimate, the implied treatment effect on days absent is 6.36 (= -1.10 + 7.46), which corresponds to a large harmful effect of Catholic schooling. Thus, despite evidence that Catholic schooling might reduce absences (and increase

[12] Locus of control is measured as the sum of the following 4-point scale variables (with each variable given a numerical value of 1 for a response of "strongly disagree", 2 for "disagree", 3 for "agree", and 4 for "strongly agree"): "I don't have enough control over the direction my life is taking"; "In my life, good luck is more important than hard work for success"; "Every time I try to get ahead, something or somebody stops me"; "My plans hardly ever work out, so planning only makes me unhappy"; and "Chance and luck are very important for what happens in my life". The sum of these scales is then standardized to be mean zero, ranging from -3 to 1.5.

tardiness), we are hesitant to draw firm conclusions because there is so much potential for bias even in the richest OLS and matching models.

The results are similar for the other outcomes shown in the table. For both the locus of control and grade repetition measures, the raw differences shown in column (1) imply large beneficial effects of Catholic schools that disappear one detailed controls are included in columns (3) and (4). On the other hand, we find a negative relationship between Catholic schooling and the likelihood of parent-reported suspension using both OLS and matching specifications. In our preferred specification in column (4), Catholic schooling is associated with a five percentage-point reduction in suspension. However, the "selection on observables equals selection on unobservables" condition implies large harmful effects of Catholic schools for all outcomes. We again stress that, although we are wary to interpret the Altonji et al. (2005a) condition as being literally true, the bias calculations in column (5) are so large that they illustrate a substantial potential for bias in all of these cases.

Overall, the matching and OLS results show no consistent effect of Catholic schooling on non-cognitive outcomes other than a notable reduction in the likelihood of suspension in eighth grade. In all cases, estimates based on the Altonji et al. (2005a) approach imply that any modest beneficial effects on non-cognitive outcomes would be fully accounted for by selection bias if the strength of selection on unobservables is only a small fraction of the strength of selection on unobservables.

VII. Relation to Previous Work: A Comparison of ECLS-K and NELS:88

The results presented above paint a pessimistic picture of the efficacy of Catholic schooling relative to the previous literature. Several empirical studies, including Evans and Schwab (1995), Sander (1996), Neal (1997), Grogger et al. (2000), Jepsen (2003), and Altonji et al. (2005a), find evidence of positive effects of Catholic schooling on student outcomes. Sander's (1996) and Jepsen's (2003) results are most relevant to those found here, as both of these authors analyze primary schooling and achievement; both find small positive effects of Catholic primary schooling on test scores, although statistically insignificant in the case of Jepsen (2003). Even though Altonji et al. (2005a) do not focus on primary schooling, they note that Catholic eighth graders have substantial test score advantages even conditional on a rich set of controls available in the National Educational Longitudinal Survey of 1988 (NELS:88).

Rather than attempting to reconcile all of the potential sources of variation in the findings of these studies, we opt to focus on an illustrative example that highlights the importance of controlling for achievement differences that exist prior to school entry. None of the previous studies had access to test scores early in kindergarten, as these measures are unique to ECLS-K. It is thus potentially informative to assess what the estimates in ECLS-K would be if these data did not include early-kindergarten test scores, but instead only included those covariates found in education datasets such as NELS:88 or High School and Beyond.

In Table 5, we use various control sets to present estimates from NELS:88 and ECLS-K of the effects of Catholic schooling on the five outcomes common to both data sets: eighth grade math and reading scores, the locus of control scale, an indicator for whether a student repeated a grade at any time up to eighth grade, and an indicator for whether the student was suspended in the past school year. Columns (1) and (3) present raw differences. In columns (2) and (4), we report estimates from kernel density matching models, where the propensity score is a function of control variables that are common to both data sets, including student characteristics (sex and race/ethnicity), family background (log family income, maternal and paternal education), parent marital status, family structure (including indicators for whether both parents live in the child's household), region, and urbanicity. All of these variables, which correspond to the control set used in Altonji et al.'s (2005a) analysis of NELS:88, are listed in Appendix Table 1. Column (5) includes kindergarten test scores in the estimation of the propensity score.

The similarity between the estimates in columns (1) and (3) is striking: the bivariate relationships between Catholic schooling and all five outcomes are remarkably similar in the two data sets. For the non-cognitive outcomes, the estimates in columns (2) and (4) are also similar, implying that the controls are similarly predictive of Catholic schooling and these outcomes in both datasets. For test scores, the estimates in column (4) are slightly smaller than those in column (2), implying that selection on these observables is slightly stronger in ECLS-K than in NELS:88 for these outcomes.

Finally, a comparison of columns (2), (4), and (5) highlights the central role of kindergarten test scores. The mean differences imply large positive effects of Catholic primary schooling on eighth grade reading scores and small positive (in NELS:88) or small, insignificant negative (in ECLS-K) effects on math scores. Based on either column (2) or (4), a researcher might conclude that Catholic primary schooling boosts test scores, while column (5) supports the opposite conclusion. Clearly, the kindergarten test scores have a larger practical effect on the estimates than any other difference between the two data sets. This comparison emphasizes the importance of controls for achievement prior to school entry in analyzing the effects of Catholic primary schooling, or potentially in analyzing the effects of any primary school intervention on achievement.

VIII. Conclusions

A substantial body of research has investigated the effects of Catholic schooling on student outcomes. Much of the literature has focused on Catholic high schools, typically finding that Catholic schooling boosts educational attainment as well as student achievement. In this paper, we instead assess the effects of Catholic primary schooling. An advantage of this focus lies in our ability to carefully control for the achievement levels of students near the beginning of the kindergarten year, before much formal schooling has taken place. Catholic school students have large advantages in both mathematics and reading test scores at the start of kindergarten, and the mathematics gap declines substantially between kindergarten and eighth grade. The reading gap neither diminishes nor grows over time. These temporal patterns suggest that much of the test score advantage of eighth graders in Catholic schools reflects selection bias, in the form of differences in skills that existed prior to kindergarten.

In order to further assess the role of selection bias, we pursue an empirical approach based on simple OLS estimates, propensity score matching estimators, and the techniques developed by Altonji, Elder, and Taber (2005a) to use selection on observed variables to quantify the importance of selection on unobserved variables. Although the identifying assumptions differ across these methods, several patterns in the results do not. Catholic primary schooling is consistently associated with lower mathematics achievement, with estimated effects ranging from two to ten percentile-point reductions in test scores. Catholic schooling also does not appear to substantively increase reading scores in fifth and eighth grades. The estimates based on the Altonji, Elder, and Taber (2005a) approach suggest that the small positive matching and OLS estimates are driven by selection bias – for example, if the association between unobservable determinants of eighth-grade reading scores and Catholic schooling is only 5 percent as strong as the corresponding association between the observable determinants and Catholic schooling, selection bias is solely responsible for the OLS estimate.

When we consider non-cognitive outcomes such as measures of attendance, locus of control, grade repetition, and suspension, we again find little evidence of a positive Catholic primary schooling effect. Matching estimates of Catholic schooling effects imply small beneficial effects on absences and suspension, a modest harmful effect on tardiness, and small, statistically insignificant effects on locus of control and grade repetition.

Taken together, the estimates presented in this paper do not point to any discernible beneficial effects of Catholic primary schooling, at least in terms of the outcomes available in ECLS-K. In fact, the only unambiguous finding that emerges is that Catholic primary schooling reduces math scores. Uncovering the mechanisms underlying this negative effect is an important area for future research, especially considering that it exists in spite of arguably better peer group quality at Catholic schools in comparison to public schools. One possible explanation is lower teacher quality in Catholic schools relative to public schools. In 2007-2008, private elementary school teachers had an average salary of $35,730, compared to an average of $51,660 in public schools (Snyder and Dillow, 2012). This pay gap exists both unconditionally and conditional on a variety of teacher characteristics, such as education and experience. Given this gap, it is quite conceivable that Catholic schools face difficulties in attracting high-quality teachers (Brekke (2013) documents programs intended to attract teachers to low-wage Catholic schooling positions in several cities). Other potential explanations for lower Catholic school achievement include superior curriculum design in public schools.

Another potential goal of future research is to shed more light on the effects of Catholic primary schooling on non-cognitive outcomes, including how these effects vary across demographic subgroups. The estimates presented here do not provide strong evidence for beneficial Catholic primary schooling effects on non-cognitive outcomes, but they do not conclusively rule out small beneficial effects either. Given the recent proliferation of research suggesting that non-cognitive skills may be more important than test scores for predicting adult outcomes, future work on the influence of Catholic schooling on non-cognitive skills will be essential to assess the overall effectiveness of Catholic primary schools.

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Figure 1: Average Test Score Percentiles by Survey Wave and School Type, ECLS-K

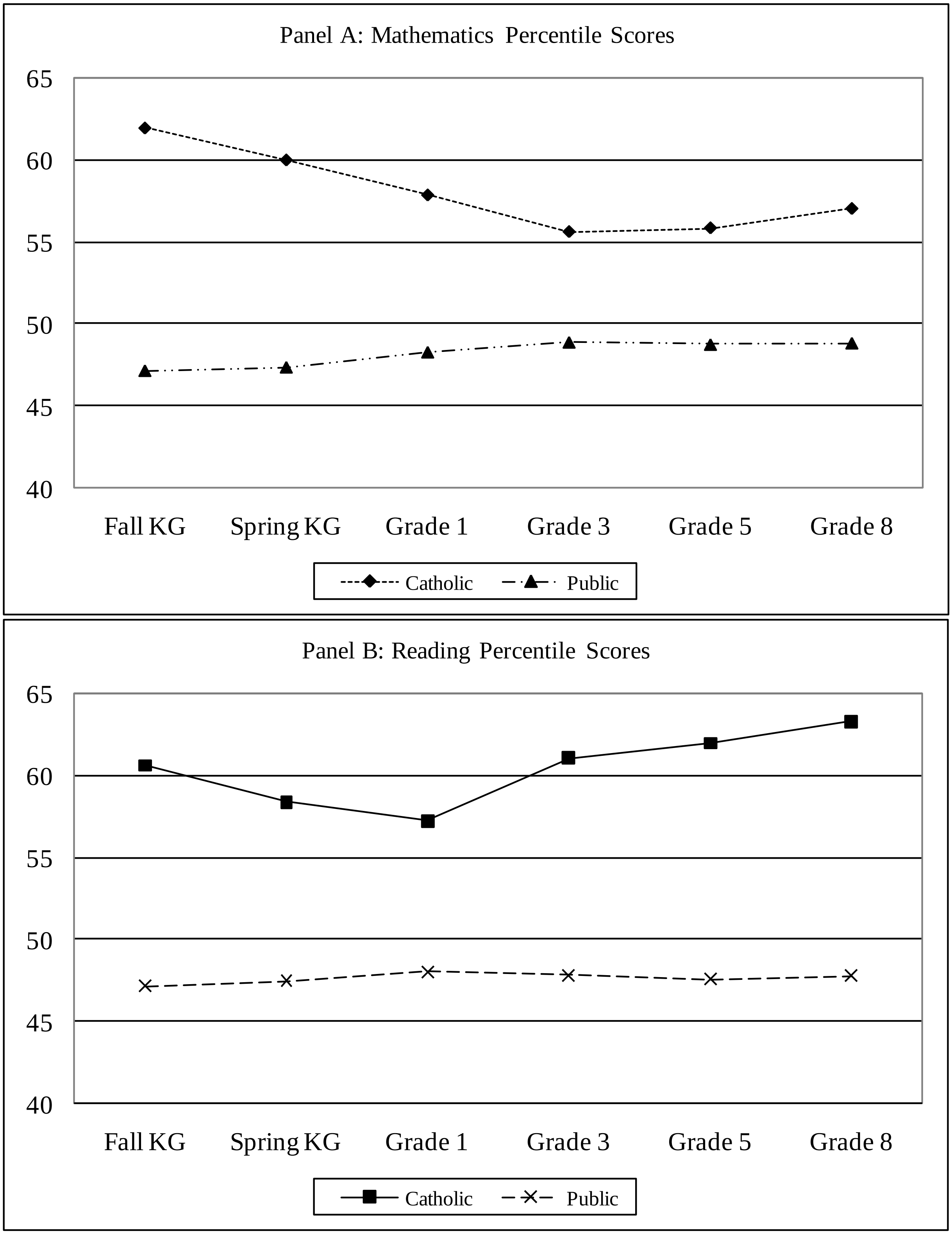

Table 1: OLS Estimates of the Effect of Catholic Primary Schooling on 5th and 8th Grade Test Score Percentiles, ECLS-K

Outcome	(1)	(2)	(3)	(4)
8th Grade Math Scores	7.17	-1.50	-5.96	-4.62
	(1.69)	(1.35)	(1.31)	(1.33)
Adjusted R^2	0.01	0.39	0.49	0.51
Observations	6,590	6,590	6,400	6,270
8th Grade Reading Scores	13.55	5.18	0.93	1.69
	(1.69)	(1.45)	(1.40)	(1.44)
Adjusted R^2	0.02	0.36	0.44	0.46
Observations	6,540	6,540	6,360	6,230
5th Grade Math Scores	5.98	-3.40	-7.53	-6.22
	(1.58)	(1.32)	(1.26)	(1.25)
Adjusted R^2	0.00	0.45	0.55	0.57
Observations	8,250	8,250	7,990	7,810
5th Grade Reading Scores	11.73	2.06	-1.98	-1.62
	(1.74)	(1.32)	(1.30)	(1.32)
Adjusted R^2	0.02	0.43	0.51	0.52
Observations	8,250	8,250	7,980	7,800
Controls				
Kindergarten Test Scores		X	X	X
Additional Covariates			X	X
State Indicators				X

Notes: The test score percentile variables are created based on all valid test scores in a particular survey and range from 1 to 100. The entries for each model are the coefficient, standard error in parentheses, and the regression R^2. The variables comprising "Additional Covariates" are described in the text and are listed in Appendix Table 1. Standard errors are robust to clustering among students attending the same school. All sample sizes are rounded to the nearest 10 to comply with Institute for Education Sciences restricted-use data publication policy.

Table 2: Propensity Score Estimates of the Effects of Catholic Primary Schooling on 5th and 8th Grade Test Score Percentiles, ECLS-K

	Matching Method		
Outcome	Kernel	Nearest Neighbor	Caliper / Radius
8th Grade Math Score Percentiles	-5.83 (1.26)	-6.79 (2.09)	-6.12 (2.00)
8th Grade Reading Score Percentiles	1.02 (1.40)	0.77 (1.87)	0.67 (2.35)
5th Grade Math Score Percentiles	-7.80 (1.46)	-9.77 (1.68)	-9.41 (2.01)
5th Grade Reading Score Percentiles	-2.33 (1.43)	-2.72 (2.28)	-2.01 (1.80)

Notes: Standard errors are in parentheses. The variables used to create the estimated propensity scores include all variables in column (3) of Table 1. The kernel, caliper, and nearest neighbor methods are described in the text.

Table 3: Estimates of Bias in OLS Estimates of the Effects of Catholic Primary Schooling Based on Altonji et al. Methodology

Outcome	(1)	(2)	(3)
	OLS Estimate	Estimated Bias	Implied Ratio
8th Grade Math Scores	-5.96	14.01	-0.43
	(1.31)	(1.30)	(0.11)
8th Grade Reading Scores	0.93	16.91	0.05
	(1.40)	(1.48)	(0.09)
5th Grade Math Scores	-7.53	11.26	-0.67
	(1.26)	(1.24)	(0.15)
5th Grade Reading Scores	-1.98	13.71	-0.14
	(1.30)	(1.33)	(0.10)

Notes: The estimates in column (1) are identical to those shown in column (3) of Table 1. The estimates in column (2) are the estimates of bias based on the condition $\frac{cov(CS_i, X_i^{'}\gamma)}{var(X_i^{'}\gamma)} = \frac{cov(CS_i, \varepsilon_i)}{var(\varepsilon_i)}$, where estimates of γ are obtained from models that impose that Catholic schools do not affect outcomes. The estimates in column (3) are the OLS estimates in column (1) divided by the estimated bias in column (2), representing the ratio of selection on unobservables to selection on observables that would be consistent with a zero effect of Catholic primary schooling on the specific test score. Standard errors are in parentheses. Standard errors in columns (2) and (3) are obtained via 200 bootstrapped samples drawn with replacement within school clusters to allow for correlations among students attending the same school.

Table 4: Estimates of the Effect of Catholic Primary Schooling on 5th and 8th Grade Non-Cognitive Outcomes, ECLS-K

	OLS Estimates			Matching Estimates	Implied Bias
Outcome	(1)	(2)	(3)	(4)	(5)
Days Absent, 5th Grade	-1.88	-1.45	-1.10	-1.06	-7.46
	(0.34)	(0.34)	(0.36)	(0.36)	(2.15)
Adjusted R^2	0.00	0.02	0.09		
Observations	7,280	7,280	7,100	7,100	7,100
Days Tardy, 5th Grade	0.12	0.51	0.97	0.99	-13.75
	(0.46)	(0.46)	(0.48)	(0.44)	(2.89)
Adjusted R^2	0.00	0.01	0.06		
Observations	6,750	6,750	6,570	6,570	6,570
Locus of Control Scale, 8th Grade	0.15	0.07	0.02	0.03	1.34
	(0.03)	(0.03)	(0.03)	(0.03)	(0.14)
Adjusted R^2	0.01	0.06	0.10		
Observations	6,550	6,550	6,370	6,370	6,370
Repeated a Grade, K-8	-0.10	-0.03	0.01	0.00	-0.40
	(0.01)	(0.01)	(0.01)	(0.02)	(0.04)
Adjusted R^2	0.00	0.13	0.21		
Observations	6,610	6,610	6,420	6,420	6,420
Suspended in 8th Grade	-0.14	-0.11	-0.07	-0.05	-0.40
	(0.02)	(0.02)	(0.02)	(0.01)	(0.06)
Adjusted R^2	0.01	0.03	0.15		
Observations	5,900	5,900	5,740	5,740	5,740
Controls					
Kindergarten Test Scores		X	X	X	X
Additional Covariates			X	X	X

Notes: "Additional Covariates" are described in the text and are listed in Appendix Table 1. Standard errors, in parentheses, are robust to clustering among students attending the same school. All sample sizes are rounded to the nearest 10 to comply with Institute for Education Sciences restricted-use data publication policy. The estimates in column (4) are matching estimates using kernel density matching, as in column (1) of Table 2. The estimates in column (5) are the estimates of bias based on the Altonji et al. (2005) approach, as in column (2) of Table 3.

Table 5: Kernel Density Propensity Score Weighting Estimates of the Effects of Catholic Primary Schooling in ECLS-K and NELS:88

	NELS:88		ECLS-K		
Outcome	(1)	(2)	(3)	(4)	(5)
8th Grade Math Scores	7.97	1.96	7.17	-1.88	-5.39
	(1.56)	(1.08)	(1.69)	(1.58)	(1.53)
Observations	15,460	13,580	6,590	6,590	6,590
8th Grade Reading Scores	12.29	6.66	13.55	5.01	1.48
	(1.28)	(1.13)	(1.15)	(1.59)	(1.72)
Observations	15,460	13,570	6,540	6,540	6,540
Locus of Control Scale, 8th Grade	0.16	0.04	0.15	0.06	0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	12,290	10,760	6,550	6,550	6,550
Repeated a grade, K-8	-0.05	-0.03	-0.10	-0.03	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	15,960	14,010	6,610	6,610	6,610
Suspended in 8th Grade	-0.09	-0.08	-0.14	-0.08	-0.08
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	10,420	9,230	5,900	5,900	5,900
NELS:88 Controls		X		X	X
Kindergarten Test Scores					X

Notes: "NELS:88 Controls" include those variables available in both NELS:88 and ECLS-K, including the measures of demographics, family background, parent marital status, region, urbanicity, parental education, and family income listed in Appendix Table 1. Standard errors, in parentheses, are robust to clustering among students attending the same school. All sample sizes are rounded to the nearest 10 to comply with Institute for Education Sciences restricted-use data publication policy.

Appendix Table 1: Summary Statistics by School Sector, ECLS-K

	Full Sample (N=8,260)		Catholic Kindergarten (N=1,150)	Public Kindergarten (N=7,110)
Variable	Mean	S. D.	Mean	Mean
Percentile Math Scores				
Fall Kindergarten	53.01	28.23	64.66	51.12
Spring Kindergarten	53.25	27.87	63.20	51.62
1st Grade	53.03	28.25	60.19	51.86
3rd Grade	52.43	28.47	56.79	51.72
5th Grade	51.54	28.61	55.83	50.85
8th Grade	51.44	28.53	57.02	50.48
Percentile Reading Scores				
Fall Kindergarten	50.59	28.38	62.28	48.69
Spring Kindergarten	51.47	27.98	61.15	49.89
1st Grade	52.32	27.91	59.70	51.11
3rd Grade	52.84	28.10	62.06	51.34
5th Grade	51.91	28.38	61.88	50.30
8th Grade	51.37	28.24	61.96	49.52
Non-Cognitive Outcomes				
Days Absent, 5th Grade	6.55	7.44	5.31	6.75
Days Tardy 5th Grade	2.99	6.92	3.53	2.90
Locus of Control Scale	0.01	0.62	0.14	-0.01
Repeated a Grade, K-8	0.09	0.29	0.05	0.10
Suspended in 8th Grade	0.13	0.34	0.04	0.16
Demographics				
Female	0.50	0.50	0.50	0.50
Asian	0.06	0.23	0.06	0.05
Hispanic	0.14	0.35	0.14	0.14
Black	0.13	0.33	0.04	0.14
Native American	0.04	0.18	0.02	0.04
Multi-race	0.03	0.16	0.02	0.03
Race missing	0.00	0.04	0.00	0.00

Appendix Table 1: Summary Statistics by School Sector, ECLS-K (cont.)

	Full Sample (N=8,260)		Catholic Kindergarten (N=1,150)	Public Kindergarten (N=7,110)
Variable	Mean	S. D.	Mean	Mean
Family Structure				
Live with Mother + G'parent	0.05	0.23	0.03	0.06
Live with Father + G'parent	0.01	0.08	0.00	0.01
Live with Mother Only	0.17	0.37	0.08	0.18
Live with Father Only	0.01	0.12	0.01	0.01
Live with Other Family	0.03	0.17	0.02	0.03
Family Structure Missing	0.05	0.22	0.03	0.05
Parental Marital Status				
Separated	0.04	0.20	0.02	0.04
Divorced	0.08	0.27	0.05	0.08
Widowed	0.01	0.09	0.01	0.01
Never Married	0.12	0.32	0.04	0.13
Marital Status Missing	0.05	0.22	0.03	0.06
Region				
Midwest	0.28	0.45	0.38	0.26
South	0.31	0.46	0.16	0.34
West	0.20	0.40	0.21	0.20
Urbanicity				
Suburban	0.39	0.49	0.34	0.40
Rural	0.26	0.44	0.13	0.29
Parental Education				
Mother's Education	13.06	3.23	14.18	12.88
Missing Mother's Education	0.04	0.19	0.02	0.04
Father's Education	11.03	5.77	13.10	10.70
Missing Father's Education	0.19	0.40	0.10	0.21
Log Family Income	10.28	1.99	10.84	10.19
Family income Missing	0.03	0.17	0.01	0.03
Log Family Size	1.39	0.42	1.41	1.39
Catholic School Kindergarten	0.14	0.35	1.00	0.00

Appendix Table 2: Propensity Score Estimates of the Effects of Catholic Primary Schooling on 5th and 8th Grade Test Score Percentiles, ECLS-K, UNWEIGHTED

	Matching Method		
Outcome	Kernel	Nearest Neighbor	Caliper / Radius
8th Grade Math Score	-4.92	-5.57	-4.77
Percentiles	(0.86)	(1.19)	(1.02)
8th Grade Reading Score	0.93	-0.44	0.09
Percentiles	(0.81)	(1.23)	(1.39)
5th Grade Math Score	-7.40	-8.19	-8.44
Percentiles	(0.70)	(0.96)	(1.09)
5th Grade Reading Score	-0.99	-1.66	-1.45
Percentiles	(0.89)	(1.08)	(0.94)

Notes: Standard errors are in parentheses. The variables used to create the estimated propensity scores include all variables in column (3) of Table 1. The kernel, caliper, and nearest neighbor methods are described in the text, and are identical to the methods used in Table 3.

Appendix Table 3: Selected Matched and Unmatched Summary Statistics, ECLS-K

	Public (N=7110)	Catholic (N=1150)	Public Matched (N = 6820)
Fall Kindergarten Percentile Test Scores			
Math	51.12	64.66	64.51
Reading	48.69	62.28	61.99
Demographics			
Female	0.50	0.50	0.51
Asian	0.05	0.06	0.06
Hispanic	0.14	0.14	0.13
Black	0.14	0.04	0.04
Native American	0.04	0.02	0.02
Multi-race	0.03	0.02	0.02
Race missing	0.00	0.00	0.00
Family Structure			
Live with Mother + G'parent	0.06	0.03	0.03
Live with Father + G'parent	0.01	0.00	0.00
Live with Mother Only	0.18	0.08	0.09
Live with Father Only	0.01	0.01	0.01
Live with Other Family	0.03	0.02	0.02
Family Structure Missing	0.05	0.03	0.02
Parental Marital Status			
Separated	0.04	0.02	0.02
Divorced	0.08	0.05	0.05
Widowed	0.01	0.01	0.01
Never Married	0.13	0.04	0.05
Marital Status Missing	0.06	0.03	0.02
Region			
Midwest	0.26	0.38	0.40
South	0.34	0.16	0.17
West	0.20	0.21	0.19
Urbanicity			
Suburban	0.40	0.34	0.36
Rural	0.29	0.13	0.13
Parental Education			
Mother's Education	12.88	14.18	14.36
Missing Mother's Education	0.04	0.02	0.01
Father's Education	10.70	13.10	13.22
Missing Father's Education	0.21	0.10	0.09
Log Family Income	10.19	10.84	10.96
Family income Missing	0.24	0.21	0.20
Log Family Size	1.39	1.41	1.43