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Differences in Educational Production Between Dutch Public and Religious Schools

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Abstract – A common finding in the economic literature on schooling effectiveness is the phenomenon of better performance by religious, and, more precisely, Catholic schools in terms of scholastic achievement, educational attainment and measurable labor market outcomes (i.e. subsequent employment status and wages). While the majority of this research in the economic sciences stems from the US, fueling the debate over public financing of private education in this country, comparatively little research directly addressing the phenomenon has been performed in the Netherlands despite evidence of a significant achievement premium to Dutch Catholic schools. This study explores the phenomenon of superior achievement of Catholic over other (public and Protestant) primary schools in the Netherlands.

Although this is a common finding across other countries, the case of the Netherlands differs in that it is unlikely the premium to Catholic versus public and Protestant education has to do with systematic differences in funding or administrative selection of better/worse students across these educational sectors. However, self-selection of schools by parents may be a significant source of selectivity bias. Therefore, extra attention is taken in controlling for parental self-selection for students into the three main school sectors (Catholic, public and Protestant) when estimating the causal effect of each on scholastic achievement through the use of an IV technique. We also control for a wide variety of (potentially achievement enhancing) educational practices that may be more pervasive and/or efficient in the Catholic sector to attempt to explain the persistent achievement advantage of Catholic schools in Holland.

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1 Introduction

The finding that Catholic schools produce better results in terms of student achievement and labor market outcomes relative to their non-Catholic counterparts has become increasingly common in the educational literature. This finding seems to hold across studies using data from different countries and various levels of education. Recent analyses using data from the US, Australia and the Netherlands have shown students from Catholic schools to have higher test scores, levels of schooling attainment, and more favorable subsequent labor market outcomes such as greater earnings and better employment prospects.[1] This is surprising in light of the fact that Catholic schools tend to have larger classes and lower per-pupil costs.

Amidst the attention given to improving education in the US, the phenomenon of better academic results found for Catholic school students has been brought to the forefront of the educational reform agenda. The perception that US Catholic and, more generally, religious schools are advantageous in terms of achievement has fueled the current debate on school choice and, more precisely, prompted the push for government funded subsidies for enrolment in private schools. For instance, in 2000 proposed legislation that would create tuition vouchers for private education at the primary school level was voted upon (and rejected) in the US states of California and Michigan.[2]

Despite the vast amount of attention it has received, there is still little consensus as to the validity of the hypothesis that schools in the Catholic sector indeed produce better outcomes (henceforth referred to as the "Catholic school hypothesis"). The original works purporting the positive effects of Catholic (relative to public) education include the studies by Coleman et al (1981a, 1981b). Here, controlling for a large number of pupil characteristics, the majority of the evidence is in favor of the Catholic school hypothesis. Opponents to the findings of this literature refute the evidence put forth, citing methodological problems ranging from flawed data definitions to invalid estimation techniques. Goldberger and Cain (1982) and Noell (1982) provide sharp criticisms of the work by Coleman et al arguing their results were most likely driven by non-random selection of individuals into different types of schools. More recently, several studies have implemented different strategies in an attempt to control for the endogenous selection of individuals into Catholic (and other private) versus public schools giving evidence both for and against the advantages of Catholic schooling. In turn, the question remains open as to the validity of the Catholic school hypothesis.

The phenomenon that Catholic schools perform better is by no means localized to the US. Empirical analyses in the work by Dobbelsteen et at (2002) and Levin (2001) find significantly higher achievement scores for 4th, 6th and 8th grade students from Catholic versus both Protestant and public schools in the Netherlands. However, in contrast to the case of the US, Dutch Catholic (and other religious) schools are not synonymous with the private sector (i.e. privately funded tuition costs). In fact, Dutch law mandates equal public financing across

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[1] In addition, additional studies have shown that the benefits of Catholic education may extend to other favorable social outcomes such as lower incidences of arrest and abuse of hard drugs such as cocaine (see Figlio and Ludwig, 2000).

[2] The push has also stimulated two well-known school choice experiments in the US states of Wisconsin and Ohio. More recently an important decision was reached by the US Supreme Court where voucher schemes, which allow vouchers to be used at religious schools, were deemed constitutional under federal law (see Rehnquist (2002)).

all schools, regardless of their religious persuasion. With the Dutch version of the Catholic school hypothesis at hand, the purpose of this study is to answer the following question:

> "Can the significantly higher achievement scores of Dutch Catholic students be attributed to Catholic schools themselves being better, the higher caliber of students that enroll in Catholic schools, or a combination of these factors?"

To address this question, differences between Dutch Catholic, Protestant and public primary schools in terms of administrative and teaching practices as well as curriculum are analyzed. In addition, potential biases of the estimated Catholic, Protestant and public school effects caused by non-random selection of individuals into these schooling sectors is controlled for via a two-stage model that accounts for the three possible schooling states. While educational practices cannot fully explain the Catholic school hypothesis, it is found that negative selection of individuals into Catholic schools exacerbates the expected achievement gap between these and other types of schools (Protestant and public).

The remainder of this paper is structured as follows. The second section provides a survey of the literature that has evolved around the Catholic school hypothesis. Section 3 contains a brief overview of schooling choice with respect to the three dominant schooling types (Catholic, Protestant and public) in the Netherlands. In Section 4, an exposition of the econometric models and description of the data employed in the empirical analysis is provided. Section 5 follows with the empirical results including an assessment of differences in educational practices across the various schooling sectors as well as results from a model of achievement with endogenous schooling selection. The last section summarizes and concludes.

2 Previous literature

Over the past twenty years a large collection of literature has been produced to explore and test the Catholic school hypothesis. A large part of the motivation behind this work was the controversy amongst educational reformists as how to improve educational quality in the face of declines in measurable schooling outcomes such as scores on achievement tests. Proponents of Catholic schooling place great weight on the Catholic school hypothesis advocating increasing the number of schools in this sector as a viable way to improve the quality of education. Furthermore, studies supporting greater school choice (i.e. voucher plans, government funding for non-public schools, etc.), which includes greater access to Catholic education, suggest this will also raise the standards of public education via subjection of the later to increased competition.

In order to provide a better understanding of the previous research in this area, the following section gives a brief chronological survey of the empirical literature to date that has addressed the issue. Appendix A contains a tabled overview of the nine studies covered here.

2.1 Early studies on Catholic schools and achievement

The studies by Coleman et al (1981a, 1982a, 1982b) are among the earliest to directly address quality differences between private versus public education. Using the first wave of the High School and Beyond dataset (1980) the authors set out to establish whether: average achievement measures differ across public, Catholic and "other" private schools; if any differences can be attributed to the policies of these different schooling sectors; and, which policies these might be. The study uses three strategies to uncover differences between Catholic and other types of schooling. First, the authors include several observable individual-specific characteristics in OLS regressions to control for the quality of student inputs, whose influence would otherwise be manifested in the estimated Catholic school effect. The authors find that, after controlling for "initial differences" (selection effect), there is an approximate grade level increase in math and vocabulary scores resulting from a sophomore going to a Catholic or private as opposed to a public school. A second strategy used is to nullify the selection effect by taking the difference between sophomore and senior scores in each school type. Their results show that for mathematics and vocabulary,

> ". . . the estimated (learning) rates in the private sectors from sophomore to senior are at least twice those in the public sector."

The final strategy to control for potential selection bias in the estimated school effects is to simply identify the ways in which the three schooling types differ and to assess which of these have a significant effect on achievement after controlling for student characteristics. The authors identify two related factors driven by school policy that could account for significant achievement differences, discipline and student behavior. The effect on achievement of variables describing coursework taken, homework, absenteeism, school disciplinary climate and student behavior are estimated and a counterfactual is formed by assessing the expected impact on achievement for a "representative" public school sophomore resulting from attending a school with Catholic or private school characteristics. They conclude,

> "The last portion of the analysis shows. . . that achievement is just as high in the public sector when the policies and the resulting student behavior are like those in the Catholic or other private schools."

In response to the studies by Coleman et al, Goldberger and Cain (1982) produce a criticism of the evidence put forth supporting the advantage of Catholic and other private schools. The authors provide a comprehensive exposition of the work, including the methodological errors in the studies and how these relate to their results and subsequent conclusions. First, they are dubious of the way in which Coleman et al attempt to control for selectivity and/or omitted variable bias via the inclusion of individual background variables.[3] For instance, many of the included variables are not necessarily indicative of individual characteristics prior to one's high school attendance but rather may be all or at least partially driven by their high school achievement.

The critique next addresses the second strategy taken by Coleman et al (i.e. differencing sophomore and senior achievement scores) in which it is argued the way discontinuing students (dropouts) were controlled for across the various schooling sectors was in err in favor of private schools. Third, the authors address Coleman's claim that Catholic and private school superiority can be attributed to specific policies more often enforced in these schooling sectors. The main thrust of their argument contends that many of the policies "accountable" for private school supremacy are not exogenous but are in fact driven by the background characteristics of the student body. From their critique,

[3] From Goldberger and Cain (1982), "We doubt that all 17 variables together can substitute for direct initial measures of cognitive achievement, such as would be provided by accurate reading, vocabulary and math test scores obtained just prior to entering high school. . . The list omits prior cognitive achievement, contains poorly measured background variables and is far from comprehensive."

> 'The "school policy" variables used by Coleman et al have all the appearance of being (a) primarily, reflections of student background characteristics not otherwise controlled for; (b) secondarily, endogenous outcomes reflecting school achievement; and (c) least of all, exogenous school policies. In this light, we see that Coleman et al are attributing to the public schools negative effects that reflect sources (a) and (b).'

On a more fundamental level, the authors point out well-known alternative methods developed specifically to control for unobserved factors that drive self-selection bias in behavioral models. It would seem most logical to employ a model designed to correct for selectivity bias such as the *two-step method* suggested by Heckman (1976).[4] Indeed, almost all of the studies on educational choice and schooling outcomes since this early work have taken this approach.

A reanalysis of the High School and Beyond data was performed by Noell (1982) yielding quite different conclusions. The approach extends the Coleman analysis in two ways. First, an "enhanced" specification of the model was formulated accounting for some of the, arguably obvious, controls omitted in the previous analysis such as gender, handicap status and region of residence. The results of this exercise is that the premium to Catholic schooling with respect to mathematics achievement disappears entirely while for reading only a minute significant premium remains for sophomores. Secondly, the author employs a two-stage selection model to explicitly control for any correlation between the unobserved determinants of Catholic school participation and scholastic achievement. As an exclusion restriction, the author uses a dummy variable indicating whether the individual is Catholic. Clearly, the probability of attending a Catholic school is positively related with one's religion being Catholic. The author's second contention, that being Catholic is not expected to have a direct influence on cognitive achievement, is somewhat more problematic.[5] After controlling for selectivity Noell finds the corrected achievement effect not to significantly differ from the OLS estimate.

2.2 More recent works incorporating graduation, attainment and wages

Using the same data as Coleman et al (1981a, 1982a, 1982b) and Noell (1982), Sander and Krautmann (1995) deviate from the earlier analyses in two ways. First, instead of measuring scholastic achievement as proxied by standardized test scores they address two alternative schooling outcomes, the probability of graduation and educational attainment. Second, in order to identify the Catholic school effect on the two outcome measures, four restrictions are formed by interacting regional indicators of concentrated Catholic population areas with urban status and a fifth by interacting Catholic religion and urban status. This identification strategy implies there must be a critical mass of Catholics in a given area before schools will be built to cater to their needs while at the same time the existence of Catholic schools in these regions is not the sole factor for Catholic migration to these areas.[6]

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[4] For an excellent theoretical exposition of how selectivity bias potentially confounds assessments of public versus private schooling outcomes the reader is referred to the study by Murnane et al (1985).

[5] The studies by Chiswick (1983, 1988) have since shown a significant influence of religion on achievement.

[6] Note this implies a one-way causality between number of Catholic residents and Catholic school creation within a given region whereas the causality may in fact run in both directions. An example of work that addresses the possible two-way causality between school type and place of residence is the study by Tyler (1994).

The authors estimate two separate models; a "dropout rate" model to determine the probability of a sophomore not graduating with his/her class and an "educational attainment" model to estimate the amount of schooling attained six years after their senior year. The results of the dropout rate model, both corrected and uncorrected for potential selectivity bias, show significantly positive effects of Catholic school attendance on expected graduation probabilities. For the uncorrected educational attainment model the authors find a significant positive effect of Catholic school education on educational attainment six years after an individual's senior year. However, once selectivity is taken into account the point estimate of the Catholic school effect turns negative and insignificant. In addition, the estimated correction term is marginally significant and *positive* indicating an upward bias in the uncorrected Catholic school effect. Thus, their evidence shows that the non-random selection of above-average achievers into Catholic schools can account for the observed attainment premium associated with that type of schooling.

Sander and Krautmann's analysis is taken one step further by Evans and Schwab (1995) who, in addition to analyzing the probability of finishing high school, address the likelihood of entering a four-year college (conditional upon successful high school graduation). The uncorrected results pertaining to high school graduation are quite similar to those of Sander and Krautmann. The representative student graduating from a Catholic high school is expected to have a 14% higher chance of entering a four-year college.

The authors next consider the bias in the uncorrected estimates caused by the omission of critical variables related to inherent ability, peer effects, family/household inputs, region-specific finance and labor market conditions. To account for the potential harm stemming from the omission of these critical factors they include in their model sophomore test scores, seven peer effect indicators, family/household inputs, and indicators of state residence, both separately and collectively. The general result is that the inclusion of these controls causes either a slight or negligible decline in the difference in graduation and college entrance rates between those attending Catholic versus public schools.

Finally, the study performs two exercises to control for non-random selection of individuals into Catholic schools due to school admissions policies and parental choice, respectively. First, as a weak test of whether more stringent Catholic school admissions policies may in fact cause a significant bias in the estimated effects, the authors conduct the original regressions on the Catholic school sub-sample controlling for the existence of entrance exams and/or waiting lists. The evidence shows there to be no significant difference in expected graduation or college entrance probabilities between schools with and without the entrance criteria. To control for possible bias caused by parental choice several two-stage selectivity corrected models are estimated.[7] After correcting for selectivity five of the six graduation model estimates of the Catholic school effect prove to be highly significant, pointing to increased graduation rates ranging from 0.114 to 0.141. The results are less consistent across the college enrollment specifications where point estimates of the Catholic

[7] The following five exclusion restrictions are signaled out to properly identify the models: an indicator of Catholic religion; the proportion of Catholics in county where student attends school; an interaction of Catholic religion and church attendance indicators; interaction of Catholic religion indicator and proportion of Catholics in county; and, a triple interaction of Catholic religion indicator, church attendance and the proportion of Catholics in county.

school effect vary from 0.071 to 0.240 with four of the five proving significant at conventional levels.

Sander (1996) carries out yet another work using the High School and Beyond dataset addressing the Catholic school effect on sophomore scores of non-Hispanic whites in vocabulary, mathematics, science and reading tests. Again, an important focus of the paper is to control for potential selectivity bias. However, the author deviates from the prior studies by allowing the effect of Catholic schooling to vary with respect to duration of attendance. To this end, two indicator variables are included in the achievement equations denoting participation in one to seven years and eight years of Catholic grade school, respectively.[8] The main results of the uncorrected models give evidence to an advantage for participants in eight years of Catholic grade school who are expected to have an average of one and three-quarters more correct answers on both the math and vocabulary tests.[9]

The identification strategy used to correct for potential selectivity bias is identical to that used by Sander and Krautmann (1995) and will therefore not be repeated here. The main results from the selectivity-corrected model show an increase in both the magnitude and significance of the estimated (eighth year) Catholic school effect. The expected increase in number of correct answers caused by Catholic school attendance is 3.44, 2.48 and 2.04 in math, vocabulary and reading, respectively, and all are significant at conventional levels. Unfortunately, the only reported descriptive statistics are sample means so that there is no "yardstick" such as standard deviation with which to evaluate these estimated effects.

Goldhaber (1996) takes a slightly different approach to examine the apparent disparity in effectiveness between public and private schools in the US. The author is not only interested in how different schooling regimes influence achievement in terms of standardized 10th grade test scores on math and reading, but also how differences in educational quality across schooling regimes effect school choice.[10] To this end, a fully recursive system of three equations are estimated. The first two equations are much like that found in the previous literature (cf Sander, 1996) where a probit equation is first estimated to predict schooling choice and a selectivity-correction term is then calculated and included in a linear second-stage achievement equation. However, the model expands upon this approach by then using predicted achievement from the corrected second stage as a regressor in a structural probit equation of school choice. In this way, it is possible to estimate the effect of school quality as proxied by student achievement (controlled for social background and purged of selectivity bias) on school choice.

The first-stage equation specifies the choice between public and private school as a function of a host of controls to account for individual characteristics and family background

[8] Note the model implies that there may be an effect of Catholic *primary* school on standardized test scores in *secondary* education. However, Sander presents no results with controls for type of secondary school attended nor the interaction between sector of primary and secondary education, which would serve as a test of the relative complementarity between Catholic primary and secondary education (i.e. the premium to staying within the Catholic sector).

[9] The total possible number of correct answers on the math, vocabulary, reading and science tests are 38, 21, 19 and 20, respectively.

[10] From the article, "This paper seeks to unify the above two areas of study by developing and estimating a model of school choice in which choice of, and performance in, school sector is treated as endogenously determined."

in addition to variations in costs and availability of private schooling.[11] The second-stage equation models achievement in terms of 10^{th} grade test score as a function of a student's initial 8^{th} grade test score, several school and class-specific controls, time-specific variables thought to influence achievement, and all first-stage variables *except* the private school cost and availability measures. Note the latter variables are excluded from the second-stage equation to properly identify the effect of schooling regime on achievement. The full model is rounded out with a third enhanced school-choice equation that includes expected student achievement predicted from the estimated second-stage equation.

The model is estimated using all private, Catholic, and other elite versus public schools. In none of the estimations is there any evidence of significant selectivity bias. Goldhaber next formulates counterfactuals he terms "corrected differentials" to measure how much more or less a "representative" 10^{th} grader would have achieved in an alternate schooling sector with *identical* school and class characteristics. The results of this exercise show that much of the uncorrected raw difference between the private and public sector schools disappears once teacher and class characteristics are controlled for.[12] Next, the achievement differentials are *decomposed* into those stemming from the differences in the sector-specific *returns* to the school, class and individual characteristics versus those attributable to differences in the average levels of these characteristics between each sector. In simple terms, it is a test of whether achievement differentials can be attributable to the differences in educational production across sectors or differences in school and class characteristics as well as caliber of student in each sector. The results are clear-cut,

> "Clearly, the majority of the raw mean differentials between school sectors can be attributed to differences in the characteristics of students and schools rather than the returns to these characteristics."

The study by Neal (1997) examines the effects of Catholic secondary schools in the US on three measurable outcomes: high school graduation rates, college graduation rates, and wages.[13] The author pays particular attention to possible heterogeneity in the magnitude of the Catholic school effect across different groups of individuals. To this end, the sample is stratified by urban and minority statuses and estimations carried out on each group separately.

In addressing the identification problem Neal discounts the use of Catholic religion as an exclusion restriction and, as an alternative, constructs two measures of Catholic school availability for each county in the US. The first measure is the number of Catholics as a proportion of the county population.[14] The second measure is that of direct availability defined as the number of Catholic secondary schools per square mile. Because most public school systems provide busing free of charge, transportation costs should significantly affect the marginal costs of education for those families with a preference for Catholic schools.

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[11] The cost and availability of private schools are proxied by indicators of region and degree of urbanization.

[12] For instance, the largest corrected achievement differential found is only 0.06 standard deviations.

[13] The data used is quite novel in that it combines information from the National Catholic Educational Association (a virtual directory of all Catholic secondary schools in the US), the National Longitudinal Survey of Youth, and the 1980 US Census, creating a very specialized data set with which to analyze the Catholic school effect.

[14] The rationale is that Catholic schools in areas with high Catholic population densities receive larger subsidies and can subsequently charge lower tuition compared to those areas more sparsely populated with Catholics.

Clearly, a higher density of Catholic schools in a given area will reduce the marginal costs of families with a taste for this type of schooling.

The uncorrected probit regressions of high school graduation controlling for individual characteristics, family background and county demographics produce a significant 10% premium to Catholic school attendance on the expected graduation rate for those in urban areas. The estimate increases to 26% for urban minorities moving from a public to Catholic school. Neal next performs the analysis accounting for possible selectivity bias using a bivariate probit analysis and finds no significant evidence that positive selection is driving the uncorrected results. Rather, the corrected graduation effects become even larger implying a *negative* selection of individuals into Catholic schools.[15] A simple comparison of the predicted gap in graduation rates between whites and minorities proves to be negligible in suburban compared to urban areas where the differential equals 10%. Moreover, Neal finds little variance in minority high school graduation rates in the Catholic school sector with respect to county size whereas this variance is much larger in the public sector (i.e. minority graduation rates for public schools in counties with larger populations are significantly lower than in counties with small populations). Therefore, he concludes that any marginal benefit enjoyed by urban minorities associated with Catholic schools is because their public school alternatives are relatively poor compared to those in suburban areas.

The analysis shifts next to estimating college graduation rates. Here, a similar negligible effect of Catholic secondary school is found on the college graduation rate of suburban individuals. However, with respect to those in urban areas Catholic schooling is significantly correlated with higher college graduation rates, regardless of minority status.[16] All corrected estimates of the Catholic school effect prove to be too imprecise to yield significant results.

In the final part of the analysis Neal performs three standard OLS regressions of the logarithm of urban male wages initially on a variety of controls, sequentially adding high school and college graduation indicators as regressors.[17] Using the simple OLS regression without controls for educational attainment a significant estimated minority wage premium attributable to Catholic school participation equals approximately 31%. The regression that includes an indicator for high school graduation results in a drop of the Catholic school wage effect for minorities to 27%. Finally, when indicators for both high school and college graduation are included the expected minority wage gain due to Catholic school participation is 23%. Therefore, Neal concludes that the indirect effect of Catholic school on minority wages, via increased educational attainment, is about 8%.

Deviating from the previous studies that focus on schools in the US, Vella (1999) offers an analysis of Catholic versus public schools using data from the Australian educational

[15] The estimates imply expected increases from Catholic high school attendance on the graduation rates of representative urban public school whites and minorities on the order of 18 and 30%, respectively.

[16] The college graduation probability for the typical urban minority is expected to rise from 11 to 27% as a result of attending a Catholic secondary school while the same increase for a representative white student is from 26 to 38%.

[17] Due to a lack of valid instruments, the author does not attempt to control for potential endogeneity of Catholic school participation with respect to wages.

system.[18] Similar to previous analyses the author attempts to estimate the impact of Catholic school attendance on high school graduation rates prompting the use of a bivariate probit regression. The strategy to control for self-selection follows that of the majority of the preceding studies, relying on an indicator of Catholic religion to identify Catholic school enrollment, although the author also includes a control for native-born individuals as an identification restriction.

The uncorrected estimate of the Catholic school effect on high school graduation rates is positive and highly significant. A calculation of the average treatment effect of Catholic school attendance gives evidence to an expected 18% increase in the probability of graduation, which proves to be quite large as the graduation rate for the public school sector is less than 30%. Controlling for potential bias caused by self-selection results in an estimate that is almost identical to the uncorrected single equation model. Unsurprisingly, tests show no significant bias resulting from self-selection of academically better (worse) individuals into Catholic schools.

The study extends the high school graduation outcome measure by estimating educational attainment (in years) and allowing for participation in post-secondary education. An expected 10% increase in the probability of obtaining higher education resulting from a Catholic school "treatment" effect is found. Finally, the last part of the analysis looks at the effect of Catholic education on early labor market outcomes such as employment probabilities and wage rates. In terms of employment rates, the estimated effect of attending a Catholic school translates into an expected increase in the probability of being employed by approximately 7%. A regression of the logarithm of hourly wage rates on Catholic schooling in addition to the other various controls yields a positive yet insignificant treatment effect.

The most recent study included in this review is that by Figlio and Stone (1999). Here the number of outcomes under scrutiny is expanded to include tenth grade standardized mathematics test scores, probability of high school graduation, two years of college attainment, two years of college attainment at a "selective" institution, and two years of college attainment where the major was mathematics, science or engineering. The authors further develop the literature by generalizing the school-choice decision (and potential bias stemming from self-selectivity) to accommodate more than two types of schooling.[19] To do this, a method first implemented by Dubin and McFadden (1984) is used to jointly model the choice and resulting outcomes between public schools and their private religious and non-religious counterparts. The model employs a multinomial logit (MNL) regression with three states (for public, religious and non-religious school types) for the school choice equation. Next, the results are used to estimate two variables denoting the predicted probabilities of enrolling in religious and non-religious private schools. The variables are then included in the second-stage outcome equation in place of the dichotomous participation indicators of the two respective schooling types.

Single-equation baseline regressions (uncorrected for selectivity bias) show religious schools to be significantly associated with better schooling outcomes. For instance, attending

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[18] The author makes use of the 1985 Australian Longitudinal Survey from which he extracts a sample of individuals that reported themselves as having completed their education at the time of the survey.

[19] Individuals in non-religious private schools are included separately in the analysis rather than being pooled together with those religious schools or discarded altogether, providing a more realistic model of school choice than has been done in previous studies.

a religious school is expected to increase 10^{th} grade test scores in mathematics by 2.5 to 3.2%. Moreover, there seems to be little difference between the achievement effect of Catholic versus all religious schools. The simple uncorrected models show that non-religious private schools only have significant positive effects on the probabilities of attending at least two years of college and going to a selective college for at least two years. Once probit models are used in place of linear probability specifications for the discrete outcome measures, non-religious private schools are also expected to have a marginally significant (at the 10%-level) positive effect on the probability of high school graduation.

Following suit with much of the previous work, an indicator of Catholic religion is used as an instrument for school choice resulting in an increase in the religious school treatment effects. Moreover, this result is robust to the inclusion of fourteen additional indicators that allow for the identification of religions other than Catholicism. This implies a *negative* selection of students into religious private schools; there is a systematic mechanism by which less academically inclined individuals enroll in religious schools. In contrast, after controlling for possible endogeneity the treatment effect of non-religious private schools decreases pointing towards a *positive* selection of individuals into these types of schools. The question of instrument validity poses a major problem however; for all specifications save one (completion of two years of college studying math, science or engineering) weak tests of instrument exogeneity reject the null hypothesis that Catholic religion is a valid instrument.

The authors make use of two alternative instruments denoting states with "right-to-work" or "duty-to-bargain" laws as instruments for school choice.[20] The use of the state law indicators as valid instruments for school choice is supported because, although it has been shown that these public policies may affect the actual or perceived *distribution* of achievement outcomes, there is no evidence that they have a direct effect on *mean* performance levels. In addition, it is plausible that the existence of such laws have a significant influence on parental decisions with respect to school choice. Intuitively, the authors state that parents of more academically inclined students on average opt to send their children to more "streamlined" private schools in those areas in which the public sector is highly unionized.[21] The alternative identification strategy using public policy/state law instruments is implemented after showing a significant correlation between all instruments (individually and jointly) and school choice as well as a lack of direct influence between these instruments and student achievement. The results of this exercise can be summarized as follows:

- private religious and non-religious schools have no significant effect on 10^{th} grade mathematics test scores;
- there is a significant increase in the probability of being enrolled in college for at least two years resulting from private school participation;
- there exists a significantly positive private school treatment effect on the probability of attending a selective college.

[20] In general, right-to-work laws secure the right of employees to decide whether or not to join or financially support a union; in 1999 there were 21 states in the US with such laws in place. Duty-to-bargain laws give employees the right to engage in collective bargaining with respect to conditions of employment.

[21] However, this model implicitly assumes that the area of residence is exogenous so that parents do not "vote with their feet" by settling in (or moving to) states with a non-unionized public sector rather than send their kids to private schools.

The final part of the study poses the reader with two questions. First, why is there no observed difference in outcomes between the private and public schooling sectors with respect to science and mathematics achievement or graduation probability? A partial explanation may lie in differences in teacher quality across the two schooling sectors; public school teachers on average take a significantly larger amount of education within the subject they teach than do their private school counterparts. In addition, the average number of science and mathematics units taken and the amount of time spent in class per week in the former seem to be larger for public schools. Finally, the amount of homework assigned by private school teachers is significantly less than that given by instructors in public schools.[22] However, although there may be significant differences between schooling sectors with respect to these three characteristics, their validity is not formally tested by examining whether there are significant achievement differentials between the schooling sectors that can be attributed to these characteristics (i.e. <u>after</u> explicitly controlling for these factors in achievement regressions).

The second question asks why would parents send their children to more expensive private schools if there appears to be no significant increase in student performance? The authors hypothesize that achievement is but one of many desired outcomes from schooling. Parents may alternatively want their children to be educated in a more disciplined environment, have more exposure to religion and/or extracurricular activities, or be surrounded by a specific type of peer group. Indeed, the data seems to support this hypothesis as private schools (at least in the NELS data) show higher levels of disciplinary action, more opportunity to participate in extracurricular activities, and the possibility of obtaining religious education not offered by public schools.

2.3 Literature survey conclusion

The focus of the literature reviewed attempts to address some or all of the following questions:

- Do Catholic, religious and/or private schools perform better than those in the public sector?
- What is the magnitude of differences in outcomes across these sectors (if any) and are they significant?
- Can we attribute any significant outcome differences to sector-specific factors of educational production or are they caused by unobservable factors correlated with participation in a particular schooling sector?

The answer to the first question seems quite clear; much of the literature suggests that the performance of Catholic schools is, on average, better than that of schools in the public sector. However, it must be noted that there is more evidence supporting the superiority of Catholic education using educational attainment and labor market outcomes as opposed to achievement in terms of test scores.

With respect to the second question, the literature provides mixed evidence. Although there seems to be ample support that *significantly* better outcomes are found in Catholic (and

[22] For this explanation the study cites Betts (1996) who reports a significant positive correlation between amount of homework assigned and academic achievement (conditional on a portion of the homework being graded and returned to the student).

religious schools) versus those in the public sector, the magnitude of these effects are unclear at best. The conclusion as to whether a given significant effect is small or large depends greatly on the outcome being measured.

At first glance, the positive Catholic effect on achievement (test scores) appears to be quite small but vary greatly depending on the study. For instance, Noell (1982) estimates an expected 0.91 percentile increase (equivalent to just 0.09 standard deviations) in sophomore reading test scores associated with Catholic school attendance while the largest gain found in Goldhaber is equal to just 0.06 standard deviations in achievement. In contrast, Sander only reports the absolute expected gain in terms of numbers of correct questions where a Catholic school sophomore is expected, on average, to answer 2.04 more questions correctly on a 19 question exam (equal to an increase of over 10%). Unfortunately, Sander fails to report descriptive statistics making an objective assessment of this result difficult.

On the other hand, due to a higher incidence of reporting of descriptive statistics in studies measuring effects in terms of discrete outcomes, we find more transparent evidence as to the efficacy of Catholic schooling from these works. For example, Neal (1997) reports large increases in the high school graduation probabilities of urban whites and minorities (17 and 30%, respectively) attributable to Catholic school participation. The increases certainly seem impressive when compared to the mean graduation rates for these groups (75 and 62% for whites and minorities, respectively). In addition, Neal finds a jump in college graduation rates of urban whites and minorities from 11 to 27 and from 16 to 30%, respectively, associated with Catholic school attendance. The effect of Catholic school on the probability of college enrollment also appears to be relatively large. The lowest estimated increases in college enrollment probability attributable to a Catholic education are found by Evans and Schwab (1995) equaling 7 and 10% (corrected and uncorrected for selectivity, respectively), representing approximately a quarter to a third of the mean college enrollment of public school individuals in their sample (32%).[23] A word of caution must accompany these results as the studies are not cost-benefit analyses nor do they compare the benefits of Catholic schooling to other policy proposals. Therefore, judgements as to whether to implement policies to stimulate Catholic schools should be held back until these results can be weighed against those expected from other policy measures.

In attempting to answer question three the literature supplies us with perhaps the most surprising finding of all. Indeed, the evidence points towards a potential *downward* bias in the estimated Catholic school effect associated with unobserved (omitted) variables that are correlated with the choice to enroll in this type of schooling. Sander and Krautmann (1995), Evans and Schwab (1995), Sander (1996) and Vella (1999) all find evidence of significant *negative* selection into Catholic schools implying that those who (non-randomly) enroll in this type of schooling are expected to have *worse* outcomes than the average individual.[24] The direction of the bias implies that the outcome premiums found for Catholic schooling, at least in the US and Australia, cannot be accounted for by the systematic enrollment of superior students in this schooling sector by parents or via "cream skimming" by educational administrative policies.

[23] Interestingly enough, using Australian data Vella (1999) also finds an average treatment effect of Catholic secondary education on the probability of college enrollment on the order of 10%.

[24] In addition, similar negative selection is found in other studies such as Neal (1997) and Figlio and Stone (1999) however the bias is not statistically significant.

3 Educational choice in the Netherlands

Almost all of the above-reviewed studies consider the outcomes of Catholic and/or private religious schooling relative to those in the public sector in the context of the US. As this paper is focussed on testing the Catholic school hypothesis in the context of Dutch primary education it is necessary to shed some light as to the differences in the two educational systems, especially with respect to school choice. As will be shown later, this has a profound impact on the assumptions made and model used to jointly estimate the effect of Catholic schooling in the presence of free school choice. To this end, the following section briefly describes the historical background and resulting structure of school choice in the Netherlands and contrasts it to that of the US and other Western European countries.

The studies by Dronkers (1995) and Dijkstra et al (2001) report that in Europe the freedom of school choice originates historically from socio-political struggles in the 19th century. In Austria, Belgium, France, Germany and the Netherlands three interactions occurred between the church versus the state, the "old" single-church 18th century regime and the "new" 19th century regime tolerant of religious freedom, and the newly emerging versus the traditionally dominant classes. One common result of these social phenomena across Europe was the ability for parents to more or less freely choose between public and religious-subsidized schooling sectors. However, many aspects of the newly established freedom of school choice are unique to the Dutch version of school choice.

During this time educational choice in the Netherlands was largely linked to other activities divided along religious lines such as voting behavior, union membership, club memberships, etc. Three dominant cultural "pillars" emerged for the Catholic, Protestant and non-religious/public subcultures. In turn, school choice became largely dictated by the pillar one belonged to as opposed to based on the quality of schools in the available sectors. In this respect the Dutch experience with school choice differs with that of most other countries. While the majority of other Western countries have traditionally offered the choice between schools in the public sector and those controlled by the existing state church (usually either Catholic or Protestant), in the Netherlands the two religious pillars each established their own sector of significant size.[25] The significant prevalence of the two religious Dutch schooling sectors can be evidenced by national statistics that show the distribution of primary schools in 1993 to be 37.6% public, 21.7% Catholic and 35.1% Protestant. Therefore, in the Dutch context it seems logical to account for more than two relevant sectors in the empirical model of schooling choice and outcomes; a more general simultaneous model of school choice and educational outcomes is needed.[26]

By the beginning of the 20th century, heated debates arose in which the religious pillars argued for equal subsidization and treatment by the state for their schools. As a result of these debates legislation was incorporated into the Dutch constitution of 1917 that has since dictated individual freedom of education and school choice. This powerful law has had a profound impact on the Dutch educational system and literally shaped the unique existing framework of school choice in the Netherlands. From Dronkers (1995):

[25] For instance, the private sector in the US is largely dominated by the Catholic Church, with which approximately 90% of private schools are affiliated.

[26] Such as the approach by Figlio and Stone (1999) described above.

> "This unchanged article in the constitution prescribes the equal subsidization by the state of all school sectors: They are subjected to strong control of equal examinations, salary, capital investment, and so forth by the national government. . ."[27]

In turn, under the realistic assumption that most schools adhere to national law, all Dutch primary schools are subject to the same funding and investment scheme.[28] This is in stark contrast to the case of the US for which a majority of the costs of non-public schooling is borne out of the pockets of parents. Therefore, it seems highly unlikely that any difference in outcomes across Dutch schooling sectors is due to funding differentials. Although Dutch private schools are allowed to charge some extra fees, the amount charged is almost certainly not large enough to explain significant differences in terms of outcomes between schools. For instance, Dronkers (1995) reports that the average fees charged were 200 guilders per year and used primarily for extracurricular activities. Moreover, the above-mentioned works claim that restrictions on the use of extra funds for teacher grants and smaller classes have also kept the within-sector variance of schooling inputs at a minimum, preventing the emergence of educational hierarchies such as the elite "public" primary schools and Ivy League universities in England and the US, respectively.

There is yet another characteristic of the Dutch educational system resulting from the above-mentioned legislation that helps validate two implicit assumptions made in the empirical model that follows. Namely, we assume that residential location with respect to school choice is exogenously determined and administrative selectivity of the best students (also known as "cream skimming") is negligible. The motivation for these assumptions lies in Dutch law allowing parents the possibility to send their children to a school of their choice regardless of geographic location.[29]

This is quite different then the case of the US where children in the public sector are generally required to attend a school within their own neighborhood or district. Thus, much attention has been drawn to educational choice in the US where parents, given a desired level of public school quality, are said to "vote with their feet" by opting to reside in neighborhoods with expensive housing and relatively good public schools or locate in areas with cheap housing, low quality public schools, and send their children to a costly private school.[30] This implies that in the US location of family residence is likely to be simultaneously determined with school choice and should therefore be explicitly modeled as an endogenous variable. Note none of the studies reviewed above account for the simultaneous residence and school choice decision (although Neal (1997) does at least recognize this potential pitfall in his model). However, due to the flexible nature of the law as it applies to Dutch school choice

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[27] The equal funding and treatment for all schools, regardless of religious persuasion was initially guaranteed for primary schools. However, by 1972 the law was extended to cover all levels (primary, secondary and higher) of education.

[28] In the Netherlands the amount of resources a school receives is largely dependent on the number of students adjusted for the composition of student body with respect to socioeconomic status and special needs (i.e. physically and mentally handicapped individuals). For more on this issue, the reader is referred to the discussion of student weights in Section 4.3, below.

[29] Dijkstra et al (2001) cites the abolishment of "obliged attachment" areas in the 1970's and 1980's.

[30] Theoretically, this idea was formalized in the pioneering work of Tiebout (1956) where economic agents are assumed to locate based on their relative demand and supply of public goods provided in the particular areas available to them.

and residential location, exogeneity of residential location with respect to school choice does not seem to be such a far-fetched assumption.

With respect to the second assumption, that administrative selectivity bias in the Netherlands is negligible, Dijkstra et al (2001) note that the ability of parents to freely choose schools regardless of residential location effectively increases the level of competition non-public schools face, which in turn greatly diminishes the degree to which they can be selective with respect to enrollment. Moreover, the authors note that the significant size of the private schooling sector exacerbates this competitive effect also significantly lowering the possibility of non-public schools to attract (admit) superior pupils.

In sum, the structure of school choice in the Netherlands provides us with a special case with which to test the Catholic school hypothesis. The Dutch educational system can clearly be distinguished from that of the US (the context of all but one of the studies reviewed above) by the attempt of the former to ensure educational equity vis-à-vis legislation pertaining to funding and capital investment, expenditure restrictions, and absolute freedom of school choice with respect to geographic location. In essence, a parallel can then be drawn between the Dutch system of school choice (characterized by the interaction of geographic mobility and equal financing) and the heavily publicized debate in the US over the use of and experimentation with public funding for privately provided education. Proponents of such funding schemes (better known as voucher programs) argue that parents who opt to send their children to non-public schools should be issued publicly funded vouchers that they may put towards tuition for better quality private sector schools. The idea is that such a program will put fledgling public sector schools in greater direct competition (for students) with their private school counterparts eventually forcing the former to raise their standards.[31] Although not the focal point of this paper, the contrast between the school choice in the Netherlands that mimics a universal voucher system and the current debates in other countries addressing the effectiveness of voucher programs is nevertheless intriguing, from Dronkers (1995):

> "The Dutch case is therefore interesting for other societies to observe for the possible effects of free parental choice combined with equal subsidization and treatment of schools within the same school type – the effects in the Dutch example not being biased by the creaming-off of the most able students, by the financial possibilities of different sectors, or by the geographical constraints on parental choice. . ."

On a more practical note, the structure of school choice in the Netherlands provides us with so me insight as to how to more properly model educational outcomes in the presence of selectivity bias (i.e. allowing for more than two educational sectors to choose from) and, equally important, pitfalls that we need not be concerned with such as endogeneity of residential location and administrative selectivity bias.

4 Estimation methods

In order to properly address the Catholic school hypothesis it is necessary to formulate and test models that account for the estimated achievement premium to Catholic education found in previous work. Therefore, our analysis takes two approaches to identify the nature of this phenomenon in the context of the Netherlands. The first is quite straightforward; to explicitly control for specific school practices in the educational achievement equation. The second approach is to attempt to control for bias of the Catholic school effect caused by selectivity via an instrumental variables (IV) model. The following section will take each of these approaches in turn.

4.1 Modeling educational inputs

Obviously, there is a wide array of educational practices schools use in terms of administration, curriculum and teaching methods. In our context, the exploitation of any such information might very well explain the significant achievement difference between schools in the Catholic and other sectors. More specifically, if particular educational practices prove to have significant positive effects on scholastic achievement <u>and</u> these practices are more prevalent or effective on average in the Catholic schooling sector, then simply controlling for these practices should account for some or all of the Catholic schooling effect. Therefore, our first strategy estimates models of the following form:

$$y_{ics} = \alpha + X_i'\beta + C_c'\gamma + S_s'\delta + \varepsilon_{ics} \tag{1}$$

where y_{ics} is the percentile score of pupil i in class c of school s on a standardized test (in arithmetic or language); X_i, C_c and S_s are matrices of variables pertaining to the individual,

[31] The relationship between educational effectiveness and school choice has been tested in the recent empirical studies by Hoxby (1994, 2000b).

class and school, respectively; and ε_{ics} is an individual-specific error term.[32] Naturally, of particular interest are the estimated parameters associated with the school denomination indicators included in S_s. Of central importance to this approach is to identify and control those characteristics of an individual's educational experience that may be conducive to achievement. More simply, we include extra controls for various educational practices in the vector of class-specific variables C_c.[33] It is then possible to verify whether the Catholic school point estimate "picks up" the effects of these previously unobserved characteristics (i.e. whether the resulting Catholic school effect diminishes or disappears altogether). To this end, we include controls for the following characteristics:

- amount of time per week (in minutes) allocated to mathematics, language and reading instruction;
- whether homework is assigned and to which types of students;
- type of curriculum and how closely teacher follows instructional material;
- style in which class is taught;
- frequency of testing based on curriculum, diagnostic and external materials;
- composition of class with respect to cognitive ability.[34]

In addition, we now include parental education in the educational production function. Under the assumption that the education level of one's parents can serve as a proxy for the support given and importance put on scholastic achievement in a student's home, should higher educated parents more often send their children to Catholic schools, then this social background control may also account for the achievement disparity between Catholic schools and those in the other sectors.[35]

4.2 Modeling selectivity bias

The second proposed explanation of the Catholic school phenomenon contends that there may be an upward bias in the estimated Catholic schooling effect due to self-selection. That is, there may exist a systematic non-random mechanism by which more (less) academically capable students attend schools in the Catholic (Protestant or public) sectors. If we cannot observe and control for characteristics that mark those more academically inclined pupils, who on average attend Catholic schools, the estimated effectiveness of this schooling sector

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[32] It should be noted that equation (1) is estimated accounting for the possible correlation of individual error terms (within classes) when modeling educational inputs. That is, robust standard errors are calculated using Roger's (1993) generalization of the estimator put forth by Huber (1967), which takes into account possible correlations between the errors of observations *within* groups (in our context classes). The exact procedure used for obtaining robust variance in the presence of a group clustered error structure can be found in StataCorp (2001), pp. 254-258.

[33] An obvious argument against this approach is that educational practices themselves may be endogenous with respect to achievement. However, as the objective is to assess whether practices *within* Catholic schools can account for the achievement premium in this sector, endogeneity of these variables and/or possible correlation between these practices and Catholic schooling should not interfere but rather aid us in our assessment.

[34] It is noted that class composition with respect to cognitive ability in our context is only *potentially* an educational practice as the study by Dobbelsteen et al (2002), which uses the same data, finds that the majority of schools in our sample are not able to sort high and low-ability individuals into separate classes.

[35] Moreover, the control provides a good "yardstick" with which to compare the effectiveness of the different schooling practices.

will be overstated. Of the studies reviewed above that controlled for self-selection of students, all but one modeled the choice between two schooling states. However, a model with only two states may prove to be overly restrictive for our purposes. If one recalls the unique structure of school choice in the Netherlands discussed in Section 3.3 there are at least three significant schooling sectors from which to choose. Therefore, it follows that to properly address potential upward selection bias in the estimated Catholic school effect we will need a more generalized model that accounts for more than two schooling sectors.

Two such generalized approaches addressing selectivity bias with polychotomous states have been developed by Dubin and McFadden (1984) and Lee (1983). The first approach is analogous to an instrumental variables (IV) technique using a multinomial logit equation to estimate the predicted probability of choosing a given choice state. The predicted probabilities are then included in the second-stage equation replacing the potentially endogenous indicators of choice participation. The latter approach also involves estimating a multinomial logit in the first-stage, but instead formulates selectivity-correction terms similar to those used in a "Heckman" selectivity model, which are then included in state-specific second-stage equations. In the context of public versus private school achievement Figlio and Stone (1999) use both approaches but only report their findings using the first.[36] The model here makes use of the technique developed by Dubin and McFadden (and later used by Figlio and Stone) while employing an identification strategy akin to that of Neal (1997).

To make clear how selectivity may affect our estimates let us define the achievement outcome of individual *i* enrolled in school type *d* (where *d* equals 1, 2, or 3 denoting schooling states in the public, Protestant or Catholic sectors) as a function of: the regressor matrices *X*, *C* and *S* found in equation (1) describing all characteristics pertaining to individual, class and school characteristics thought to affect achievement <u>except</u> school denomination; dummy variables I_d defined as indicators of individual *i*'s participation in school type *d*; and, a deviation from the predicted mean achievement, the composite error term ($\xi_{icsd} = \varepsilon_{icsd} + u_i$), due to the idiosyncratic match between individual *i* and schooling sector *d* (ε_{icsd}) and a more general unobserved individual-specific effect (u_i) or

$$y_{icsd} = \alpha + X_i'\beta + C_c'\gamma + S_s'\delta + I_d'\eta + \xi_{icsd} \tag{1'}$$

Equation (1') then shows endogeneity bias may arise when one or both of the unobservable components (ε_{icsd} and u_i) of ξ_{icsd} are correlated with school choice. For example, should parents of students that have a *comparative advantage* with respect to achievement in school type *d* tend to send their children to this sector (i.e. $E(\varepsilon_{icsd} \mid X_i, C_c, S_s, I_d=d)>0$), the positive correlation between school choice and the unobservable component of achievement (ε_{icsd}) will cause the estimated mean effect of school type *d* on achievement to be biased upward. Similarly, an upwardly biased estimate of the average effect of this school type will occur if there is some unobserved individual effect that proves to be conducive to achievement (i.e. $E(u_i \mid X_i, C_c, S_s, I_d=d)>0$) and significantly correlated with this choice of schooling due to educational preferences unrelated to achievement (i.e. preference for a religious or moral education, extra curricular activities, etc.)[37]

[36] However, the authors note that the findings using both approaches are qualitatively similar.

[37] Note that although parental choice is labeled as the mechanism behind any potential correlation between the disturbances and school sector, this could also occur due to administrative selection of schools (i.e. selective admissions (expulsions) of better (worse) students).

As mentioned above, we implement an IV technique to circumvent the potential bias caused by a correlation between the disturbances and school choice indicators. In the first-stage, we employ two generalizations of the simple logistic regression called a *conditional logit* (CL) and a special case of the CL called a *multinomial logit* (MNL) to obtain predicted probabilities of participating in one of the three schooling sectors. The CL model is specified as follows:

$$P(I_d = d) = \frac{\exp(W_i'\psi_d + Z_{icsd}'\omega)}{\sum_{d=1}^{3} \exp(W_i'\psi_d + Z_{icsd}'\omega)} \qquad (2)$$

where W and Z denote regressor matrices containing variables used to describe school choice (those contained in X_i, C_c and S_s as well as instrumental variables) grouped into those that vary only across individuals (termed *characteristics*) and those that vary by choice state but also possibly by school, class and individual (labeled *attributes*), respectively.[38] Substitution of the predicted participation probabilities of being in a particular schooling state for the actual school indicators yields the educational production function purged of selectivity

$$y_{icsd} = \alpha + X_i'\beta + C_c'\gamma + S_s'\delta + \hat{P}_d'\eta + \xi_{icsd} \qquad (3)$$

Of central importance to any non-experimental estimation technique is the identification strategy used to isolate the unbiased (exogenous) treatment effect of a potentially endogenous regressor. The model specified above achieves identification by the use of credible instruments, which are analogous to exclusion restrictions in a selection model. For an instrument to be of sufficient *quality* and *validity* it must significantly explain the given treatment (in our case school choice) <u>and</u> have no direct influence on the outcome being measured (i.e. test score achievement), respectively.[39] Here we employ two types of instruments in order to achieve identification.

First, following a majority of the previous studies religion of the child is used to describe school choice. The instrument is defined as the interaction of the two parent religion indicators yielding the dummy variables "Child Catholic" and "Child Protestant", equal to one

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[38] When all regressors vary only by individual this model collapses into an MNL, specified as follows: $P(I_d = d) = \frac{\exp(W_i'\psi_d)}{\sum_{d=1}^{D} \exp(W_i'\psi_d)}$. Furthermore, in practice one set of the alternative-specific parameter vectors in the MNL must be normalized to zero yielding the following expected probabilities for the reference and non-reference categories

$$P(I_d = r) = \frac{1}{1 + \sum_{\substack{d=1 \\ d \neq r}}^{3} \exp(W_i'\psi_d)} \text{ and } P(I_d = d) = \frac{\exp(W_i'\psi_d)}{1 + \sum_{\substack{d=1 \\ d \neq r}}^{3} \exp(W_i'\psi_d)}, \text{ respectively.}$$

[39] Alternatively speaking, the variable(s) in question must be included in the first-stage choice equation and (legitimately) excluded from the second-stage equation of primary interest. A more elaborate discussion of the dangers of using instruments that are invalid or of poor quality can be found in Levin (2002) (see Chapter 4).

when both parents are of the same faith and zero, otherwise.[40] Clearly, religion should have a significant effect on the choice of whether or not to send a child to a religious school. However, this strategy is not without its pitfalls. A large amount of literature has cited the fact that certain religious groups tend to have higher scholastic achievement. Therefore, because religion may very well have a direct influence we might expect it to fail the second criteria.

The second identification strategy used is similar to that in the study by Neal (1997). This involves identifying the treatment effects of Catholic, Protestant and public education on achievement by using measures of relevant school availability in each of these sectors to isolate the exogenous variation in school choice. From the data at hand we can tell in which city or town each school is located and from this information map each observation to its respective *gemeente* or municipality (more or less equivalent to a county in the US).[41] Next, data from the Dutch Central Bureau of Statistics (CBS) are drawn upon to provide us with the total number of primary schools (broken out by schooling sector) in the gemeente making it possible to construct measures of school availability within the relevant area of school choice.[42] These measures were further discounted by gemeente size (measured in sq. km.) to provide the within-gemeente densities of schools in each sector.[43] Given the free geographic mobility of school enrollment encompassed by the structure of school choice in the Netherlands, the gemeente seems the most logical geographic area from which to choose a school (educational market). That is, although parents may send their kids to a school in a different *stadsdeel* (city district) or even a different city, it is not likely one would choose one outside of the gemeente in which they reside.

In addition to using school density as a proxy for school availability we also estimate specifications that include the *relative* supply of schools in each sector as instruments. An obvious argument against the use of density of schools is that this type of measure may very well be sensitive to scale effects. Clearly, estimated effects on school choice using absolute measures of school availability such as density with respect to area may be prone to (unobserved) influences of gemeente size, even after controlling for degree of urbanization. A direct way to account for this is to use the relative supply of schools in each gemeente, which is insensitive to the size of the gemeente. Therefore, we create three measures of the relative (within-gemeente) supply of schooling, defined as the ratio between the number of schools in a given sector to all other types of schools within the gemeente:

$$\text{Relative supply of public schools} = \left({}^{\#\text{ of public}}\!\!\big/_{\#\text{ of Protestant} + \#\text{ of Catholic}} \right)$$

$$\text{Relative supply of Protestant schools} = \left({}^{\#\text{ of Protestant}}\!\!\big/_{\#\text{ of public} + \#\text{ of Catholic}} \right)$$

$$\text{Relative supply of Catholic schools} = \left({}^{\#\text{ of Catholic}}\!\!\big/_{\#\text{ of public} + \#\text{ of Protestant}} \right)$$

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[40] Note, although I have casually interpreted the interaction of parent religion as the religion of a child this is quite stringent in that <u>both</u> parents must be of the same faith for a child to be considered part of the religious group.

[41] This was done using the CBS publication *<u>Plaatsnamen in Nederland</u>* (1997).

[42] This information was obtained directly from the CBS.

[43] Gemeente size was taken from the CBS publication *<u>De landelijke wijk- en buurtindeling</u>* (1993).

Note that one would expect this identification strategy to pass the first criteria of instrument quality set forth above. If transportation cost proves to be a significant determinant of school choice (i.e. parents are more likely to send their children to schools that are closer to home), then a greater availability of a household's preferred school type within the relevant educational market should equate to a smaller expected distance to this type of school and hence, a higher participation probability in this schooling sector. Whether or not the density and relative supply of schools meet the second criteria of instrument validity is a bit more problematic. The work by Hoxby (1994, 2000b) implies that competition among schools in the US does in fact have a direct effect on public sector achievement (see footnote 31, above). In addition, the study by Coleman and Hoffer (1987) points out that the relatively large degree of *social capital* in areas with a high concentration of Catholics can explain the beneficial effects associated with this type of schooling. Therefore, under the reasonable assumption that there is a greater availability of Catholic schools in areas with a higher concentration of Catholics, there may be an enhanced Catholic community effect that significantly influences scholastic achievement thereby rendering our identification strategy invalid. In the European context, Dronkers (1995) refutes the social capital explanation:

> ". . . the Coleman and Hoffer explanation (1987) of the church as a community is inappropriate for European society, where the church no longer represents a significant community."

Instead, the author contends a more important driver of an educational community (at least in the Netherlands) is the deliberate choice of parents and teachers to participate in a particular school regardless of its religious persuasion or pedagogical orientation. Furthermore, Roeleveld and Dronkers (1994) claim that deliberate school choice occurs more frequently in communities with more equal relative supplies of school in the dominant sectors and that educational effectiveness is higher in these areas.[44]

4.3 Data and descriptive statistics

The following analysis makes use of PRIMA, a longitudinal survey containing information on Dutch pupils who were enrolled in grades 2, 4, 6 and 8 in the 1994/1995 school year. Several instruments were used in collecting the data: administrative sources, test results and questionnaires for the teachers, parents and headmasters of approximately 800 primary schools throughout the Netherlands. Of these, 400 were chosen to form a nationally representative sample of "regular" schools used in this analysis. Here, the sample is limited to only the observations on individuals from grades 4, 6 and 8, as the information on class size for grade 2 is too unreliable.[45]

The PRIMA survey provides a wealth of information at the pupil, class, and school levels. Pupils' individual scores on arithmetic and language tests transformed into grade-level percentile rankings to measure school performance are regressed on various pupil, class and school characteristics. At the individual level we include indicators of gender and weight

[44] If this is indeed the case, then clearly the instrumental validity of our school availability identification strategy should be put into question. This is tested empirically below.

[45] The documentation of the PRIMA survey states that a considerable number of teachers in grade 2 seem to have given erroneous information on class size. Probably they filled in the total number of pupils enrolled at that grade level instead of the number in their own class.

factors accounting for socioeconomic status (SES) as covariates. The weight factor of a pupil indicates his/her social background and is used to determine the amount of resources a school is given for a certain pupil; schools get more money for pupils with a disadvantaged background than for those with a "normal" background.

The weight factor ranges from 1.00 to 1.90 and is defined as follows:

Weight factor value	Definition of weight factor category
1.90	Pupils with foreign-born parents that satisfy one of the following conditions: 1) father, mother or guardian has at most a VBO-level education;[46] 2) the primary earning parent or guardian has either a job involving physical labor, or has no income from labor at the time of interview.
1.70	Pupils whose parents are transients.
1.40	Pupils living in a boarding school or a foster home, and whose parents are masters of a ship.
1.25	Pupils of whom either parents or guardians have at most an education at VBO-level.
1.00	Other pupils (reference group).

In addition, individual level variables are included denoting parental education, student age and an indicator whether a pupil is significantly older than the "normal" student in his/her grade level. The latter serves as an indicator of grade repetition or late school entry.[47] Class level controls include class size, an indicator of teacher's gender and years of experience, share of classmates that are female, average SES, and indicators of a dual teacher (whether more than one teacher has taught the class) and multi-grade class (whether the class combines individuals from more than one grade level). At the school level indicators are included denoting degree of urbanization around school, average socioeconomic status (SES), total enrollment and, of primary interest to our analysis, school denomination (i.e. Catholic, Protestant and public).[48],[49]

The sample has been limited to only those observations without missing values for the baseline variables used in the analysis. In addition, observations for individuals attending other types of schools (including Islamic and those using specialized pedagogical methods/ideologies) have been omitted. Finally, in order to implement the identification strategy set above, it was

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[46] VBO stands for Voorbereidend Beroepsonderwijs or secondary vocational education, which serves as the lowest form of secondary schooling in the Dutch educational system.

[47] This variable is defined as equaling one if the student was born before the 1st of October in 1986, 1984 and 1982 for those in the 4th, 6th and 8th grades, respectively, and zero, otherwise.

[48] Urbanization controls consist of four dummy variables derived from a five-point scale of address density (the number of addresses per sq. km.) within the city and ranges from one (the reference group) to five for the following categories: greater than or equal to 2,500; from 1,500 to 2,499; 1,000 and 1,499; 500 and 999; and less than 500. These measures were taken from the CBS publication *De landelijke wijk- en buurtindeling* (1993).

[49] The class and school level variables used as well as those pertaining to school availability denote lagged indicators of these characteristics the year prior to the survey (1993-94), whereas achievement and measures based on IQ are proxied by scores on tests taken at the beginning of the survey year (1994-95).

necessary to limit our sample to only those observations that had access to all of the three dominant school types (i.e. whose gemeente contained at least one of each type of school).[50]

Appendix B contains descriptive statistics for the variables used in the analysis broken out by grade level and school sector. The differences in raw averages of test scores across school types give clear evidence as to the motivation of this study. Catholic schools produce average arithmetic test scores that are 4.8 to 7.4 percentile points higher than those of public schools and 4.8 to 7.0 points higher than their Protestant school counterparts. The average language score differences are not as pronounced ranging from a –0.2 to 3.8 percentile difference between Catholic and public and from 1.8 to 4.3 between Catholic and Protestant schools, respectively.

Investigation of the descriptive statistics corresponding to the baseline variables shows there to be little or no difference in the data across the various school types. Average enrollment seems to be lower for Protestant schools at all three grade levels. In addition, the dispersion in school enrollment is noticeably larger for public schools in the 4th and 6th grade sub-samples. The distribution of SES is remarkably similar across the three schooling types regardless of grade level; there is never more than a 5% difference when comparing the proportion of students in a particular SES weight category across school types. The distribution of parental education across school type is also quite similar with no more than a 9% difference in incidence of parental education for a particular category (i.e. the difference with respect to the proportion of fathers with intermediate vocational education (MBO) between 6th grade students in Catholic and public schools). Similarly, there is little difference in gender composition across school type or grade level. With respect to instructors, 6th grade teachers in Protestant schools have an abnormally low average level of experience relative to those in the Catholic and public sectors. The composition of teachers with respect to gender becomes significantly more male-dominated as grade level increases however there is little difference exhibited across school types. A lower proportion of 6th and 8th grade Catholic classes have had two or more teachers in the observed year implying a lower incidence of instructor absenteeism within this schooling sector. In addition, the Catholic schooling sector exhibits fewer classes catering to multiple grade levels than do the Protestant or public sectors.

Perhaps the most informative descriptive statistics concern the variables to be used in the identification strategies. Namely, the variables describing parental religion and availability of the three various school types all give indication of their importance with respect to school choice. Just under half (49.7%) of 4th graders enrolled in Catholic schools have parents of the same faith and over half have at least one parent that is Catholic.[51] In contrast, the proportion of Catholic school students whose parents are both Protestant is just over 1%. In Protestant schools the percentage of parents that are religious is slightly less however the same fundamental story holds. That is, a relatively large proportion (36%) of

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[50] The resulting number of observations after excluding those with missing values of baseline variables (other than school enrollment whose missing values are imputed with mean values for the regressions that follow) and enrolled in "Other" schools is 4,567, 4,217 and 4,410 for the 4th, 6th and 8th grades, respectively. Further limiting the sample to those observations that have access to all three schooling sectors caused these numbers to drop to 3,387, 3,090 and 3,328.

[51] Unfortunately, religion of parents is only available for the 4th graders and therefore this identification strategy cannot be implemented in the case of the 6th and 8th grades.

Protestant school participants come from families where both parents adhere to this faith compared to 4.6% of students in these schools whose parents are both Catholic.

A survey of the supply of the various types of schools across the sample also proves to be quite informative. More precisely, the concentration of Protestant and Catholic schools in the gemeente seems to be closely related to school choice in the religious schooling sectors. For example, in the areas in which the 4th grade Catholic school observations lie there is on average approximately one Catholic school for every 3.7 km^2 where the concentration of Protestant schools is one for every 6.8 km^2.[52] In those areas in which the survey participants attend Protestant schools the availability of religious schools is reversed where, on average, there is one Protestant school for every 4.9 km^2 and one Catholic school for every 7.0 km^2. The concentration of public schools across all three subsamples is almost constant averaging one school per 4.5, 4.4 and 4.8 km^2 for the public, Protestant and Catholic sector subsamples, respectively. Finally, the relative supply of Catholic schools, proxied as the ratio of Catholic versus other types within the gemeente, shows strong differences across school sectors. In areas surrounding our 4th grade sample of Catholic schools the average ratio of Catholic to Protestant schools is over twice as high as in areas surrounding schools in the public sector sample and over 4.5 times as large as those surrounding the Protestant school sample. Although smaller in value, the average ratio of Catholic to public schools is also larger in areas surrounding the Catholic versus other school samples.

5 Empirical results

5.1 Controlling for educational inputs across sectors

Table 1a provides results to the baseline achievement regressions of the three grade levels associated with equation (1). Immediately one takes notice of the general result whereby Catholic school students have significantly higher achievement in both arithmetic and language. The estimates suggest that, compared to the average public school student, an average Catholic school student scores 5.4 to 7.6 percentile points higher on the arithmetic exam depending on the grade level. Interestingly enough, 4th graders in Catholic school do not exhibit the same increase in language achievement over their public school counterpart. However, there is an expected advantage of 6.5 and 4.2 percentiles in language achievement for Catholic students in the 6th and 8th grades, respectively. Additional estimates of interest, although not the focus of this paper, are those pertaining to student gender, individual SES, and teacher experience. In brief, female students tend to perform worse in arithmetic while the individual SES indicators are generally large, negative, and significant at conventional levels (especially for the SES categories 1.25 and 1.9). Individuals that are late entrants or held back a grade have on average significantly lower achievement.[53]

[52] Although we use the 4th grade sample as an example, the pattern of schooling availability is quite similar across the three grade levels.

[53] Despite significance of the missing value indicator for this variable denoting non-ignorable non-response bias, limiting the sample to only those observations with non-missing values yields almost identical results with respect to the late entrant indicator.

<table>
<tr><td colspan="7">Table 1a – OLS Baseline Regressions of Educational Achievement (Reference Group is Public School)</td></tr>
<tr><td></td><td colspan="3">Math</td><td colspan="3">Language</td></tr>
<tr><td></td><td>Grade 4</td><td>Grade 6</td><td>Grade 8</td><td>Grade 4</td><td>Grade 6</td><td>Grade 8</td></tr>
<tr><td>Protestant school</td><td>2.569
(2.492)</td><td>1.490
(2.031)</td><td>1.270
(1.986)</td><td>-0.500
(2.543)</td><td>3.555
(1.696)**</td><td>0.045
(1.602)</td></tr>
<tr><td>Catholic school</td><td>5.446
(2.267)**</td><td>7.562
(1.757)***</td><td>7.459
(1.736)***</td><td>-0.323
(2.361)</td><td>6.490
(1.611)***</td><td>4.244
(1.249)***</td></tr>
<tr><td>Student gender (1=female)</td><td>-3.874
(0.956)***</td><td>-7.514
(0.939)***</td><td>-8.262
(0.854)***</td><td>1.065
(0.802)</td><td>2.719
(1.016)***</td><td>0.100
(0.899)</td></tr>
<tr><td>SES weight factor = 1.25</td><td>-4.865
(1.206)***</td><td>-5.181
(1.572)***</td><td>-5.222
(1.522)***</td><td>-4.917
(1.245)***</td><td>-6.972
(1.507)***</td><td>-2.689
(1.212)**</td></tr>
<tr><td>SES weight factor = 1.4</td><td>0.967
(4.305)</td><td>9.706
(6.034)</td><td>3.426
(10.181)</td><td>-13.253
(7.474)*</td><td>8.647
(6.886)</td><td>6.386
(12.012)</td></tr>
<tr><td>SES weight factor = 1.7</td><td>-10.190
(8.744)</td><td>-16.163
(8.268)*</td><td>-12.816
(7.671)*</td><td>-2.470
(9.180)</td><td>-4.065
(8.395)</td><td>-19.541
(4.378)***</td></tr>
<tr><td>SES weight factor = 1.9</td><td>-8.863
(1.910)***</td><td>-7.033
(2.310)***</td><td>-7.598
(2.434)***</td><td>-17.203
(1.875)***</td><td>-15.663
(2.310)***</td><td>-15.069
(1.983)***</td></tr>
<tr><td>Student age in months</td><td>0.651
(0.118)***</td><td>0.131
(0.136)</td><td>0.068
(0.106)</td><td>0.243
(0.115)**</td><td>0.144
(0.134)</td><td>0.054
(0.103)</td></tr>
<tr><td>Student age missing</td><td>-10.811
(13.525)</td><td>-4.193
(18.591)</td><td>-41.947
(3.599)***</td><td>-13.874
(3.561)***</td><td>30.013
(4.813)***</td><td>-38.959
(3.136)***</td></tr>
<tr><td>Late entrant indicator</td><td>-10.274
(1.763)***</td><td>-15.744
(1.961)***</td><td>-15.390
(1.729)***</td><td>-7.043
(1.692)***</td><td>-12.691
(1.904)***</td><td>-12.594
(1.716)***</td></tr>
<tr><td>Late entrant indicator missing</td><td>32.187
(13.178)**</td><td>0.961
(19.275)</td><td>37.968
(6.012)***</td><td>32.135
(3.659)***</td><td>-25.196
(5.527)***</td><td>38.822
(3.919)***</td></tr>
<tr><td>Mother LBO
(secondary vocational)</td><td>-0.038
(1.873)</td><td>-0.924
(2.466)</td><td>2.331
(2.476)</td><td>3.743
(2.010)*</td><td>-2.210
(2.028)</td><td>5.520
(2.076)***</td></tr>
<tr><td>Mother MAVO (lower
secondary general)</td><td>5.222
(2.080)**</td><td rowspan="3">2.928
(2.672)</td><td rowspan="3">6.407
(2.583)**</td><td>6.117
(2.000)***</td><td rowspan="3">2.149
(2.331)</td><td rowspan="3">12.024
(2.284)***</td></tr>
<tr><td>Mother HAVO/VWO
(upper secondary)</td><td>8.281
(2.246)***</td><td>9.658
(2.351)***</td></tr>
<tr><td>Mother MBO
(intermediate vocational)</td><td>5.264
(2.353)**</td><td>4.889
(2.084)**</td></tr>
<tr><td>Mother HBO
(higher vocational)</td><td>11.690
(2.593)***</td><td rowspan="2">3.790
(2.889)</td><td rowspan="2">6.464
(2.892)**</td><td>11.439
(2.384)***</td><td rowspan="2">5.330
(2.865)*</td><td rowspan="2">16.267
(2.603)***</td></tr>
<tr><td>Mother WO
(higher university)</td><td>17.468
(4.098)***</td><td>13.144
(3.365)***</td></tr>
<tr><td>Father LBO
(secondary vocational)</td><td>3.020
(2.065)</td><td>1.651
(2.378)</td><td>5.620
(2.337)**</td><td>4.099
(1.708)**</td><td>3.325
(1.869)*</td><td>1.188
(1.930)</td></tr>
<tr><td>Father MAVO (lower
secondary general)</td><td>1.911
(2.094)</td><td rowspan="3">2.986
(2.731)</td><td rowspan="3">7.771
(2.664)***</td><td>2.145
(2.020)</td><td rowspan="3">3.741
(2.327)</td><td rowspan="3">3.117
(2.164)</td></tr>
<tr><td>Father HAVO/VWO
(upper secondary)</td><td>4.552
(2.464)*</td><td>4.749
(2.067)**</td></tr>
<tr><td>Father MBO
(intermediate vocational)</td><td>-1.348
(2.195)</td><td>6.165
(2.050)***</td></tr>
<tr><td>Father HBO
(higher vocational)</td><td>0.896
(2.187)</td><td rowspan="2">8.602
(2.923)***</td><td rowspan="2">11.847
(2.884)***</td><td>3.774
(2.332)</td><td rowspan="2">10.387
(2.520)***</td><td rowspan="2">6.132
(2.301)***</td></tr>
<tr><td>Father WO
(higher university)</td><td>4.695
(2.676)*</td><td>9.355
(2.527)***</td></tr>
<tr><td>Mother education
missing</td><td>-0.425
(2.430)</td><td>4.629
(2.393)*</td><td>0.521
(2.399)</td><td>-2.806
(2.679)</td><td>2.783
(2.151)</td><td>1.837
(1.987)</td></tr>
<tr><td>Father education
missing</td><td>-0.383
(2.344)</td><td>1.383
(2.075)</td><td>-1.482
(2.361)</td><td>-0.071
(2.299)</td><td>1.210
(2.041)</td><td>-0.178
(2.009)</td></tr>
</table>

Table 1a (continued) – OLS Baseline Regressions of Educational Achievement (Reference Group is Public School)						
	Math			Language		
	Grade 4	Grade 6	Grade 8	Grade 4	Grade 6	Grade 8
Class size	0.275 (0.155)*	0.001 (0.150)	0.046 (0.132)	-0.226 (0.156)	-0.304 (0.118)**	-0.054 (0.103)
Teacher gender (1=female)	2.640 (2.112)	-2.605 (1.447)*	4.111 (1.574)***	-1.771 (2.560)	-2.883 (1.507)*	2.281 (1.127)**
Teacher experience in years	0.059 (0.120)	0.166 (0.084)**	0.270 (0.092)***	-0.107 (0.122)	0.159 (0.085)*	0.126 (0.075)*
Share of class female	0.840 (7.089)	-9.439 (5.550)*	1.013 (5.643)	-1.308 (6.943)	-12.488 (5.018)**	-2.260 (4.315)
Class average SES	-0.071 (14.648)	18.911 (17.462)	5.962 (13.691)	-5.127 (16.523)	5.439 (13.288)	-4.273 (10.661)
Dual teacher class	1.827 (1.843)	1.435 (1.448)	0.970 (1.472)	2.612 (1.970)	1.402 (1.604)	0.096 (1.322)
Dual grade class	-0.200 (2.207)	0.010 (1.715)	-0.616 (1.697)	-1.105 (2.574)	-0.388 (1.779)	0.759 (1.257)
School average SES	-12.211 (15.093)	-28.699 (17.950)	-16.142 (13.034)	-20.081 (17.276)	-23.490 (14.904)	-11.136 (10.800)
School enrollment	0.017 (0.010)*	-0.005 (0.008)	0.015 (0.009)*	0.014 (0.011)	0.002 (0.009)	0.014 (0.006)**
School enrollment missing	-4.831 (3.457)	-4.197 (3.304)	-0.947 (2.949)	-9.814 (8.270)	-0.790 (3.985)	-0.780 (2.359)
Urbanization = 2	3.362 (3.022)	1.923 (2.382)	4.730 (2.803)*	-3.140 (3.529)	1.280 (3.226)	2.729 (2.312)
Urbanization = 3	3.252 (3.186)	4.371 (2.552)*	8.287 (2.452)***	-6.103 (3.450)*	0.156 (2.963)	4.558 (2.103)**
Urbanization = 4	3.455 (3.453)	2.063 (2.398)	6.177 (2.805)**	-3.836 (3.693)	-1.307 (3.148)	4.615 (2.259)**
Urbanization = 5	0.097 (3.839)	1.084 (2.827)	7.456 (3.221)**	-6.573 (4.012)	-3.789 (3.444)	3.672 (2.842)
Constant	-12.852 (17.317)	52.830 (17.907)***	35.713 (20.038)*	69.097 (17.671)***	69.944 (18.712)***	52.031 (16.864)***
Adjusted R^2	0.1240	0.1390	0.2119	0.1632	0.1692	0.2306
Observations	3387	3090	3328	3387	3090	3328

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
Robust standard errors taking account of correlated disturbance terms within classes reported in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

above the lowest form of secondary education is most important for both the arithmetic and language achievement of 4th and 8th graders.[54] The effect of father's education is more sporadic however, a consistent pattern is found with respect to higher education, which has a significantly positive effect on both achievement measures across all three grade levels. Moving to the class level controls there is evidence that teacher experience has a positive influence on 6th and 8th grade student achievement in both arithmetic and language. Finally, relative to 8th graders attending schools in densely populated cities, those in medium urbanized areas (urbanization levels 3 and 4) have higher scores on average in both achievement measures.

[54] Because more detailed information on parental education was available only for the lowest grade level these variables are broken into seven indicators for the 4th grade sample and four for the 6th and 8th grades.

In accordance with our first strategy we next include specific educational practices in the baseline equation to test the hypothesis that schooling practices which prove to be superior in terms of boosting achievement may be employed more often or more effectively by schools in the Catholic sector. Explicitly controlling for possible achievement-enhancing practices that are more prevalent and/or effective in Catholic schools should account for at least part of the premium to this type of schooling found in the baseline estimates. To this end, we will assess the resulting Catholic school effect after including in separate baseline regressions controls for the following: time spent in class on math, language and reading; homework assignment practices; curriculum employed; teaching style most often used; frequency of testing; and, student composition of class with respect to cognitive ability.

Time spent in class on mathematics, language and reading is calculated in minutes. The latter two are both included for the language achievement equations while only the former is used to estimate arithmetic achievement outcomes. Controls for homework practices consist of three indicators denoting if the teacher never or rarely assigned homework, only assigned homework to weaker students, or assigned homework to all students (those whose whole grade level did not receive homework serves as the reference group). Curriculum controls include thirteen indicators identifying various methods most often used by the teacher to teach mathematics and language at each grade level. In addition, three dummy variables measure the extent to which the instructor follows the associated teaching material. The possible answers are “almost always”, “only the important parts”, “hardly ever” and “no associated instructional material” (the reference group). Indicators for teaching style include: classical lecture style (the reference group); primarily classical with individual <u>or</u> group supplement; alternative classical, individual and/or group supplement; primarily in homogeneous groups (i.e. individuals of a similar cognitive level); primarily in heterogeneous groups (i.e. individuals of differing cognitive levels); homogeneous groups including different grade levels; and, primarily individual instruction. The testing frequency controls include three sets of indicators for curriculum, external and diagnostic exams. Each set includes five indicators denoting the number of tests administered per year in the following categories: one to two tests, three tests, five or more tests, number unknown, and none (the reference group).

Taking from previous work by Dobbelsteen et al (2002), the existence of classmates with similar cognitive ability has been shown to have a significant positive effect on achievement. Therefore, repeating the exercise here we control for the cognitive composition of a class by including the variable “number of classmates with similar IQ”.[55] Finally, we estimate specifications in which all seven schooling practices are included in each of the grade level/achievement regressions.

The results of the seven educational practice exercises are recorded on Table lb. Only the estimated coefficients of the Protestant and Catholic school indicators are reported for each of the regressions. Notable results from the battery of regressions are sporadic at best and at times confounding. Comparing the baseline Catholic coefficients in the second row of the table to the corresponding point estimates that follow we find that in 17 of the 21 pairwise baseline/control contrasts does the estimated Catholic school effect decrease as a result of controlling for educational practices. However, with respect to language achievement we find

[55] This is simply a tally of the number of classmates within a student’s class that falls within a plus or minus one-half grade-level standard deviation IQ bandwidth around the individual’s own IQ.

this number to be much smaller where just over half (12 of the 21) of the comparisons show a decreased Catholic school effect after controlling for schooling practices.

Table 1b - Effects of School Denomination on Achievement Controlling for Educational Practices (Reference Group is Public School)

		Math			Language		
Specification		Grade 4	Grade 6	Grade 8	Grade 4	Grade 6	Grade 8
Baseline	Protestant	2.569 (2.492)	1.490 (2.031)	1.270 (1.990)	-0.500 (2.543)	3.555 (1.696)**	0.045 (1.602)
	Catholic	5.446 (2.267)**	7.562 (1.757)***	7.459 (1.736)***	-0.323 (2.361)	6.490 (1.611)***	4.244 (1.249)***
Time allocation	Protestant	1.721 (2.509)	0.987 (2.028)	1.677 (2.028)	-1.539 (2.513)	3.319 (1.714)*	0.227 (1.661)
	Catholic	5.353 (2.284)**	7.109 (1.733)***	7.585 (1.760)***	-0.209 (2.378)	6.306 (1.538)***	4.236 (1.290)***
Homework	Protestant	1.549 (2.485)	0.590 (2.022)	0.481 (2.055)	-1.564 (2.528)	2.751 (1.690)	-0.619 (1.630)
	Catholic	5.628 (2.335)**	6.689 (1.704)***	6.779 (1.775)***	-0.080 (2.392)	6.550 (1.640)***	3.920 (1.290)***
Curriculum	Protestant	0.248 (2.569)	0.070 (2.028)	2.223 (2.045)	-1.054 (2.674)	2.618 (1.713)	-0.858 (1.511)
	Catholic	4.846 (2.387)**	6.394 (1.676)***	7.778 (1.774)***	0.572 (2.458)	6.055 (1.633)***	4.273 (1.345)***
Teaching style	Protestant	0.672 (2.588)	0.201 (2.036)	-0.874 (2.033)	-1.475 (2.561)	2.460 (1.710)	-0.325 (1.637)
	Catholic	4.933 (2.369)**	6.259 (1.735)***	5.665 (1.854)***	-0.292 (2.443)	5.568 (1.594)***	3.925 (1.329)***
Testing frequency	Protestant	1.929 (2.421)	0.496 (2.085)	1.011 (2.021)	1.363 (2.671)	3.776 (1.778)**	-0.247 (1.510)
	Catholic	4.369 (2.152)**	6.323 (1.675)***	7.325 (1.751)***	1.226 (2.398)	5.993 (1.691)***	3.125 (1.364)**
Peer effect	Protestant	1.642 (2.406)	0.995 (1.967)	1.220 (2.022)	-1.092 (2.525)	3.014 (1.690)*	-0.274 (1.556)
	Catholic	4.256 (2.190)*	6.645 (1.659)***	7.263 (1.787)***	-0.558 (2.338)	5.731 (1.541)***	3.901 (1.279)***
All controls	Protestant	0.693 (2.446)	-0.918 (2.283)	0.579 (2.035)	0.524 (2.764)	3.721 (1.647)**	0.269 (1.594)
	Catholic	2.749 (2.011)	5.618 (1.959)***	4.575 (1.781)**	2.158 (2.524)	7.168 (1.889)***	3.497 (1.358)**

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: pupil's gender (1=female), four individual SES category indicators, pupil's age in months, pupil's age missing indicator, late entry indicator (1=late entry), late entry missing indicator, class size, teacher's gender (1=female) and experience (in years), share of females in pupil's class, class average SES (ranging 1-1.9), indicators for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, school enrollment missing indicator, and degree of urbanization.
Number of observations for 4th, 6th and 8th grades are 3,387, 3,090 and 3,328, respectively.
Robust standard errors taking account of correlated disturbance terms within classes are reported in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

A few patterns emerge when assessing Catholic school effectiveness after controlling for specific educational practices across the grade levels. Namely, including controls for teaching style, testing frequency and classroom composition with respect to student IQ cause a drop in the estimated baseline Catholic premium over public schools in arithmetic achievement across all grades. The results show that controlling for teaching style causes a drop by about one-quarter (1.8 out of 7.5 percentiles) of the 8th grade Catholic school arithmetic premium. Similarly, at the 4th grade level including controls for testing frequency and peer effects in the arithmetic regressions takes away approximately one-fifth of the Catholic school effect (20 and 22%, respectively). However, it must be noted that only in the case of controlling for peer effects does the decrease cause the estimated Catholic school premium to become insignificant at the conventional 5%-level and even then, the practice-controlled coefficient does not significantly differ from the estimated baseline Catholic school effect.

With respect to language the results are more scattered. Again, controlling for teaching style and peer effects consistently accounts for part of the Catholic school achievement premium across all grade levels, yet the magnitude of these effects is small in both absolute terms and relative to the baseline Catholic school premium. Conversely, controlling for curriculum actually increases the expected language achievement premium to Catholic versus public schools at all grade levels. The "corrected" estimates also imply an increase in the Catholic school premium for all 6th grade language specifications except for teaching style and peer effects. Therefore, the results suggest that the incidence and/or effectiveness of these schooling practices in Catholic schools is in fact lower and/or inferior to their public sector counterparts. We must again note that the changes in these point estimates serve only as a rough indicator as in no case does their statistical significance change. Finally, the most notable effect from the language regressions comes from controlling for 8th grade testing frequency. Here we find controlling for testing practices is met with a 26% decrease in (1.1 out of 4.2 percentiles) the Catholic school premium. In addition, this "corrected" premium falls in significance from the 1% to the 5%-level. Again, in no instance do the practice-controlled coefficients differ from the estimated baseline effects.

Of course, there may be similarity in the average effects of the various educational practices on achievement. Therefore, a better strategy may be to test for all of the controls simultaneously. The last two rows of Table 1b offer the results of this exercise. When all controls are implemented we find that the arithmetic premium to Catholic versus public schools drops considerably across all three grades. The most dramatic decrease is found for 4th grade where the point estimate of the Catholic school effect drops by approximately one-half and becomes insignificant at conventional levels. The 8th grade estimate declines by 39% and is less precise (decreases in significance to the 5%-level). The relative decrease for the 6th grade estimate is more modest equaling only 26% and retains its significance at the 1%-level. To the contrary, the language regressions controlled for all effects simultaneously result in an *increase* in the estimated Catholic school premium for the lower two grades.[56] Finally, we find the familiar decrease for the 8th grade where the point estimate drops by almost 15% and declines in significance to the 5%-level.

It is worthy to note that the exercise above estimates only the main effects of the various controls on achievement. In other words, what is estimated are the average effects of the given educational practices across all schooling sectors. However, it may be plausible that

[56] The 4th grade Catholic school effect increases by a factor of 7.7 but remains insignificant at any reasonable level of confidence. The relative increase of the 6th grade effect is just 10% and remains significant at the 1%-level.

certain schooling practices are particularly complementary within a specific schooling sector. For instance, although the use of a given type of curriculum has no significant effect across the whole sample, perhaps its effect on achievement is significant within the environment of a particular schooling sector. To this end, the preceding regressions have also been performed accounting for interactions between the various practices and schooling sectors. The result of this exercise provides even less of a clear picture than the simpler models that account only for the main effects. That is, no consistent pattern emerges showing a significant complementarity between Catholic schools and educational practices that can account for the estimated baseline premium to this type of education.

In sum, the general result is that the controls for educational practices at our disposal do not sufficiently explain the Catholic school premium (over public schools) in scholastic achievement. Only the number of classmates with similar IQ (peer effect) produces a consistent negative effect on the estimated Catholic school premium across all grade levels for both measures of achievement. Moreover, in only one of the 29 cases where we observe a decrease in the Catholic school effect did the practice-controlled estimated premium become insignificant at conventional levels (but not significantly different from the corresponding baseline estimate).[57]

5.2 Controlling for selectivity bias

We now turn to the second possible explanation for the Catholic school hypothesis, selectivity bias. The following section presents the results of the various IV procedures used to purge the estimates of the Catholic school effect of any potential bias caused by self-selection into the various schooling sectors. The first two columns of Tables 2a through 2e show the results of baseline estimates of the Catholic and Protestant school effects shown above in Table 1a. The following two columns in each table contain the results of the first-stage MNL or CL models of school choice while the last two report the results of the corresponding selectivity-corrected achievement equations.

5.2.1 First-stage results

To accommodate the possibility of selectivity bias we now turn to the two-stage procedure formalized above. To this end, the first-stage CL and MNL equations illustrated above have been performed using the baseline variables in addition to our instruments, student religion (for the 4th grade only) and relative within-gemeente school availability. Again, in light of the fact that we employ no less than 33 variables in the models that follow, for sake of brevity we restrict our reporting to only those coefficient estimates of primary interest (i.e. the effects of our instruments and schooling sector indicators).

Rather then just list the raw first-stage coefficients, we also present the estimated marginal effects of the instruments on the expected participation *probabilities* in brackets. This is because, in contrast to the simpler binary logit (i.e. a logit model with two choices), the interpretation of estimated coefficients from a MNL or CL model is more complex.

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[57] The specification controlling for all practices resulted in the positive baseline Catholic school effect on 4th grade math to become insignificant.

Table 2a - 4th Grade Achievement Using Student Religion Identification Strategy (Reference Group is Public School)						
	Baseline equations		First-stage multinomial logit equations		Second-stage selectivity-corrected equations	
	Math	Language	Protestant	Catholic	Math	Language
Protestant school	2.569 (2.492)	-0.500 (2.543)			3.668 (4.657)	-1.484 (4.277)
Catholic school	5.446 (2.267)**	-0.323 (2.361)			9.336 (3.931)**	-1.716 (3.618)
Child Protestant			2.755 (0.188)*** [0.638]	-0.686 (0.284)** [-0.458]		
Child Catholic			0.027 (0.222) [-0.182]	2.724 (0.133)*** [0.538]		
Observations	3387	3387	3387	3387	3387	3387
Adjusted/pseudo R^2	0.1240	0.1632	0.2976		0.1217	0.1632
Test of instrument exclusion from first-stage (p-value)			860.63 (0.0000) $\chi^2_{crit(4)} = 9.49$			
Hausman test for IIA (p-value)			5.05 (0.9999) $\chi^2_{crit(36)} = 23.27$	2.24 (0.9999) $\chi^2_{crit(36)} = 23.27$		
Overidentification test of instrument validity (p-value from F-test)					2.11 (0.1238) $F_{crit(2, 202)}$=3.04	0.07 (0.9333) $F_{crit(2, 202)}$=3.04
Hausman exogeneity test p-value					0.2362	0.8685

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: four individual SES dummies, a dummy for pupil's gender (1=female), pupil's age in months, missing age dummy, dummy for late entry/repeater (1=late entrant), missing late entry dummy, share of females in class, class average SES (ranging 1-1.9), teacher's gender (1=female) and experience (in years), dummies for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, enrollment missing dummy, 12 dummies denoting parental education level, two missing parental education dummies, and four degree of urbanization dummies.
Robust standard errors taking account of correlated disturbance terms within classes reported in parentheses. Marginal probability of one-unit change in covariates reported in brackets. Marginal effects calculated at sample means of independent variables.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Whereas the sign of a coefficient in the simple binary case always "agrees" with the estimated effect on the participation probability in a given state (i.e. a positive (negative) coefficient indicates an unambiguous increase (decrease) in the expected probability of participating in the given state), in the more general CL/MNL models this need not be the case.[58]

We start with the first identification strategy for the 4th grade, using student religion indicators for Protestant and Catholic as instruments. The results in the third and fourth columns of Table 2a show that religion of a child indeed has a large significant impact on the sector of schooling he or she will attend. The average student whose parents are both Protestant is 63.8% more likely to attend a school in the Protestant sector and 45.8% less likely to be in a Catholic school. As one might expect, the pattern is reversed for Catholic students who are 53.8% more likely to attend a school of the same religious persuasion. However, despite an estimated decrease of 18.2%, the results suggest that being Catholic does not significantly affect one's probability of attending a Protestant school.

[58] The marginal effects here were computed using Stata 7.0 and LIMDEP 7.0 for the MNL and CL models, respectively. For more on calculating marginal effects of CL and MNL models the reader is referred to Liao (1994).

Table 2b - 4th Grade Achievement Using School Availability Identification Strategy (Reference Group is Public School)						
	Baseline equations		First-stage mixed logit equations		Second-stage selectivity-corrected equations	
	Math	Language	Protestant	Catholic	Math	Language
Protestant school	2.569 (2.577)	-0.500 (2.428)			4.172 (10.722)	11.274 (9.479)
Catholic school	5.446 (2.446)**	-0.323 (2.322)			11.057 (9.826)	7.658 (7.921)
Relative school availability			0.678 (0.030)*** [0.126]	[0.169]		
Observations	3387	3387	10161		3387	3387
Adjusted/pseudo R^2	0.1240	0.1632	0.2429		0.1316	0.1744
Test of instrument exclusion from first-stage (p-value)			506.84 (0.0000) $\chi^2_{crit(1)} = 3.84$			
Overidentification test of instrument validity (p-value from F-test)					2.31 (0.0799) $F_{crit\ (2,\ 116)}=2.68$	1.99 (0.1190) $F_{crit\ (6,\ 116)}=2.18$
Hausman exogeneity test p-value					0.6631	0.4366

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: four individual SES dummies, a dummy for pupil's gender (1=female), pupil's age in months, missing age dummy, dummy for late entry/repeater (1=late entrant), missing late entry dummy, share of females in class, class average SES (ranging 1-1.9), teacher's gender (1=female) and experience (in years), dummies for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, enrollment missing dummy, 12 dummies denoting parental education level, two missing parental education dummies, and four degree of urbanization dummies.
Robust standard errors taking account of correlated disturbance terms within gemeentes reported in parentheses. Marginal probability of one-unit change in covariates reported in brackets. Marginal effects calculated at sample means of independent variables.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Moving on to our second identification strategy (Table 2b) we find that the expected effect of relative school availability on choice of school sector is highly significant.[59] When evaluated at the 4th grade total sample mean of relative school supply, the marginal effects imply an expected probability attending school in the Protestant and Catholic sectors of 9.4 and 12.5%, respectively.[60] Table 2c contains the 4th grade results when both student religion and relative supply instruments are employed. Again, we find significantly positive effects of being Protestant and Catholic on the probabilities of attending these types of schools, respectively. In addition, having Protestant parents is expected to decrease the probability of enrolling in Catholic school by 14.7%. School availability once again proves to be positive, however the marginal effects have slightly decreased. At the 4th grade average, relative school availability is now expected, *ceteris paribus*, to increase the probability of attendance in these types of schools by 6.6 and 8.8%, respectively.

[59] We have also experimented with using only school density and both measures of school availability however the resulting first- and second-stage estimates are qualitatively similar. Only the results of relative school supply are presented here because they outperformed the other two specifications in terms of instrument quality and validity.

[60] The total sample means of relative school supply for the 4th, 6th and 8th grades are 0.742, 0.745 and 0.753, respectively.

Table 2c - 4th Grade Achievement Using Religion and School Availability Identification Strategy (Reference Group is Public School)

	Baseline equations		First-stage mixed logit equations		Second-stage selectivity-corrected equations	
	Math	Language	Protestant	Catholic	Math	Language
Protestant school	2.569 (2.577)	-0.500 (2.428)			3.028 (5.072)	1.334 (4.207)
Catholic school	5.446 (2.446)**	-0.323 (2.322)			8.628 (4.818)*	0.442 (4.125)
Child Protestant			2.646 (0.190)*** [0.494]	-0.590 (0.288)** [-0.147]		
Child Catholic			0.016 (0.225) [0.003]	2.331 (0.138)*** [0.580]		
Relative school availability			0.746 (0.032)*** [0.089]	[0.119]		
Observations	3387	3387	10161		3387	3387
Adjusted/pseudo R^2	0.1240	0.1632	0.3549		0.1322	0.1728
Test of instrument exclusion from first-stage (p-value)			937.31 (0.0000) $\chi^2_{crit(5)} = 11.07$			
Overidentification test of instrument validity (p-value from F-test)					2.49 (0.0350) $F_{crit(5,\ 116)}$=2.29	3.97 (0.0003) $F_{crit(5,\ 116)}$=2.29
Hausman exogeneity test p-value					0.4598	0.8688

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: four individual SES dummies, a dummy for pupil's gender (1=female), pupil's age in months, missing age dummy, dummy for late entry/repeater (1=late entrant), missing late entry dummy, share of females in class, class average SES (ranging 1-1.9), teacher's gender (1=female) and experience (in years), dummies for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, enrollment missing dummy, 12 dummies denoting parental education level, two missing parental education dummies, and four degree of urbanization dummies.
Robust standard errors taking account of correlated disturbance terms within gemeentes are reported in parentheses. Marginal probability of one-unit change in covariates reported in brackets. Marginal effects calculated at sample means of independent variables.
* significant at 10%; ** significant at 5%; *** significant at 1%.

The third and fourth columns of Tables 2d and 2e contain the results of the first-stage equations using the second identification strategy on the 6th and 8th grade groups. The results for the two higher grades are remarkably similar to that of the 4th grade. That is, the within-gemeente relative school supplies both have a strong positive impact on the school choice decision. Moreover, the results for the 4th and 8th grades are almost identical; the average marginal effects of relative school supply on the 8th grade attendance probabilities for the Protestant and Catholic sectors equal 10.2 and 12.7%, respectively. The estimated marginal effects for the 6th grade are somewhat stronger resulting in average marginal school supply effects measuring 14.1 and 17.2% for the Protestant and Catholic sectors, respectively.

5.2.2 Independence of irrelevant alternatives and other issues

Before reporting the results from the second-stage outcome equations, an issue specific to the use of the CL and MNL models must first be addressed, the assumption of independence of irrelevant alternatives (IIA). IIA is a property of the CL/MNL model by which the relative

Table 2d – 6th Grade Achievement Using School Availability Identification Strategy (Reference Group is Public School)

	Baseline equations		First-stage mixed logit equations		Second-stage selectivity-corrected equations	
	Math	Language	Protestant	Catholic	Math	Language
Protestant school	1.490 (1.970)	3.555 (1.787)**			-2.480 (7.303)	5.232 (6.312)
Catholic school	7.562 (1.862)***	6.490 (1.791)***			4.843 (6.317)	11.070 (4.906)**
Relative school availability			0.938 (0.037)*** [0.189]	[0.231]		
Observations	3090	3090	9270		3090	3090
Adjusted/pseudo R^2	0.1390	0.1692	0.2866		0.1376	0.1761
Test of instrument exclusion from first-stage (p-value)			630.00 (0.0000) $\chi^2_{crit(1)} = 3.84$			
Overidentification test of instrument validity (p-value from F-test)					0.76 (0.5176) $F_{crit(3, 113)}=2.68$	2.22 (0.0898) $F_{crit(3, 113)}=2.68$
Hausman exogeneity test p-value					0.9313	0.0694

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: four individual SES dummies, a dummy for pupil's gender (1=female), pupil's age in months, missing age dummy, dummy for late entry/repeater (1=late entrant), missing late entry dummy, share of females in class, class average SES (ranging 1-1.9), teacher's gender (1=female) and experience (in years), dummies for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, enrollment missing dummy, six dummies denoting parental education level, two missing parental education dummies, and four degree of urbanization dummies.
Robust standard errors taking account of correlated disturbance terms within gemeentes are reported in parentheses. Marginal probability of one-unit change in covariates reported in brackets. Marginal effects calculated at sample means of independent variables.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2e - 8th Grade Achievement Using School Availability Identification Strategy (Reference Group is Public School)

	Baseline equations		First-stage mixed logit equations		Second-stage selectivity-corrected equations	
	Math	Language	Protestant	Catholic	Math	Language
Protestant school	1.270 (2.150)	0.045 (1.717)			-1.624 (8.903)	0.579 (6.990)
Catholic school	7.459 (1.793)***	4.244 (1.412)***			11.522 (8.008)	10.079 (5.751)*
Relative school availability			0.686 (0.030)*** [0.135]	[0.169]		
Observations	3328	3328	9984		3328	3328
Adjusted/pseudo R^2	0.2119	0.2306	0.2103		0.2132	0.2374
Test of instrument exclusion from first-stage (p-value)			515.57 (0.0000) $\chi^2_{crit(1)} = 3.84$			
Overidentification test of instrument validity (p-value from F-test)					1.50 (0.2180) $F_{crit(3, 121)}=2.68$	1.66 (0.1789) $F_{crit(3, 121)}=2.68$
Hausman exogeneity test p-value					0.2625	0.1161

Dependent variable is percentile ranking of pupils' scores on standardized arithmetic and language tests.
All regressions include controls for the following: four individual SES dummies, a dummy for pupil's gender (1=female), pupil's age in months, missing age dummy, dummy for late entry/repeater (1=late entrant), missing late entry dummy, share of females in class, class average SES (ranging 1-1.9), teacher's gender (1=female) and experience (in years), dummies for dual-teacher and multi-grade class, school average SES (ranging 1-1.9), total school enrollment, enrollment missing dummy, six dummies denoting parental education level, two missing parental education dummies, and four degree of urbanization dummies.
Robust standard errors taking account of correlated disturbance terms within gemeentes are reported in parentheses. Marginal probability of one-unit change in covariates reported in brackets. Marginal effects calculated at sample means of independent variables.
* significant at 10%; ** significant at 5%; *** significant at 1%.

probability or *odds* of choosing between choice states is insensitive to the addition/exclusion of alternative states. This property poses a specification problem as the odds between any two choice states are also held constant when added or omitted alternatives are in fact *relevant*; that is, when states that are close substitutes are added or excluded from the choice set.[61] To this end, the CL/MNL model implicitly assumes that all alternatives in the choice set are *distinct* from one another. Violation of the IIA assumption, where relative probabilities are dependent on alternative states, occurs when states that are "relevant" (not truly distinct) to existing alternatives are introduced into or discarded from the choice set.[62]

In our context, among the schooling choices public, Protestant and Catholic schools one might expect the two latter alternatives to be significantly close substitutes for one another such that the inclusion of say, the alternative Protestant to the choice set public and Catholic should decrease the relative probability of attending school in the latter. Should the two religious states not be significantly distinct, a full decrease in the probability of attending the Catholic choice state resulting from the inclusion of the Protestant alternative will not be realized. This is because proportional shares from both the Catholic and public school states will be used to make up the probability of attending the newly included Protestant schooling alternative so as to preserve the original odds between the two original choice states. If this is the case, our specification violates the implicit IIA assumption made by CL/MNL models and will tend to overstate the relative probabilities of choosing a religious school, and therefore we should employ a different method in describing choice among the alternatives in this set.

The study by Hausman and McFadden (1984) develops a relatively simple test to check whether estimates violate the IIA assumption. The central notion of the test is that if a given alternative is truly irrelevant (distinct from other alternatives), then the remaining estimates after dropping this alternative from the choice set should not change *systematically*.[63] Under the null hypothesis there is no systematic change in the coefficients common to both choice sets. Inspection of the IIA test results corresponding to the first-stage regression on Table 2a we find that the test statistics are far below their respective critical values. Therefore, in this case we can do not reject the null hypotheses put forth and conclude that the specification does not violate the implicit IIA assumption made by the MNL model.

[61] Debreu (1960) was among the first to call attention to the potential inapplicability of the multinomial logit model with the existence of similar alternatives.

[62] For a simple example of the problem we refer the reader to the "red bus/blue bus" anecdote found in most econometric texts to explain the IIA property of CL/MNL models.

[63] The procedure takes the following form of a Hausman-type specification test: $\chi^2 = \left(\hat{\beta}_r - \hat{\beta}_u\right)' \left[\hat{V}_r - \hat{V}_u\right] \left(\hat{\beta}_r - \hat{\beta}_u\right)$, where $\hat{\beta}_r, \hat{V}_r$ and $\hat{\beta}_u, \hat{V}_u$ are the estimated coefficients and variance-covariance matrices of the restricted (with one alternative dropped) and unrestricted models, respectively. The test statistic is asymptotically distributed as chi-squared with k degrees of freedom equal to the difference in number of estimated parameters between the two models.

Unfortunately, for all specifications that follow the statistic is negative allowing us to neither reject nor accept the null hypothesis that our model complies with the IIA assumption.[64]

5.2.3 Quality/validity of instruments and second-stage results

As mentioned in Section 3.4.2, the first criterion for variables to properly identify the causal effect of a treatment (serve as instruments) is that they must be able to explain a significant portion of the variation in treatment status. To assess whether our restrictions "pass" this criterion, included in the first-stage portion of each table are results of Wald tests of the null hypothesis that the estimated coefficients associated with each instrument set is jointly equal to zero. In all specifications for all grade levels, the test statistics far exceed their respective critical values and therefore in every instance we can strongly reject the null hypothesis and be assured that our instruments are of sufficient quality.

The second criterion for variables to be credible instruments is that they must not exert a significant direct influence on the second-stage outcome (be exogenous with respect to the dependent variable of primary interest).[65] To this end, we apply two diagnostic exercises to investigate this possibility. First, as a "rough" test of instrument exogeneity we include the instruments themselves in baseline equations and assess their joint significance via Wald tests. Second, we consider tests of overidentifying restrictions (also called Basmann or Sargan tests).[66] Under the null hypotheses an instrument set is deemed not to have a direct influence on the second-stage outcome measure (i.e. they are not significantly correlated with the second-stage residuals). Alternatively speaking, the tests determine whether the variables in question can be legitimately excluded from the outcome equation.[67]

A potential problem that enters our application of these tests is the grouped nature of some of the variables we use to identify school choice. Shore-Sheppard (1996) and Hoxby and Passerman (1998) show that there is a substantial downward bias on standard errors and subsequent upward bias in overidentification test statistics when there is intra-group correlation in the sample with respect to the instruments being used. In turn, should there be little or no within-group variation among our instruments, our overidentification tests will too often reject the null hypothesis that our identifying variables are valid. To correct for any potential bias caused by the grouped structure of the school availability instruments we have

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[64] An evident property of the simple Hausman-type test is the possibility of producing negative test statistics due to the possible lack of semi-definiteness in the calculated difference of the restricted and unrestricted variance-covariance matrices. However, Hausman and McFadden (1984) derives an alternative restricted covariance matrix that always leads to a positive test statistic and claim that "in no case have we found this alternative statistic to be so large as to come close to any reasonable critical value for a χ^2 test." (see footnote 4 in Hausman and McFadden (1984)).

[65] For example, Figlio and Stone (1999) note that, due to the direct influence of being Catholic on achievement *conditional* on school choice, the use of Catholic religion as an identification restriction may in fact exacerbate the bias caused by self-selection.

[66] Unfortunately, the first specification we consider is *exactly identified* (i.e. there are an equal number of endogenous regressors and instruments) so that an overidentification test could not be applied. For a more elaborate discussion of identification and overidentifying restrictions see Davidson and MacKinnon (1993), pp. 232-237.

[67] It is acknowledged that both procedures are "crude" in that they have been constructed for use in linear simultaneous equations models and therefore may not be perfectly applicable in the current non-linear setting.

calculated robust standard errors clustered by gemeente for specifications using this strategy.[68]

The second-stage achievement equations have been run for both arithmetic and language using the predicted probabilities of Protestant and Catholic school attendance derived from the first-stage results in place of the actual (potentially endogenous) schooling sector participation indicators. The second-stage estimation results for the 4th, 6th and 8th graders can be found in the last two columns of Tables 2a through 2e. In each column, below the IV estimates for the school denomination indicators we include the results of our "rough" overidentification tests.[69] In addition, Hausman tests are reported to evaluate whether the corrected estimates differ significantly from those produced by the baseline equations.[70]

The "corrected" point estimates we find are quite startling. The first IV procedure for 4th grade (Table 2a) produces a significant Catholic school premium over public schools in arithmetic achievement of 9.3 percentiles, representing a 71.4% increase over the original baseline estimate! In contrast, the point estimate for language achievement drops but remains insignificant. As can be seen from our overidentification (Wald) test the religion variables are jointly equal to zero in both baseline equations suggesting they can therefore be legitimately excluded from the second-stage equation. Hausman tests do not reject the null hypothesis of exogeneity of the school denomination indicators. Alternatively speaking, the corrected IV estimates do not significantly differ from those produced by the corresponding baseline equations.

The selectivity-corrected results using the second two instrument sets are similar. When only availability is used to identify school choice (Table 2b) we find a Catholic school effect that is over twice as large as that of the baseline. However, the estimate is far less precise not significantly differing from zero. The corrected Catholic effect on language increases by more than a factor of 20 but remains insignificant. Using both the religion and school availability instruments (Table 2c) we find the estimated Catholic school arithmetic premium increases by 58.4% and remains significant at the 10%-level. The language point estimate increases slightly and becomes even less precise.[71] As to whether these estimates should be "trusted", inspection of the overidentification tests show that the validity of the first instrument set is marginally rejected for the arithmetic specification while instrument validity for the second is strongly rejected for both specifications. In addition, even if the instrument sets had passed our validity tests, the corrected point estimates are never deemed significantly different from the baseline effects per the Hausman tests of exogeneity.

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[68] However, the second-stage standard errors have not been corrected for the two-step procedure used. Because of this, all reported standard errors and subsequent p-values on inference tests should be recognized as lower bounds. As we will see, this makes little difference with regards to the interpretation of the final results.

[69] Although more stringent, results from the Sargan tests applied to the overidentified specifications (no less than three instruments describing two endogenous regressors) in Tables 2b through 2e are qualitatively similar to those of the "rough" overidentification tests reported on the tables.

[70] The Hausman tests are performed simply by including our instrumented endogenous regressors in the baseline equations and assessing their joint significance via F-tests.

[71] Note that, although not the focal point of this study, all but one of the corrected Protestant school point estimates for 4th grade also increase dramatically yet imprecision renders them insignificant across all specifications.

The second-stage results for the 6th grade differ from those of the younger students. Here we find the Catholic school effect on arithmetic achievement has decreased by 36% (from 7.6 to 4.8 percentiles) and become insignificant even at the 10%-level. Conversely, the expected effect of Catholic school attendance on language achievement has now increased by 70.6% (from 6.5 to 11.1 percentiles). Overidentification tests do not reject the null hypothesis that relative school availability has no direct influence on arithmetic achievement. However, with respect to language this hypothesis is marginally rejected (at the 10%-level). Despite the large changes in the point estimates, only the exogeneity of Catholic school participation with respect to language achievement is marginally rejected.

The corrected Catholic school effects for the 8th grade also rise dramatically. In arithmetic the selectivity-corrected premium to Catholic school increases by 54.5% (from 7.5 to 11.5 percentiles) while for language achievement the premium more than doubles (from 4.2 to 10.19 percentiles)! However, due to imprecision associated with the two-stage procedure the arithmetic estimate becomes insignificant while the language coefficient drops in significance to the 10%-level. Again, the overidentification tests ensure instrument validity in both equations yet Hausman tests suggest that endogeneity does not significantly bias the baseline estimates.

What can we gather from the resulting estimates? Let us limit our focus only to those specifications for which our instrumentation strategy is valid and momentarily downplay the results of the Hausman exogeneity tests. In this hypothetical world the results suggest that the uncorrected estimates of the Catholic school achievement effect on 4th grade arithmetic, 6th grade language and, 8th grade arithmetic and language are biased downward. This implies there may be *negative* selection into Catholic schools such that individuals enrolled in this sector perform worse (for the grade/achievement dimension combinations under scrutiny) than the average (observationally similar) individual in the sample would attending this sector.[72] Moreover, this evidence of negative selectivity into Catholic schools implies that they are even more productive than the previous baseline results would suggest.

In summary, disregarding the imprecision of our corrected estimates, the bulk of the evidence from this exercise points towards a possible downward bias in the uncorrected estimates of Catholic schooling.[73] Therefore, we conclude that selectivity bias cannot explain the Catholic school hypothesis.

6 Concluding remarks

With the current talk of educational reform taking place in many countries it is no wonder the widespread phenomenon of better performance by religious, and more precisely Catholic, schools in terms of scholastic achievement, educational attainment and measurable labor market outcomes (i.e. subsequent employment status and wages) has gained a great deal of attention. A lengthy literature survey provided earlier of both the foundation work and more

[72] At odds with this finding is the result pertaining to the corrected effect of Catholic school on 6th grade math achievement where the estimated premium has decreased and become insignificant suggesting an upward bias in the corresponding baseline effect implying *positive* selection into this sector.

[73] Nonetheless, it is important to note however that the support for negative or positive selection is not substantiated statistically as none of the tests of exogeneity for the Catholic school indicator is significant at the conventional 5%-level.

recent studies addressing the Catholic school hypothesis gives a good overview of the empirical knowledge that has been accumulated thus far and the theoretical issues that have arisen in gaining this knowledge. However, the bulk of this work has been done in the context of the US education system. Comparatively, little research directly addressing the validity of the Catholic school hypothesis as it pertains to the Netherlands has been performed despite evidence of a significant achievement premium to Dutch Catholic schools.

The preceding study explores this phenomenon for primary schooling in the Netherlands by first asking whether the Catholic school hypothesis is indeed credible in the context of the Dutch educational system. Should the hypothesis hold true so that Catholic schooling is a more effective form of primary education, then one might expect the educational practices of the schools in this sector to be accountable for any significant achievement premiums. In this sense the hypothesis really implies a beneficial supply-side effect of Catholic schools in the Dutch educational market. To this end, the first strategy of the study investigates the effects of a wide array of educational practices to ascertain whether they could confirm the Catholic school hypothesis. Note that an affirmation of the hypothesis via the educational practice strategy would conveniently lend counsel to policy formulation for reform by pinpointing which methods are most effective. However, this strategy uncovers only a partial explanation for the phenomenon. While controlling for teaching style, testing frequency and classroom composition with respect to student IQ causes a decrease in the estimated effect of Catholic schools, these practices fail to completely account for the apparent premium to this type of schooling. Furthermore, the decreases resulting from controlling for these educational practices is rarely statistically significant.

A second possible explanation for the phenomenon that Catholic schools produce better results stems from the demand side of the schooling market in which pupils are (non-randomly) selected by their parents into schools that lie in one of the three dominant educational sectors. Should the parents of children with an (unobservable) above-average capacity to excel academically more often enroll their kids in Catholic schools, then uncorrected estimation techniques will tend to attribute this unobserved component of achievement to the schooling effect itself causing an upward bias in its estimate. The second strategy employed therefore involves controlling for non-random selection through the estimation of a model that explicitly accounts for the schooling decision, effectively filtering out the potential bias on the sector-specific schooling effect caused by selectivity. The results of this exercise are quite surprising. Although the IV Catholic school effects never differ significantly from those produced by the baseline equations, the majority of the corrected point estimates become larger. These results suggest that the uncontrolled Catholic school effects may be biased by *negative* rather than *positive* selection and therefore selectivity cannot explain the Catholic school hypothesis, but rather reinforce this phenomenon.

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Appendix A Overview of selected empirical studies

Study	Description and estimation method	Controls/identification used	Main results	Conclusion (excerpts from studies)
Coleman, Hoffer and Kilgore (1982a)	Analysis of effect of Catholic school on changes in vocabulary, reading and mathematics achievement scores from sophomore to senior year using first wave (1980) of High School and Beyond dataset. Ordinary least squares	Family income, parental education, number of siblings, minority status, home literacy and educational influence proxies (i.e. books at home, parental thoughts on education, etc.), homework, attendance and behavior indicators.	• Achievement of juniors in Catholic schools (3rd year of secondary school) is approximately equal to that of seniors (4th year of secondary school) in the public sector. • Catholic schools are more beneficial to minorities and narrow the achievement gap with respect to race. • Catholic school policies (more homework, higher attendance, better behavior, etc.) account for superior results.	"The three types of analysis carried out in this paper provide strong evidence that there is, in vocabulary and mathematics, higher achievement for comparable students in Catholic and other-private schools than in public."
Noell (1982)	Reanalysis of Coleman et al (1982a) study using High School and Beyond dataset. Two-stage selectivity model with dichotomous first stage.	Same controls with indicators for gender, handicapped status, geographic region and 8th grade college attendance expectations. Use of Catholic religion indicator as identification restriction.	Estimated Catholic school effect on sophomore reading achievement are as follows: • Uncorrected OLS effect of 0.911 percentile points (0.0911 standard deviations). • Selectivity-corrected effect of 1.131 percentile points (1.131 standard deviations). Negligible effect of selectivity bias on scholastic achievement.	". . . the superior achievement of Catholic schools is not confirmed. . . Catholic school pupils are found to do no better-or worse-than public school pupils."
Sander and Krautman (1995)	Examines the effect of Catholic school on high school dropout rates and educational attainment using third follow-up (fourth wave) of High School and Beyond dataset. Two-stage selectivity model with dichotomous first stage/linear second stage and bivariate probit.	Family income, parental education, number of siblings, and minority status. Four urban-region interactions used as identification restrictions.	Estimated Catholic school effect on graduation probability (relative to public schools) are as follows: • Both corrected (bivariate) and uncorrected probit models show an expected increase in graduation rate of 10%. Negligible effect of selectivity bias resulting from self-selection. Estimated Catholic school effect on educational attainment are as follows: • Uncorrected single-equation model shows expected increase in schooling attainment of one-third of a year. • Selectivity-corrected two-step model shows insignificant effect on educational attainment. Significant upward bias on uncorrected Catholic school effect resulting from self-selection.	". . . Catholic high schools have a large negative effect on the high-school dropout rate. . . seniors in Catholic high schools are no more likely to acquire more schooling than seniors in public high schools if adjustments are made for selectivity and other background factors."

Appendix A Overview of selected empirical studies (continued)

Study	Description and estimation method	Controls/identification used	Main results	Conclusion (excerpts from studies)
Evans and Schwab (1995)	Analysis of effect of Catholic school on high school graduation and college enrollment using first three waves (1980, 1982, 1984) of the High School and Beyond dataset. Bivariate probit.	Essentially same variables as Coleman et al (1982a) plus controls for single-parent household, degree of religiosity and age at sophomore year of high school. Indicators of Catholic religion, Catholic church attendance, proportion of Catholic population in county, and their interactions used as identification restrictions.	Estimated Catholic school effect on graduation probability (relative to public schools) are as follows: • Uncorrected single-equation probit model shows an expected increase in high school graduation rate of 10 to 12% for "representative" individual attending Catholic versus public school. • Selectivity-corrected two-step model shows a significant 11 to 14% increase in expected graduation rate. Estimated Catholic school effect on college enrollment (relative to public schools) are as follows: • Uncorrected single-equation model shows expected increase in probability of entering a four-year college of 10 to 14%. • Corrected two-step model shows expected increase in probability of entering a four-year college of 7 to 24%. No evidence of significant bias on estimated graduation or college entrance probabilities caused by administrative screening of Catholic schools.	". . . teens enrolled in Catholic schools have a significantly higher probability of completing high school and starting college." A particularly strong Catholic school effect found for students in urban areas.
Sander (1995)	Examines the effect of eight years of consecutive attendance in Catholic school on vocabulary, reading, mathematics and science achievement scores using third follow-up (fourth wave) of High School and Beyond dataset. Two-stage selectivity model with dichotomous first stage/linear second stage.	Family income, parental education, geographic location, religion and urban indicators. Five religion-region interactions used as identification restrictions.	Uncorrected estimated effects of eight years of Catholic schooling on sophomore standardized test scores are as follows: • 0.75 out of 38 possible correct answers for mathematics. • 0.75 out of 21 possible correct answers for vocabulary. Selectivity-corrected estimated effects of eight years of Catholic schooling on sophomore standardized test scores are as follows: • 3.44 out of 38 possible correct answers for mathematics. • 2.48 out of 21 possible correct answers for vocabulary. • 2.04 out of 19 possible correct answers for reading.	". . . a positive Catholic grade school effect on the tenth grade test scores of non-Hispanic whites is driven by non-Catholics who attend Catholic grade schools. Thus, non-Hispanic white Catholics who attend Catholic grade schools (in other words, the vast majority of the Catholic grade school population) do not receive a superior education as measured by test scores."

Appendix A Overview of selected empirical studies (continued)

Study	Description and estimation method	Controls/identification used	Main results	Conclusion (excerpts from studies)
Goldhaber (1996)	Analyzes the effects of school type on achievement and of school quality on school choice using the National Educational Longitudinal Survey of 1988 (NELS88). Two-stage selectivity model with dichotomous first stage/linear second stage.	Family income, single parent home, parental education, parental interest in child's education and educational resources in home, prior day care attendance, amount of money saved for further education, gender, race, ethnicity, learning disability, repetition of school year, and controls for time-specific influences. Indicators of region and degree of urbanization used as identification restrictions.	• Families that are Catholic, Jewish, with higher incomes and more educational resources at home are more likely to send their kids to private schools. • Expected mathematics achievement gains from private (versus public) schools significantly influence private school enrollment.	"Clearly, the majority of the raw mean differentials between school sectors can be attributed to differences in the characteristics of students and schools rather than the returns to these characteristics."
Neal (1997)	Analysis of effect of Catholic school on high school and college graduation, and future wages using National Longitudinal Survey (1979). Two-stage selectivity model with dichotomous first stage/linear second stage and bivariate probit	Parental education and marital status, population measures. Catholic school availability measures (number of Catholics as proportion of total county population and number of Catholic schools per square mile) used as identification restrictions.	Estimated Catholic school effect on graduation probability (relative to public schools) are as follows: • Uncorrected single-equation probit model shows an expected increase in high school graduation rate of 10 and 26% for "representative" urban whites and minorities, respectively. • Corrected bivariate probit model shows an expected increase in high school graduation rate of 17 and 30% for "representative" urban whites and minorities, respectively. Expected wage increases attributable to Catholic school participation are approximately 8%.	"In the urban minority sample, Catholic schooling dramatically increases the probability of high school graduation. . . appears to increase college graduation rates. . . translate into future wage gains. . . In sum, these results do not indicate that Catholic schools are superior to public schools in general. . . they suggest that Catholic schools are similar in quality to suburban public schools, slightly better than the urban public schools that white students usually attend, and much better than the urban public schools that many minorities attend."

Appendix A Overview of selected empirical studies (continued)

Study	Description and estimation method	Controls used	Main results	Conclusion (excerpts from studies)
Vella (1999)	Analysis of Catholic school effect on high school graduation, college enrollment, and early labor market behavior using the Australian Longitudinal Survey (1985). Two-stage selectivity model using both dichotomous and linear first stage/linear second stage, bivariate probit and ordinal probit.	Parental education and marital status, number of siblings and gender. Indicators of Catholic religion and being Australian-born serve as identification restrictions.	Estimated Catholic school effect on graduation probability (relative to public schools) are as follows: • Uncorrected (probit) and corrected (bivariate probit and instrumental variables models) models show an average treatment effect of Catholic schooling on high school graduation probability of 18%. • The same models limited to Catholic sub-sample and only using Australian-born for identification shows an average treatment effect of Catholic schooling on high school graduation probability of 17%. Uncorrected single-equation ordinal probit model limited to Catholic sub-sample shows an average treatment effect of Catholic schooling on obtaining post-secondary education of 10%. Uncorrected single-equation probit model limited to Catholic sub-sample shows an average treatment effect of Catholic schooling on probability of being employed of 7%. Uncorrected OLS model limited to Catholic sub-sample shows a positive (0.008) but insignificant effect of Catholic schooling on logarithm of wages.	"This paper provides further evidence that attendance at Catholic school significantly enhances the probability of completing high school and obtaining tertiary education. . . We also find that individuals from Catholic schools are more likely to find employment and are paid higher wages. . ."

Appendix A Overview of selected empirical studies (continued)

Study	Description and estimation method	Controls used	Main results	Conclusion (excerpts from studies)
Figlio and Stone (2000)	Analysis of Catholic school effect on sophomore standardized mathematics test scores, high school graduation rate, two years of college attainment, two years of college attainment at a "selective" institution, and two years of college attainment where major was mathematics, science or engineering. Data used is a custom combination of the National Educational Longitudinal Survey of 1988 (NELS88) and privately produced data set collected by Dun and Bradstreet. Two-stage selectivity model with polychotomous first-stage/linear and dichotomous second stage (depending on outcome).	Socioeconomic status based on family income, parental education, and parental occupation, gender, race, Hispanic ethnicity, single parent home, religious denomination, logarithm of the eighth-grade mathematics test score, urbanization and region. Use of Catholic religion indicator and indicator of states with "right-to-work" and "duty to bargain" laws as identification restrictions.	Uncorrected single equation estimates show: • Negligible effect of private non-religious schools on sophomore standardized mathematics test scores and probability of high school graduation. • Positive effect of private religious and non-religious schools on probability of two years of college attainment, attendance at a "selective" institution, and following a major of mathematics, science or engineering. Corrected two-step estimates using religion as instrument show: • Positive effects of private religious and non-religious schools on all outcome measures. Corrected two-step estimates using state law-based instruments show: • Negative effects of private religious and non-religious schools on probability of high school graduation. • Positive effects of private religious and non-religious schools on probability of two years of college attainment and attendance at a "selective" institution.	"It is also widely believed that private schools are generally superior to public schools. We find some evidence to support this last belief, but not in the case of the academic outcomes that have been generally explored in the previous literature-test scores and high school completion probabilities. Instead we find positive private school treatment effects only regarding the probability of two years of college attendance and the probability of selective college attendance. Regarding the traditional measures of academic performance, only for a few distinct subgroups do we find that private schools outperform public schools in mathematics test performance. . ."

Appendix B Descriptive statistics

Grade 4- Baseline characteristics and instruments	Public					Protestant					Catholic				
Variables	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Percentile arithmetic score	966	49.734	28.639	1	96	840	48.563	27.765	1	96	1581	54.984	27.316	1	96
Percentile language score	966	56.361	27.392	1	100	840	52.782	28.394	1	100	1581	56.176	27.071	1	100
Class size	966	25.764	5.677	11	37	840	23.055	5.786	8	36	1581	25.755	5.666	8	37
# Similar students	966	5.784	3.892	0	19	840	4.699	3.288	0	17	1581	6.924	4.970	0	32
School enrollment	921	240.610	128.717	23	552	803	192.009	81.625	27	425	1534	246.024	104.747	60	508
School enrollment missing	966	0.047	0.211	0	1	840	0.044	0.205	0	1	1581	0.030	0.170	0	1
School average weight factor	966	1.189	0.128	1	1.567	840	1.216	0.195	1	1.88	1581	1.195	0.157	1	1.78
Weight factor = 1.00	966	0.565	0.496	0	1	840	0.492	0.500	0	1	1581	0.531	0.499	0	1
Weight factor = 1.25	966	0.334	0.472	0	1	840	0.354	0.478	0	1	1581	0.359	0.480	0	1
Weight factor = 1.40	966	0.000	0.000	0	0	840	0.010	0.097	0	1	1581	0.006	0.075	0	1
Weight factor = 1.70	966	0.006	0.079	0	1	840	0.001	0.035	0	1	1581	0.004	0.062	0	1
Weight factor = 1.90	966	0.094	0.292	0	1	840	0.144	0.351	0	1	1581	0.101	0.301	0	1
Gender (1=female)	966	0.503	0.500	0	1	840	0.510	0.500	0	1	1581	0.485	0.500	0	1
Student age in months	966	90.888	5.057	61	123	838	91.154	5.252	57	128	1579	90.775	5.416	57	142
Student age missing	966	0.000	0.000	0	0	840	0.002	0.049	0	1	1581	0.001	0.036	0	1
Repeat grade indicator	966	0.138	0.345	0	1	837	0.157	0.364	0	1	1580	0.135	0.342	0	1
Repeat grade indicator missing	966	0.000	0.000	0	0	840	0.004	0.060	0	1	1581	0.001	0.025	0	1
Percent of class female	966	0.500	0.110	0.2	1	840	0.510	0.137	0	1	1581	0.492	0.109	0	0.82
Class average weight factor	966	1.172	0.129	1	1.6	840	1.220	0.201	1	1.87	1581	1.184	0.154	1	1.67
Years of teacher experience	966	17.133	8.579	2	37	840	15.794	7.741	2	34	1581	17.853	8.111	1	35
Dual teacher class	966	0.420	0.494	0	1	840	0.238	0.426	0	1	1581	0.440	0.497	0	1
Gender of teacher (1=female)	966	0.834	0.372	0	1	840	0.938	0.241	0	1	1581	0.824	0.381	0	1
Multi-grade class	966	0.243	0.429	0	1	840	0.315	0.465	0	1	1581	0.157	0.364	0	1
Public school	966	1.000	0.000	1	1	840	0.000	0.000	0	0	1581	0.000	0.000	0	0
Protestant school	966	0.000	0.000	0	0	840	1.000	0.000	1	1	1581	0.000	0.000	0	0
Catholic school	966	0.000	0.000	0	0	840	0.000	0.000	0	0	1581	1.000	0.000	1	1
Mother maximum LO (primary)	788	0.126	0.332	0	1	610	0.130	0.336	0	1	1270	0.103	0.304	0	1
Mother LBO (secondary vocational)	788	0.197	0.398	0	1	610	0.246	0.431	0	1	1270	0.194	0.396	0	1
Mother MAVO (lower secondary general)	788	0.203	0.403	0	1	610	0.205	0.404	0	1	1270	0.190	0.392	0	1
Mother MBO (intermediate vocational)	788	0.173	0.378	0	1	610	0.175	0.381	0	1	1270	0.213	0.410	0	1
Mother HAVO/VWO (upper secondary vocational/general)	788	0.133	0.340	0	1	610	0.111	0.315	0	1	1270	0.133	0.340	0	1
Mother HBO (higher vocational)	788	0.142	0.349	0	1	610	0.121	0.327	0	1	1270	0.139	0.346	0	1
Mother WO (higher university)	788	0.027	0.161	0	1	610	0.011	0.107	0	1	1270	0.028	0.164	0	1
Father maximum LO (primary)	741	0.127	0.333	0	1	584	0.127	0.333	0	1	1198	0.125	0.331	0	1
Father LBO (secondary vocational)	741	0.185	0.388	0	1	584	0.231	0.422	0	1	1198	0.170	0.376	0	1
Father MAVO (lower secondary general)	741	0.127	0.333	0	1	584	0.125	0.331	0	1	1198	0.129	0.336	0	1
Father MBO (intermediate vocational)	741	0.174	0.379	0	1	584	0.223	0.416	0	1	1198	0.220	0.414	0	1
Father HAVO/VWO (upper secondary vocational/general)	741	0.124	0.330	0	1	584	0.098	0.297	0	1	1198	0.104	0.306	0	1
Father HBO (higher vocational)	741	0.179	0.384	0	1	584	0.156	0.363	0	1	1198	0.166	0.372	0	1
Father WO (higher university)	741	0.084	0.277	0	1	584	0.041	0.199	0	1	1198	0.085	0.279	0	1
Mother education missing	966	0.184	0.388	0	1	840	0.274	0.446	0	1	1581	0.197	0.398	0	1
Father education missing	966	0.233	0.423	0	1	840	0.305	0.461	0	1	1581	0.242	0.429	0	1

Appendix B Descriptive statistics (continued)

Grade 4- Baseline characteristics and instruments	Public					Protestant					Catholic				
Variables	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Student Catholic	966	0.108	0.310	0	1	840	0.046	0.211	0	1	1581	0.497	0.500	0	1
Student Protestant	966	0.062	0.241	0	1	840	0.360	0.480	0	1	1581	0.011	0.106	0	1
Mother Catholic	966	0.165	0.371	0	1	840	0.081	0.273	0	1	1581	0.588	0.492	0	1
Father Catholic	966	0.156	0.363	0	1	840	0.080	0.271	0	1	1581	0.533	0.499	0	1
Mother Protestant	966	0.104	0.305	0	1	840	0.430	0.495	0	1	1581	0.038	0.191	0	1
Father Protestant	966	0.093	0.291	0	1	840	0.386	0.487	0	1	1581	0.033	0.178	0	1
Public schools per sq. km. in gemeente	966	0.223	0.198	0.02	0.84	840	0.227	0.221	0	0.77	1581	0.210	0.233	0	0.84
Protestant schools per sq. km. in gemeente	966	0.194	0.164	0.01	0.611	840	0.203	0.199	0	1.06	1581	0.147	0.182	0	0.61
Catholic schools per sq. km. in gemeente	966	0.155	0.146	0.01	0.473	840	0.143	0.140	0	0.49	1581	0.273	0.161	0	0.65
Ratio of public to other schools in gemeente	966	0.685	0.385	0.11	2.33	840	0.623	0.336	0.06	1.71	1581	0.432	0.288	0.06	1.33
Ratio of Protestant to other schools in gemeente	966	0.640	0.537	0.05	3.00	840	0.887	0.806	0.07	3.50	1581	0.333	0.435	0.04	3.50
Ratio of Catholic to other schools in gemeente	966	0.859	1.561	0.04	5.67	840	0.431	0.540	0.04	2.50	1581	1.639	1.358	0.05	5.67

Appendix B Descriptive statistics (continued)

Grade 6 – Baseline characteristics and instruments	Public					Protestant					Catholic				
Variables	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Percentile arithmetic score	870	52.002	28.347	1	100	864	52.060	28.458	1	100	1356	56.813	27.239	1	100
Percentile language score	870	54.569	28.588	1	100	864	55.716	27.083	1	100	1356	57.564	27.551	1	100
Class size	870	26.099	5.634	9	36	864	28.071	5.763	8	37	1356	26.268	5.815	12	38
# Similar students	870	6.253	4.326	0	19	864	5.936	4.041	0	16	1356	6.448	4.454	0	22
School enrollment	830	259.027	135.362	23	552	791	220.948	101.312	27	530	1314	240.341	100.420	60	508
School enrollment missing	870	0.046	0.210	0	1	864	0.084	0.278	0	1	1356	0.031	0.173	0	1
School average weight factor	870	1.177	0.119	1	1.57	864	1.179	0.161	1	1.88	1356	1.217	0.180	1	1.881
Weight factor = 1.00	870	0.547	0.498	0	1	864	0.535	0.499	0	1	1356	0.478	0.500	0	1
Weight factor = 1.25	870	0.338	0.473	0	1	864	0.367	0.482	0	1	1356	0.392	0.488	0	1
Weight factor = 1.40	870	0.001	0.034	0	1	864	0.012	0.107	0	1	1356	0.001	0.038	0	1
Weight factor = 1.70	870	0.003	0.059	0	1	864	0.001	0.034	0	1	1356	0.003	0.054	0	1
Weight factor = 1.90	870	0.110	0.313	0	1	864	0.086	0.280	0	1	1356	0.126	0.332	0	1
Gender (1=female)	870	0.500	0.500	0	1	864	0.486	0.500	0	1	1356	0.487	0.500	0	1
Student age in months	860	115.391	5.846	55	141	847	115.226	5.344	105	136	1338	115.635	5.601	99	148
Student age missing	870	0.011	0.107	0	1	864	0.020	0.139	0	1	1356	0.013	0.114	0	1
Repeat grade indicator	862	0.173	0.378	0	1	847	0.158	0.365	0	1	1338	0.191	0.393	0	1
Repeat grade indicator missing	870	0.009	0.096	0	1	864	0.020	0.139	0	1	1356	0.013	0.114	0	1
Percent of class female	870	0.503	0.123	0	1	864	0.489	0.136	0	1	1356	0.488	0.109	0	1
Class average weight factor	870	1.185	0.145	1	1.9	864	1.174	0.165	1	1.88	1356	1.216	0.190	1	1.9
Years of teacher experience	870	19.991	8.916	2	38	864	14.875	9.035	1	35	1356	17.994	8.191	1	36
Dual teacher class	870	0.425	0.495	0	1	864	0.566	0.496	0	1	1356	0.290	0.454	0	1
Gender of teacher (1=female)	870	0.407	0.492	0	1	864	0.531	0.499	0	1	1356	0.502	0.500	0	1
Multi-grade class	870	0.347	0.476	0	1	864	0.424	0.494	0	1	1356	0.319	0.466	0	1
Public school	870	1.000	0.000	1	1	864	0.000	0.000	0	0	1356	0.000	0.000	0	0
Protestant school	870	0.000	0.000	0	0	864	1.000	0.000	1	1	1356	0.000	0.000	0	0
Catholic school	870	0.000	0.000	0	0	864	0.000	0.000	0	0	1356	1.000	0.000	1	1
Mother maximum LO (primary)	673	0.095	0.294	0	1	698	0.080	0.272	0	1	1216	0.137	0.343	0	1
Mother LBO (secondary vocational)	673	0.392	0.489	0	1	698	0.474	0.500	0	1	1216	0.426	0.495	0	1
Mother MAVO or MBO (lower secondary general or intermediate vocational)	673	0.340	0.474	0	1	698	0.328	0.470	0	1	1216	0.336	0.472	0	1
Mother HBO or WO (higher vocational or higher university)	673	0.172	0.378	0	1	698	0.117	0.322	0	1	1216	0.102	0.303	0	1
Father maximum LO (primary)	717	0.068	0.253	0	1	713	0.073	0.260	0	1	1215	0.128	0.335	0	1
Father LBO (secondary vocational)	717	0.402	0.491	0	1	713	0.445	0.497	0	1	1215	0.413	0.493	0	1
Father MAVO or MBO (lower secondary general or intermediate vocational)	717	0.265	0.442	0	1	713	0.293	0.456	0	1	1215	0.286	0.452	0	1
Father HBO or WO (higher vocational or higher university)	717	0.265	0.442	0	1	713	0.189	0.392	0	1	1215	0.172	0.378	0	1
Mother education missing	870	0.226	0.419	0	1	864	0.192	0.394	0	1	1356	0.103	0.304	0	1
Father education missing	870	0.176	0.381	0	1	864	0.175	0.380	0	1	1356	0.104	0.305	0	1
Public schools per sq. km. in gemeente	870	0.229	0.200	0.016	0.84	864	0.191	0.196	0.01	0.77	1356	0.188	0.197	0	0.84
Protestant schools per sq. km. in gemeente	870	0.214	0.189	0.012	0.82	864	0.221	0.244	0.02	1.06	1356	0.127	0.159	0	0.611
Catholic schools per sq. km. in gemeente	870	0.160	0.144	0.006	0.47	864	0.107	0.117	0.01	0.49	1356	0.273	0.163	0	0.645
Ratio of public to other schools in gemeente	870	0.667	0.374	0.11	2.33	864	0.593	0.344	0.06	1.71	1356	0.444	0.311	0.06	1.55
Ratio of Protestant to other schools in gemeente	870	0.648	0.520	0.05	3.00	864	1.078	0.842	0.07	3.50	1356	0.327	0.470	0.04	3.50
Ratio of Catholic to other schools in gemeente	870	0.845	1.537	0.04	5.67	864	0.320	0.422	0.04	2.50	1356	1.671	1.393	0.05	5.67

Appendix B Descriptive statistics (continued)

Grade 8 – Baseline characteristics and instruments			Public					Protestant					Catholic		
Variables	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Percentile arithmetic score	982	51.675	27.783	1	100	894	52.092	28.125	1	100	1452	59.072	26.674	1	100
Percentile language score	982	55.198	27.184	1	100	894	54.669	26.695	1	100	1452	58.949	26.513	1	100
Class size	982	27.339	6.746	9	39	894	27.016	5.681	12	37	1452	26.212	5.872	9	39
# Similar students	982	6.711	4.874	0	20	894	5.913	4.232	0	20	1452	7.523	5.566	0	32
School enrollment	946	239.625	123.205	30	552	844	208.619	113.240	27	530	1416	262.949	130.603	60	625
School enrollment missing	982	0.037	0.188	0	1	894	0.056	0.230	0	1	1452	0.025	0.156	0	1
School average weight factor	982	1.178	0.141	1	1.778	894	1.184	0.164	1	1.881	1452	1.197	0.157	1	1.88
Weight factor = 1.00	982	0.541	0.499	0	1	894	0.517	0.500	0	1	1452	0.492	0.500	0	1
Weight factor = 1.25	982	0.349	0.477	0	1	894	0.381	0.486	0	1	1452	0.402	0.490	0	1
Weight factor = 1.40	982	0.001	0.032	0	1	894	0.009	0.094	0	1	1452	0.005	0.069	0	1
Weight factor = 1.70	982	0.005	0.071	0	1	894	0.000	0.000	0	0	1452	0.005	0.069	0	1
Weight factor = 1.90	982	0.104	0.305	0	1	894	0.093	0.290	0	1	1452	0.096	0.295	0	1
Gender (1=female)	982	0.516	0.500	0	1	894	0.525	0.500	0	1	1452	0.513	0.500	0	1
Student age in months	979	139.149	5.661	100	163	838	139.249	5.596	87	158	1431	139.784	5.856	113	170
Student age missing	982	0.003	0.055	0	1	894	0.063	0.242	0	1	1452	0.014	0.119	0	1
Repeat grade indicator	980	0.174	0.380	0	1	838	0.171	0.376	0	1	1431	0.194	0.396	0	1
Repeat grade indicator missing	982	0.002	0.045	0	1	894	0.063	0.242	0	1	1452	0.014	0.119	0	1
Percent of class female	982	0.516	0.130	0	0.778	894	0.521	0.137	0	1	1452	0.514	0.120	0.1	0.92
Class average weight factor	982	1.187	0.163	1	1.789	894	1.180	0.164	1	1.9	1452	1.193	0.155	1	1.82
Years of teacher experience	982	18.097	6.971	2	33	894	18.961	7.416	2	33	1452	19.804	7.970	1	40
Dual teacher class	982	0.357	0.479	0	1	894	0.413	0.493	0	1	1452	0.292	0.455	0	1
Gender of teacher (1=female)	982	0.229	0.420	0	1	894	0.332	0.471	0	1	1452	0.318	0.466	0	1
Multi-grade class	982	0.467	0.499	0	1	894	0.493	0.500	0	1	1452	0.311	0.463	0	1
Public school	982	1.000	0.000	1	1	894	0.000	0.000	0	0	1452	0.000	0.000	0	0
Protestant school	982	0.000	0.000	0	0	894	1.000	0.000	1	1	1452	0.000	0.000	0	0
Catholic school	982	0.000	0.000	0	0	894	0.000	0.000	0	0	1452	1.000	0.000	1	1
Mother maximum LO (primary)	777	0.104	0.306	0	1	757	0.098	0.297	0	1	1324	0.114	0.318	0	1
Mother LBO (secondary vocational)	777	0.386	0.487	0	1	757	0.460	0.499	0	1	1324	0.456	0.498	0	1
Mother MAVO or MBO (lower secondary general or intermediate vocational)	777	0.346	0.476	0	1	757	0.328	0.470	0	1	1324	0.309	0.462	0	1
Mother HBO or WO (higher vocational or higher university)	777	0.163	0.370	0	1	757	0.115	0.319	0	1	1324	0.121	0.326	0	1
Father maximum LO (primary)	815	0.080	0.271	0	1	746	0.094	0.292	0	1	1322	0.107	0.309	0	1
Father LBO (secondary vocational)	815	0.399	0.490	0	1	746	0.422	0.494	0	1	1322	0.419	0.494	0	1
Father MAVO or MBO (lower secondary general or intermediate vocational)	815	0.283	0.451	0	1	746	0.298	0.458	0	1	1322	0.286	0.452	0	1
Father HBO or WO (higher vocational or higher university)	815	0.238	0.426	0	1	746	0.186	0.390	0	1	1322	0.188	0.391	0	1
Mother education missing	982	0.209	0.407	0	1	894	0.153	0.360	0	1	1452	0.088	0.284	0	1
Father education missing	982	0.170	0.376	0	1	894	0.166	0.372	0	1	1452	0.090	0.286	0	1
Public schools per sq. km. in gemeente	982	0.228	0.212	0.016	0.84	894	0.205	0.207	0.01	0.765	1452	0.185	0.217	0	0.84
Protestant schools per sq. km. in gemeente	982	0.191	0.176	0.007	0.816	894	0.181	0.190	0.02	1.061	1452	0.133	0.185	0	1.06
Catholic schools per sq. km. in gemeente	982	0.166	0.157	0.006	0.639	894	0.121	0.131	0.01	0.493	1452	0.271	0.167	0	0.65
Ratio of public to other schools in gemeente	982	0.689	0.406	0.11	2.33	894	0.652	0.386	0.06	1.71	1452	0.409	0.285	0.06	1.55
Ratio of Protestant to other schools in gemeente	982	0.600	0.481	0.04	3.00	894	0.971	0.880	0.07	3.50	1452	0.332	0.485	0.04	3.50
Ratio of Catholic to other schools in gemeente	982	0.858	1.457	0.04	5.67	894	0.401	0.554	0.03	2.50	1452	1.735	1.342	0.05	5.67