Private schools and student achievement

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Abstract

We investigate the effects of private schools on reading and numeracy scores using rich population data. Conditional on lagged test scores and narrowly defined neighborhood indicators, Catholic and non-Christian faith private schools on average raise test scores by 0.18 standard deviations or more relative to the average public school, while non-Catholic Christian private schools have negligible effects. The effects of secular private "prep" schools are similar to those of Catholic schools, but selection bias is a greater concern in this case. We use school-specific estimates of effectiveness to investigate private school choice decisions and the determinants of private school effectiveness.

<A>1 Introduction

Advocates of school choice have long argued that private school vouchers can generate improvements in the quality of education, by allowing students to enroll in better schools or in schools that are better matches and by leveraging market pressures to motivate school leaders to deliver effective programs (Friedman 1962). Among other things, this classic hypothesis requires that a substantial number of parents can and do access private schools that are more effective than their public school alternatives.

This paper contributes to this debate by providing new estimates of private school effectiveness under a universal voucher program. Beginning in 1977, British Columbia (B.C.) Canada has provided operating grants to private schools, and since 1989 this grant has been worth 35-50% percent of the public school operating grant. Almost all private schools receive this funding and are referred to as “independent” schools within B.C.’s system, which most closely resembles Denmark’s in key features (see Table 1). Government-funded private schools may use any admissions criteria that do not violate human rights laws and may charge any amount of tuition. They must operate as non-profits and meet provincial curriculum standards, which they may supplement with faith-based or other types of content. They must administer the same standardized tests as public schools, and school-level results are publicly disseminated and widely discussed (Federation of Private Schools Associations 2015). Teachers must be provincially certified but are not covered by the public sector collective agreement.

Our longitudinal records follow five population cohorts from grade 4 (age 8-9) to grade 7 who attend 767 schools, of which 112 are private. Student records include basic demographic characteristics, residential postal code and school attended in each year, as well as results from centrally graded, low stakes reading and numeracy tests administered in grades 4 and 7. We estimate school effectiveness by including fixed school effects in a test score model that includes polynomials in lagged test scores and a set of individual covariates. Taking advantage of our geographically dense population data, we include neighborhood indicators corresponding to geographic units that contain an average of 19 households nationally (Statistics Canada 2010) and an average of 2.8 unique students in our sample.[1] These indicators control for many of the neighborhood-level characteristics that may drive selection into private schools (e.g., average family income or the quality of local public school alternatives). We conduct a number of robustness checks that provide general support for our specification and obtain reassuringly similar estimates of the private school effect from an alternative specification that replaces the lagged test score with a student fixed effect.

We find that the average private school is more effective than the average public school in both reading and numeracy, with an effect size of between 0.12 and 0.15 standard deviations. This result should not be interpreted as evidence that the average public school is weak - as a stand-alone jurisdiction, B.C. outranked every other country and Canadian province on the 2015 PISA tests in reading and ranked below only eight countries and the Canadian province of Quebec in mathematics (Council of Ministers of Education, Canada 2016). Interestingly, Lefebvre, Merrigan and Verstraete (2011) find positive test score effects of secular private high schools in

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[1] The size of the typical Canadian postal code is similar to those that have been used as geographic controls in studies of pupil achievement in England, which typically include 15 contiguous housing units (see Gibbons and Silva 2011). In contrast, the average U.S. zip code includes a population in excess of 7000 (https://www.zip-codes.com/zip-code-statistics.asp).

Quebec, which provides a similar universal subsidy to private schools. Studies of other jurisdictions that provide universal subsidies to private schools find little or negative test score effects associated with secular and religious private schools (see Nghiem et al. (2015) for evidence from Australia and Hinnerich and Vlachos (2017) for Sweden).[2] Our results also paint a more positive picture of private school effectiveness than the literature on targeted vouchers in the U.S., which uses lottery designs or discontinuities to identify the effect of private schools on test scores.[3] While these estimates are highly credible, the effects they identify may not generalize: schools that participate in small-scale voucher programs may not be representative of private schools that receive funding under a universal program (e.g., Abdulkadiroglu, Pathak,

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

[3] Earlier studies of New York, Washington D.C. and Dayton, Ohio's voucher programs find small average effects, with somewhat more promising results for some subgroups (see Epple, Romano, and Urquiola 2017 for a review). Other studies find negative effects of voucher schools in Ohio (Figlio and Karbownik 2016), Indiana (Waddington and Berends 2018) and Louisiana (Abdulkadiroglu, Pathak, and Walters 2018). However, Mills and Wolf (2017) find that the year-three results of the Louisiana program are less negative than those from the first year of the program and may be neutral or even slightly positive.

and Walters 2018); and the effects of these schools for voucher students may differ from their average effects.[4]

We then investigate variation in effectiveness within the private school sector, beginning with heterogeneity among four private school types. One group stands out as being little or no more effective on average than the average public school: Christian schools from non-Catholic denominations. The other three groups - Catholic schools, non-Christian faith schools (Sikh, Jewish, Muslim) and secular "prep" private schools - all outperform public schools on average, with similar effect sizes among the three groups. In the case of Catholic schools, the positive and moderately large effect we estimate (0.18 standard deviations in reading and 0.27 standard deviations in numeracy) stands in contrast to previous work that finds negligible or small negative effects on the average achievement of students at private Catholic schools in the U.S. (Jepsen 2003; Altonji, Elder, and Taber 2005; Carbonaro 2006; Lubienski, Crane, and Thule 2008; Reardon 2009; Elder and Jepsen 2014).[5]

[4] For example, peer effects may depend both on the share of high or low ability peers and on a student's own ability level (Hoxby and Weingarth 2006; Carrell, Fullerton, and West 2009; Imberman, Kugler, and Sacerdote 2009; Lavy, Paserman, and Schlosser 2012; Lavy, Silva, and Weinhardt 2012; Burke and Sass 2013; Fruehwirth 2013; Feld and Zolitz 2017; and Garlick 2018).

[5] A smaller literature on longer term outcomes finds positive effects of Catholic schools on high school graduation, college attendance and earnings (Evans and Schwab 1995; Neal 1997;

We extend our investigation of heterogeneity by estimating a full set of school effects for the universe of public and private schools. With an average of 150 observations per school, these estimates are in general precise enough to allow us to investigate a key question– whether private school students enroll in schools that are more effective than their local public alternatives when they opt out of the public system.[6] Compared to their guaranteed public school, we estimate that roughly 60 percent of private school students attend a school that is (statistically significantly) more effective in reading, while 14 percent attend a school that is *less* effective in reading; 54 percent attend a school that is more effective in numeracy while 22 percent attend a school that is less effective in numeracy. Students enrolled in non-Catholic Christian schools are substantially more likely than other private school students to be enrolled in a school that is less

Altonji, Elder, and Taber 2005). Using a lottery-based design Chingos (2018) finds no effect of attending a voucher school on college enrolment.

[6] Our data include more than seven times the number of students enrolled in private Catholic schools than the data used by Elder and Jepsen (2014) and Nghiem et al. (2015), and more than nine times the number of students enrolled in private secular schools than the data used by Lefebvre, Merrigan, and Verstraete (2011) and Nghiem et al. (2015). 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).

effective than their guaranteed public alternative, and substantially less likely to be enrolled in a more effective school.[7]

These results demonstrate that most families who choose private schools do not have to trade off academic quality in order to gain access to other characteristics of private schools that they value. Those families who do choose less effective private schools may be poorly informed (Kane and Staiger 2002; Neilson, Allende and Gallego 2019) or may believe that these schools have other characteristics that they value (e.g., Hastings and Weinstein 2008; Burgess et al. 2015; Beuermann et al. 2022; Ainsworth et al. 2020). Understanding the motivation for school choice is important: if, as recent evidence suggests, parents base their choice on peer quality rather than school effectiveness (Abdulkadiroglu et al. 2020), school choice does not incentivize school effectiveness regardless of whether peer quality and school effectiveness are correlated. Interestingly, we find that prep school students are least likely to attend a school with relatively lower mean test scores than their guaranteed school, but some of these students are enrolled in schools that are no more effective than their guaranteed school. This pattern raises the possibility that some prep schools maintain a reputation for quality by using selective admissions policies rather than providing value-added (MacLeod and Urquiola 2015). In contrast, a sizeable share of non-Catholic Christian students attends a school that is both no more effective than their guaranteed public school and where mean test scores are lower. Given the widespread

[7] It is worth bearing in mind that we are measuring effectiveness via value-added between grades 4 and 7; school effectiveness in earlier grades, unobserved in our data, may or may not be correlated with effectiveness in later grades.

availability of school-level test scores data in B.C., it seems unlikely that these families are poorly informed. More likely, these families prioritize faith instruction or believe that their school is more effective in other dimensions that they value. Recent evidence that schools vary in how effectively they shape a range of non-tested or non-cognitive outcomes that are correlated with long-run outcomes (e.g., Beuermann et al. 2022; Beuermann and Jackson 2020; Jackson et al. 2020a, b) suggests that the test score value-added that we study is indeed only one important dimension of school effectiveness.

Finally, we use our estimates of school effects to explore several hypotheses about what factors may contribute to the success of highly effective private schools. We focus our investigation on observable school characteristics that can be influenced by tools uniquely available to private schools. Despite their positive performance on average, we find that many faith schools perform poorly relative to the average public school in our study. This result suggests that faith education does not provide a sure path to school effectiveness. We further find that private schools that spend more than public schools on operating expenses are no more effective on average than private schools that do not, and that some of the relative private school advantage is associated with enrolling fewer and more academically homogeneous students. We discuss other potential mechanisms and their implications for policy in our concluding comments.

<A>2 Institutional context

<B>Public school choice and funding During the period of study, students in B.C. were guaranteed access to a single public school based on their attendance zone. Before July 2002, enrolment in a public school serving a different attendance zone required permission from the principals of both the guaranteed school and the preferred school. Since July 2002, students have been free to enroll in any attendance zone school in the province that has space available after guaranteed students have enrolled. School transportation is not provided. When these schools are over-subscribed, principals must prioritize within-district students and school boards may choose to prioritize siblings of current students. Within these categories, principals have discretion in enrolment choices. Parents may also choose a public magnet program. French Immersion, which is the most popular, attracts about 10 percent of Kindergarten students in B.C. (B.C. Ministry of Education 2011). Early entry into this program occurs in Kindergarten or grade 1, and space is often allocated by lottery. Entry into a small number of “late” French Immersion programs occurs at the beginning of grade 6.

Along with capital funding, the B.C. Ministry of Education provides a per-student operating grant to school districts, with a supplement for each student who is Aboriginal, gifted or disabled, or in an English as a Second Language (ESL) program. Public districts and schools have no authority to raise any additional revenue and are required to offer the provincial curriculum. Hiring, firing and remuneration of public school teachers is governed by strict rules specified in a collective agreement between the Province and the powerful union that represents B.C. public school teachers.

<B>Private school choice and funding

B.C. has provided public grants to private schools since 1977 (Federation of Private Schools Associations 2015). These per student grants are pegged at 50 percent of the corresponding grant to public schools when a private school’s operating costs do not exceed the average operating cost of public schools in the same district, and at 35 percent when they do (B.C. Ministry of Education 2005). The total amount of funding allocated to private schools is not capped, and publicly funded private schools are not constrained in their selection of students. In 2005, the supplemental grant for special education students in private schools was increased from half to the full amount paid to public schools.

Private schools must operate as non-profits, offer the provincial curriculum, hire qualified B.C. teachers and participate in standardized tests. 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 schools in our sample serve a variety of faith communities, including Catholics, Protestants, Sikhs, Jews and Muslims. Secular schools include academically focused "prep schools" and a small number that offer Montessori or Waldorf programs or programs for students with special learning needs. Tuition fees range widely, from roughly one thousand dollars at some faith schools to $20,000 or more at top-ranked prep schools. Private schools also receive donations from individuals, foundations and organizations. Catholic schools, for example, receive construction funding from both the Diocese and the local parish, and further capital and operating funding from the parish (Catholic Private Schools 2017).

<B>Testing and accountability

B.C. requires public and provincially funded private schools to administer standardized reading and numeracy tests in grades 4 and 7 each year. Centralized grading ensures that a consistent standard is applied across schools.[8] Test 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, a private 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) that 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 and they are readily available online.

<A>3 Data

Our estimates are based on two administrative databases collected and maintained by the B.C. Ministry of Education. The student-level enrolment database records school enrolment as of September 30. Our extract consists of five cohorts of grade 4 students who were enrolled in a public or private school in our region of study[9] between 1999/2000 and 2003/2004, and follows

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[8] Hinnerich and Vlachos (2017) 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.

[9] Our region of study includes fourteen school districts in B.C.'s Lower Mainland area, which consists of the city of Vancouver and 15 surrounding municipalities. Its population of about 2.5

them for the next four years. Students remain in our data unless they leave the region. Individual records include information about the language spoken at home, gender and indicators of whether the student self-identified as Aboriginal in any year, was registered in ESL or special education (i.e. a gifted or disabled program), or was enrolled in French Immersion, along with their 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 available in a separate online appendix that can be accessed on *Education Finance and Policy's* Web site at https://direct.mit.edu.edfp.

The second database provides student-level data on participation and scores on standardized tests in grades 4 and 7. We merge students' test scores with the enrolment database via the unique student identifier provided in both files. Since we follow each of our five cohorts of grade four students for four years (through grade 7), we have grade 4 test scores for the 1999/2000 through 2003/04 school years and grade 7 test scores for 2002/03 through 2006/07. Test scores are normalized to have a mean of zero and standard deviation of one in each year at the provincial

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million in 2007 was roughly comparable to that of the Denver, Baltimore or Pittsburgh MSAs. 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, forming a continuous and distinct commuting zone.

level. As our sample is an extract from the provincial data, the within-sample means and standard deviations differ slightly from these values.

<A>4 Methodology

Our approach to identifying school effectiveness relies on demonstrating the robustness of our estimates across two specifications of the test score model that differ in their identifying assumptions, and by providing evidence in support of these assumptions where possible.

The widely used value-added model (Koedel, Mihaly, and Rockoff 2015) relies on the assumption that selection on unobserved time-invariant student ability is fully accounted for by lagged test scores and observed student characteristics. With observations for each student in grades 4 and 7, we can estimate this model of grade 7 test scores:

$$y_{i7}^{j} = \alpha SchoolType_{i7} + \lambda f(y_{i,4}^{j}, y_{i,4}^{k}) + X_{i7}'\beta + \varepsilon_{i7}^{j} \qquad (1)$$

where y_{i7}^{j} is the test score of student i in subject j in grade 7, $SchoolType_{i7}$ is an indicator of the type of school attended in grade 7, X_{i7} is a vector of individual and neighborhood characteristics (home language, Aboriginal identity, gender and postal code) and cohort effects, $f(y_{i,4}^{j}, y_{i,4}^{k})$ is a polynomial in grade 4 test scores in subjects j and k and ε_{i7} is a stochastic error. We initially define $SchoolType_{i7}$ as an indicator of private versus public school attendance. In other specifications, we include indicators of private school type or a full set of school fixed effects.

The second model controls directly for unobserved time-invariant student ability via student fixed effects:

$$y_{ig}^{j} = \alpha SchoolType_{ig} + Z'_{ig}\beta + grdyr_{t(ig)} + student_i + \varepsilon_{ig}^{j} \qquad (2)$$

where y_{ig}^{j} is the test score of student i in subject j in grade $g = 4{,}7$, $SchoolType_{ig}$ is an indicator that of the type of school attended in grade g, Z_{ig} is a vector of time-varying individual and neighborhood characteristics, $grdyr_{t(ig)}$ is a grade-by-year fixed effect, $student_i$ is a student fixed effect, and ε_{ig} is a stochastic error.

The OLS estimate of the coefficient of interest in both models, α, will capture differences in school effectiveness across sectors that come about via differences in school inputs and differences in peer effects. To the extent that competition from private schools leads to improved public school quality, it will underestimate the true effect of private schools on student achievement (see Urquiola 2016 for a review of relevant empirical evidence).

Apart from being a possibly incomplete proxy for unobserved student characteristics, the inclusion of lagged test scores in the value-added model may introduce bias, since test scores are prone to measurement error. Despite these issues, a number of studies of U.S. pilot and charter schools have validated the use of the value-added model with observational data by comparing their results to those of lottery designs (Bifulco, Cobb, and Bell 2009; Abdulkadiroglu et al. 2011; Angrist, Pathak, and Walters2013; Angrist et al. 2016, 2017; Dobbie and Fryer 2013; Deming 2014).[10] The student fixed effects model constrains the coefficient on the lagged test

[10] Unlike most studies that estimate school and teacher effects using student-level data from adjacent years (i.e. *t* and *t-1),* the testing regime in our environment requires us to use test scores from *t* and *t-3.* Fazlul et al. (2021) investigate the correlation between standard estimates of

score to be zero – learning is not allowed to be cumulative. This restriction is supported by growing evidence that the effects of lagged inputs in the education production function dissipate rapidly (see for example Kane and Staiger 2008, Jacob, Lefgren, and Sims 2010, and Andrabi, Khwwja, and Zajonc 2011, in the context of teacher effects). Since we observe test scores three years apart, any bias introduced by this restriction relative to the value-added model is likely to be very small.

The key to understanding the remaining differences between the identifying assumptions in the two models lies in the fact that, while the value-added model uses identifying variation both among students who do not change school type between observed test years (stayers) and among those who do (movers), the student fixed effects model uses only the latter. Both estimators remain valid under broad patterns of inter-sector mobility among movers. If more students move from public schools to private schools than vice versa, for example, this would not violate the identifying assumption in either model – the student fixed effect model conditions on the actual sequence of schools at which each student is observed, and the value-added model does so implicitly via the lagged test score. Both estimators are valid even if mobility rates differ among high and low ability students, or if high ability students are more likely to move to private schools. School mobility may also depend on fixed or time-varying non-academic characteristics of school types.

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school and district effects and estimates using lagged *t-1* versus *t-2* test scores. They find that these correlations are high.

To better understand potential sources of bias in each model, we decompose the error term in equations (1) and (2) as:

$$\varepsilon_{ig} = \phi_{is(g)} + \mu_{ig} + \zeta_{ig} + \varsigma_{ig=7} \tag{3}$$

where $\phi_{is(g)}$ is a match-specific effect between student i and the school sector type s that he/she attends in grade $g = 4{,}7$; μ_{ig} is a moving cost incurred if a student changes school sectors shortly before grade g; ζ_{ig} captures transitory shocks to student achievement; and $\varsigma_{ig=7}$ is an innate student learning speed effect that affects grade 7 test scores. We assume that $\phi_{is(g)}$, ζ_{ig} and $\varsigma_{ig=7}$ have mean zero for every student, grade, and school type in our sample. We discuss the potential for each of these error components to bias our estimates of private school effectiveness in turn. In Section 5, we present a battery of robustness checks that address these sources of concern.

We first consider potential bias associated with students who don't change school sectors between grades 4 and 7, i.e., "stayers". For these students, who contribute to identification of private school effects only in the value-added model, we are concerned about selection with respect to components of the error term that affect student test scores differently in grade 4 and 7 and therefore contribute to test score growth. Since match quality is the same in both grades when students don't change school sectors, $\phi_{is(g=4)} = \phi_{is(g=7)}$, it is not a potential source of bias among stayers. The remaining three components of our error term in equation (3) vary across grades within a student/sector pair and therefore may affect test score growth among stayers. If a move to a new school sector prior to grade 4 disrupts learning more in the short-run than in the longer-run, test scores of students who change school sectors will be depressed in grade 4 relative to grade 7, $|\mu_{i4}| > |\mu_{i7}|$, and their test score growth will be overestimated. These moving costs will bias the value-added estimator if they are systematically related to enrolment in one sector rather than the other. For example, if more students move from public to private schools prior to grade 4 than vice versa, we will overestimate the relative quality of private schools. Idiosyncratic shocks, ζ_{ig}, affect test scores differently in grades 4 and 7, by definition. They can bias the value-added estimator if the difference between the average shock in grades 4 and 7, which will affect mean test score growth, varies systematically across sectors. We can think of no obvious story for why this might be the case among students who don't change sectors. Finally, student learning speeds, $\varsigma_{ig=7}$, affect test score growth by definition. If innately faster learners sort into private schools prior to grade 4 and remain there, for example, $\varsigma_{ig=7}$ will be correlated with the private school indicator in grade 7 and the value-added model will overestimate the relative effectiveness of private schools among stayers.

We next consider potential sources of bias that could arise in both specifications from sorting among students who change sectors between grades 4 and 7. Again, moving costs, idiosyncratic shocks and innate learning speeds have grade-specific effects and therefore may contribute to test score growth. Unlike the case of stayers, match effects also vary across grades for students who switch sectors and therefore contribute to test score growth for these students. Among sector movers, each of these student-by-grade varying shocks will bias our estimators if they are correlated with student-by-grade variation in private school enrolment:

$$\mathrm{cov}[(\varepsilon_{ig}, private_{ig})|private_{i7} \neq private_{i4}] \neq 0$$

There are two ways this might occur in our model. First, consider the case where the expected value of grade-specific shocks (ε_{ig}) among sector movers varies across grades but is the same for students who move from private to public schools as for those who move from public to private. In this scenario, student-by-grade level shocks will be correlated with private school enrolment only if the number of students who move from private to public schools between grades differs from the number who move from public to private. For example, suppose sector movers are positively selected with respect to test score growth:

$$E[(\varepsilon_{i7} - \varepsilon_{i4})|private_{i7} \neq private_{i4}] > 0$$

Our estimators will attribute this component of test score growth to the relative effectiveness of the school sector that the student attends in grade 7. If the number of movers in each direction is the same, then the upward bias in the estimate of private school effectiveness among movers who attend private school in grade 7 is exactly offset by the upward bias in the estimate of public school effectiveness among movers who attend public school in grade 7. However, if a greater number of students move from public to private schools than vice versa, then we will overestimate the effectiveness of private schools relative to public schools, with the magnitude of the bias increasing in the share of movers who switch from public to private schools.

The second case arises when the expected value of grade-specific shocks among sector movers varies in ways that are different for students who move from private to public schools than for those who move from public to private:

$$E[(\varepsilon_{i7} - \varepsilon_{i4})|private_{i7} = 1, private_{i4} = 0] \neq E[(\varepsilon_{i7} - \varepsilon_{i4})|private_{i7} = 0, private_{i4} = 1]$$

In this case, student-by-grade level shocks will be correlated with student-by-grade variation in private school enrolment regardless of the number of students who move in either direction, and both estimators will be biased among movers. Given our definition of ε_{ig} in equation (3), this implies, for example, that our estimators will overestimate the relative effectiveness of private schools among movers if the gain in match quality, $\phi_{is(7)} - \phi_{is(4)}$, is greater on average among public to private movers than among private to public movers, if moving costs that affect grade 7 test scores, μ_{i7}, are smaller on average among public to private movers,[11] if a negative idiosyncratic shock to grade 4 test scores, ζ_{i4}, is more likely to precipate a move from the public to the private sector than vice versa, or if students with higher innate learning speeds, $\varsigma_{i(g=7)}$, are more likely to move from the public sector to the private sector than vice versa.

<A>5 Results

<B>Descriptive statistics

Our population of interest is all students enrolled in grade 4 in 1999/2000 through 2004/2005, and who advance one grade in each of the following three years, to grade 7.[12] We exclude students who enrolled in a francophone school board, a Montessori or Waldorf private school, or who attended a school that enrolled fewer than five students in seventh grade in a given year.[13]

[11] This might occur if, for example, a greater share of private to public moves occurred between grades 6 and 7 rather than between grades 4 and 5, compared to public to private moves.

[12] Grade repetition and accelerated advancement through grades are infrequent in B.C. elementary schools. All but 1.7% of public school students and 2.2% of private school students in our data extract advance one grade in each of the three years following grade 4.

[13] Instruction is offered in French to non-francophone students via French Immersion programs. Francophone students may choose to attend one of a small number of schools operated by the

The enrolment-based restriction removed a small number of private schools that offer services exclusively to students with special needs from our sample.

Panel A of Table 2 presents selected school characteristics by school type. The students in our population of interest attend 767 different schools, of which 655 are public and 112 are private. Of the private schools, 39 are Catholic, 46 are associated with other Christian denominations, 10 are associated with other faiths, and 17 are secular prep schools. Over 80% of private schools are in funding group 1, qualifying for a per student subsidy equal to 50% (versus 35%) of the public school subsidy because their operating costs do not exceed those of public schools in their district. Most schools in our data offer Kindergarten through grade 7. Of the 655 public schools in our sample, 70 offer grade 7 but not grade 4 and 118 offer grade 4 but not grade 7. Of the 112 private schools, four offer grade 7 but not grade 4, and three offer grade 4 but not grade 7. The typical grade configuration among these schools is Kindergarten to grade 4 or 5, followed by grade 5 or 6 to grade 8, although there are many combinations observed in the data.

Panel B of Table 2 presents descriptive statistics for the population of interest when observed in grade 7. Over 10 percent of grade 7 students attend a private school. Those who are enrolled in private prep schools are the most positively selected with respect to several observable characteristics that have a known association with test scores (see Friesen and Krauth 2011). Students enrolled in Catholic schools are also positively if slightly less strongly selected, students enrolled in non-Christian faith schools are less positively selected, and those enrolled in other Christian schools are not strongly selected. The remaining rows of Panel B show that

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public francophone school board. We exclude Francophone public schools and Waldorf or Montessori private school from our analysis because they follow a different curriculum.

private school students on average achieve higher test scores than public school students, but this difference varies by school type. Students enrolled in prep schools excel; their average grade 7 test scores are 0.87 and 0.92 standard deviations above the overall provincial mean in reading and numeracy. On average, Catholic school students score 0.44 and 0.54 standard deviations higher than the overall provincial mean in reading and numeracy respectively, other Christian school students score 0.27 and 0.26 standard deviations higher, and non-Christian faith school students score 0.35 and 0.48 standard deviations higher. The share of public school students with missing grade 7 test scores is 7.5% in reading and 8.8% in numeracy, more than twice the corresponding shares of private school students. Figure 1 presents kernel density estimates of student-weighted distributions of school mean test scores of public and private schools. These estimates reveal that only a small proportion of private school students are enrolled in a private school where the mean test score is less than the average public school mean test score.

Panel C shows that almost 15% of the students who attend a private school in grade 7 attended a public school in grade 4, while 1.5% of those who attend a public school in grade 7 attended a private school is grade 4. As the number of public school students in grade 7 is about 10 times the number of private school students, the number of public to private movers is roughly the same as the number of private to public movers. Those who switched from public to private schools earned lower test scores on average and were more likely to have a missing test score in grade 4 than private school stayers, while those who switched from private to public schools earned higher test scores on average and were less likely to have a missing test score in grade 4 than public school stayers. Among movers, those who switched from public to private schools had higher grade 4 test scores but were also more likely to have a missing score than those who switched from private to public schools. A full set of demographic characteristics of movers and stayers in each sector can be found in Table 1 of the online appendix.

<B>Estimates of the relative effectiveness of public and private schools

Table 3 presents our estimates of the effects of attending a private school on test scores. The top two panels show results from the valued-added model, first without and then with individual covariates. All specifications of the value-added model include a private school indicator, quadratics in lagged reading and numeracy test scores and year effects. Columns 2 and 4 add postal code indicators. The third panel of Table 3 show results from the student fixed effects model. Two key points emerge from this table. First, estimated private school effects are positive and statistically significant in all specifications. Second, when we control for selection on neighborhood-level unobservables via postal code fixed effects, the value-added and student fixed effects specifications yield very robust, and very similar, estimates: 0.148 versus 0.133 in reading and 0.147 versus 0.119 in numeracy.

While the differences in magnitudes of the private school effects across the two models are not large enough to lead to qualitatively different conclusions, we provide further estimates with the goal of shedding light on some of the factors that might explain why the value-added model yields slightly larger point estimates than the student fixed effects model. Along with differences in the method for controlling for individual heterogeneity, the value-added and student fixed effects estimates reported in Table 3 are estimated for a different sample of schools[14] and are

[14] The value-added specification is estimated on a sample of students enrolled in grade 7, and thus the estimated private school effect is identified from the test scores of students enrolled in

identified from a different group of students (sector stayers and movers versus sector movers only).[15] To understand how this might contribute to differences in the results, we begin by restricting our sample to students who attend schools that offer both grades 4 and 7, in order to make the estimates from the two models more directly comparable. In the first two columns of Table 4 we present estimates from the value-added model in this sub-sample of schools for sector stayers and sector movers only, and in the third column we present estimates from the fixed effects model.[16] Differences between the estimates in columns 1 and 2 reflect any treatment effect heterogeneity between sector movers and stayers, as well as any differences in sources of selection bias that arise from estimating the value-added model on movers versus stayers (as discussed in Section 4). The estimates in columns 2 and 3 are both identified from movers only, so differences between them reflect neither treatment effect heterogeneity nor differences in selection bias. Instead, they reflect differences that arise from using students fixed effects versus lagged test scores to control for unobserved student-level heterogeneity among movers. In numeracy, the point estimates are very robust across columns, ranging from 0.101 to 0.118. In

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schools that offer grade 7. Some of these schools do not offer grade 4. The student fixed effects specification is estimated on the same sample of *students*, but the sample comprises both their grade 4 observations and their grade 7 observations. It thus identifies the private school effect from a broader set of schools, including some that offer grade 4 but not 7.

[15] As detailed in Table 1 of the online appendix, the average characteristics of students who move between the public and private sectors tend to lie somewhere between those of private school stayers and public school stayers.

[16] Complete estimates of this specification are available on request.

the case of reading, the point estimate in column 2 (valued-added model, movers only; 0.189) is substantially larger than the point estimates in columns 1 (value-added model, stayers only; 0.132) and column 3 (student fixed effects model, identified off movers only; 0.128). Heterogeneous effects between movers and stayers cannot account for this pattern of results, nor can selection bias (estimates in columns 1 and 3 are similar although estimated from different samples that are subject to different sources of selection bias). Therefore, the most likely explanation for the anomalous results in column 2, and likewise for the small differences between the estimates from the value-added and student fixed effects models in the full sample, is differences in the extent to which the unobserved heterogeneity among movers is absorbed by grade 4 test scores versus the student fixed effect.[17] In this respect, the fixed effects specification is more general, since it controls for all time-invariant unobserved heterogeneity that might be related to selection into private schools, not just heterogeneity that is correlated with grade 4 test scores.

In the remaining columns of Table 4 we report estimates from the student fixed effects model for sub-samples that are restricted in ways that are meant to limit potential threats to identification. As described in Section 4, the fixed effects estimator will be biased if a negative idiosyncratic

[17] The observed pattern of larger point estimates for movers compared to stayers in the value-added model is also consistent with a model in which the benefit from attending a private school takes the form of a once-and-for-all bump to achievement. For private school stayers, this bump to achievement will be absorbed by the lagged grade 4 test score while for movers it will not. However, this story cannot explain why the effect size for movers in the student fixed effects model is smaller than the effect size for movers in the value-added model.

shock to grade 4 test scores, ζ_{i4}, is more likely to precipate a move from the public to the private sector than vice versa, or if moving costs that affect grade 7 test scores, μ_{i7}, are smaller on average among public to private movers than vice versa. The sub-sample used to produce the results in column 5 excludes students who changed school sectors immediately after grade 4, eliminating the first potential source of bias; the sub-sample used to produce the results in column 6 excludes students who changed school sectors immediately after grade 6, eliminating the second potential source of bias. The point estimates in column 5 are slightly smaller than the full sample results reported in column 4 (0.123 versus 0.133 in reading; 0.111 versus 0.119 in numeracy), and the point estimates in column 5 slightly larger (0.138 versus 0.133 in reading; 0.131 versus 0.111 in numeracy). This provides reassuring evidence that these particular forms of selection bias are not driving our results in important ways. In column 7 we report estimates for a sub-sample of compulsory movers - students who are required to change schools between grades 4 and 7 because their grade 4 school does not offer grade 7. Sector moves among these students are less likely to be systematically related to idiosyncratic shocks to grade 4 or 7 test scores than voluntary moves. Again, the estimates from this sub-sample are reassuringly similar to those from the other samples. Estimates of the value-added model on these sub-samples are also similar (see Table 2 of the online appendix).

As described in Section 4, our estimates of private school effects among movers will be biased even if the selection process is the same for private to public and public to private movers, if the sizes of the two groups differ. As reported in Table 1 of the online appendix, the difference in group size is fairly small (1620 public to private movers versus 1400 public to private movers). Define:

$$\theta = E[(\varepsilon_{i7} - \varepsilon_{i4})|private_{i7} \neq private_{i4}]$$

For students who move from public to private schools, the average change in test scores will be the private school effect plus the mean change in the error term among sector movers, $(\alpha + \theta)$, while for students who move from public to private schools the average change in test scores will be $(-\alpha + \theta)$. With data on the average change in test scores for each group of movers, we can use these expressions to solve for θ and α. This estimator of α is unbiased even when the number of movers in each group differs. Panel A of Table 5 shows the results of this calculation from the raw data. The average gain in test scores among students who move from public to private schools is 0.174 in reading and 0.194 in numeracy; the loss in test scores among students who move from private to public schools is 0.082 in reading and 0.052 in numeracy. The implied estimate of the private school effect, $\hat{\alpha}$, is 0.128 in reading and 0.123 in numeracy. These point estimates are very similar to the point estimates for movers from the student fixed effects model presented in column 3 of Table 4. They also suggest that sector movers are positively selected relative to stayers with respect to unobserved factors that affect test score growth ($\hat{\theta} > 0$), e.g., due to improvements in match quality. In Panels B and C of Table 5, we undertake a similar exercise conditional on observable characteristics using our regression specifications. The specification in panel B is the same as Table 3 Panel B, but we have replaced the single indicator for attending a private school in grade 7 with three indicators for the sector (public/private) of the grade 4 and 7 schools; the reference category is students that attended public school in both grade 4 and grade 7. The specification in Panel C is the same as Panel B, but the dependent variable and all control variables are measured in differences between grade 7 values and grade 4 values; this is equivalent to a model with student fixed effects, estimated on first differences. The magnitudes of the private school effect implied by these specifications when we account non-random selection of sector movers are very similar to the Panel A estimates based on the raw data, and to our main estimates in Tables 2 and 3. Moreover, the results from the raw data, the value-added model, with and without postal codes fixed effects and the student fixed effects model, with and without postal codes fixed effects are remarkably robust, ranging between 0.12 and 0.13 standard deviations in both reading and numeracy.

<B>Heterogeneity among private schools

Important forms of heterogeneity in private school effects emerge when we disaggregate them by private school type. The results presented in Table 6 are based on a value-added specification that includes postal code indicators and where we have replaced a single indicator for attending a private school with a vector of indicators corresponding to four categories of private school: Catholic, other Christian, other faith and prep schools.

We find positive, statistically significant and substantial effects for Catholic schools, with effect sizes of 0.184 standard deviations in reading and 0.270 standard deviations in numeracy. While affiliation with a Catholic community or Catholic religiosity has been shown to be positively associated with academic results after controlling for a range of covariates (Altonji, Elder, and Taber 2005), we are aware of no formal or even anecdotal evidence that would suggest that this form of selection bias could account for effects of these magnitudes. Point estimates for other (non-Christian) faith schools and prep schools are similar in magnitude to those for Catholic schools, with effect sizes ranging from 0.219 to 0.244 standard deviations. In contrast, effect sizes for schools associated with non-Catholic Christian denominations are small in reading (0.064 standard deviations) and effectively zero in numeracy. As a robustness check, we re-estimate this model on a sample that excludes movers. Consistent with the results reported in columns (1) and (2) of Table 4, the results for the disaggregated private school group are slightly smaller when estimated from stayers only (see Table 3 of the online appendix).

We further explore heterogeneity in private school effectiveness by estimating equation (1) with a full set of school fixed effects in place of the $SchoolType_i$ indicator. Figure 2 presents kernel density estimates of the distributions of the estimated fixed effects for private and public schools respectively.[18] The school-weighted distributions in Panel A show substantial overlap: many public schools outperform the average private school, and many private schools fall short of the average public school. A comparison of these figures to their student-weighted counterparts in Panel B shows that enrolment in private schools where the point estimate of the school effect is smaller than that of the average public school in reading is relatively small, but this is less obviously the case for numeracy.

Whether attending a private school improves a student's test scores will depend on the effectiveness of the school they attend relative to a counterfactual school. We investigate this question using a wild bootstrap inference procedure (clustered at the school level) to test whether the fixed effect of the school that each child i attends, $\psi_{s(i)}$, differs significantly from that of their guaranteed public school, $\psi_{p(i)}$.[19] As reported in columns (2) and (5) of Table 7, more than

[18] We normalize estimated school fixed effects to have zero mean in our sample. The public school means are -0.019 in reading and -0.017 in numeracy; the private school means are 0.155 in reading and 0.137 in numeracy.

[19] Our inferences are based on a wild cluster bootstrap as described in Cameron and Miller (2015) and MacKinnon (2015). Define $d_i = \psi_{s(i)} - \psi_{p(i)}$. We test the one-sided null hypotheses

half of private school students attend a school for which we reject the one-sided hypothesis that $\psi_{s(i)} \leq \psi_{p(i)}$ at the 5% level of significance, i.e., where we are confident that the private school they attend is more effective than their guaranteed public school. This share is below 50 percent only among students attending non-Catholic Christian schools. Among those attending other types of faith schools (including Catholic schools), roughly two-thirds attend a school that is statistically significantly more effective than their guaranteed public school in reading and two-thirds attend a school that is statistically significantly more effective in numeracy. Conversely, the share of private school students who attend a school that we can be confident is *less* effective than their guaranteed public school is 13.6% in reading and 22.1% in numeracy. This share is

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$H_0: d_i \geq 0$ and $H_0: d_i \leq 0$ for each student. Our inference procedure is as follows. We estimate the specification with school fixed effects to obtain the vector of estimates $\hat{d}_i = \hat{\psi}_{s(i)} - \hat{\psi}_{p(i)}$, and construct the Wald statistics $w_i = \hat{d}_i / se(\hat{d}_i)$, where $se(\hat{d}_i)$ is a cluster-robust estimate of the standard error of $\hat{d}_i$. Then, in each replication $b = 1, \ldots, B$ of the wild cluster bootstrap, we construct the vector of differences $\hat{d}_i^b = \hat{\psi}_{s(i)}^b - \hat{\psi}_{p(i)}^b$ and Wald statistics $w_i^b = (\hat{d}_i^b - \hat{d}_i)/se(\hat{d}_i^b)$ where $se(\hat{d}_i^b)$ is a cluster-robust estimate of the standard error of $\hat{d}_i^b$. For each student i, we define the bootstrap critical values of the Wald statistic, $c_i^{0.05}$ and $c_i^{0.95}$, as the 5th and 95th percentiles of $\{w_i^1, \ldots, w_i^B\}$, respectively. Finally, for each student i, we reject $H_0: d_i \geq 0$ if $\hat{d}_i < 0$ and $w_i < c_i^{0.05}$; and reject $H_0: d_i \leq 0$ if $\hat{d}_i > 0$ and $w_i > c_i^{0.95}$. In columns (1) and (4) of Table 7, we report the proportion of students for whom we reject $H_0: d_i \geq 0$, and in columns (2) and (5) we report the proportion for whom we reject $H_0: d_i \leq 0$.

highest among students attending a non-Catholic Christian school: 20.1% in reading and 38.1 % in numeracy.[20]

We further investigate these school choice decisions by regressing indicators that private school students attend a school that is significantly more effective than their guaranteed public school on a set of student characteristics. The first specification reported in Table 8 controls for student characteristics but does not include guaranteed school fixed effects. We see that higher achieving students are more likely to opt out to a private school than lower-achieving students, and this is true for both "better" private schools and those that are "not better". Students who speak Chinese or Punjabi at home, on the other hand, are less likely to opt out to a private school than those who speak English. Students in English as a Second Language programs are less likely to attend a "not better" private school than non-ESL students, but this characteristic doesn't affect their likelihood of attending a "better" private school. Aboriginal students are somewhat less likely to opt out to "better" private schools than to "not better" private schools. The results from the second specification, which adds guaranteed school fixed effects, are very similar to those from

[20] Results from the student fixed effects model paint a somewhat less favorable view of private school choice. In reading, these estimates indicate that a smaller share of private school students attend a school that is significantly more effective (50% in the student fixed effects model compared to 60% in the value-added model) and a larger share attends a school that is significantly less effective (22% compared to 14%). The pattern is similar in numeracy, although the differences across models are smaller: 47% attend a private school that is more effective (compared to 54% in the value-added model); and 25% attend a private school that is less effective (compared to 22% in the value-added model). Full results are available upon request.

the first specification. This implies that observed sorting into both “better” and “not better” is not driven by differences in the distribution of student characteristics between neighborhoods; rather it reflects patterns of school choice within neighborhoods.

Particular care should be taken when considering the implications of these results for minority students who enroll in private schools that are no more effective at raising test scores than their guaranteed public schools. Recent evidence demonstrates that schools also vary in their effectiveness at shaping a range of non-tested or non-cognitive outcomes that are in turn correlated with long-run outcomes (e.g., Beuermann et al. 2022; Beuermann and Jackson 2020; Jackson et al. 2020a), and that these other forms of effectiveness may be particularly important for disadvantaged or marginalized students (Jackson et al. 2020b). Alternatively, it is possible that private schools that are less effective for the *average* student may not be less effective for the students that choose them. We investigate this issue next, in the context of robustness checks on our main specification.

<B>Robustness checks

<C>*Heterogeneity among students*

Our empirical specification assumes that school effectiveness enters linearly into student test scores, is additively separable from other determinants of test scores, and is homogeneous for all students in a given school or sector. The reality may be more nuanced, and it is possible that private schools might be more effective for some students than others. Indeed, this may explain why a non-trivial subset of private school students are enrolled in schools that appear to be less effective than their guaranteed public school. To address this possibility, we estimate a Oaxaca-Blinder decomposition of the difference in mean test scores between public and private school students that allows the coefficients on lagged test scores and other observable characteristics to differ between public and private school students. This decomposition allows us to estimate the extent to which differences in average test scores between public and private school students are a consequence of differences in their observable characteristics, versus differences in the "returns" to those characteristics in the determination of test scores. The latter allows for a richer notion of school effectiveness, in which private schools have the potential to be more effective for some students than others and is potentially important given that Table 2 indicates there are meaningful differences between the characteristics of public and private school students.

We report this decomposition in Table 9. Focusing on the estimates in column 1, in which we control for neighborhood characteristics using census data, the gap between average public and private school reading scores is 0.394 standard deviations. Of this, 0.237 standard deviations (60 percent) is explained by differences in the observable characteristics of public and private school students, and the remaining 0.157 standard deviations (40 percent) is explained by differences in the returns to those characteristics. However, virtually all of the difference in characteristics (0.213 standard deviations, or 54% of the overall gap) is accounted for by the lower lagged test scores of public school students, with differences in mean neighborhood education and income making up the balance. There is some evidence of meaningful differences in the returns to neighborhood education levels and income in public versus private schools – with differential returns to neighborhood education levels widening the gap and differential returns to neighborhood income narrowing it – but these are imprecisely estimated and partially offsetting. As a consequence, most of the gap between average public and private reading scores "explained" by differences in returns to observable characteristics is in fact accounted for by differences in regression intercepts for the two sectors (0.140 standard deviations, or 33 percent of the total gap). This result is consistent with our assumption that private school effectiveness enters test scores additively. Notably, we see no significant differences in returns to private versus public schools that are specific to minority home language or Aboriginal students; this evidence does not support the hypothesis that the relatively low rates of enrolment in private schools among these students is a response to treatment effect heterogeneity. Estimates for the numeracy specification (column 3) are very similar, as are estimates based on specifications with postal code fixed effects (columns 2 and 4). In the latter, however, postal code fixed effects account for the lion's share of differences between public and private school test scores, reducing the amount explained by lagged test scores by more than half. On the whole, therefore, the estimates in Table 9 do not provide compelling evidence that there is material between-student heterogeneity in private school effectiveness, at least for the characteristics that we observe in our data, nor do they provide compelling evidence against the simplifying assumptions of our model with additively separable school effectiveness.

<C>*Missing test scores and systematic exclusion from testing*

Missing test scores arise in our data when students are excused from a test or are absent, or when data collection and processing errors prevent matching a student's test score to their enrolment record. As reported in Table 2, test scores are more likely to be missing for public school students (7-9%) than private school students (3-4%). This substantial difference raises the concern that students may be systematically excluded from test-taking. If public and private schools rely on different criteria for exclusion, this could bias our estimates of private school effectiveness. To investigate this possibility, we regress our estimated school fixed effects on the school-specific proportion of missing test scores, a private school indicator, and their interaction. For both reading and numeracy, neither the coefficient on the proportion missing nor its interaction with the private school indicator is statistically significant at conventional levels.[21] There is therefore no evidence that schools systematically exclude students from testing in a way that might bias our estimates of public or private school effectiveness.

<B>What accounts for variation in school effectiveness?

The evidence we present above shows that private schools are over-represented in the upper tail of the distribution of academic effectiveness. An important question then is how they achieve this. To better understand the features that are associated with school effectiveness in our data, we estimate the following regression model:

$$\hat{\alpha}_{s(i)} = \phi private_{s(i)} + X'_{s(i)}\beta + private_{s(i)}X'_{s(i)}\gamma + \mu_i$$

where $\hat{\alpha}_{s(i)}$ is the estimated school effect associated with the school s attended by student i, $private_{s(i)}$ is an indicator that school s is private, and $X_{s(i)}$ is a vector of school characteristics. We focus on several measurable school characteristics over which private schools may have more control than public schools. We report selected estimates in Panel A of Table 10, omitting those characteristics that are not statistically or economically significant (full results are available on request). Among the coefficients missing from Table 10, because we found no discernable relationship to school effectiveness, are indicators of the level of per-student operating costs.

[21] The estimated coefficient on the proportion of missing reading scores is 0.049 with a standard error of 0.118; and the estimated coefficient on the interaction term is -0.475 with a standard error of 0.623. The estimated coefficient on the proportion of missing numeracy scores is -0.083 with a standard error of 0.213; and the estimated coefficient on the interaction term is 2.24 with a standard error of 1.85. Standard errors in both regressions are clustered at the school level.

This result is consistent with recent evidence that standard measures of school resources, including per-pupil expenditures and the share of teachers who are certified or have advanced degrees, do not explain variation in effectiveness among charter schools (e.g., Angrist et al. 2013; Dobbie and Fryer 2013). We also found no statistically significant effect of single-sex schooling. Variables that do seem to matter are ones that private schools have greater latitude than public schools to control via admissions procedures: enrolment levels, heterogeneity in home language and dispersion in reading and numeracy scores. Enrolling a larger number of students in total is associated with less school effectiveness, as is greater dispersion in reading scores and home language (in the case of numeracy). Greater dispersion in numeracy scores is associated with greater school effectiveness.

We then estimate a Oaxaca-Blinder decomposition of the mean (private-public) gap in school fixed effects and report the results in Panel B of Table 10. In reading, the raw gap is 0.174 standard deviations. Of this, greater homogeneity in grade 7 reading scores contributes 0.026 standard deviations (14 percent) to the private school advantage and smaller average school size accounts for a further 0.017 standard deviations (9 percent). In numeracy, greater homogeneity in grade 7 reading scores contributes 0.029 standard deviations (18 percent) to the private school advantage and smaller school size contributes 0.028 standard deviations (19 percent). These estimated relationships between school effects and school characteristics obviously cannot be interpreted as causal. However, they suggest that private schools on average are able to create relatively favorable environments for learning by keeping enrolment numbers low and admitting a somewhat more homogeneous set of students. Whether smaller and more homogeneous schools are more likely to implement some of the practices that have been found effective among charter schools, such as frequent teacher feedback, greater instructional time, high expectations, high-quality tutoring, and data-driven instruction (Dobbie and Fryer 2013), remains a question for future work.

<A>6 Conclusion

Our results present a new and more complete picture of the relative effectiveness of private and public schools with respect to reading and numeracy skill development. The public schools that serve as our counterfactual are embedded in a vibrant public school choice environment and achieve excellent results by international standards. When compared to these highly effective public schools, the private schools in our data on average perform even better.

The implications of our results for policy depend critically on the nature of the underlying mechanisms that allow most private schools to succeed. We find no evidence that private schools with higher operating expenditures are more effective than lower-expenditure schools. Most private schools in our data are affiliated with a faith-based organization and many of these schools perform poorly relative to the average public school. This suggests that private faith education does not provide a sure path to school effectiveness, consistent with evidence that *public* faith schools are no more effective than public secular schools on average (Gibbons and Silva 2011). At the same time, we find that many faith schools are highly effective, and we cannot rule out the possibility that those schools' faith-based teachings and practices are important contributors to their success.

Data limitations prevent us from investigating several other important possibilities. On one hand, the relative success of the private school sector may reflect differences in institutional design and practice, conditional on student and teacher composition and on funding levels, that could be replicated in the public sector. Recent evidence of the success of so-called *No Excuses* charter schools in the United States, which employ similar approaches as the stereotypical private school, including the requirement that students wear uniforms, high expectations for student conduct and an emphasis on clear and frequent communication with parents, suggests one set of potential mechanisms that might be emulated by the public sector.[22] Alternatively, the academic success of the average private school may be somehow inherent to the market-based provision of education services, i.e. the market mechanism may simply create stronger incentives for schools to perform academically given the resources that they have, relative even to those created in a vibrant school choice environment. If this is the case, policies that expand the private school sector may improve the quality of education overall. On the other hand, if effective private schools are poaching high quality teachers or peers from public schools, policies that expand the private school sector may harm public education.[23] Identifying the relative importance of these underlying mechanisms should be a priority for future research on private school effectiveness.

[22] See, for example, Dobbie and Fryer (2011) and Angrist, Pathak, and Walters (2013), although evidence from Chabrier, Cohodes and Oreopoulos (2016) is less promising

[23] Behrman et al. (2016) show that private schools in Chile attract better teachers than public schools while drawing higher-productivity individuals into the teaching profession in general.

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Tables

Table 1. Characteristics and program features of some universal voucher programs

Jurisdiction	Scope and history		Share of students enrolled	For-profit	Allowed features		
	Since	Per-student value			Selective admissions	Religious affiliation	Tuition top-up
Chile[a]	1981	100%[b]	47%	Yes	Yes	Yes	Yes
Denmark[a]	1855	~80% [c]	12%	No	Yes	Yes	Yes
Holland[a]	1917	100% [b]	70%	No	Yes	Yes	No
New Zealand[a,d]	1989	~30% [c]	15%	Yes	Yes	Yes	No
Sweden[a]	early 90s	~80%[c]	10%	Yes	No	Yes	No
British Columbia	1977 [e]	33-50% [b,f]	13% [g]	No [e]	Yes [e]	Yes [e]	Yes [e]

Notes: [a]Source: Epple et al. (2017). [b]As share of per-student operating grant to public schools. [c]As share of per-student public school expenditure. [d] Refers to private schools only, exclusive of "integrated" schools. See Epple et al. (2017) for details.[e,g] Source: Federation of Private Schools Associations (2015). [f] Source: B.C. Ministry of Education (2005).

Table 2: Selected school and student characteristics, by school type

	(1) All Public	(2) All Private	(3) Private School Type: Prep	(4) Private School Type: Catholic	(5) Private School Type: Christian	(6) Private School Type: Other faith
A. School characteristics						
No. of schools	655	112	17	39	46	10
Funding group 1	n/a	92	10	37	37	8
Funding group 2	n/a	20	7	2	9	2
Grade 4 only	118	3	1	0	2	0
Grade 7 only	70	4	1	0	2	1
Grades 4 and 7	467	105	15	39	42	9
B. Student characteristics (Grade 7)						
No. of students	94,888	10,967	2,151	4,301	4,028	487
% of the sample	89.6	10.4	2.03	4.06	3.81	0.460
French Immersion	0.084	0.019	0.028	0.035	0.000	0.000
Home language:						
English	0.671	0.739	0.708	0.816	0.719	0.351
Chinese	0.120	0.075	0.121	0.044	0.092	0.000
Punjabi	0.070	0.026	0.031	0.003	0.010	0.324
Other	0.141	0.165	0.152	0.138	0.181	0.329
Aboriginal	0.058	0.008	0.004	0.010	0.009	0.002
Female	0.487	0.492	0.498	0.515	0.459	0.542
Neighborhood mean:						
Immigrant	0.071	0.067	0.072	0.073	0.055	0.097
High school	0.248	0.244	0.227	0.244	0.254	0.236
Some college	0.296	0.281	0.238	0.289	0.301	0.237
Bachelor's	0.172	0.212	0.353	0.201	0.145	0.232
Family income/$10000	6.69	7.87	11.9	7.06	6.66	7.09
Grade 4 reading score	0.018	0.346	0.708	0.276	0.252	0.139
Grade 4 numeracy score	0.069	0.346	0.721	0.266	0.242	0.243
Missing grade 4 reading	0.073	0.030	0.026	0.024	0.035	0.045
Missing grade 4 numeracy	0.078	0.033	0.029	0.028	0.038	0.043
Grade 7 reading score	0.042	0.459	0.870	0.440	0.269	0.347
Grade 7 numeracy score	0.134	0.508	0.917	0.539	0.255	0.479
Missing grade 7 reading	0.075	0.033	0.022	0.030	0.038	0.078
Missing grade 7 numeracy	0.088	0.038	0.026	0.034	0.046	0.064
C. Students who change sectors (public/private) between Grade 4 and Grade 7, by Grade 7 school type						
Share	0.015	0.148	0.260	0.078	0.164	0.129
Grade 4 reading score	0.110	0.224	0.554	-0.003	0.114	-0.510
Grade 4 numeracy score	0.107	0.312	0.604	0.062	0.239	-0.351
Missing Grade 4 reading	0.035	0.064	0.032	0.057	0.086	0.143
Missing Grade 4 numeracy	0.040	0.070	0.036	0.074	0.092	0.127

Notes: see text and Data Appendix for details of sample selection and for variable definitions. Column (1) of Panel C reports characteristics of students who attended a private school in Grade 4 and a public school in Grade 7. Columns (2)-(6) of Panel C report characteristics of students who attended a public school in Grade 4 and a private school of the indicated type in Grade 7.

Table 3: Estimates of private school effects on test scores

Model	(1)	(2)	(3)	(4)
	Reading		Numeracy	
A. Value-added, no individual controls	0.174***	0.144***	0.174***	0.131***
	[0.026]	[0.021]	[0.039]	[0.032]
R^2	0.491	0.685	0.454	0.686
# of observations	92,198	92,198	91,152	91,152
# of unique postal codes		32,908		32,722
B. Value-added, with individual controls	0.174***	0.148***	0.188***	0.147***
	[0.026]	[0.022]	[0.037]	[0.033]
R^2	0.502	0.691	0.480	0.692
# of observations	92,198	92,198	91,152	91,152
# of unique postal codes		32,908		32,722
C. Student fixed effects	0.130***	0.133***	0.127***	0.119***
	[0.027]	[0.021]	[0.033]	[0.026]
R^2	0.839	0.880	0.833	0.876
# of observations	184,396	184,396	182,304	182,304
# of unique postal codes		37,064		36,898
Postal code fixed effects		x		x

Notes: Dependent variable in panels A and B is student's Grade 7 test score. Specification in Panel A controls for year effects and quadratics in grade 4 reading and numeracy scores. Specification in Panel B controls for year effects, quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language, and Aboriginal identity. Dependent variable in Panel C is the student's test score in grade $g = 4, 7$. Control variables in Panel C include grade-by-year fixed effects, English as a Second Language, and a full set of student fixed effects. Standard errors are clustered at the school level and reported in brackets; standard errors for the specification with student and postal code fixed effects are estimated via a cluster-wild bootstrap. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 4: Robustness of estimated private school effects to sources of student mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	Value-added		Student fixed effects				
Sample	Schools with G4 & G7, stayers	Schools with G4 & G7, movers	Schools with G4 & G7, all	All students & schools	Excluding G4/G5 movers	Excluding G6/G7 movers	Compulsory movers only
			Reading				
Private	0.132***	0.189***	0.128***	0.133***	0.123***	0.138***	0.113***
	[0.025]	[0.058]	[0.032]	[0.021]	[0.033]	[0.027]	[0.050]
# of schools	541	544	568	761	745	738	586
# of obs	56,821	17,600	150,883	184,396	165,578	162,630	29,124
			Numeracy				
Private	0.101**	0.110	0.118***	0.119***	0.111***	0.131***	0.090
	[0.040]	[0.071]	[0.038]	[0.025]	[0.038]	[0.036]	[0.063]
# of schools	541	545	569	763	747	738	585
# of obs	56,253	17,309	149,181	182,304	163,788	160,914	28,748

Notes: Specification in columns (1) and (2) is the same as Table 3 Panel B, column 2: dependent variable is student's Grade 7 FSA test score; and control variables include year effects, quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language, Aboriginal identity, and a full set of postal code fixed effects. Specification in columns (3)-(7) is the same as Table 3 Panel C, columns (2) and (4): dependent variable is the student's FSA test score in grade $g = 4, 7$, and additional control variables include grade-by-year fixed effects, English as a Second Language, and a full set of postal code fixed effects. Estimates in column (4) replicate estimates from Panel C of Table 3. Samples used to produce estimates in columns (1)-(3) include only students who attend schools that offer both grades 4 and 7; column (1) further restricts the sample to students who did not change school sectors between grades 4 and 7, column (2) instead restricts the sample to students who changed school sectors between grades 4 and 7. Standard errors clustered at the school level. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 5: Estimates of private school effects and selection effects implied by differences in test score growth between students who move from private to public versus public to private schools

Model	(1) Reading	(2)	(3) Numeracy	(4)
A. Raw data				
Mean change in test scores between grades 4 & 7				
Public to private movers	0.174***		0.194***	
Private to public movers	-0.082***		-0.052*	
Implied private school effect, $\hat{\alpha}$	0.128		0.123	
Implied selection term, $\hat{\theta}$	0.046		0.071	
# of observations	92,198		91,152	
B. Value-added model				
Mean change in test scores between grades 4 & 7				
Public to private movers	0.199***	0.187***	0.179***	0.176***
Private to public movers	-0.060***	-0.061**	-0.084***	-0.082***
Implied private school effect, $\hat{\alpha}$	0.129	0.124	0.132	0.129
Implied selection term, $\hat{\theta}$	0.069	0.063	0.047	0.047
R^2	0.502	0.691	0.480	0.692
# of observations	92,198	92,198	91,152	91,152
# of unique postal codes		32,908		32,722
C. Student fixed effects (first differences)				
Mean change in test scores between grades 4 & 7				
Public to private movers	0.144***	0.137***	0.142***	0.158***
Private to public movers	-0.116***	-0.102***	-0.112***	-0.099**
Implied private school effect, $\hat{\alpha}$	0.130	0.119	0.127	0.129
Implied selection term, $\hat{\theta}$	0.014	0.018	0.015	0.029
R^2	0.022	0.388	0.036	0.427
# of observations	92,198	92,198	91,152	91,152
# of unique postal codes		32,908		32,722
Postal code fixed effects		x		x

Notes: Panel A reports mean changes in test scores between grades 4 and 7. The specification in panel B is the same as Table 3 Panel B, but we have replaced the single indicator for attending a private school in grade 7 with three indicators for the sector (public/private) of the grade 4 and 7 schools; the reference category is students that attended public school in both grade 4 and grade 7. The specification in Panel C is the same as Panel B, but the dependent variable and all control variables are measured in differences between grade 7 values and grade 4 values; this is equivalent to a model with student fixed effects, estimated on first differences. See the notes to Table 3 for additional information about sample and specification. Standard errors are clustered at the school level and reported in brackets; standard errors for the specification with student and postal code fixed effects are estimated via a cluster-wild bootstrap. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 6: Estimates of private school effect on grade 7 test scores from the value-added model, by private school type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reading				Numeracy			
	Catholic	Christian	Other Faith	Prep	Catholic	Christian	Other Faith	Prep
Estimate	0.184***	0.064**	0.219***	0.244***	0.270***	-0.011	0.229**	0.202***
	[0.028]	[0.028]	[0.085]	[0.054]	[0.042]	[0.040]	[0.103]	[0.070]
R^2	0.692				0.693			
# students	92,198				91,152			

Notes: Specification in columns (1)-(4) is the same as reported in column (2) of Table 3 Panel B, except that it includes an indicator for private school type instead of a single private school dummy. Similarly, the specification in columns (5)-(8) is otherwise the same as that reported in column (4) of Table 3 Panel B. Controls include year effects, quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language, Aboriginal identity, and postal code indicators. Standard errors clustered at the school level and reported in brackets. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 7: Share of students enrolled at a school where school effects and mean test scores are significantly above/below their guaranteed public school

	Share of students enrolled at schools where … than at student's guaranteed public school					
	Reading			Numeracy		
	(1) Fixed effect is significantly less	(2) Fixed effect is significantly more	(3) Mean test score is less	(4) Fixed effect is significantly less	(5) Fixed effect is significantly more	(6) Mean test score is less
All private	0.136	0.594	0.144	0.221	0.535	0.252
Prep	0.091	0.694	0.065	0.160	0.561	0.127
Catholic	0.112	0.663	0.118	0.124	0.671	0.216
Christian	0.201	0.445	0.199	0.381	0.347	0.355
Other faith	0.048	0.696	0.277	0.118	0.681	0.277
Public	0.070	0.104	0.136	0.094	0.107	0.154

Notes: Authors' calculations based on estimates from the value-added specification with fixed school effects. This specification is identical to that reported in columns (2) and (4) of Table 3 Panel B, except that we have replaced the private school indicator with a full vector of 766 fixed school effects. See the notes to Table 3 for additional information about specification. In columns (1) and (4), we report the proportion of students for whom we reject the hypothesis that the fixed effect of the school they attend is greater than or equal to the fixed effect of their guaranteed public school at the 5% level of significance. In columns (2) and (5) we report the proportion of students for whom we reject the hypothesis that the fixed effect of the school they attend is less than or equal to the fixed effect of their guaranteed public school at the 5% level of significance. In columns (3) and (6), we report the proportion of students enrolled at a school whose mean test score is less than at their guaranteed public school. Inferences in columns (1), (2), (4), and (5) are based on the wild bootstrap procedure, clustered at the school level, described in footnote 19.

Table 8: Predictors of being enrolled in a private school that is/not significantly more effective than the guaranteed public school

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Enrolled in a more effective private school				Enrolled in a private school that is not more effective			
	Reading		Numeracy		Reading		Numeracy	
Student characteristics								
Grade 4 reading score	0.020***	0.017***	0.016***	0.014***	0.011***	0.009***	0.015***	0.012***
Grade 4 reading score squared	0.001	0.001	0.001	0.000	-0.000	-0.000	0.000	0.000
Grade 4 numeracy score	0.007**	0.004	0.008***	0.005**	0.006**	0.006**	0.005*	0.004
Grade 4 numeracy score squared	0.000	-0.000	-0.000	-0.001	-0.001	-0.000	-0.000	0.000
ESL	-0.012	-0.014	-0.009	-0.009	-0.043***	-0.042***	-0.045***	-0.046***
Chinese home language	-0.031**	-0.062***	-0.034***	-0.053***	-0.026***	-0.029***	-0.025**	-0.040***
Punjabi home language	-0.040**	-0.036**	-0.031*	-0.020	-0.031***	-0.030**	-0.040***	-0.046***
Other home language	0.001	-0.010	0.006	-0.002	0.027	0.029	0.021	0.020
Aboriginal identity	-0.062***	-0.056***	-0.059***	-0.052***	-0.036***	-0.030***	-0.041***	-0.035***
Female	-0.003	-0.003	-0.003	-0.002	-0.000	-0.001	-0.001	-0.002
R^2	0.015	0.085	0.013	0.077	0.012	0.084	0.013	0.093
# of observations	83,555	83,555	82,005	82,005	81,692	81,692	81,255	81,255
Guaranteed school fixed effects		x		x		x		x

Notes: author's calculations based on regressions using fixed school effects from the specification reported in Table 7. The dependent variables in columns (1)-(4) are indicators for being enrolled in a private school whose fixed effect is significantly more than that of the student's guaranteed public school, as defined in columns (2) and (5) of Table 7. The dependent variables in columns (5)-(8) are indicators for being enrolled in a private school whose fixed effect is *not* significantly more than that of the student's guaranteed public school. Sample in columns (1)-(4) consists of Grade 7 students enrolled in a public school or in a private school whose fixed effect is significantly more than that of the student's guaranteed public school. Sample in columns (5)-(8) consists of Grade 7 students enrolled in a public school or in a private school whose fixed effect is *not* significantly more than that of the student's guaranteed public school. Columns (2), (4), (6), and (8) include 407 fixed effects for the student's guaranteed public school. All regressions include year fixed effects in addition to the indicated student characteristics. Standard errors are clustered at the school level and available on request. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 9: Oaxaca-Blinder decomposition of the difference between average test scores of public and private school students

	(1)	(2)	(3)	(4)
	Reading		Numeracy	
Mean public test score	0.077***		0.156***	
Mean private test score	0.471***		0.519***	
Difference	0.394***		0.363***	
Due to differences in X	0.237***	0.089***	0.199***	0.062***
Grade 4 test scores	0.213***	0.090***	0.185***	0.073***
ESL	-0.001	0.000	-0.001**	-0.001*
Home language	0.000	-0.001	-0.017**	-0.011***
Aboriginal	0.004***	0.001***	0.004***	0.001***
Female	0.000	-0.000	0.000	0.000
Neighborhood % immigrant	-0.000		-0.000	
Neighborhood mean education	0.016**		0.020**	
Neighborhood mean income	0.006**		0.009**	
Year effects	-0.001	-0.002	-0.001	-0.002
Due to differences in β	0.157***	0.080***	0.166***	0.080***
Grade 4 test scores	-0.006	-0.000	-0.019***	0.000
ESL	0.008***	0.001	0.011***	0.002
Home language	0.003	-0.000	-0.011	-0.000
Aboriginal	-0.000	-0.000	-0.007	0.000
Female	-0.001	-0.000	0.010	-0.000
Neighborhood % immigrant	0.006		0.016	
Neighborhood mean education	0.083		0.187*	
Neighborhood mean income	-0.045*		-0.075***	
Year effects	-0.031	-0.019	-0.045	-0.035**
Intercept	0.140	0.098***	0.099	0.114***
Interaction	0.000	0.001	-0.002	0.004
Due to postal code fixed effects		0.224***		0.218***
# of students	92,198	92,198	91,152	91,152
Neighborhood controls	x		x	
Postal code fixed effects		x		x

Notes: Estimates are based on a threefold Oaxaca-Blinder decomposition of the difference between average test scores of public and private school students. Individual controls in all four columns include quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language (ESL), and Aboriginal identity. Specifications in columns (1) and (3) also include neighborhood controls based on census data: mean family income in the student's Census EA/DA, proportion of household heads in the student's Census EA/DA who are immigrants, and whose highest level of education is high school, a trade certificate, some college and a bachelor's degree. Specifications in columns (2) and (4) include postal code fixed effects and are estimated from within-postal code differences. Standard errors are clustered at the school level and available on request. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Table 10: Regression of estimated school fixed effects on selected school characteristics and Oaxaca-Blinder decomposition of the difference between average school fixed effects of public and private schools, selected estimates

	(1) Reading	(2) Numeracy
A. Estimates		
Private	0.794*	1.54***
	[0.458]	[0.584]
StDev(Grade 7 reading score)	-0.566***	-0.621***
	[0.142]	[0.169]
StDev(Grade 7 numeracy score)	0.279**	0.888***
	[0.123]	[0.146]
StDev(English)	0.076	-0.240**
	[0.079]	[0.097]
Grade 7 enrolment	-0.001***	-0.001***
	[0.000]	[0.000]
Private*StDev(Grade 7 reading score)	-0.251	-1.27**
	[0.395]	[0.534]
Private*Grade 7 enrolment	-0.001	-0.002*
	[0.001]	[0.001]
R^2	0.193	0.223
# of observations	92,197	91,150
B. Oaxaca-Blinder Decomposition		
Difference	0.174***	0.154***
Due to differences in X	0.035***	0.061***
St. dev. Grade 7 reading score	0.026***	0.029***
St. dev. English	-0.005	0.015**
Grade 7 enrolment	0.017***	0.028***
Due to differences in β	0.088**	-0.005
St. dev. Grade 7 reading score	-0.220	-1.11**
Grade 7 enrolment	-0.042	-0.133**
Intercept	0.794*	1.54***
Interaction	0.052	0.098**
St. dev. Grade 7 reading score	0.012	0.058**
Grade 7 enrolment	0.017	0.054*

Notes: Dependent variable is estimated school fixed effect from the specification reported in Table 6. Additional regressors (statistically insignificant and not reported) include the share of students whose home language is English and interactions between the private school indicator and: the standard deviation of the Grade 7 numeracy score, the share of English home language, the standard deviation of English home language, an indicator that a private school is high cost, and an indicator that a private school is single sex. Year effects are fully interacted with the private school indicator. Panel A reports selected coefficient estimates in this specification; Panel B reports the threefold Oaxaca-Blinder decomposition of the difference between the average student-weighted school fixed effects of public and private schools. Decomposition results displayed do not sum to total difference because insignificant results are not reported. Standard errors are clustered at the school level and available on request. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

Figure 1: Kernel density estimates of the student-weighted distribution of school mean test scores, by public and private schools

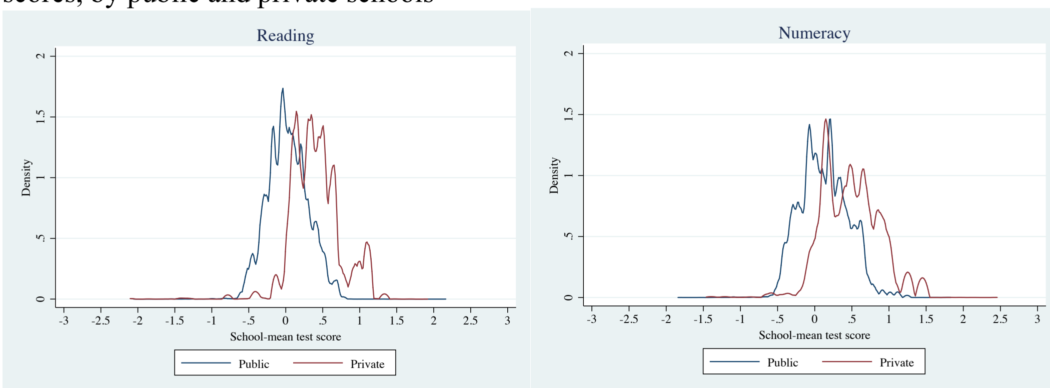
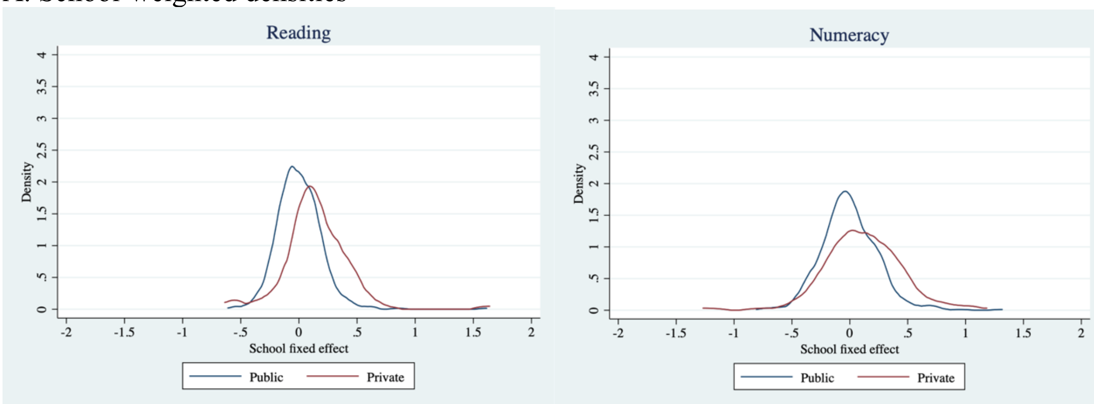
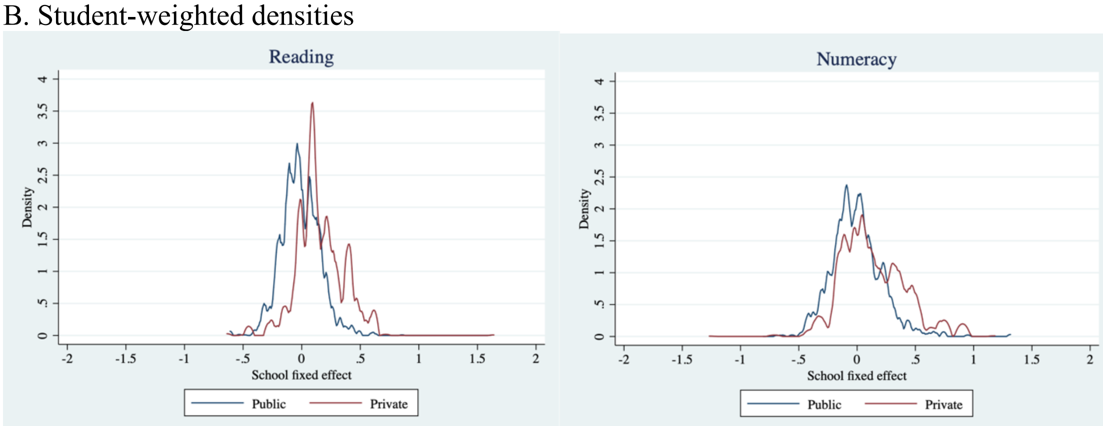


Figure 2: Kernel density estimates of the distribution of estimated school effects, by public and private schools

A. School-weighted densities



B. Student-weighted densities



Source: Author's calculations based on the specification with fixed school effects reported in Table 3 Panel C.

Appendix

Coding Census Neighborhood Characteristics

To proxy for the student's socioeconomic status, we match their residential postal code to the most recent public-use estimates of Census neighborhood characteristics 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.

Table 1: Selected student characteristics of movers and stayers, public and private schools

	(1)	(2)	(3)	(4)	(5)	(6)
		Stayers			Movers	
	Public & private	Public	Private	Both directions	Public to private	Private to public
No. of students	102,835	93,488	9,347	3,020	1,620	1,400
% of the sample	97.1	88.3	8.83	2.85	1.53	1.32
French Immersion	0.079	0.084	0.020	0.034	0.004	0.070
Home language: English	0.680	0.672	0.766	0.602	0.582	0.626
Chinese	0.115	0.121	0.058	0.116	0.169	0.054
Punjabi	0.064	0.069	0.020	0.108	0.059	0.164
Other	0.142	0.140	0.157	0.187	0.209	0.162
Aboriginal	0.054	0.058	0.007	0.024	0.018	0.031
Female	0.489	0.487	0.503	0.454	0.431	0.481
Neighborhood share						
Immigrant	0.071	0.071	0.067	0.071	0.070	0.072
High school	0.248	0.248	0.244	0.244	0.241	0.248
Some college	0.295	0.296	0.282	0.281	0.278	0.286
Bachelor's	0.176	0.172	0.210	0.202	0.222	0.180
Neighborhood family income	6.79	6.68	7.829	7.56	8.11	6.94
Grade 4 Reading score	0.050	0.016	0.366	0.170	0.224	0.110
Grade 4 Numeracy score	0.095	0.068	0.351	0.215	0.312	0.107
Grade 4 Missing reading	0.069	0.074	0.024	0.050	0.064	0.035
Grade 4 Missing numeracy	0.074	0.079	0.026	0.056	0.070	0.040
Grade 7 reading score	0.083	0.042	0.469	0.239	0.403	0.043
Grade 7 numeracy score	0.170	0.134	0.509	0.309	0.500	0.076
Missing grade 7 reading	0.072	0.075	0.032	0.054	0.041	0.069
Missing grade 7 numeracy	0.084	0.088	0.039	0.059	0.038	0.084

Notes: see text and Data Appendix for details of sample selection and construction and for variable definitions. Column (1) reports characteristics of students who attended a school in the same sector (public or private) in both grade 4 and grade 7. Column (2) reports characteristics of students who attended a public school in grades 4 and 7. Column (3) reports characteristics of students who attended a private school in grades 4 and 7. Column (4) reports characteristics of students who attended schools in different sectors (public or private) in grades 4 and grade 7. Column (5) reports characteristics of students who attended a public school in grade 4 and a private school in grade 7. Column (6) reports characteristics of students who attended a private school in Grade 4 and a public school in Grade 7.

Table 2. Robustness of estimated private school effects to sources of student mobility

	(1)	(2)	(3)	(4)
Model	Value added model			
Sample	All students	Excluding G4/G5 movers	Excluding G6/G7 movers	Compulsory movers only
Private	0.148***	0.144***	0.145***	0.146
	[0.022]	[0.023]	[0.023]	[0.095]
# of schools	624	619	603	390
# of observations	92,198	82,789	81,315	14,562
Private	0.147***	0.140***	0.146***	0.231**
	[0.033]	[0.033]	[0.036]	[0.093]
# of schools	625	620	603	385
# of observations	91,152	81,894	80,457	14,374

Notes: Specification is the same as Table 3 Panel B, columns (2) and (4). Dependent variable is student's Grade 7 FSA test score. Control variables include year effects, quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language, Aboriginal identity, and a full set of postal code fixed effects. Estimates in column (1) replicate estimates from Panel B of Table 3. Standard errors clustered at the school level. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reading				Numeracy			
	Catholic	Christian	Other Faith	Prep	Catholic	Christian	Other Faith	Prep
Estimate	0.184***	0.061**	0.204**	0.221***	0.262***	-0.016	0.205**	0.181**
	[0.029]	[0.030]	[0.088]	[0.057]	[0.042]	[0.042]	[0.086]	[0.084]
R^2	0.696				0.697			
# of students	89,510				88,484			

Notes: Standard errors clustered at the school level and reported in brackets. Specification in columns 1-4 is the same as reported in column 2 of Table 3 Panel B, except that it includes an indicator for private school type instead of a single private school dummy, and the sample has been restricted to students who are enrolled in the same sector (public or private) in both grades 4 and 7. Similarly, the specification in columns 5-8 is otherwise the same as that reported in column 4 of Table 3 Panel B. Controls include year effects, quadratics in grade 4 reading and numeracy scores, gender, home language (Chinese, Punjabi, other non-English), English as a Second Language, Aboriginal identity, and postal code indicators. *** indicates statistical significance at the 0.01 level, ** indicates significance at the 0.05 level, and * indicates significance at the 0.1 level.