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Historical religious concentrations and the effects of Catholic schooling ☆

Danny Cohen-Zada [a,*], Todd Elder [b]

[a] Ben-Gurion University, Department of Economics, P.O. Box 653, Beer-Sheva 84105, Israel
[b] Michigan State University, Department of Economics, 110 Marshall-Adams Hall, East Lansing, MI 48824-1038, USA

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ABSTRACT

The causal effects of Catholic schooling on student outcomes have proven challenging to estimate, with several previous studies using the proportion of a geographic unit's population which is Catholic as a potentially exogenous source of variation in the availability of Catholic high schools. We propose a new approach which instead relies on the historical distribution of religious preferences. Specifically, we find that county-level Catholic shares measured at the end of the 19th century are far more strongly associated with Catholic school attendance than are current Catholic shares. Using several strategies, we show that historical Catholic shares are likely to be exogenous to student outcomes conditional on the current distribution of religion. Estimates based on this identification strategy point to smaller Catholic schooling effects than those implied by OLS, in contrast to instrumental variables estimates from previous studies.

1. Introduction

Numerous studies have attempted to quantify the causal effects of Catholic school attendance on student outcomes.[1] Acknowledging that selection of students into Catholic schools is non-random but lacking experimental data, researchers have typically relied on instrumental variables (IV) strategies to disentangle causal effects from spurious correlations due to sorting. Several creative instruments have been proposed, including those based on a student's own religion, the religious composition of the local population (both due to Evans and Schwab (1995)), and the local availability of Catholic schools (Neal, 1997).

Although these IV strategies were all plausibly valid for identifying Catholic schooling effects, Altonji et al. (2005a) recently provided several indications that the proposed exclusion restrictions fail in practice. First, the instruments are strongly related to student outcomes among eighth graders attending public schools. Since public eighth graders almost never attend Catholic high schools, a reduced-form relationship between an instrument and outcomes in this subsample suggests that the instrument directly affects outcomes and is therefore not excludable. Second, 2SLS estimates have typically yielded implausibly large Catholic school effects, much larger than the corresponding OLS estimates. Third, the proposed instruments are strongly associated with observable determinants of outcomes, and Altonji et al. (2005a) argue that this selection on observables implies substantial selection on unobservables as well. These authors conclude that the prospects for finding valid exclusion restrictions in this setting are poor, so they develop new methods that allow for the estimation of bounds on treatment effects in the absence of valid instruments (Altonji et al., 2005b).[2]

In this paper, we propose a new strategy for identifying the effects of Catholic schooling. We use student-level data from the National Educational Longitudinal Study of 1988 (NELS:88) and the Early Childhood Longitudinal Study – Kindergarten Cohort (ECLS-K), together with county-level data from several sources on the Catholic share in the population at different points in time, to show that the fraction of Catholics in the county population in 1890 (the earliest date that this measure is available) can serve as a potentially useful instrument for Catholic school attendance.[3] First, we find that the 1890 local Catholic share is substantially more powerful than the current Catholic share in explaining the current supply of Catholic schools, many of which were established in the early 20th century. Consequently, it is also a stronger predictor of current

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* Corresponding author. Fax: +972 8 6472941.
E-mail addresses: danoran@bgu.ac.il (D. Cohen-Zada), telder@msu.edu (T. Elder).

[1] Coleman et al. (1982), Murnane (1984), Tyler (1994), Evans and Schwab (1995), Sander and Krautmann (1995), Sander (1996), Neal (1997), Vella (1999), Grogger and Neal, 2000, Jepsen (2003), and Dee (2005) are prominent examples.

[2] Jepsen and Montgomery (2009) find that measures of the local availability of community colleges are exogenous to educational attainment, providing reason for optimism that location-based instruments can be useful in some circumstances.

[3] Cohen-Zada (2009) similarly found county-level historical Catholic population shares in 1890 to be a valid instrument for private school competition.

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Catholic school attendance. Second, a host of evidence suggests that historical Catholic shares are much more likely to be exogenous to student outcomes than are current Catholic shares. For example, following the logic of Altonji et al. (2005a), the 1990 Catholic share has a significantly positive effect on college attendance and on 12th grade math test scores in a sample of public eighth graders, but historical Catholic shares (from 1890 and 1906, specifically) are not significantly related to these outcome measures. We also use ECLS-K data to show that current Catholic shares are correlated with math and reading test scores of children in the fall of their kindergarten year (obviously before any students had attended Catholic high schools), while Catholic shares in 1890 are not correlated with these outcome measures.

Although current Catholic shares do not appear to be useful instrumental variables for identifying Catholic schooling effects, these measures still play a large role in our preferred estimation strategy, which involves using measures of historical Catholic shares as instrumental variables while directly controlling for current Catholic shares (and Catholic religion) in models of outcomes. The inclusion of current Catholic shares does not substantially weaken the identifying power of historical shares, while it has the advantage of proxying for contemporary unobservables that are potentially correlated with the historical shares. For example, as we show below, the fraction of a county that is of Hispanic ethnicity is correlated with both current and historical Catholic shares, with one-standard-deviation increases in current and 1890 Catholic shares being associated with 7- and 4-percentage point increases in the fraction of the population that is Hispanic, respectively. However, *conditional* on the current Catholic share, a one-standard-deviation increase in the 1890 Catholic share is only associated with a 0.3-percentage point increase in the fraction Hispanic. Since this pattern exists across many observable measures in NELS:88, one might suspect that it also holds among unobservables, which would imply that historical Catholic shares are exogenous to student outcomes conditional on local Catholic shares. In fact, we find essentially no relationship between 1890 Catholic shares and outcomes in kindergarten in ECLS-K or in the NELS:88 public eighth grade subsample when the current Catholic share is included as a control.

In contrast to instrumental variables estimates from previous studies, our central results point to smaller Catholic schooling effects than those implied by OLS. This pattern implies that students positively select into Catholic schools, as Altonji et al. (2005b) found, but that Catholic schools may have a modest causal effect on educational attainment. While it is outside the scope of this paper to explore the specific mechanisms by which Catholic schooling may affect educational attainment, the previous literature has provided several explanations. First, Catholic schools may deter class misbehavior by tightly enforcing discipline; Coleman et al. (1982) report that the percentage of students who assess the enforcement of discipline as "excellent" or "good" is much higher in Catholic schools than in public schools (72% versus 42%). Second, Catholic schools can more easily regulate attendance and provide better peer groups than public schools, which cannot reject students in their jurisdiction (Figlio and Ludwig, 2000). Third, religious instruction in Catholic schools may alter the preferences of teens, reducing the likelihood of undertaking activities that lead to dropping out (Figlio and Ludwig, 2000). Finally, Catholic schools benefit from extraordinarily committed teachers, and Koedel (2008) provides recent evidence that teacher quality positively affects high school completion rates.[4] Our results leave the door open for an effect of Catholic schooling on educational attainment through one of these mechanisms.

2. Data

We match student-level data from NELS:88 and ECLS-K to county-level data on local Catholic shares at different points in time, created from several sources. This section describes the data and sample construction. For more detail about individual variable coding and treatment of missing values, interested readers should see Appendix A.

2.1. NELS:88 and ECLS-K

NELS:88 is a nationally representative sample of eighth graders that was initially conducted in 1988 by the US National Center for Education Statistics (NCES). This survey included 24,599 students from 1032 schools, with subsamples of these respondents resurveyed in 1990, 1992, 1994, and 2000 follow-ups. The survey provides information on household and individual backgrounds and on achievement and behavior measured prior to high school. For all students included in the base-year sample, NELS:88 includes detailed Census zip code-level information on their eighth grade school, which allows for identification of the zip code in which the school is located; we treat this as the zip code of the student's home.[5] This allows for a merging with the county-level data described below on the local Catholic share in the population at different points in time. Our central outcome measures are indicators for high school graduation, college attendance (defined as being enrolled in a four-year university as of the date of the 1994 survey), and 12th grade math and reading Item Response Theory test scores. Although NCES includes sampling weights for each follow-up, our results are largely insensitive to the use of these weights, so we present unweighted estimates below.

In order to investigate whether a particular instrument is exogenous to student outcomes, we also analyze the base year of the ECLS-K survey, which includes 18,644 kindergarteners from over 1000 schools in the fall of the 1998–1999 school year. The outcomes here are a child's percentile rank on fall 1998 (shortly after entering kindergarten) math and reading IRT tests. The advantage of this study is that it provides descriptive information on a child's achievement upon entry into formal schooling.[6] As in NELS:88, the base year survey includes information on the school's zip code, which permits merging of these data with county-level measures of the fraction of the population that it Catholic.[7]

2.2. Historical Catholic share data

Data on the share of Catholics in the population in 1952, 1971, 1980 and 1990 were taken from the *Religious Congregation and Membership in the United States* (Jones et al., 2000). Similarly, the American Religion Data Archive contains historical data on the number of Catholic members in each county in 1890, 1906, and

[4] Bryk et al. (1993), Coleman et al. (1982), Coleman and Hoffer (1987) and Figlio and Ludwig (2000) all note the relative dedication of Catholic school teachers as a possible mechanism underlying the educational attainment gap between public and Catholic school students.

[5] These zip code characteristics of the eighth grade school are only available on the NELS:88 Restricted Use files, available by special request from NCES.

[6] Although some ECLS-K students attend Catholic primary school, so that a reduced-form correlation between test scores and Catholic shares could in principle reflect the effect of Catholic primary school, the fall 1998 assessments are given so early in the kindergarten year that it is reasonable to assume that essentially no in-school human capital accumulation has yet taken place.

[7] Zip code is only available in the base year ECLS-K Restricted Use Geographic Identifier file, available by special permission from NCES. Further information about the ECLS-K sampling design can be found on the NCES website: http://nces.ed.gov/pubsearch/pubsinfo.asp?pubid=2004089. Note that below we present unweighted estimates for models involving ECLS-K data, but the results are robust to the use of sampling weights.

1916, originally collected by the US Census (US Bureau of the Census, Religious Bodies, 1890, 1906, 1916). The Geospatial and Statistical Data Center at the University of Virginia provides county-level data on population sizes in 1890, 1910, and 1920, also originally taken from the Census of the population of the respective years (US Bureau of the Census, 1890, 1910, 1920).

We obtained the share of Catholics in each county's population by dividing the number of Catholic members of each county by its total population. Since data on population size in 1906 and 1916 are not available, we averaged the population in two adjacent censuses; for example, we proxy for the total population in 1906 by calculating the simple average of the 1900 and 1910 populations.

3. Historical Catholic shares, the supply of Catholic schools, and Catholic school attendance

We first consider evidence that the locations of Catholic high schools are largely determined by historical demographic patterns rather than current circumstances. The NCES administers the biennial Private School Surveys to collect detailed information on private schools in the US, and among the 8643 Catholic schools operating in the 1989–1990 school year (one year after NELS:88 respondents typically entered high school), the survey reports the year that 8226 were constructed (US Department of Education, 1989–1990).[8] Table 1 presents the distribution of Catholic schools with respect to the year they started operating, along with analogous distributions for all other religious private schools (in the "Other religious" column) and all non-religious private schools (the "Non-sectarian" column) in operation in 1990. The top panel shows that among Catholic schools operating in 1990, more than half began operating by 1950 and more than a quarter by 1920. In contrast, fewer than 10% of non-Catholic private schools began operating before 1920 and fewer than 20% before 1950. The bottom panel shows similar patterns among high schools only.[9] Although public school-level data analogous to the Private School Survey is not publically available for 1990, data from NCES's "Fast Response Survey System" in 1995 indicate that 24% of public high schools in operation in that year were built before 1950 (US NCES, 1996). Overall, Catholic schools tend to be much older than other secondary schools, both public and private.

Figs. 1a and 1b provide more direct evidence that the locations of Catholic high schools are largely driven by historical circumstances. The figures depict the proportion of students in NELS:88 with a Catholic school in their own zip code as a function of the proportion of the zip code's population that is Catholic in 1890 and 1990, respectively. The size of the data point is proportional to the number of children at each integer value of the Catholic share, so in 1890, when the modal value of the Catholic share was zero, the largest circle corresponds to a value of zero. The 1890 Catholic share is relatively more powerful in explaining the existence of a Catholic high school in the student's zip code in 1990, with a Pearson correlation coefficient of 0.23 for 1890 and 0.17 for 1990 (implying R^2 values of the fitted lines of 0.05 and 0.03, respectively). This pattern is consistent with the fact that many Catholic high schools were built in the first half of the 20th century, so they are presumably located near Catholic population centers from that period.

Similarly, Figs. 2a and 2b depict the proportion of students attending a Catholic high school by the proportion Catholic in the population in 1890 and 1990, respectively. Again, the correlation for 1890 is modestly higher than that for 1990 (0.17 versus 0.14), which is not surprising given Figs. 1a and 1b – a household's choice between public and Catholic schools is likely influenced by the supply of Catholic schools, so historical Catholic shares are important for both the location of Catholic schools and for attendance decisions.

Table 1
Private school ages by sector.

Year	Percentage of current schools that began operating this year or earlier		
	Catholic	Other religious	Non-sectarian
Panel A: all schools			
1900	14.3	6.5	5.9
1910	19.5	8.0	7.3
1920	26.5	9.9	9.6
1930	37.6	11.7	12.1
1940	42.1	13.2	14.5
1950	50.2	19.1	17.7
1960	70.3	28.0	24.4
1970	82.5	40.8	49.0
1980	85.4	76.6	80.4
Panel B: high schools only			
1900	17.4	3.3	9.6
1910	22.0	4.6	11.2
1920	26.5	6.4	14.0
1930	36.5	7.5	17.0
1940	41.2	8.6	18.8
1950	48.0	13.1	22.0
1960	66.3	18.5	28.5
1970	84.8	31.2	54.8
1980	88.8	77.9	83.9

Note: Authors' calculations from the NCES-administered Private School Survey of 1989–1990.

Although these raw correlations are suggestive, Table 2 illustrates the relative importance of current and historical religious composition more clearly. Column (1) of the table presents estimates from a linear probability model of Catholic school attendance as a function of the Catholic share in the population in 1890 and 1990 (*pcath1890* and *pcath1990*, hereafter). Both regressors are measured in standard deviation units, so that the coefficient of 0.037 (0.003) on *pcath1890* implies that a one-standard-deviation increase in *pcath1890* (with no analogous shift in *pcath1990*) would increase Catholic school attendance by 3.7 percentage points, a large effect relative to the mean attendance rate of 0.06. In contrast, a one-standard-deviation shift in *pcath1990* with no accompanying change in *pcath1890* would increase the rate of Catholic schooling by only 0.5 percentage points. Put another way, since the standard deviation of *pcath1890* is roughly 0.095, an increase in *pcath1890* from 0 to 1, representing a shift from a county with no Catholic residents to one with *only* Catholics, is associated with an estimated 40-percentage point increase in the rate of Catholic high school attendance. A similar shift in current Catholic shares would increase Catholic high school attendance by only 3 percentage points (since the standard deviation of *pcath1990* is 0.175). Column (2), which includes a detailed vector of student-level controls, yields similar conclusions.

Columns (3) and (4) of the table present models in which the dependent variable is an indicator for the presence of a Catholic high school in the student's own zip code, while in columns (5) and (6) the dependent variable is a binary measure of attendance in a Catholic school in eighth grade. The story is remarkably similar across columns – the 1890 Catholic share is much more strongly associated with Catholic school attendance and the supply of Catholic schools than is the current (1990) local Catholic share. In fact, for a given level of *pcath1890*, the current religious composition of a county appears unrelated (or possibly negatively related, judging from columns (4) and (6)) to measures of Catholic schooling. These patterns provide compelling evidence that historical religious

[8] See http://nces.ed.gov/surveys/pss/ for more information about these data.

[9] The PSS includes information on the grade span of schools, and our definition of "high school" corresponds to schools which include 9th, 10th, 11th, or 12th grades, as well as schools including "high school-like" curricula that is ungraded.

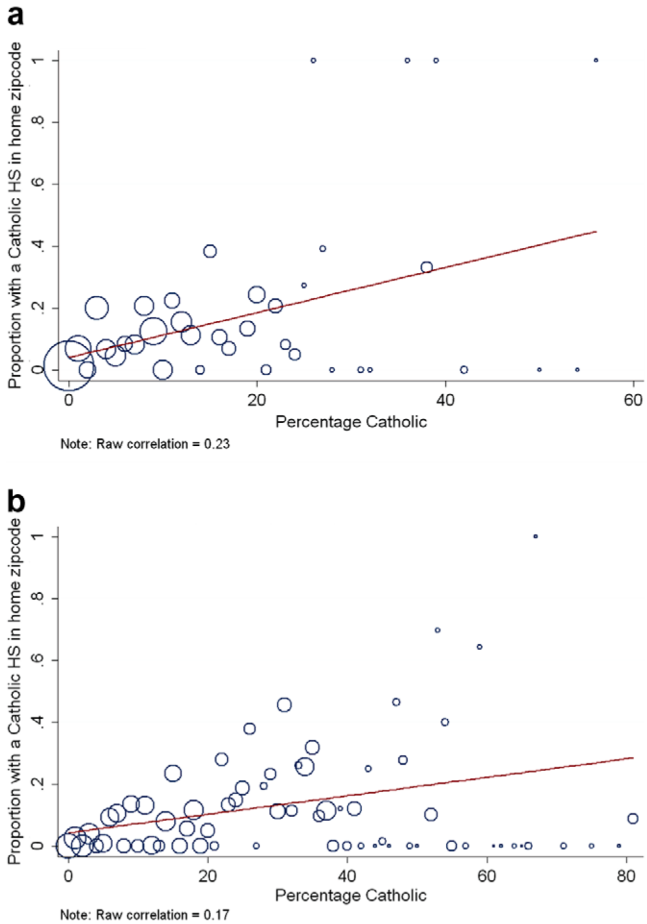

Fig. 1. (a) Proportion of students with a Catholic school in their own zip code by county Catholic share – 1890. (b) Proportion of students with a Catholic school in their own zip code by county Catholic share – 1990.

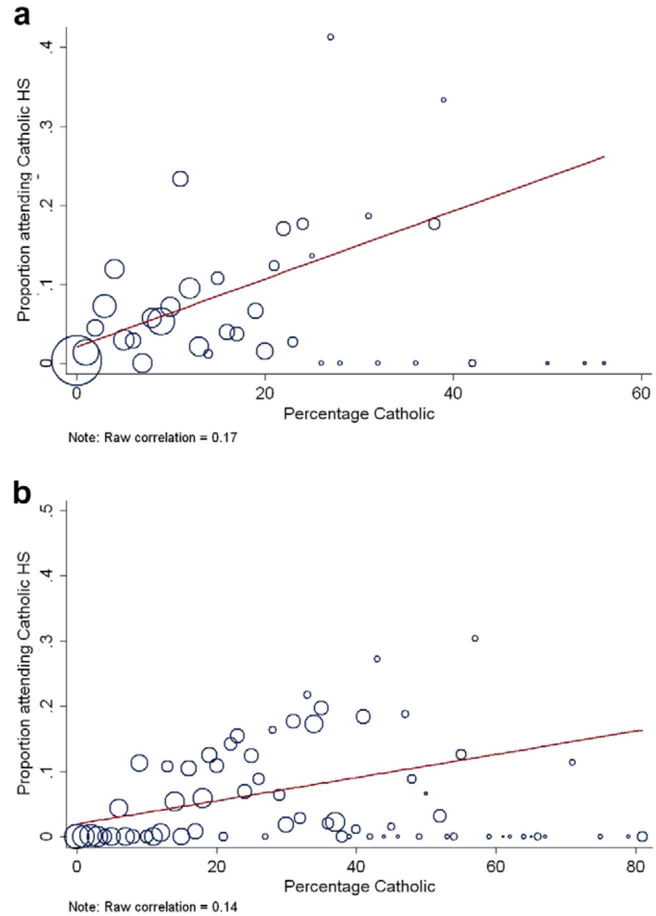

Fig. 2. (a) Catholic school attendance by county Catholic share – 1890. (b) Catholic school attendance by county Catholic share – 1990.

preferences are the primary determinants of both the location and enrollment of Catholic schools.[10]

4. Evidence for the exogeneity of historical Catholic shares

Historical Catholic shares are strongly predictive of Catholic school attendance, but they are useful sources of variation for identifying Catholic schooling effects only if they are exogenous to student outcomes. We attempt to assess the validity of these potential exclusion restrictions in three ways: by measuring the correlation between these candidate instruments and observed characteristics in NELS:88, by examining the reduced-form relationship between the instruments and outcomes among public eighth grade attendees in NELS:88, and by estimating the association between the instruments and outcomes among kindergarteners in ECLS-K.

Table 3 reports summary statistics for the NELS:88 and ECLS-K data. For NELS:88, the variables listed represent all covariates used in outcome models, as well as the outcomes themselves. Columns 1 and 2 report means and standard deviations of the variables, while columns 3 through 6 report the slope from a univariate regression of the variable listed in the first column on the variable listed in the column heading. For example, the value of −0.02 in the row labeled "*Female*" and the column labeled "*pcath1990*" indicates that an increase of *pcath1990* from zero to one, representing a shift from a county population with no Catholic members to one with all Catholics, is associated with a 2-percentage point decline in the average of the "*Female*" dummy variable. In the fourth column, *pcath1890* is the only regressor, while the fifth column, labeled "*pcath1890|pcath1990*", shows the coefficient on *pcath1890* in models that also include *pcath1990* as a control, i.e., estimates of α_1 in models such as

$$X_i = \alpha_0 + \alpha_1 pcath1890_i + \alpha_2 pcath1990_i + u_i, \quad (1)$$

where X_i denotes the value of an observable characteristic for individual *i*. Finally, the numbers reported in column 6 represent the difference in the mean value of the row variable by the value of *CH*. Numbers in bold are distinguishable from zero at the 5% significance level. Since the *pcath* measures do not vary within county, we use clustered standard errors that allow for arbitrary within-county correlation for all analyses.

As expected, selection into Catholic schools appears non-random, with Catholic school attendees showing significant advantages on most dimensions in column 6 (notably parental education, log family income, and all measures of eighth grade achievement and behavior). Neither *pcath1990* nor *pcath1890* appear to be as good as if they were randomly assigned, although the differences in these columns are less dramatic than in the *CH* column. However, conditional on *pcath1990*, the association between *pcath1890* and observable student characteristics is quite weak – in column 5, only one of the 16 measures of eighth grade achievement (eighth grade GPA) is significantly correlated with *pcath1890* conditional on *pcath1990*, compared to 5 for *pcath1890* unconditionally, 10

[10] The implications of Table 2 are unchanged if the models are estimated as probits rather than linear probability models. For example, the estimated average marginal effect associated with *pcath1890* in column (2) of the table is 0.028, while the marginal effect associated with *pcath1990* is 0.005.

Table 2
Estimates of the relationship between county Catholic share, the supply of Catholic schools, and Catholic school attendance.

	Catholic HS attendance		Catholic HS in own zip code		Catholic eighth grade attendance	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable						
pcath1890	0.037 (0.003)	0.029 (0.003)	0.068 (0.004)	0.061 (0.005)	0.058 (0.004)	0.054 (0.004)
pcath1990	0.005 (0.003)	0.005 (0.004)	0.001 (0.004)	−0.012 (0.005)	0.003 (0.004)	−0.015 (0.005)
R^2	0.030	0.123	0.050	0.089	0.044	0.150
Covariates?	No	Yes	No	Yes	No	Yes

Note: The entries for each model are the coefficient and standard error in parentheses. Covariates are described in the text.

for *pcath1990*, and all 16 for Catholic school attendance. Similarly, the R^2 from a regression of *pcath1890* on *pcath1990* is 0.5336, and this value rises by less than 0.006 when all observable covariates are included in the regression. This implies that, conditional on

Table 3
Summary statistics in NELS:88 and ECLS-K.

NELS:88 variables	Mean	SD	Univariate regression on			
			pcath1990	*pcath1890*	*pcath1890|pcath1990*	*CH*
Female	0.52	0.50	−0.02	−0.03	−0.04	−0.03
Asian	0.05	0.22	**0.04**	**0.09**	0.04	0.00
Hispanic	0.11	0.31	**0.67**	**0.43**	0.03	−0.01
Black	0.09	0.28	**−0.22**	**−0.13**	0.11	−0.01
Mother only in house	0.14	0.35	0.03	0.07	**0.11**	**−0.04**
Married	0.02	0.15	0.03	−0.07	**−0.18**	**0.07**
Urban	0.21	0.41	**0.38**	**0.75**	**0.83**	**0.36**
Suburban	0.43	0.50	**0.95**	**0.65**	−0.29	0.01
Father's education	12.74	4.00	0.13	−0.48	−1.34	**1.14**
Mother's education	13.08	2.53	0.22	0.42	0.20	**0.81**
Log of family income	9.91	2.07	**−0.42**	−0.26	−0.05	**0.19**
Eighth grade reading	51.41	9.85	0.50	1.07	−0.02	**3.76**
Eighth grade math	51.56	10.02	**2.80**	**2.99**	−0.36	**2.89**
Delinquency index (range 0–4)	0.60	1.05	**0.30**	**0.39**	0.31	**−0.14**
Student got in fight	0.23	0.50	0.02	0.02	0.03	**−0.02**
Student performs below abilities	0.23	0.35	−0.04	−0.01	0.04	**−0.10**
Student rarely completes homework	0.17	0.31	−0.01	0.01	0.03	**−0.10**
Student frequently absent	0.09	0.24	**−0.02**	0.00	0.02	**−0.05**
Student frequently tardy	0.04	0.16	**0.03**	0.01	−0.01	**−0.02**
Student inattentive in class	0.18	0.32	**−0.06**	−0.05	0.00	**−0.08**
Student frequently disruptive	0.11	0.26	**−0.05**	**−0.06**	−0.03	**−0.03**
Limited english proficiency	0.02	0.14	**0.03**	0.00	−0.03	**−0.01**
Student repeated grade	0.06	0.23	**−0.02**	0.00	0.03	**−0.04**
Student had grade trouble	0.50	0.80	**0.40**	**0.28**	−0.12	**−0.14**
Risk index	0.65	0.93	0.04	0.06	0.22	**−0.32**
Eighth grade GPA	2.98	0.74	**−0.12**	**−0.25**	**−0.35**	**0.21**
Unpreparedness index	10.77	2.13	−0.23	−0.20	0.03	**0.19**
HS graduate	0.88	0.33	**0.06**	0.04	−0.05	**0.11**
Enrolled in 4-year college in 1994	0.33	0.47	**0.48**	**0.33**	−0.06	**0.29**
12th grade math score	51.86	9.54	**5.35**	**4.22**	−0.62	**3.74**
12th grade reading score	51.55	9.45	**2.43**	1.86	−0.42	**3.06**
Catholic high school attendance	0.06	0.23	**0.31**	**0.41**	**0.37**	**1.00**
pcath1890	9.30	9.48				
pcath1916	13.98	14.78				
pcath1952	18.00	16.95				
pcath1971	20.23	17.06				
pcath1990	21.04	17.49				

ECLS-K variables			Univariate regression on		
			pcath1990	*pcath1890*	*pcath1890|pcath1990*
Female	0.52	0.50	0.06	0.04	−0.02
Asian	0.05	0.22	**0.11**	**0.08**	−0.04
Hispanic	0.11	0.31	**0.51**	**0.52**	0.25
Black	0.09	0.28	**−0.25**	−0.05	0.24
Urban	0.40	0.49	0.01	**0.70**	**0.70**
Suburban	0.38	0.49	**0.90**	0.30	−0.77
Father's education	13.59	2.36	**1.50**	0.08	−1.09
Mother's education	13.40	2.17	**0.98**	0.42	−0.65
Log of family income	10.51	0.96	**0.69**	0.07	−0.92
Kindergarten reading score	50.48	28.87	**21.20**	5.50	−24.15
Kindergarten math score	50.48	28.87	**13.80**	2.00	−21.43

pcath1990, there is little association between *pcath1890* and observable determinants of outcomes.[11]

4.1. Reduced-form relationships between Catholic shares and outcomes

We next turn to more direct evidence on the viability of Catholic population shares as instrumental variables, focusing on their reduced-form relationships with outcomes. To see why reduced forms can be informative about the validity of potential instruments, it is helpful to introduce some notation. Let the structural equation relating an outcome Y_i to Catholic school attendance CH_i be given by

$$Y_i = \alpha CH_i + X_i'\beta + \varepsilon_i, \tag{2}$$

where X_i is a vector of observable characteristics that includes a constant. Similarly, assume that the population model linking CH_i to X_i and the candidate instrument Z_i can be written as

$$CH_i = \rho Z_i + X_i'\delta + v_i \tag{3}$$

where X_i is uncorrelated with both errors and Z_i is uncorrelated with v_i (note that both of these assumptions, as well as the linearity of X_i in (2) and (3), can be relaxed with no substantive effect other than the need for additional notation). As is well known, the asymptotic bias in an IV estimate of α is $\frac{\mathrm{cov}(\widetilde{Z}_i,\varepsilon_i)}{\rho \mathrm{var}(\widetilde{Z}_i)}$, where $\widetilde{Z}_i$ denotes the residuals from a linear projection of Z_i on X_i.[12]

Consider the reduced-form relationship between an instrument and an outcome:

$$Y_i = \tau Z_i + X_i'\gamma + \xi_i. \tag{4}$$

An OLS estimate of τ based on (4) converges to

$$\frac{\mathrm{cov}(\widetilde{Z}_i,\widetilde{Y}_i)}{\mathrm{var}(\widetilde{Z}_i)} = \frac{\mathrm{cov}(\widetilde{Z}_i,\alpha C\widetilde{H}_i+\varepsilon_i)}{\mathrm{var}(\widetilde{Z}_i)} = \frac{\mathrm{cov}(\widetilde{Z}_i,\alpha\rho\widetilde{Z}_i+\alpha v_i+\varepsilon_i)}{\mathrm{var}(\widetilde{Z}_i)}$$

$$= \alpha\rho + \frac{\mathrm{cov}(\widetilde{Z}_i,\varepsilon_i)}{\mathrm{var}(\widetilde{Z}_i)} \tag{5}$$

Expression (5) shows why reduced forms are typically uninformative about the exogeneity of Z_i, because a non-zero estimate of τ could reflect either the indirect effect of Z_i on outcomes through CH_i, given by $\alpha\rho$, or the direct effect, given by $\frac{\mathrm{cov}(\widetilde{Z}_i,\varepsilon_i)}{\mathrm{var}(\widetilde{Z}_i)}$.

However, in a population in which the probability of receiving treatment is zero, ρ is identically zero because $CH_i = 0$ for any value of Z_i. This implies that any association between the candidate instrument and an outcome can be attributed to the direct effect, which is zero if and only if the asymptotic bias in an IV estimate of (2) is zero.

In NELS:88, the probability of receiving treatment is approximately zero for one group – students who attend public grammar schools as eighth graders. Fewer than 0.3% (30 out of 10,143) of these public eighth graders attended Catholic high school, so if a candidate instrument does not influence outcomes apart from its effect on Catholic schooling, it should have no reduced-form association with outcomes in this subsample. The top panel of Table 4 presents estimates of τ among public eighth graders for the four NELS:88 outcomes and five candidate instruments: *pcath1890*, *pcath1990*, and Catholic shares measured in three intermediate time periods: 1916, 1952, and 1971. All models include all covariates listed in Table 3 as well as indicators for state of residence. The results are striking: while the 1990 Catholic share has positive and significant effects on both college attendance and math test scores, these effects decline monotonically as one goes back in time, and in 1890 they are no longer distinguishable from zero. The rightmost column of the table, labeled *pcath1890|pcath1990*, displays estimates of the coefficient on *pcath1890* in reduced forms that also include *pcath1990* as a control. The point estimate of 0.02 (0.08) is the smallest coefficient listed in the "attend college" row, and is less than 10% of the magnitude of the estimate in the *pcath1990* column. It is worth emphasizing that an insignificant estimate of τ here, while not guaranteeing exogeneity, does provide some reassurance. On the other hand, there is ample reason for pessimism about an identification strategy based on the excludability of *pcath1990* from models of college attendance.

In a similar spirit, the bottom panel of the table presents estimates of the "effect" of Catholic shares on test scores among recently enrolled kindergarteners in ECLS-K. The test scores are measured in standard deviation units, so the estimate on *pcath1990* of 1.26 (0.29) for math implies that a 10-percentage point increase in current Catholic share increases math scores by roughly one-eighth of a standard deviation, even though these students had obviously never attended Catholic high school (or attained *any* formal education, since the ECLS tests are administered early in the kindergarten school year). Kindergarten assessments measure very different skills than tests given in high school, but it is natural to be skeptical about the exogeneity of *pcath1990* in light of these strong associations. Note that the estimated effects again decline monotonically as one moves back in time for both math and reading scores. As was the case in the top panel, by 1890 all effects are indistinguishable from zero, both in the models that include *pcath1990* as a control and in the models that do not.

Taken together, the findings of Tables 3 and 4 imply that historical Catholic population shares are less likely to be endogenous to student outcomes than current Catholic shares. Table 4 also suggests that the strength of the relationship between outcomes and the distribution of religious preferences increases roughly monotonically in the year in which these preferences are measured. While historical Catholic shares from years such as 1952 and 1971 may provide more credible identifying information than present-day measures, it is preferable to use shares measured at points more distant in the past, particularly in years before most of the extant Catholic schools were built. In our view, shares from 1916 and earlier are most likely to be useful for identifying Catholic schooling effects, with *pcath1890* being arguably the "best" measure of all, especially in models that directly control for current Catholic shares. Three procedures failed to produce evidence that historical shares are endogenous after conditioning on the current Catholic shares, which apparently proxy well for unobservables correlated with both historical religious preferences and outcomes. This pattern contrasts sharply with the findings of Altonji et al. (2005b), who find abundant evidence against the validity of several other potential instrumental variables that have been proposed in this setting.[13]

[11] In order to further investigate the relationship between the candidate instruments and observables, we adopt Altonji et al. (2005b) methods for assessing bias under the assumption that the relationship between an instrument and unobservable determinants of outcomes is identical to the relationship between the instrument and observables. Heuristically, this procedure measures the association between an instrument and an index of all observable characteristics, with each observable's contribution to the index being proportional to its effect on an outcome. For all four outcomes we study below, the implied bias estimate is insignificantly different from zero in models using *pcath1890* as an instrument while directly controlling for *pcath1990*, but this is not the case when *pcath1990* is used as an instrument. For example, for college attendance the *p*-value associated with the hypothesis of no bias is 0.05 using *pcath1990* as an instrument, and 0.28 using *pcath1890* as an instrument controlling for *pcath1990*.

[12] See Wooldridge (2002, pp. 107–108) and Altonji et al. (2005a, p. 805) for applications of this asymptotic bias calculation.

[13] Altonji et al. (2005a) found that Catholic religion, one's current proximity from a Catholic high school, and the interaction between Catholic religion and proximity all appear endogenous based on eighth grade subsample results and correlations with observable determinants of outcomes.

Table 4
Reduced-form associations between Catholic shares and outcomes among public eighth graders in NELS:88 and kindergarten students in ECLS-K.

	Coefficient on					
	pcath1890	*pcath1916*	*pcath1952*	*pcath1971*	*pcath1990*	*pcath1890|pcath1990*
NELS:88						
College attendance	0.05 (0.06)	0.14 (0.07)	0.18 (0.06)	0.19 (0.07)	0.27 (0.07)	0.02 (0.08)
HS graduation	−0.06 (0.05)	−0.01 (0.05)	−0.03 (0.05)	0.02 (0.05)	0.02 (0.05)	−0.03 (0.06)
12th grade math	−0.23 (0.76)	−0.28 (0.85)	0.02 (0.84)	1.51 (0.89)	1.71 (0.87)	−1.64 (1.09)
12th grade reading	−2.20 (0.90)	−2.08 (1.01)	−2.54 (1.00)	−1.16 (1.05)	−0.82 (1.03)	−2.02 (1.31)
ECLS-K						
Kindergarten math	0.25 (0.27)	0.26 (0.26)	0.80 (0.27)	1.11 (0.29)	1.26 (0.29)	−0.40 (0.41)
Kindergarten reading	−0.11 (0.29)	0.27 (0.27)	1.09 (0.29)	1.38 (0.30)	1.53 (0.31)	−0.67 (0.43)
State indicators?	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (1) Each entry in the table represents the coefficient on the variable given in the column heading from a linear regression of the outcome listed in the row heading, all covariates, and state indicators. The entries for each model are the coefficient and standard error in parentheses. The column labeled "*pcath1890|pcath1990*" displays the coefficient on *pcath1890* in models including *pcath1990* as a covariate. Standard errors are clustered at the county level. (2) All models include indicators for demographics (gender, race, and ethnicity); family education, income, marital status, and household structure; eighth grade test scores; other eighth grade student characteristics; and state indicator variables. (3) Sample sizes: $N = 7295$ (college attendance), $N = 7507$ (HS graduation), $N = 7257$ (12th grade reading and math), $N = 10{,}800$ (kindergarten math), $N = 10{,}243$ (kindergarten reading).

Finally, we caution that although we cannot find any evidence of bias in the empirical strategy we pursue below, potential problems may still exist. Most importantly, since Cohen-Zada (2009) found that historical Catholic shares are correlated with the level of private school concentration (and therefore competition among private schools), these measures may also be correlated with differential private school quality.[14] Specifically, if counties with large values of *pcath1890* have unusually high-quality private schools, then estimates of Catholic schooling effects using *pcath1890* as an instrumental variable will overstate the average effect of Catholic schools. While we cannot address this concern directly, we do find that *pcath1890* is unrelated to county-level measures of public school quality, both unconditionally and conditional on *pcath1990* (detailed results available by request). It is therefore plausible that *pcath1890* is unrelated to private school quality, but if not, the IV estimates below are likely biased toward finding beneficial Catholic schooling effects. As a result, we view the estimates below as plausibly identifying an upward bound on the causal effects of Catholic schooling.[15]

5. Estimates of the causal effects of Catholic high school attendance

Table 5 presents OLS and 2SLS estimates of the effect of Catholic schools on the four outcomes described above. Column 1 presents the OLS estimates, which uniformly imply that Catholic school attendance has a substantial positive effect for students. However, since Catholic school attendance is correlated with almost all observed covariates in NELS:88 (recall that column 6 of Table 3 showed large advantages for Catholic school students in many "desirable" characteristics, such as family income and parental education), these estimates are likely inconsistent for the true Catholic schooling effect.

In column 6 of Table 5 we follow in the spirit of prior studies and report 2SLS estimates using the 1990 Catholic share as an instrumental variable. The estimated effects are much larger than the corresponding OLS estimates for college attendance and math scores, and slightly larger for high school graduation and reading scores. Similar to the 2SLS estimates in previous studies, these coefficients imply strong negative selection into Catholic schools. Column 2 presents 2SLS estimates using the 1890 Catholic share as an instrument. The *F*-statistic for first-stage significance, 169.8, is twice as large as the *F*-statistic in column 6, consistent with the notion that historical Catholic shares are more relevant to Catholic school attendance than are current Catholic concentrations. For all outcomes, the 2SLS estimates are now much smaller than those based on current Catholic share as an instrumental variable, but the estimated treatment effects are still larger than the OLS estimates. Finally, columns 3–5 show estimates based on Catholic shares measured in intermediate years; the first-stage *F*-statistics for the three intermediate years are all smaller than that in column 2 and all larger than that in column 6.

For the reasons given in Section 4, we are not confident that *any* estimate in Table 5 is consistent for the causal effects of Catholic schooling, so we next turn to our preferred specification. Column 1 of Table 6 presents 2SLS estimates using the Catholic share in 1890 as an instrument while controlling for the current Catholic share in the population. The *F*-statistic on the excluded instrument, 81.25, considerably exceeds the Stock and Yogo (2005) weak-instrument threshold. In fact, a comparison with the *F*-statistic from the sixth column of Table 5 implies that 1890 Catholic shares, conditional on 1990 Catholic shares, have roughly the same explanatory power for Catholic school attendance as the 1990 Catholic shares themselves. Controlling for *pcath1990* in 2SLS estimates not only substantially reduces the potential for bias; it does so while paying little cost in terms of the strength of the instrument.

If historical Catholic shares are truly exogenous with respect to outcomes conditional on *pcath1990*, the use of additional historical Catholic shares as instrumental variables should not substantially alter the 2SLS estimates. In column 2 of Table 6, we include 1906 and 1916 Catholic shares as additional instruments, and we also include quadratic functions of all three share measures in an effort to improve precision.[16] The estimates from these specifications

[14] We are grateful to an anonymous referee for suggesting this possibility to us.

[15] Another potential threat to validity could arise if historical Catholic shares are related to county-level measures of population density, which could affect the composition of individuals enrolling in Catholic schools. The empirical results presented below are insensitive to the inclusion of linear and quadratic functions of population density, suggesting that this source of bias does not have serious consequences in practice. We thank an anonymous referee for pointing out this issue.

[16] Cohen-Zada (2009) found that quadratic functions of historical Catholic shares improved precision relative to linear models in the context of the effects of private school competition on student outcomes. In our case, the point estimates are largely insensitive to the use of the squared term as an additional instrument, although precision increases somewhat. For example, for college attendance, the estimate from a linear specification using *pcath1890*, *pcath1906*, and *pcath1916* as instruments is 0.09, with a standard error of 0.14. Additionally, *pcath1906* and *pcath1916* are strongly related, with a Pearson correlation coefficient of 0.91, so models including measures from either but not both years yield very similar results. Full results are available from the authors upon request.

Table 5
Estimates of the effect of Catholic high school attendance on outcomes.

	OLS	2SLS with excluded instruments				
		pcath1890	*pcath1916*	*pcath1952*	*pcath1971*	*pcath1990*
Outcome						
College attendance	0.14 (0.02)	0.46 (0.12)	0.55 (0.14)	0.49 (0.13)	0.56 (0.14)	0.95 (0.19)
HS graduation	0.03 (0.01)	−0.03 (0.09)	0.04 (0.10)	−0.02 (0.09)	0.08 (0.10)	0.06 (0.12)
12th grade math	1.25 (0.24)	1.67 (1.57)	−0.16 (1.87)	1.49 (1.71)	3.90 (1.81)	5.45 (2.24)
12th grade reading	0.82 (0.30)	−2.66 (1.92)	−1.79 (2.26)	−1.79 (2.08)	0.97 (2.17)	1.69 (2.66)
State indicators?	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for first-stage significance of instruments		169.8	120.3	148.3	135.3	87.9
p-value		0.000	0.000	0.000	0.000	0.000

Notes: (1) The entries for each model are the coefficient, standard error in parentheses, and the regression R^2. Standard errors are clustered at the county level. (2) All models include indicators for demographics (gender, race, and ethnicity); family education, income, marital status, and household structure; eighth grade test scores; other eighth grade student characteristics; and state indicator variables. (3) Sample sizes: $N = 7295$ (college attendance), $N = 7507$ (HS graduation), $N = 7257$ (12th grade reading and math).

Table 6
2SLS and bivariate probit estimates of the effect of Catholic high school attendance.

	2SLS estimates		Bivariate probit marginal effects	
	(1)	(2)	(3)	(4)
Outcome				
College Attendance	0.07 (0.15)	0.08 (0.11) [0.69]	0.10 (0.07)	0.08 (0.06)
HS graduation	−0.09 (0.12)	−0.12 (0.09) [0.85]	0.03 (0.07)	0.03 (0.06)
12th grade math	−0.43 (2.22)	0.31 (1.91) [0.24]	–	–
12th grade reading	−3.60 (2.11)	−2.64 (2.36) [0.07]	–	–
Excluded instruments				
pcath1890	Yes	Yes	Yes	Yes
pcath1890^2	No	Yes	No	Yes
Quadratics in *pcath1906* and *pcath1916*	No	Yes	No	Yes
F-stat for first-stage significance of instruments	81.25	68.89	–	–
p-value	0.000	0.000	–	–

Notes: (1) The entries for the 2SLS models are the coefficient, standard error in parentheses, and the *p*-value associated with Sargan's χ^2 test for overidentifying restrictions in brackets, where appropriate. For the bivariate probit models, the entries are the marginal effect and standard error of the marginal effect (in parentheses). Standard errors are clustered at the county level. (2) All models include indicators for demographics (gender, race, and ethnicity); family education, income, marital status, and household structure; eighth grade test scores; other eighth grade student characteristics; state indicator variables; and *pcath1990*. (3) Sample sizes: $N = 7295$ (college attendance), $N = 7507$ (HS graduation), $N = 7257$ (12th grade reading and math).

differ only modestly from those in column 1. Based on Sargan's χ^2 test of overidentifying restrictions (with relevant *p*-values reported in brackets), for all four outcomes we fail to reject the assumption that the instruments are exogenous at the 5% significance level, which again suggests that historical Catholic shares are valid instruments when controlling for current Catholic shares.

While OLS coefficients imply large beneficial effects of Catholic schooling, the 2SLS estimates in column 2 paint a less clear picture. They are noisier than OLS, but it is still informative that they are smaller (less positive) across all four outcomes. Most notably, compared to the large OLS and 2SLS estimates in Table 5 for college attendance, the point estimate here implies a smaller but still sizeable effect of roughly 8 percentage points.

Several previous studies of Catholic schools have used bivariate probit specifications rather than linear 2SLS for binary outcomes, so the final two columns of Table 6 report estimates of marginal effects from bivariate probit specifications for college attendance and high school graduation. In column (3), *pcath1890* is excluded from the outcome models, while column (4) adds its square and quadratics in *pcath1906* and *pcath1916* as additional instruments. For college attendance, the estimates in columns (3) and (4) are quite similar to those in column (2), again implying large (but imprecisely estimated) Catholic schooling effects that are slightly smaller than those from OLS. The high school graduation point estimates are positive in these two columns and are quite similar to those from OLS – all three specifications point to a 3-percentage point effect of Catholic schooling on high school graduation.

In sum, the point estimates in Table 6 imply little effect of Catholic schooling on student achievement, as measured by test scores, but leave the door open for a substantial positive effect on educational attainment. We cannot reject the hypothesis of no effect on test scores, although we *can* reject large positive effects. Unfortunately, like all previous studies in the Catholic schooling literature, our 2SLS estimates are somewhat noisy, preventing a precise statement about the effect on college attendance. The preferred estimates imply that Catholic schooling boosts college attendance rates by 8 percentage points, a large effect that is nonetheless smaller than OLS and insignificantly different from zero. As argued above in Section 4, we interpret these estimates as an upper bound on the causal effects of Catholic schooling due to the potential for correlation between historical Catholic shares and differential Catholic school quality. In order to make more definitive statements about the effect of Catholic schooling on outcomes like college attendance, future work involving large datasets with geographic identifiers, such as US Census long form data, may prove fruitful.[17]

[17] While data sources such as the Census long form data set (available by special permission from the US Census Bureau) have the advantage of large sample sizes, they do not include information on Catholic schooling. As a result, the best one can hope for when using these data are reduced-form relationships between measures like *pcath1890* and outcomes, which can be informative about treatment effects. For directly estimating the treatment effects of Catholic schools, we know of no data source larger than NELS:88.

6. Conclusions and directions for future research

We have proposed a new identification strategy for estimating the treatment effects of Catholic schools. We use county-level measures of Catholic population shares in 1890, 1906, and 1916 as instruments for Catholic school attendance while controlling for the current distribution of religious preferences in a geographic area. These historical Catholic shares are substantially better predictors of Catholic school attendance than are current Catholic shares. Moreover, the components of the historical Catholic shares that are orthogonal to current Catholic shares appear exogenous to outcomes in NELS:88, for two primary reasons. First, there is no significant reduced-form association between these measures and student outcomes among eighth graders attending public schools in NELS:88 or kindergarteners in ECLS-K. Second, observable determinants of outcomes, such as family income or measures of achievement prior to high school, are substantially less correlated with historical Catholic shares than with current Catholic shares. These patterns imply that an identification strategy based on historical measures is clearly preferable to one based on current religious concentrations – it is dominant in terms of both identifying power and the likelihood of bias.

The preferred point estimates are uniformly smaller (less positive) than the corresponding OLS coefficients, implying that students positively select into Catholic schools. However, one should not infer too much from this pattern, since the two sets of estimates are never statistically different from each other at standard significance levels. Both OLS and IV are consistent with a beneficial effect of Catholic schooling on college attendance rates, but there is essentially no evidence for a tangible benefit for test scores. Notably, these findings are quite similar to those of Altonji et al. (2005b), who estimate Catholic schooling effects based on new methods that do not require valid instrumental variables. The similarity between the estimates presented in this paper and those of Altonji et al., even though the assumptions in the two studies differ markedly, provides some reassurance that both sets of estimates are credible.

Finally, the finding that the local supply of Catholic schools is more closely associated with historical than current religious preferences can shed light on questions of interest apart from the causal effects of Catholic schooling. For example, understanding the sensitivity of Catholic school attendance to the local supply of Catholic schools can substantially improve attempts to analyze the impact of voucher programs on school choice. This is especially important in light of *Zelman v. Simmons-Harris*, in which the US Supreme Court ruled that publically-funded voucher programs that allow for attendance at religious schools do not necessarily violate the constitutional separation of church and state.

Appendix A. Data appendix

NELS:88

The variables used in the empirical analysis can be classified into several categories: demographic variables, family background, eighth grade test scores, eighth grade performance in school, and outcomes.

Demographic variables include indicators for whether a respondent is female, Asian, Hispanic, black, and Catholic.

Family background variables include measures of

- Family composition: separate 0–1 indicators for whether the student lives with his/her mother and father, mother and male guardian, father and female guardian, mother only, or father only (with the excluded category being "other relative or non-relative").
- Parents' marital status: separate 0–1 indicators for divorced, widowed, separated, never married, and not married but living in a marriage-like relationship (with the excluded category being "married").
- Mother's and father's education are continuous variables ranging from 8 to 18 years, and log family income is created using the midpoints of the ranges of the categorical family income variable provided by NCES.

Both eighth grade test scores were taken from NELS standardized values from Item Response Theory scaled scores.

Eighth grade performance-in-school measures include a NCES-coded delinquency index, indicators for whether the student got into a fight, performs below ability, rarely completes homework, is frequently absent from school, is frequently tardy, is inattentive in class, is disruptive in class, has a "behavior problem" according to parents, has limited English proficiency, or repeated any grade 4 through 8. Other continuous measures include a NCES-coded "risk index", "grade index", and an "unpreparedness index".

Outcomes include 12th grade math and reading test scores, which were taken from NELS standardized values from Item Response Theory scaled scores. Outcomes also include an indicator for whether received high school diploma as of the third NELS:88 follow-up and an indicator for whether the student was enrolled in a 4-year college as of April 1994.

All family background covariates are set equal to the sample mean when missing, and new 0–1 indicators for missing values are created for each of the original variables. Observations were excluded when information on the sector of the school (e.g., Catholic or public) was missing, or if any other variable described above was missing. Sample sizes for the empirical analyses depend on the particular dependent variable analyzed since observations are lost due to missing values of the dependent variable.

ECLS-K

The variables used in the empirical analysis can be classified into three categories: demographics, family background, and outcomes.

Demographic variables include indicators for whether a respondent is female, Asian, Hispanic, black, Native American, multiracial, has missing race, and Catholic.

Family background variables include measures of mother's and father's education, which are continuous variables ranging from 8 to 18 years, and log of family income, which is created using the midpoints of the ranges of the categorical family income variable provided by NCES.

Outcomes include kindergarten math and reading test scores, which were taken from Item Response Theory scaled scores.

All family background covariates are set equal to the sample mean when missing, and new 0–1 indicators for missing values are created for each of the original variables. Sample sizes for the empirical analyses depend on the particular dependent variable analyzed since observations are lost due to missing values of the dependent variable.

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