PRELIMINARY
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Private schools and student achievement

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Abstract

We use population data encompassing students enrolled in both private and public primary schools to investigate the relative effectiveness of private schools, and to investigate the distribution of high achieving students across public and private schools. We find that private schools on average outperform public schools by between 0.10 and 0.25 standard deviations on standardized reading and numeracy tests. Effect sizes are similar for private "prep" schools, Catholic schools and non-Christian faith schools. Other (non-Catholic) Christian private schools are no more successful than public schools on average with respect to test scores. School effectiveness varies substantially across private schools, and about one-third of private schools are not as effective as the average public school. Some low-achieving prep schools give the appearance of academic quality by enrolling high ability students. Many low quality faith schools attract students despite producing low test scores on average. School quality varies substantially among "elite" private schools that enroll high ability students.

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1 INTRODUCTION

The provision of public funding to private schools remains a hotly contested issue. Voucher advocates have long argued that private schools can fill an important niche in the education market by offering diverse approaches to education, and can operate more effectively than public schools. In contrast to public schools, which they claim are hamstrung by top-down management structures that limit their discretion to innovate, strong union rules that prevent them from disciplining or dismissing poor teachers, and the challenges of serving a diverse student body, private schools can offer innovative programs, engender effective school cultures through strong ties to communities that share common values, dismiss teachers who underperform, and apply selective enrolment rules to leverage peer effects and generate complementarities among students. With greater access to private resources, private schools can attract better or more experienced teachers, and offer smaller classes and programs that are specially designed to serve their students.

However, offering a high quality academic environment is only one of several strategies that private schools can use to attract tuition paying students. If school quality is not directly observable, some schools may be able to establish a reputation for quality by selecting high ability students rather than by providing superior learning environments (MacLeod and Urquiola 2015). Other schools may choose to offer non-academic amenities such as religious education or a desirable peer group, or may inflate grades and provide easy access to credentials (Brunello and Rocco 2008).

Evidence from several advanced economies suggests that private schools on average do not deliver superior academic performance as measured by standardized test scores.[1] The most credible research to date, based on evaluations of small-scale U.S. voucher programs that allocate vouchers via lottery, finds little if any effect of private schools on test scores (with the possible exception of small benefits for African-Americans students in some programs), despite evidence of positive and significant effects on high school

[1] Evidence of the relative performance of private versus public schools in developing economies is generally favorable (e.g. Andrabi et al. 2011; Alderman et al. 2001; Angrist 2002, 2006; Muralidharan and Sundararaman 2015; Singh 2015).

graduation (see Epple et al. 2015a for a review). Abdulkadiroglu et al. (forthcoming 2017) find sizeable *negative* effects of the Louisiana Scholarship Program on test scores. Estimates of effects on the achievement of the *average* student at a broader set of private schools tend to resemble those for the typically low-income group of voucher recipients at schools that accept them. The more recent studies of effects for the average student finds either no test score effect or a small *negative* effect associated with private Catholic high schools (Altonji et al. 2005) and private faith primary schools (Elder and Jepsen 2014; Nghiem et al. 2015). Estimates of test score effects for private secular primary schools are more promising (Nghiem et al. 2015).

This paper is the first to use student level population data to study the effects of private schools on educational outcomes. Our longitudinal records from fourteen school districts in British Columbia (B.C.), Canada, follow five cohorts of students from grade 4 (aged 8-9) to grade 7, and include basic demographic characteristics, residential postal code and school attended in each year, along with results from centrally graded standardized tests administered in grades 4 and 7 in reading and numeracy. B.C. provides a little-known but interesting institutional environment in which to study private school outcomes. Since 1977, the provincial government has provided universal vouchers, typically worth half of the basic allocation provided for each student attending a private school, and all but a handful of private schools receive this voucher. B.C. voucher schools have access to a wide range of strategies for remaining competitive with public schools while charging tuition fees. They are fully autonomous with respect to personnel decisions, pay and school calendar, they can hire, fire and remunerate teachers subject only to provincial labor standards legislation and they can set tuition fees at any level they choose. Some schools may use this autonomy to provide an environment that attracts parents seeking academic quality. B.C. voucher schools are not restricted in their admissions policy so long as they do violate the Canadian Charter of Rights and Freedoms or the provincial Human Rights Code. If spillovers are important, some schools may choose to apply selective admissions criteria to achieve academic quality via peer effects. Since public information about school-level performance on standardized tests is widely disseminated in B.C., some schools may be able to falsely establish or enhance their reputations for

academic quality by "cream-skimming" high ability students. Finally, unlike public schools, B.C. voucher schools may provide a faith-based learning environment and offer religious instruction. Some schools may choose to compete by providing faith-based education in addition to or instead of enhancing academic quality.

We begin our investigation by using our population data to provide new and precise estimates of the quality of Catholic schools, and to expand these estimates to include other types of faith private schools and secular "prep" schools. While previous studies have used an extensive set of individual characteristics along with lagged test scores to control for student ability, our large and geographically dense data allow us to add neighborhood effects measured in geographic units that include a maximum of 19 households.[2] Together, our controls account for about 70% of the variation in individual test scores, and do a good job of addressing selection into private schools according to the metric proposed by Altonji et al. (2005), giving us considerable confidence in the credibility of our estimates.

We find that the average private school outperforms the average public school with respect to literacy and numeracy skill development, with an effect size of about 0.19 standard deviations. This overall average masks substantial variation across types of private schools. In contrast to previous work, we find that Catholic private schools on average outperform public schools by 0.20 standard deviations in reading and 0.30 standard deviations in numeracy. Non-Christian (Sikh, Jewish, Muslim) faith private schools and secular "prep" private schools are similar, with an effect size of about 0.24 standard deviations in both reading and numeracy. Private schools associated with non-Catholic Christian denominations prove to be the exception to the rule – these schools on average do no better (and no worse) than public schools.

[2] Our data include more than seven times the number of students enrolled in private Catholic schools compared to the data used by Nghiem et al. (2015) and Elder and Jepsen (2014), and more than nine times the number of students enrolled in private secular schools compared to the data used by Nghiem et al. (2015) and Lefebvre et al. (2011). McKewan (2001) has a larger sample of private Catholic school students in his data from Chile, but only one test score per student. Other jurisdictions that provide researchers with access to population-based longitudinal student-level data typically do not include records for students enrolled in private schools (e.g. Florida, England, North Carolina and Texas).

We next break new ground by estimating a full set of individual public and private school fixed effects. Based on approximately 150 student-level observations per school on average, these estimates are sufficiently precise to allow us to characterize the full range of heterogeneity in private and public school effects. We find that the range of school effects *within* private school types is substantial. Almost half of private schools, many of which have been in operation for decades, appear to be less effective than the average public school in numeracy, and about a third of private schools are less effective in reading. We use our estimates to shed light on why students may enroll in these low-quality private schools. One possibility that has been discussed in the literature (e.g. Martinez-Mora 2006) is that these schools, while below average, may be better than students' *local* public school alternatives. We find that this is not in general the case; most students enrolled in low quality private schools have guaranteed access to a higher quality public school. However, the school mean test scores of low-quality prep schools are almost always higher than the school mean test scores of the guaranteed public school. This pattern suggests that some parents may be misled about the relative quality of their prep school by public information about school mean test scores that reflect above average student quality, while masking below average school quality. In the case of students attending low quality faith schools, we find that in many cases these schools also earn school mean test scores that are both below average and below mean test scores of guaranteed public alternatives. These results suggest, perhaps unsurprisingly, that a substantial share of parents who enroll their children in faith private schools trade off academic quality for faith-based education.

We then estimate a model that provides us with a full set of estimates of both school and student effects. The theoretical literature on private school vouchers includes a range of models that incorporate various potential features of voucher systems in a variety of educational environments (see Epple et al. 2015 for a comprehensive review). One prediction that emerges from virtually all these models is that private schools will draw off the most highly motivated and/or highest income students from the public sector. This "cream-skimming" raises concerns because, combined with peer effects, it implies that

some students left behind in public schools may be made worse off when vouchers are introduced. We use our estimates of student fixed effects to investigate the extent and magnitude of cream-skimming by private schools. We find that prep school students on average are substantially higher "ability" than the average student. However, when we compare prep school students to the students who live in the same neighborhoods, where the impact of cream-skimming is most likely to be felt, the cream-skimming effect of prep schools is substantially diminished. Students who attend private Catholic and other faith schools are also positively selected, and tend to live in average neighborhoods. Other Christian private schools admit students who are fairly close to average, so these schools have little if any cream-skimming effect on public schools.

Finally, we note several interesting features of the covariance between private school effects and school-mean student effects. Interestingly, most private schools with a below-average school effect in reading serve above-average students, as do about half of those that are below average in numeracy. These schools, which include some that would be considered fairly elite, appear to be using selective admissions policies to maintain strong academic reputations, while providing relatively low quality education. These results are consistent with well identified empirical evidence that selective schools are only sometimes more effective (e.g. Clark 2010; Abdulkadiroglu et al. 2014; Pop-Eleches and Urquiola 2013). In the case of high quality private schools, our methodology does not allow us to distinguish between the contribution of peer effects versus school inputs to school quality. However, we find that a number of high-quality schools serve low ability students. In these cases, we can rule out peer effects as the source of the school's superior performance.

2 INSTITUTIONAL CONTEXT

2.1 Public school choice and funding

Most public primary schools in British Columbia offer Kindergarten through grade 7, with high schools offering grades 8 through 12. There are many exceptions, however, with some schools offering Kindergarten through grades 3, 4, 5 or 6, some middle schools that begin in grade 6 and some "junior" high schools that begin in grade 7.

Students in B.C. are guaranteed access to a neighorhood "catchment" public school based on their residential address. They may also choose to enroll in a neighborhood public school other than their guaranteed school. Before July 2002, the provincial education authority (the Ministry of Education) mandated that out-of-catchment enrollment in a regular (non-magnet) public school required permission of the principals of both the guaranteed school and the preferred school. Since July 2002, students have been free to enroll in any neighborhood school in the province that has space and facilities available after students who reside in the catchment area have enrolled. Transportation to non-guaranteed neighborhood schools is not provided. When neighborhood schools are over-subscribed, provincial legislation requires that school boards give priority to students who reside within the district. Boards may elect to give priority to siblings of children who are already enrolled. Within these enrolment categories, principals of neighborhood schools have discretion over which students to enroll.

Parents in B.C. may also choose to enroll their children in a public magnet program. The most popular form of magnet program is French Immersion, which enrolls about 10 percent of Kindergarten students in the province (BC Ministry of Education 2011). Entry into French Immersion programs is restricted to students entering Kindergarten or grade 1, and space is often allocated by lottery.

The B.C. Ministry of Education provides operating and capital funding directly to public districts. Operating funds are provided in proportion to total district enrolment, with supplementary funding for each student who is Aboriginal, gifted or disabled, or who qualifies for English as a Second Language (ESL)[3] instruction. Public districts and schools are not authorized to raise any additional revenue, and are required to offer the provincial curriculum. Hiring, firing and remuneration of teachers is governed by strict rules specified in a collective agreement between the Province and the powerful union that represents B.C. teachers.

[3] The ESL program was renamed English Language Learning (ELL) in January 2012.

2.2 Private school choice and funding

Since 1977, British Columbia has provided universal vouchers to private schools that conform to provincial curriculum standards and meet various provincial administrative requirements (B.C. Federation of Independent Schools Associations 2015). [4] B.C. provides 50 percent of the per student public school grant to private schools whose operating costs are no higher than in the public system, and 35 percent to those whose operating costs are higher (B.C. Ministry of Education 2005). The Ministry of Education does not limit the total number of vouchers, and private schools are not constrained in their selection of students. The formula for supplementary funding for special education students in private schools changed in 2005. Private schools had historically received half as much per student special education funding as public schools; since 2005, private schools have received the full value of the public school special education supplement.

To be eligible for voucher funding, private schools must operate on a not-for-profit basis, offer the provincial curriculum, hire qualified B.C. teachers and participate in standardized testing programs. Unlike public schools, private schools may provide a faith-based learning environment and offer religious instruction. They may charge any amount of tuition, apply any admissions criteria that do not violate the Canadian Charter of Rights and Freedoms or the provincial Human Rights Code, and can hire, fire and remunerate teachers subject only to provincial labor standards legislation. Private faith schools in B.C.'s Lower Mainland serve a variety of religious communities, including Catholics, Protestants, Sikhs, Jews and Muslims. Secular schools include "prep schools" that are focused on academic excellence and university preparation, and a small number that offer Montessori or Waldorf programs, or specialized education for students with special learning needs. Tuition fees range widely, from several thousand dollars at some Waldorf schools to $20,000 or more at top-ranked prep schools. Private schools are also supported through donations from individuals and from supporting foundations and organizations. In the case of Catholic schools, for example, initial building costs are

[4] The term "independent school" has been widely used in BC province to refer to private schools that receive public funding. We use both terms interchangeably.

subsidized by both the Diocese and the local parish, with the parish contributing funds towards other capital and operating costs (Catholic Independent Schools, 2017).

2.3 Testing and accountability

All public and provincially funded private schools in British Columbia are required to administer standardized tests to students in grades 4 and 7 in reading and numeracy each year. A centralized grading system ensures that a consistent standard is applied across schools.[5] These scores do not contribute to students' academic records and play no role in grade completion, and there are no financial incentives for teachers or schools related to student performance. The Ministry of Education began posting school-average test scores on their website in 2001 (B.C. Ministry of Education 2001). The Fraser Institute, an independent research and educational organization (Fraser Institute 2008), began issuing annual "report cards" on B.C.'s elementary schools in June 2003 (Cowley and Easton 2003). These reports include school scores and rankings based on test scores. From the outset, the school report cards have received widespread media coverage in the province's print, radio and television media.

3 DATA

Our estimates are based on extracts from two administrative databases collected and maintained by the B.C. Ministry of Education. The first is an enrolment database that records the school at which each student is enrolled on September 30 of each year. Our extract includes five cohorts of grade 4 students who were enrolled in a public or private school located within the geographic boundaries of the fourteen school districts in the Lower Mainland of B.C.[6] in 1999/2000 through 2003/2004, and follows them for the following four years. Students remain in our data so long as they remain within the provincial public or private school system in this geographic region. The individual records include indicators for the language spoken in the student's home (English, Chinese, Punjabi, and other), whether the student self-identified as Aboriginal in any

[5] Hinnerich and Vlachos (2016) show that internally graded exam scores are inflated by 0.14 standard deviations on average by Swedish upper secondary voucher schools relative to those of municipal schools.
[6] The Lower Mainland consists of the city of Vancouver and its suburbs. It is geographically isolated by the Canada/U.S. border to the south, rugged mountains to the east and north, and the Salish Sea to the west.

year, whether the student was registered in ESL or special education (i.e. a gifted or disabled program), whether the student was enrolled in French Immersion, whether the school is public or private, and the student's gender. In addition, the extract provides the student's residential postal code and unique student, school and district identifiers. We attach average family income, proportion of immigrant families, and proportion of people with different levels of education in the student's Census neighborhood (enumeration area), based on a postal code match. An enumeration area is the smallest geographic area for which public-use Census data are produced, and typically comprises several hundred households. A detailed description of our procedures for locating residential postal codes within enumeration areas is provided in a data appendix.

The second database provides student-level data on participation and scores on standardized tests administered in grades 4 and 7 for the 1999/2000-2006/2007 school years. We merge students' test scores with the enrolment database via the unique student identifier provided in both files. We normalize valid FSA scores in reading and numeracy to have a mean of zero and standard deviation of one in each year.

4 METHODOLOY

4.1 Estimating school effects

We follow the recent literature and estimate a basic model of seventh grade test scores that includes a simple indicator for whether a student attends a private school along with several sets of controls, including a lagged test score. Our baseline model is:

$$y_i = \alpha Private_i + \lambda y_{i4} + X_i'\beta_1 + PC_i + year_{t(i)} + \varepsilon_i \qquad (1)$$

where y_i is a test score for student i, $Private_i$ is an indicator that she attends a private school, y_{i4} is her lagged grade 4 test score, X_i is a vector of individual student characteristics (home language, Aboriginal identity and gender), PC_i is an indicator of her residential postal, $year_{t(i)}$ is a year or cohort effect, ε_i is a stochastic error, and all variables correspond to the student's grade 7 year unless otherwise specified. The coefficient of interest, α, is the conditional mean difference in test scores between

students enrolled in private and public schools. OLS estimation of (1) provides a difference-in-differences estimate of the average effect of attending a private school in grade 7 on student test scores. This estimate will reflect both the average effect of differences in school inputs across sectors and any average difference in peer effects (see Sacerdote 2011 for a review of empirical evidence of peer effects in education). To the extent that competition from private schools leads to improved public school quality, this estimator will underestimate the effect of private schools on overall student achievement (see Urquiola 2016 for a review of relevant empirical evidence).

The key identifying assumption is that unobserved factors affecting grade 7 test scores are uncorrelated with choice of school in grade 7, conditional on grade 4 test scores and controls:

$$E\left(\varepsilon_i \middle| Private_i, y_{i4}, year_{t(i)}, X_i, PC_i\right) = 0 \qquad (2)$$

This condition will be violated to the extent that there are unobserved factors that affect test score growth between grades 4 and 7 that are systematically related to the grade 7 school choice decision, conditional on observable student characteristics and residential postal code. A further threat to identification comes from the inclusion of the lagged dependent variable in this model, since test scores are prone to measurement error. Despite these theoretical issues, this specification has been validated relative to experimental results in a range of contexts, suggesting that condition (2) may be satisfied when data sets provide an adequate set of controls (Andrabi et al. 2011; Angrist et al. 2013; Deming et al. 2014; Kane and Staiger 2008; Kane et al. 2013).

Following the recent literature, we use the method proposed by Altonji et al. (2005) to assess the degree of violation of condition (2), relative to selection on our controls, that would fully account for our estimates. Writing the grade 7 test score model (1) more compactly as:

$$y_i = \alpha Private_i + Z_i'\gamma + \varepsilon_i \qquad (3)$$

and defining $\widetilde{Private}_i$ as the residual from a regression of $Private_i$ on Z_i, the bias in the OLS estimate $\hat{\alpha}$ from (3) can be written as:

$$bias_{\hat{\alpha}} = \frac{cov(\widetilde{Private}_i, \varepsilon_i)}{var(\widetilde{Private}_i)} \tag{4}$$

If $cov(Z_i \varepsilon_i) = 0$, this expression is equal to $\frac{cov(Private_i, \varepsilon_i)}{var(\widetilde{Private}_i)}$. The denominator in this term is identified from the data. Altonji et al. (2005) show that the numerator is identified under the assumption of "equal selection" on observable and unobservable characteristics:[7]

$$\frac{cov(Private_i, Z_i'\gamma)}{var(Z_i'\gamma)} = \frac{cov(Private_i, \varepsilon_i)}{var(\varepsilon_i)} \tag{5}$$

so that we can produce an estimate of the bias term of interest under the assumption of equal selection, call this estimate $bias_{\hat{\alpha}}^{ES}$. The ratio of this bias term to the point estimate indicates the degree of selection on unobservable covariates relative to observed controls that would fully account for the point estimate if the true treatment effect were zero:

$$\delta = \frac{\hat{\alpha}}{bias_{\hat{\alpha}}^{ES}} \tag{6}$$

This "implied ratio" provides a basis for assessing the likelihood that the observed point estimate is purely the product of selection bias. For example, Elder and Jepsen (2014) obtain an implied ratio of .05 for their estimates of the effects of Catholic schooling on eighth grade reading scores, implying that the relationship between Catholic school attendance and unobserved characteristics in the test score equation would have to be only five percent as big as its relationship to observed controls selection bias to produce their point estimate of the effect of Catholic schooling on test scores under the null of no treatment effect.

We estimate two other versions of this model, one that include indicators for each type of private school (Catholic, Other Christian, Other Faith and Prep) and one that includes a full set of school indicators.

[7] With a consistent estimate of γ under the null that $\alpha = 0$, obtained from a regression of $Private_i$ on X_i, we can produce estimates of $cov(Private_i, X_i'\gamma)$, $var(X_i'\gamma)$ and $var(\varepsilon_i)$, and use these to estimate $cov(Private_i, \varepsilon_i)$ via (5).

4.2 Estimating school and student fixed effects

We next estimate a test score model that allows us to identify individual student fixed effects, which we use to assess hypotheses related to the distribution of student ability within and across public and private schools. Our baseline model for student test scores in grade g is:

$$y_{ig} = \alpha Private_{ig} + X'_{ig}\beta_1 + X'_{n(ig)t(ig)} + \beta_2 grdyr_{t(ig)} + student_i + \varepsilon_{ig} \quad \text{for } g = 4, 7 \qquad (7)$$

where X_{ig} is a vector of characteristics of individual *i* when she is in grade g, $X_{n(ig)t(ig)}$ is a vector of characteristics associated with *i*'s neighborhood (Census Enumeration Area) in the year she is in grade g, and $grdyr_{t(ig)}$ is a grade-by-year fixed effect. This alternate specification of the test score model constrains the coefficient on the lagged score, λ, to be zero. This restriction is supported by growing evidence that the effects of lagged inputs dissipate rapidly (see for example Andrabi et al. 2011, Jacob et al. 2010, and Kane and Staiger 2008 in the context of teacher effects). Since we observe test scores three years apart, any bias introduced by this restriction is likely to be very small. The student fixed effects specification has been used widely in the school quality literature, including several recent papers on the effects of charter schools (e.g. Imberman 2011). As before, we estimate two other versions of this model, one that include indicators for each type of private school (Catholic, Other Christian, Other Faith and Prep) and one that includes a full set of school indicators. We estimate this two-way fixed effects model using the procedure developed by Abowd et al. (2002).

The coefficient of interest in (7), α, is the difference in mean test scores (in grades 4 and 7) between students enrolled in private and public schools. It is identified from students who attend a different type of school in grades 4 and 7. The key identifying assumption is that, among these movers, unobserved factors that affect test scores in grade g are uncorrelated with school sector choice, conditional on transitory grade-specific shocks, student fixed effects and observable time-varying student characteristics:

$$E\left(\varepsilon_{ig} \middle| Private_{i7}, Private_{i4}, grdyr_{t(i7)}, grdyr_{t(i4)}, X_{i7}, X_{i4}, X_{n(i7)t(i7)}, X_{n(i4)t(i4)}, student_i\right)$$

$$= 0 \quad \text{for } g = 4, 7 \tag{8}$$

At least two plausible scenarios could threaten the validity of this assumption. First, students may be heterogeneous with respect to an unobserved effect in test score *growth* between grades 4 and 7. The student fixed effects estimator will be biased if this effect is correlated with patterns of student mobility. Suppose, for example, that students who switch from a public school in grade 4 to a private school in grade 7 on average have characteristics associated with higher rates of test score growth, all else equal, than those who switch from private to public. In this case, the estimator will attribute the higher rate of test score growth that is caused by this pattern of unobserved heterogeneity to the effect of the private school.

Second, transitory shocks that cause students to change schools may be correlated with unobserved factors that affect test scores. Suppose, for example, that some students change schools at the end of grade 4 following a family break-up or job loss, and this event also adversely affects student achievement in grade 4. If the student's grades recover by grade 7, the estimated quality of the grade 4 school will be biased downwards relative to the grade 7 school. If students enrolled in private schools in grade 4 experience this scenario with the same frequency and degree as those enrolled in public schools in grade 4, the estimated difference between public and private school quality will not be affected. If, for example, these types of shocks more frequently result in students moving from private school to public schools, rather than vice versa, the estimated quality of private schools will be biased downwards.

We address this second selection problem in two ways. First, we investigate the sensitivity of our results to several sample restrictions designed to eliminate bias associated with different patterns of correlation between mobility patterns and transitory shocks to student achievement. By excluding students who changed schools immediately after grade 4, we eliminate the threat of bias from shocks that precipitate a move at the end of grade 4 and affect grade 4 test scores. By excluding students who changed schools immediately after grade 6, we eliminate the threat of bias from shocks that precipitate a

move at the end of grade 6 and affect grade 7 test scores. Second, we estimate our model for a sample of students who are required by the grade configuration of schools to move between grades 4 and 7. The types of transitory shocks that lead to the dynamic selection problems described above are likely to be less frequent among compulsory movers than among students who elect to change schools.

If the identifying assumption is satisfied, estimation of (7) will produce unbiased estimates of school effects for movers. These estimates can be generalized to non-movers only if school effects are the same for movers and non-movers. This assumption would be violated, for example, if some schools do a better job of teaching reading and numeracy skills to new students who transfer in, relative to their effects on non-movers. To the extent that this is true, our estimates of school and student effects for non-movers will be biased.

5 RESULTS

5.1 Descriptive statistics

We define the population of interest as all students enrolled in the relevant set of schools in grade four in 1999/2000, 2000/2001, 2002/2003 or 2004/2005, and who advance one grade in each of the following three years, through grade 7. We restrict our attention to English language[8] public or publicly-funded private schools that enroll at least five students in the relevant grade and year. Given their small number and typically small size, we exclude students who attend a Montessori or Waldorf private school from our population of interest, along with students who attend private schools that specialize in serving students with special needs.

The upper panel of Table 1 presents selected school characteristics by school type. The students in our population of interest attend 650 different schools, of which 555 are public and 95 are private. Of the private schools, 33 are Catholic, 40 are other Christian,

[8] French language instruction is offered to English speaking students via public French Immersion programs. Attrition from these programs is very high (see Shack 2015). Francophone students may choose to attend one of a small number of schools operated by the public francophone school board.

7 are other faith, and 15 are secular prep schools. Eighty percent of private schools are in funding group 1, receiving a per student subsidy equal to half of the public school subsidy; the remaining private schools are in funding group 2, receiving a 35% subsidy. Almost all private schools in our sample offer both grades 4 and 7. In contrast, 101 public schools offer grade 4 but do not offer grade 7, and 35 schools offer grade 7 but not grade 4.

The lower panel of Table 1 presents descriptive statistics for the grade 7 population of interest. Private school students on average earn higher test scores than public school students, but this difference varies across private school types. Students enrolled in private prep schools excel; their average grade 7 test scores are 0.85 and 0.92 standard deviations above the mean in reading and numeracy. Students enrolled in faith private schools also score relatively well, averaging 0.42 standard deviations above the mean in reading and 0.54 standard deviation in numeracy at private Catholic schools, 0.26 standard deviations in reading and 0.27 standard deviations in numeracy at private other Christian schools, and 0.31 standard deviations in reading and 0.51 standard deviations in numeracy at non-Christian private faith schools, compared to 0.00 and 0.11 standard deviations among public school students. The share of public school students with missing test scores is 13% in reading and 15% in numeracy, more than twice the corresponding shares of private school students.

The remaining rows of Table 1 demonstrate the extent to which sorting across schools produces differences in the observables characteristics of students attending each school type. These patterns demonstrate the substantial potential for differences in student characteristics to account for the raw differences in mean achievement across school types. Students enrolled in private prep schools are the most positively selected with respect to several observable characteristics that have a known association with test scores. These students on average live in neighborhoods with very high mean family income and highly educated household heads. Relatively few prep school students are Aboriginal or speak Punjabi at home, characteristics that in British Columbia are associated with low mean test scores on average (see Friesen and Krauth 2011). Students

enrolled in private non-Christian faith schools also live in relatively high SES neighborhoods. However, these students are far less likely to speak English at home than any other group. Notably, over one-third of these students are Punjabi-speakers who attend Sikh faith schools. Students enrolled in Catholic schools are also positively if slightly less strongly selected with respect to neighborhood SES. They are more likely to speak English at home than public school students and are less likely to be Aboriginal. In contrast, neighborhood SES is very similar for students enrolled at other Christian private schools compared to public school students. Punjabi speakers and Aboriginal students are underrepresented at these schools, but the proportion that speaks another non-English home language (e.g. Korean, Vietnamese or Tagalog) is higher than among public school students.

The variation across school types in gender composition is also worth noting. Private prep school students are 48 percent female, the same proportion as in public schools. Catholic schools and other (non-Christian) faith private schools are disproportionately female (52 percent and 54 percent respectively), while other (non-Catholic) Christian private schools are disproportionately male (46 percent female).

5.2 Estimates of school quality

Table 2 presents our difference-in-differences estimates of the effect of attending a private school in grade 7 on seventh grade test scores from the lagged test scores model (1). The specification reported in the first column includes only the private school indicator, the lagged test score and a set of year effects, with subsequent columns adding individual covariates alone, and then individual covariates along with postal code indicators. The point estimates of the private school effect are positive and statistically significant and the effect sizes are quite stable across specifications, ranging from 0.16 and 0.19 standard deviations in reading and from 0.17 and 0.21 standard deviations in numeracy.

Several statistics reported in Table 2 can inform our assessment of the extent to which these point estimates reflect positive selection into private schools that has not been

accounted for by our controls. With the addition of the postal code indicators, the model explains almost 70% of the overall variation in individual reading and numeracy scores. The "implied ratios" indicate that selection on the unobserved factors that account for the remaining 30% of the overall variation in test scores would have to be 60% and 82% as great as the selection on the observables in reading and numeracy respectively to fully account for the point estimates. Our method of calculating the implied ratios assumes that a full set of controls could explain 100% of the variation in test scores (see Oster 2016 for a discussion). To the extent that there is measurement error in test scores that is uncorrelated with private school enrolment, these implied ratios overstate the degree of threat from selection on unobservables. Taken together, these results provide strong evidence that private schools outperform public schools on average with respect to the formation of reading and numeracy skills.

The results in Table 2 are not directly comparable to those in the previous literature, which focuses on Catholic private schools. We present estimates by private school type in Table 3. The first column reports estimates from a version of (1) that includes indicators for each of our four private school types. The second column reports estimates obtained from different sub-samples of the data. In the case of Catholic private schools, for example, the effect is estimated from a sample that excludes private school students who do not attend Catholic private schools. The model in this case includes a single indicator for Catholic private school enrolment, so that the estimate is directly comparable to several previous studies of Catholic schools (Altonji et al. 2005; Elder and Jepsen 2014), and we can apply the method of Altonji et al. (2005) to assess selection bias specifically with respect to private Catholics schools. In practice, it turns out that almost all the point estimates from the two specifications are the same, so we focus on those in column 2.

The point estimates for Catholic private schools is positive and statistically significant, and the effect sizes are 0.21 standard deviations in reading and 0.30 standard deviations in numeracy. The associated implied ratios are 0.90 and 2.42, implying that a very high degree of non-random selection on unobservable characteristics would be required to fully account for the estimated effects. These results stand in contrast to those in the

recent literature, which finds negligible or even negative effects of private Catholic schools on reading and numeracy skills (Altonji et al. 2005; Elder and Jepsen 2014; Nghiem et al 2015). Point estimates for non-Christian faith schools and prep schools are similar in magnitude and statistical significance to those for Catholic schools, with effect sizes ranging from .24 to .25 standard deviations. The implied ratios for other faith private schools are 3.53 for reading and 2.21 for numeracy, again implying that an implausibly large amount of non-random selection would be required to fully account for these point estimates. The potential for selection bias is a greater concern in the case of prep schools, where the implied ratios are 0.48 and 0.51 in reading and numeracy respectively. Other Christian private schools stand out from the other groups. The point estimates are substantially smaller (negligible in the case of numeracy), and the risk that these estimates are purely the result of selection bias is relatively high.

We next use these data to move beyond estimates of mean effects for groups of private schools by producing estimates of school effects specific to each private school in our data. Figure 1 presents kernel density estimates of the distributions of school effects for private and public schools. The school-weighted distributions in Panel A show substantial overlap in these distributions, illustrating the limitations of what can be learned from simple mean comparisons across school sectors. Many public schools outperform the average private school, and many private schools fall short of the average public school. Estimates of student-weighted distributions of school effects in the Panel B of Figure 1 show that private schools that do a relatively poor job of teaching reading have small enrollments; it is less obvious that this is the case for private schools that do a poor job of teaching numeracy skills.

We use these estimates of school effects to investigate three possible reasons why parents may choose to enroll their children in tuition-charging private schools that do a relatively poor job: (1) because these schools offer a good alternative relative to *nearby* public schools;[9] (2) because parents are making mistakes based on misleading information,

[9] Various authors have suggested that the pattern of test score estimates for Catholic schools in U.S. (i.e. positive effects only in the case of urban minorities) may be the result of Catholic

and/or; (3) because parents seek non-academic amenities that only private schools can offer. In the case of the first hypothesis, we assess the role of *local* public school quality in private school choice decisions by comparing the estimated school effect of the school attended by each private school student to the estimated school effect of their guaranteed public school. The results in the first panel of Table 4 show that 21% of private school students enroll in schools with lower school effects than the median public school in reading, and 34% in numeracy. When the comparison is made between the private school attended and the student's guaranteed public school, these shares are somewhat higher. This evidence provides no support for the hypothesis that low-quality private schools attract students by locating nearby to even lower quality public schools.

We next consider whether parents who enroll their children in low quality private schools may be basing their school choice decisions on misinformation. When school quality is not directly observable, evidence suggests that parents use publicly available school mean test scores, observable characteristics of the student body and/or word of mouth when assessing their school choice options (e.g. Hastings and Weinstein 2008; Friesen et al. 2010), and this information may not always provide a reliable indicator of actual school quality. Figure 2 shows kernel density estimates of student-weighted distributions of school-mean *test scores* for private and public schools. Compared to the share of students who attend private schools with below-average school effects (seen in Figure 1, Panel B), the share of students who attend a private school with below-average school mean test scores is somewhat smaller. These shares are shown in the second panel of Table 4. Among prep school students, very few attend a school with lower school mean test scores than their guaranteed public school in either reading or numeracy. Taken together with the comparisons of prep school effects, these results provide indirect evidence that public information about school mean test scores may mislead a substantial minority of private school parents about the relatively quality of their prep school. Among faith school students, however, the results tend to be quite different. For example, over one-third of other Christian private school students have guaranteed access to a public school where

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schools locating in urban areas where the quality of public schools is particularly low (see Martinez-Mora, 2006, for a theoretical treatment of this issue).

mean numeracy scores are higher. Among students attending other Faith private schools, the corresponding share for reading is 41%. These results suggest, perhaps unsurprisingly, that a substantial share of parents who enroll their children in faith private schools are either uninformed about school-level achievement or value faith-based education for reasons unrelated to academic achievement. Given the availability of public information, the latter seems a more plausible interpretation.

5.3 Estimates of student ability and cream-skimming

We next estimate model (2), which allows us to recover a full set of individual school and student fixed effects. We use these estimates to shed light on several hypotheses about the distribution of student "ability" across public and private schools.

As described in Section 4.2, school effects are identified in the students fixed effects model from students who change schools between grades 4 and 7, and this strategy poses a distinct set of challenges compared to the lagged test score model. We begin by investigating the robustness of our estimates of private school effects to this alternative specification as a way of establishing the validity of this approach. The first column of Table 5 reproduces the estimates from the third column of Table 3 and the second column presents the student fixed effects estimates from the full sample. The remaining columns of Table 5 present estimates or various sub-samples of the data as robustness checks designed to address the threat from *dynamic* selection into mover status. The first and second sub-samples excludes students who change school sectors immediately after grade 4 and grade 6 respectively, in order to eliminate potential bias from transitory shocks that systematically affect the school sector choice of these movers and are correlated with grade 4 (grade 7) test scores. The third sub-sample includes only students who are required to change schools between grades 4 and 7 because of the grade configuration of their grade 4 schools.

The effects estimated from the student fixed effects model are fairly consistent across sub-samples. The sign and relative magnitudes of the estimates are the same as those from the lagged test score specification; however, the effect sizes are substantially

smaller, both overall and for each private school type. These differences in effect sizes could be due to several factors. First, the models include different sets of geographic controls (postal code indicators versus Census neighborhood characteristics). Second, effects may not be homogeneous across different types of students. Specifically, private school effects may be greater among stayers than movers; since stayers do not contribute to the identification of school fixed effects in the student-fixed effects model, this could result in lower estimates. Third, estimates from the two specifications are subject to different sources of bias.

5.3.1 Cream-skimming.

Figure 3 presents kernel density estimates of the distributions of student effects in public and private schools. While the average student effect is higher in the private school sector in both reading and numeracy, the distributions in Panel A exhibit substantial overlap. Panel B reveals that prep schools are the most selective group of private schools, while Christian and Catholic private schools are also selective, but substantially less so. Table 6 provides further insight into the extent to which private schools draw off high achieving students. The first two columns show the share of students who enroll in public versus private schools, by bottom and top quartile of the distribution of student quality. Results for reading, reported in the top panel, show that the public school share of enrolment falls from 92 percent of students in the bottom quartile of student quality to only 85 percent of those in the top quartile. Enrolment share decreases across quartiles at prep schools, Catholic schools and other Christian private schools. Enrolment share at other faith private schools is the same in the bottom and top quartiles of the distribution. This pattern results in mean differences in student effects between all students enrolled in all public and private schools (reported in column 5). For reading, this difference is 0.27 standard deviations overall. It is largest for students enrolled in prep schools (0.57), followed by Catholic (0.22) and other Christian private schools (0.21). The difference in average student effects between students enrolled in other faith private schools and public schools is also positive, but small in magnitude (0.07) and statistically insignificant. Results for numeracy, reported in the lower panel of Table 6, are similar.

The final column of Table 6 reports mean differences in student effects between students enrolled in private schools and mean student effects among students who live in the same catchment area. Since neighborhoods tend to be stratified by income and family resources, a more meaningful measure of cream-skimming would compare private school students to their neighbors. Under the counterfactual assumption that private school students would otherwise be attending school with their neighbors, rather than with randomly selected public school students, cream-skimming associated with prep schools is substantially less pronounced. This pattern indicates that prep schools draw students from neighborhoods where mean student effects are above average. The same pattern emerges to a lesser degree among Catholic schools. Other Christian private schools appear to draw students from neighborhoods where mean student effects are close to average, while other faith private schools draw students from neighborhoods with below average mean student effects. In the first three cases, differences in peer quality remain large even when compared to local alternatives.

5.3.2 The relationship between school and student effects.

Each point in Figure 5 represents a single private school, with the school mean student effect on the horizontal axis and the school fixed effect on the vertical axis. Visual inspection of these plots reveals several interesting patterns.

Among schools that admit above-average students, a substantial number have below average school effects in both reading and numeracy. These schools are doing a poor job of educating good students. If parents look to school mean test scores as an indicator of school quality when making school choice decisions, these schools may be relying on selective admission procedures to generate high average test scores, as a low-cost way of establishing and maintaining reputations for academic quality despite relatively poor academic quality. All four types of private schools are represented in this group.

We also see no evidence that private schools that admit very high achieving students are more effective than those that admit more average students. These results are consistent with well identified empirical evidence that selective schools are only sometimes more effective (e.g. Clark 2010; Abdulkadiroglu et al. 2014; Pop-Eleches and Urquiola 2013).

Interestingly, all the private schools that serve below-average students in reading have reading school effects that are at or above the average. In these cases, it is reasonable to conclude that the above-average school effects reflect superior school inputs, since any peer effects associated with below-average students are unlikely to be positive - these schools do a good job of teaching reading to their reading-challenged students. In contrast, there is substantial variation in school numeracy effects among private schools that serve below-average students in numeracy. Below-average school effects in these cases do not necessarily imply that the school is using poor inputs, if weak numeracy students create a challenging peer environment in which to teach and learn numeracy skills.

6 CONCLUSION

The results in this paper point to the relative success of the average private primary school in British Columbia with respect to literacy and numeracy skill development, compared to its public counterpart. This evidence contradicts the results of most previous studies of the effects of private schools on test scores in other advanced economies; Catholic private schools in particular have been associated with no better or even relatively poor test scores in both the U.S. and Australia (e.g. Altonji et al. 2005; Elder and Jepsen 2014; Nghiem et al. 2015). This qualitative difference in our conclusions about the relative effectiveness of private schools may reflect differences in institutions. Like private schools in many jurisdictions, B.C.'s private schools enjoy substantial autonomy with respect to teacher hiring, firing, discipline and remuneration, are free to apply a wide range of admissions criteria and can charge tuition. Unlike private schools in some jurisdictions, B.C. private schools are required to hire teachers who are provincially certified, and they receive partial funding from the provincial government. Published school-level test score results include private schools, and these receive a substantial amount of media and public attention.

Our methodology provides no direct evidence of the mechanisms that may be driving our results. However, it is of some interest to note that our analysis by private school type finds positive effects for some but not all groups of faith private schools as well as

positive effects for secular prep private schools. This pattern suggests that it is not faith-based education per se that is contributing to the relative effectiveness of some private schools. This interpretation is consistent with existing direct evidence that faith *public schools* are no more effective than secular public schools (Gibbons and Silva 2011). It is also worth noting that, while private schools on average enroll higher quality students and deliver higher quality outcomes, this is not true in all private school sectors. In particular, other Christian schools enroll relatively high quality students but do not get better results on average than public schools, while other faith schools enroll fairly average students but get better than average results. This pattern is not consistent with a simple story of peer effects driving variation in school performance across public versus private school types.

While the average private school effect is positive, the range of quality among private schools is substantial, and many private schools appear to be less effective than many public schools. The survival of these schools, many of which have been in operation for decades, invites several potential explanations. We use our estimates to rule out the hypothesis that low quality private schools compete successfully at the local level by locating in neighborhoods where public schools are weak. In some cases, parents who rely on school mean test scores as indicators of school quality are unable to identify low-quality schools that enroll high achieving students. In cases where both school and student quality are relatively low, we hypothesize that these schools may offer amenities that parents value apart from their possible contributions to cognitive skill development; these likely include religious or moral instruction, and may include peer characteristics, enriched supervision and monitoring of student behavior, etc.

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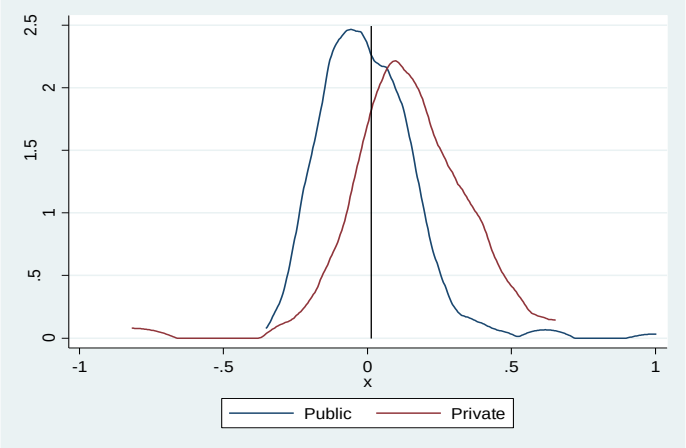
FIGURES

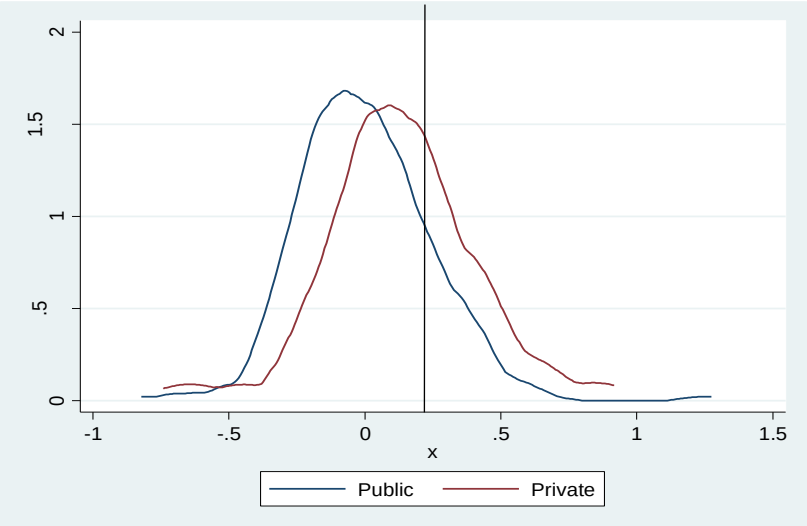
Figure 1: Kernel density estimates of the distribution of estimated school effects, by public and private schools

Panel A. School-weighted densities

Reading



Numeracy



Panel B. Student-weighted densities

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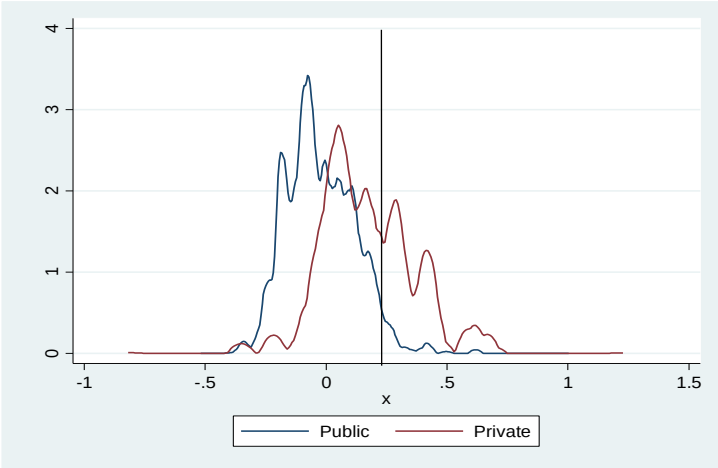

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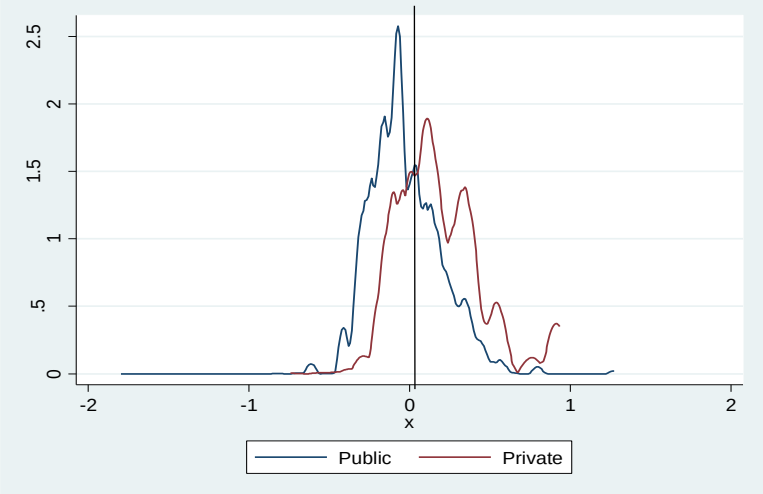
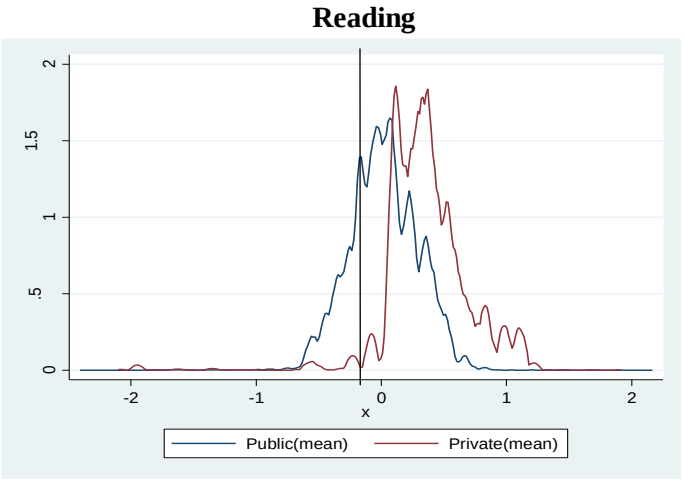


Figure 2: Kernel density estimates of (student-weighted) distribution of school mean test scores, by public and private schools



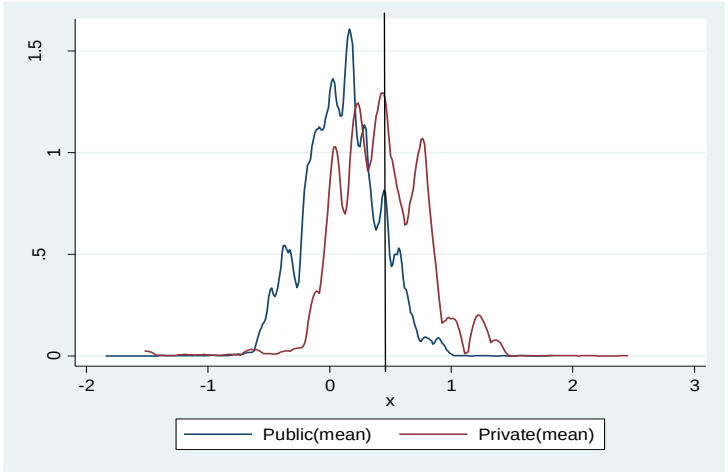


Figure 3. Kernel density estimates of the student-weighted distribution of estimated student effects

Panel A. Public and private schools

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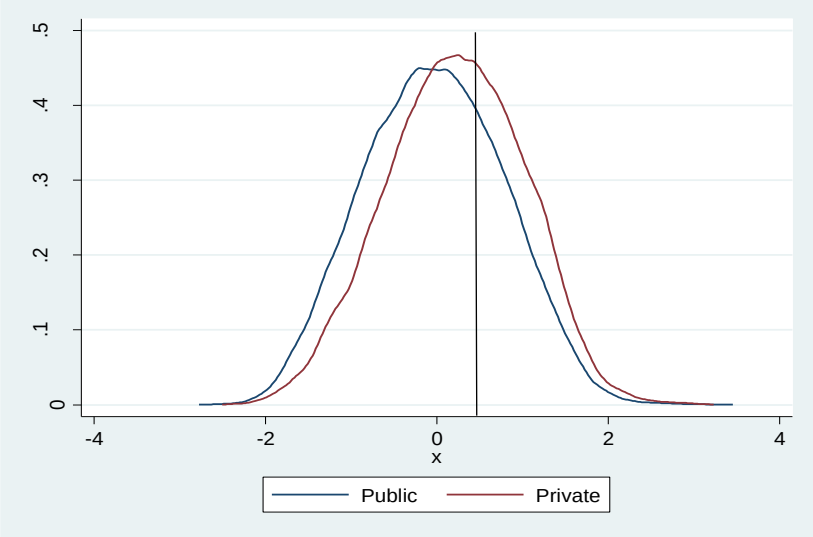

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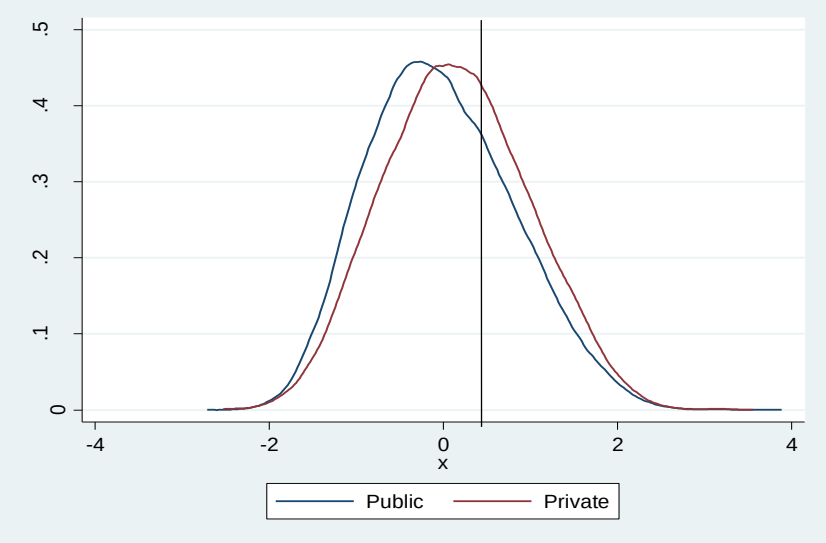

Panel B. Private school types

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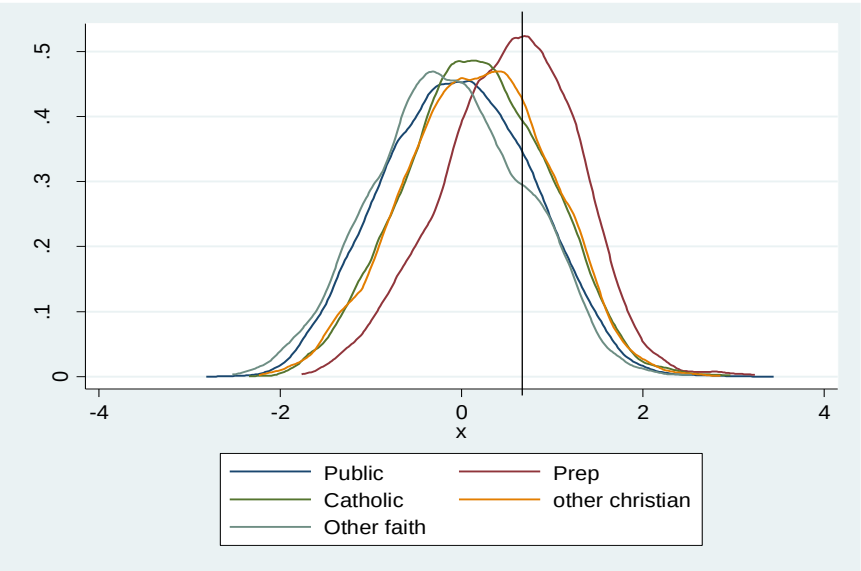

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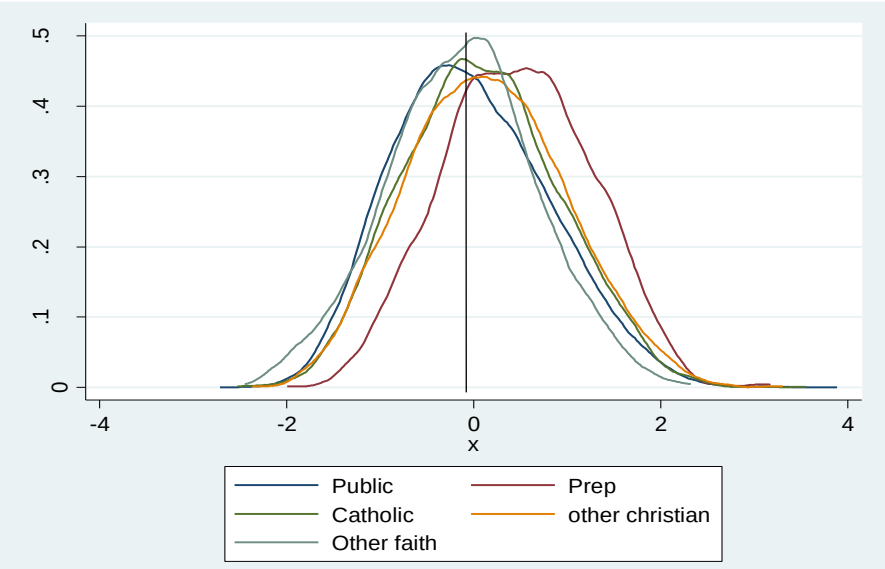
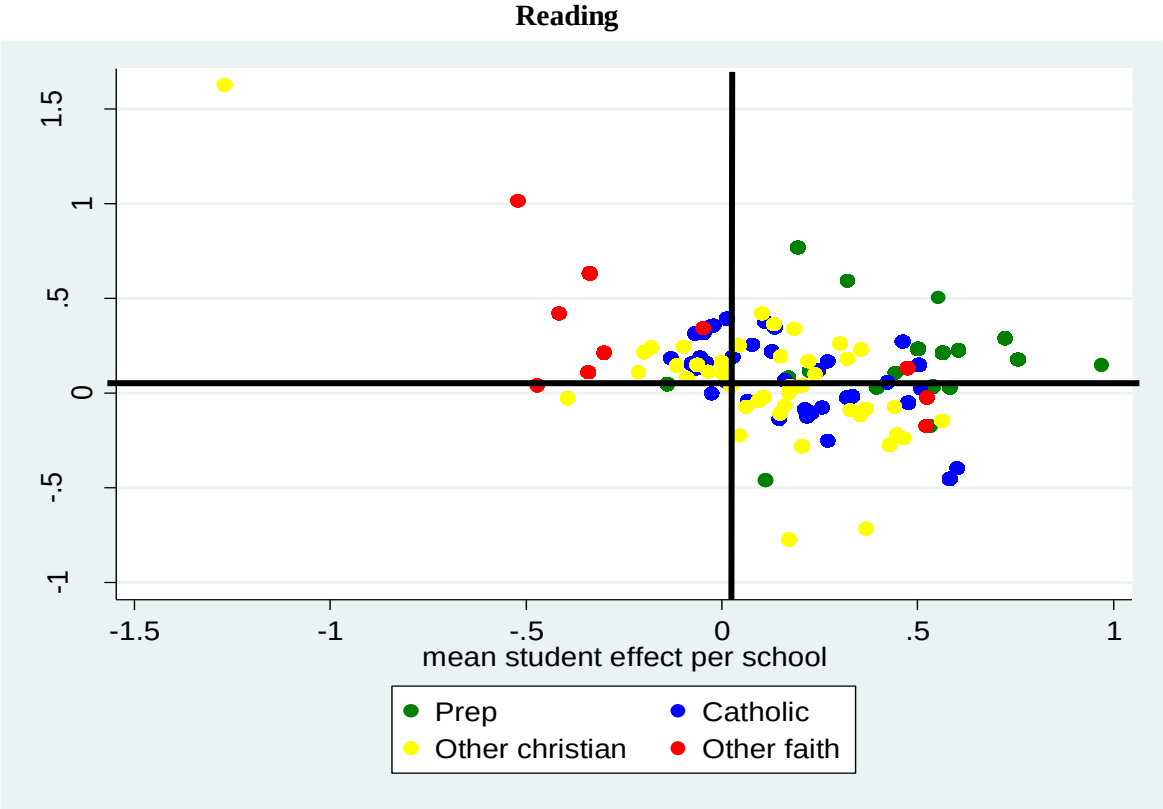
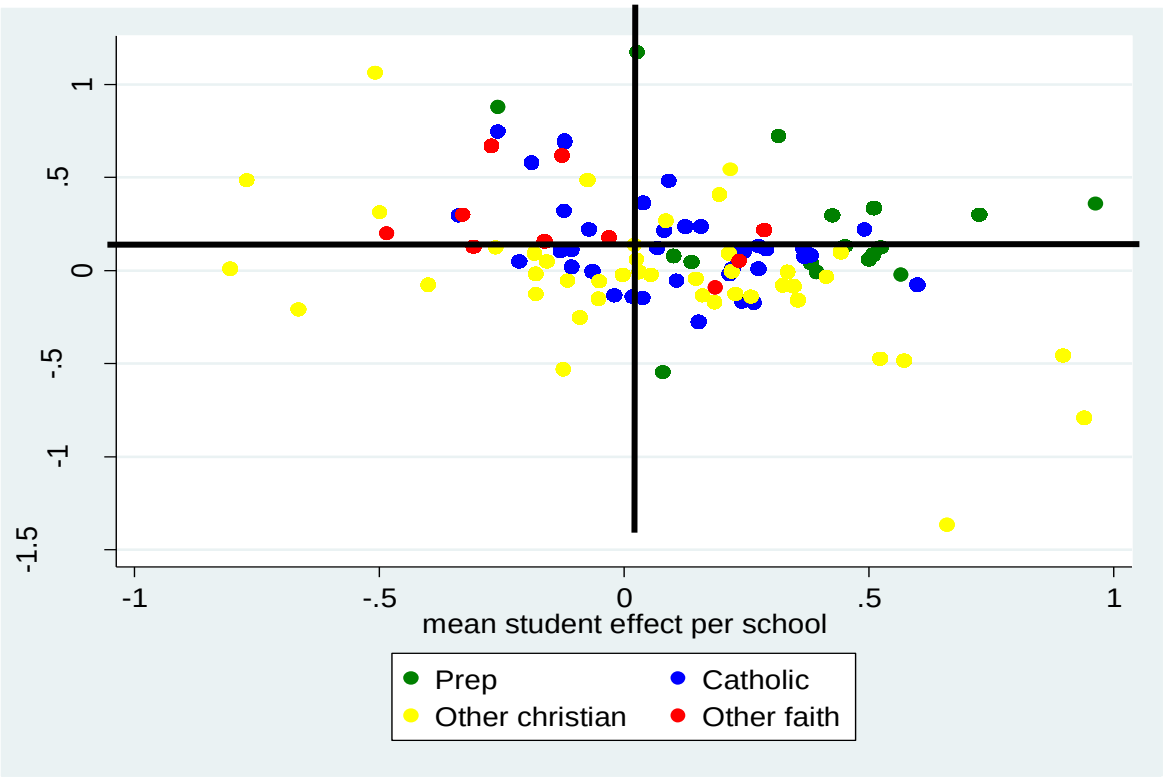


Figure 4. Estimated school effects and school mean student effects, by private school type



Numeracy


1
.5
0
-.5
-1
-1.5
-1
-.5
0
.5
1
mean student effect per school
Prep
Catholic
Other christian
Other faith

TABLES

Table 1: Selected school and Grade 7 student characteristics, by school type

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Private				
	Public	All	Prep	Catholic	Christian	Other faith
SCHOOL CHARACTERISTICS						
No. of schools	555	95	15	33	40	7
Funding group 1	.	76	9	31	31	5
Funding group 2	.	19	6	2	9	2
Grade 4 only	101	1	1	0	0	0
Grade 7 only	35	4	1	0	2	1
Grades 4 and 7	419	90	13	33	38	6
STUDENT CHARACTERISTICS (grade 7)						
No. of students	87113	11924	2379	4565	4448	532
% of sample	87.96	12.04	2.40	4.61	4.49	0.54
Reading score	0.00	0.44	0.85	0.42	0.26	0.31
Numeracy score	0.11	0.51	0.92	0.54	0.27	0.51
Missing reading score	0.13	0.06	0.06	0.05	0.07	0.10
Missing numeracy score	0.15	0.07	0.06	0.05	0.08	0.10
Home language						
English	0.66	0.73	0.71	0.81	0.72	0.35
Chinese	0.12	0.07	0.12	0.04	0.09	0.00
Punjabi	0.08	0.03	0.03	0.00	0.01	0.35
Other	0.14	0.17	0.16	0.14	0.18	0.30
Aboriginal	0.06	0.01	0.00	0.01	0.01	0.00
Female	0.48	0.49	0.48	0.52	0.46	0.54
Neighborhood mean						
Immigrant	0.07	0.06	0.07	0.07	0.05	0.10
High school	0.25	0.25	0.23	0.25	0.26	0.24
Some college	0.30	0.28	0.24	0.29	0.30	0.24
Bachelor's	0.17	0.22	0.36	0.20	0.15	0.24
Family income	6.64	7.87	11.95	7.04	6.66	7.11

Notes: see text and Data Appendix for details of sample selection and construction, and for variable definitions.

Table 2: Estimates of private school effect on grade 7 test scores

	(1)	(2)	(3)	(1)	(2)	(3)
	READING			NUMERACY		
Private	0.19***	0.19***	0.16***	0.19***	0.21***	0.17***
	[0.027]	[0.026]	[0.022]	[0.041]	[0.039]	[0.035]
Implied ratio	0.687	0.706	0.606	0.801	1.026	0.822
R-squared	0.469	0.480	0.687	0.443	0.465	0.694
# of students	84774	84774	84774	81447	81447	81447
Year effects	x	x	x	x	x	x
Individual controls		x	x		x	x
Postal code indicators			x			x

Notes: Standard errors clustered at the school level. Individual controls include grade 4 test score, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language and Aboriginal identity.

Table 3: Estimates of private school effect on grade 7 test scores, by private school type

	(1)[a]	(2)[b]	(1)[a]	(2)[b]
	READING		NUMERACY	
Catholic	0.21***	0.21***	0.29***	0.30***
	[0.027]	[0.027]	[0.044]	[0.045]
Implied ratio	-	0.980	-	2.242
R-squared	0.687	0.690	0.695	0.697
# of students	84774	78687	81447	77339
Other Christian	0.01	0.07**	0.01	0.01
	[0.035]	[0.026]	[0.041]	[0.041]
Implied ratio	-	0.362	-	0.076
R-squared	0.687	0.688	0.695	0.694
# of students	84774	78511	81447	77151
Other faith	0.29**	0.24*	0.25*	0.24*
	[0.110]	[0.102]	[0.118]	[0.110]
Implied ratio	-	3.532	-	2.214
R-squared	0.687	0.693	0.695	0.700
# of students	84774	75224	81447	73908
Prep	0.25***	0.25***	0.25***	0.25**
	[0.057]	[0.058]	[0.077]	[0.077]
Implied ratio	-	0.480	-	0.512
R-squared	0.687	0.697	0.695	0.703
# of students	84774	76734	81447	75399

Notes: Year effects, individual controls and postal code effects included in all specifications. Standard errors clustered at the school level. Individual controls include grade 4 test score, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language and Aboriginal identity. [a] Full sample. [b] Samples exclude students attending other types of private schools.

Table 4: Share of private school students enrolled at school where school effect and school mean test score are less than benchmark school.

Share of students enrolled in	School effect less than at				School mean test score less than at			
	Guaranteed school		Median public school		Guaranteed school		Median public school	
	Reading	Numeracy	Reading	Numeracy	Reading	Numeracy	Reading	Numeracy
All private	0.29	0.35	0.21	0.34	0.10	0.23	0.18	0.28
Prep	0.18	0.29	0.04	0.15	0.03	0.04	0.09	0.17
Catholic	0.21	0.24	0.08	0.19	0.05	0.21	0.14	0.22
Christian	0.44	0.52	0.43	0.60	0.15	0.34	0.26	0.39
Other faith	0.23	0.27	0.27	0.28	0.41	0.28	0.27	0.29

Notes: Authors' calculations based on estimates from school effects version of (1). See notes to Table 1 for details of specification.

Table 5: Estimates of private school effect on grade 7 reading and numeracy scores, by private school type, from lagged test score model and student fixed effects model

	(1)	(2)	(3)	(4)	(5)
	READING				
	From Table 3	Student fixed effects model[a]			
			No movers between		Compulsory movers
	All students	All students	G4/G5	G6/G7	
All private schools	0.16***	0.10***	0.12***	0.12*	0.08**
	[0.022]	[0.024]	[0.028]	[0.051]	[0.030]
R-squared	0.687	0.842	0.842	0.841	0.843
# of students	84774	169419	151950	149071	26450
Catholic	0.21***	0.11***	0.15***	0.04	0.14***
	[0.027]	[0.031]	[0.037]	[0.112]	[0.038]
Other Christian	0.01	0.03	0.05	0.07	-0.05
	[0.035]	[0.032]	[0.038]	[0.069]	[0.035]
Other faith	0.29**	0.20*	0.23*	0.59**	0.18
	[0.110]	[0.085]	[0.095]	[0.211]	[0.101]
Prep	0.25***	0.17***	0.14*	0.34**	0.18**
	[0.057]	[0.051]	[0.062]	[0.117]	[0.067]
R-squared	0.687	0.842	0.842	0.841	0.843
# of students	84774	169419	151950	149071	26450
	NUMERACY				
	From Table 3	Student fixed effects model[a]			
			No movers between		Compulsory movers
	All students	All students	G4/G5	G6/G7	
All private schools	0.17***	0.08**	0.06	0.10**	0.09
	[0.035]	[0.030]	[0.032]	[0.033]	[0.064]
R-squared	0.694	0.836	0.836	0.836	0.837
# of students	81447	166613	166613	149573	146799
Catholic	0.29***	0.13**	0.16***	0.18	0.13**
	[0.044]	[0.043]	[0.043]	[0.106]	[0.043]
Other Christian	0.01	-0.11**	-0.02	0.02	-0.11**
	[0.041]	[0.037]	[0.038]	[0.084]	[0.037]
Other faith	0.25*	0.20*	0.24*	-0.17	0.20*
	[0.118]	[0.090]	[0.105]	[0.252]	[0.090]
Prep	0.25***	0.18**	0.16*	0.35**	0.18**
	[0.077]	[0.061]	[0.078]	[0.128]	[0.061]
R-squared	0.695	0.836	0.836	0.837	0.832
# of students	81447	166613	149573	146799	25843

Notes: Standard errors clustered at the school level. [a]Dependent variable is the student's FSA test score in grade *g*. Additional control variables include grade-by-year fixed effects, English as a Second Language, Aboriginal identity, Census neighborhood characteristics and a full set of student fixed effects.

Table 6. Share of students enrolled in school type by quartile of student effects, difference between mean student effect in school type and mean student effect in benchmark schools

	(1)	(2)	(3)	(4)
		READING		
	Share of students enrolled in school type by quartile of student effects		Mean effect in school type minus mean effect among students…[a]	
	Q1	Q4 (top)	in all public schools	in attendance zone
All public	0.917	0.850	-	-0.023
	(0.003)	(0.004)	-	
All private	0.083	0.150	0.27***	0.180
	(0.003)	(0.004)	0.023	
Prep	0.015	0.034	0.57***	0.257
	(0.001)	(0.002)	0.044	
Catholic	0.036	0.056	0.22***	0.152
	(0.002)	(0.003)	0.035	
Other Christian	0.028	0.055	0.21***	0.190
	(0.002)	(0.002)	0.033	
Other faith	0.004	0.005	0.07	0.012
	(0.001)	(0.001)	0.060	
		NUMERACY		
	Share of students enrolled in sector by quartile of student effects		Mean effect in school type minus mean effect among students…[a]	
	Q1	Q4 (top)	in all public schools	in attendance zone
All public	0.910	0.857		-0.018
	(0.003)	(0.004)		
All private	0.090	0.143	0.22***	0.138
	(0.003)	(0.004)	0.020	
Prep	0.015	0.032	0.51***	0.224
	(0.001)	(0.002)	0.040	
Catholic	0.041	0.053	0.14***	0.070
	(0.003)	(0.003)	0.039	
Other Christian	0.031	0.054	0.18***	0.191
	(0.002)	(0.002)	0.034	
Other faith	0.004	0.004	0.06	??
	(0.001)	(0.001)	0.065	

Notes: Standard errors are bootstrapped. [a] Differences between school-weighted averages of estimated private school and public school mean student fixed effects, by private school type.

Data Appendix

Control variables

Controls for individual characteristics include indicators for gender, Aboriginal, English as a Second Language and language spoken at home (English, Chinese, Punjabi or other). We also control for mean household income in the Census Enumeration or Dissemination Area (EA or DA, respectively) in which the student resides, as a proxy for unobserved student background characteristics.

Coding of Neighborhood Family Income

To proxy for the student's socioeconomic status, we match their residential postal code to the most recent public-use estimates of neighborhood average income from the 1996, 2001, and 2006 Census long-form. Statistics Canada publishes average income at the Enumeration Area (EA) or the Dissemination Area (DA) level, depending on Census year. 1996 Census estimates were published at the EA level, where an Enumeration Area typically included 125 to 440 dwellings (in rural and urban areas, respectively). Since the 2001 Census, Statistics Canada has replaced EA-level estimates with estimates at the DA level. A Dissemination Area comprises 400 to 700 persons, so EAs and DAs are comparable in size.

We link postal codes to an EA/DA using Statistics Canada's Postal Code Conversion File (PCCF), which contains the longitudinal history of each postal code (postal codes are routinely retired and reused elsewhere). Postal codes are smaller than EAs/DAs, although they sometimes straddle multiple EAs or DAs. In these cases, we link the postal code to the best EA/DA using Statistics Canada's single link indicator, which identifies the EA/DA with the majority of dwellings assigned to that postal code.