Three Essays on Economics of Education

by

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Abstract

This thesis is composed of three essays on economics of education.

The first chapter is co-authored with Ciro Avitabile and Jesse Cunha and investigates the medium-term impact of early-life welfare transfers on children's learning. It studies children who were exposed to the randomized controlled trial of the Mexico's Food Support Program (*Programa de Apoyo Alimentario*), in which households were assigned to receive cash, in-kind food transfers, or nothing (a control). The findings show that in-kind transfers did not impact test scores, while cash transfers led to a significant and meaningful decrease in test scores. An analysis of the mechanisms driving these results reveals that both transfers led to an increase in child labor, which is likely detrimental to learning. In-kind food transfers, however, induced a greater consumption of several key micronutrients that are vital for brain development, which likely attenuated the negative impacts of child labor on learning.

The second chapter, jointly with Jane Friesen and Simon Woodcock, studies sorting, peer effects and school effectiveness under a universal voucher program. Using student-level longitudinal data for the population of students enrolled in private and public schools, we estimate a model of test scores that includes student effects, school effects and peer effects. Our results provide both the first estimates of the contribution of peer ability to private school effectiveness and a novel set of estimates of the effect of private school cream-skimming on the achievement of public school students under a mature voucher program. We find evidence of substantial sorting that contributes meaningfully to achievement at private schools via peer effects but has little effect on the average outcomes of those left behind in public schools.

The third chapter investigates the effect of a policy-induced increase in public school competition on private school enrollment and budget outcomes. I exploit a natural experiment created by the introduction of an open enrollment policy that expanded public school choice opportunities and increased competitive pressure on private schools. Using a new data set constructed from mandatory nonprofit information returns and school enrollment records, I find that an increase in public school competition modestly reduces private school enrollment. Catholic school enrollment is most responsive to increased public school choice, whereas other private schools such as Christian and other faith schools experience no reduction in enrollment. The negative enrollment effects are concentrated among high school age students. I find no evidence that private schools respond to this increased public school choice by adjusting their revenue and spending choices.

Keywords: private schools, peer effects, school vouchers, cream-skimming, school effectiveness, school choice, school competition, open enrollment, welfare transfers, learning outcomes.

Dedication

To Léa, Lauren, Manuela, and Amelia

.

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Chapter 1

The Medium Term Impacts of Cash and In-kind Food Transfers on Learning

[1]

1.1 Introduction

Worldwide, means tested transfer programs have become one of the most common strategies to reduce poverty (Bastagli *et al.* , 2019), and a robust body of evidence has demonstrated that transfers improve short-term outcomes (Fiszbein *et al.* , 2009). In addition to addressing short-term needs, many transfer programs also aim to increase children's human capital as a means to improve life-long outcomes and promote intergenerational mobility. Our paper contributes to the small and growing literature on the medium- and long-run impacts of transfer programs (Barham *et al.* , 2019; Araujo *et al.* , 2016; Millán *et al.* , 2020) by studying how unconditional cash and in-kind food transfers impacted the standardized test scores of primary school children in poor and remote areas of Mexico 4 to 10 years after transfers were first received.

Transfer programs can impact children's learning in various ways, both positively and negatively. First, transfers can improve nutritional intake during critical ages for mental and physical development (directly via food transfers or indirectly via cash), thus improving a key biological foundation for learning (Prado & Dewey, 2014; Almond *et al.* , 2011). Second, transfers increase the household budget which allows parents more flexibility to invest in inputs to the learning process (Dahl & Lochner, 2012); for example, parents may buy more books for their children, reduce their work hours in order to spend more time on children's educational activities, or send their children to a better (and more expensive) school. Third,

[1]co-authored with Ciro Avitabile and Jesse Cunha.

transfers can impact child labor: greater resources can reduce the necessity for child labor and free up time for learning (Edmonds & Schady, 2012), or greater resources could lead to more child labor if, for example, the family invests in assets that are complementary to labor (Basu *et al.* , 2010; Edmonds & Theoharides, 2019; De Hoop *et al.* , 2017).[2] Fourth, transfers that are conditional on school attendance can increase enrollment and time in the classroom. While higher enrollment and more time in school can increase learning (Barham *et al.* , 2017), it could also negatively impact students by increasing demands on a limited supply of teachers and school resources.[3] Finally, there are likely dynamic complementarities amongst inputs to a child's learning (Heckman & Cunha, 2007; Glewwe & Muralidharan, 2016). For example, if transfers lead to an improved biological foundation for learning, parents may subsequently increase or decrease investments in children's schooling depending on whether those investments complement or are substitutes for improved nutrition.

We investigate how these mechanisms contribute to the overall impact of cash and in-kind food transfers on children's learning in the context of the *Programa de Apoyo Alimentario* (PAL), one of Mexico's flagship anti-poverty transfer programs. We leverage the randomized controlled trial of PAL that was implemented during the program's roll-out in 2003. 208 villages in southern Mexico were randomized into three groups in which program-eligible households received either cash transfers, in-kind food transfers, or no transfers (a control). In-kind food transfers were of a similar value to the cash transfer, and both represented around 12% of pre-program household consumption. We follow approximately 4,000 children whose families were part of this experiment into primary school by merging individual-level experimental data with a nationwide census of standardized test scores in grades 3 through 6. Test score data spans the years 2007 through 2013, which allows us to study children's learning 4 to 10 years after transfers began. Unlike Mexico's other well-known anti-poverty program, *Progresa/Oportunidades*, PAL transfers were not conditional on school attendance.

Previous research (Cunha, 2014) has shown that in the short-run (approximately one year after transfers began), both in-kind and cash PAL transfers increased total household consumption by similar magnitudes, a result attributable to the fact that the cash transfer was largely inframarginal. However, certain in-kind food items were extramarginal and binding, and thus children receiving in-kind transfers consumed more micronutrients than children who received cash transfers. Pre-program, many children were deficient in key micronutrients - vitamin C, iron, and zinc - that have been shown to support brain development (Black, 2003) and the in-kind treatment induced more children to consume above the

[2] Also, see de Hoop & Rosati (2014) and Dammert *et al.* (2018).

[3] If transfers induce lower ability students to enroll, the heterogeneity of student ability in the classroom may increase, leading to less effective teaching or adverse sorting of peers within class by ability (De Giorgi & Pellizzari, 2014; Duflo *et al.* , 2011).

Recommended Daily Allowance (RDA) of these micronutrients. By linking individual-level information on consumption from the post-experiment survey in 2005 with test score results in primary education, we can thus directly assess whether improvements in the nutritional intakes translate into improvements in subsequent performance in school.

Unlike previous work on the effect of conditional and unconditional transfers on learning at the end of secondary school (e.g. Barham *et al.* , 2019; Araujo *et al.* , 2016), we study school performance in primary school where baseline enrollment was almost universal and treatment effects on learning outcomes are unlikely to be confounded by those on school enrollment and completion. We use both the household survey data that were collected as part of the experiment and administrative data that were collected concurrent with the primary school exams to study how PAL impacts other determinants of school performance, such as child labor, school quality, and parental time investment.

Our main finding is that 4 to 10 years after transfers began, relative to the control, in-kind food transfers had no impact on test scores while cash transfers negatively impacted test scores. Transfers, whether in-kind or in-cash, did not impact the likelihood that children took the tests, implying these results are not driven by sample selection. Students were tested in three subjects - math, Spanish, and a third subject which rotated yearly - and cash transfers led to reductions in scores in all three areas, with effect sizes varying between 0.12 and 0.16 standard deviations lower than students from control villages. The children in our sample varied in the age at which they were first exposed to transfers, with the oldest at 6 years of age and the youngest having been exposed since conception. While a limited sample size precludes precise comparisons, estimates suggest that the negative impacts on test scores in cash villages are concentrated among those over 2 years old when transfers began. We also find that indigenous students and those from especially poor families - two of the most disadvantaged groups in Mexico - experienced larger negative impacts on test scores for both in-kind and cash transfers.

We next explore the mechanisms that could be driving these differential impacts on test scores. First, we replicate results from (Cunha, 2014) and show that neither transfer modality improved several measures of the stock of child health, but children in in-kind communities increased their intakes of important micronutrients, such as zinc and iron. Second, we find that transfers induced children to work more, especially amongst students from cash localities. Third, we find that students from cash localities were more likely to attend community or indigenous schools, as opposed to general schools, which is likely a result of the fact that general schools in cash villages increased fees for materials, uniforms, and enrollment. General school are historically of better quality than community and indigenous schools, but the evidence suggests that parents substituted to cheaper schools when principals increased the costs of general schools. Finally, neither transfer modality seems to have impacted parental involvement in school related activities. In sum, we conclude that the lower test scores in cash villages were due to several factors: (1) principals in cash villages increased the fees for general schools, (2) children in cash localities moved to lower quality indigenous or community schools, and (3) that the cash transfers increased the returns to child labor which induced a substitution of labor and learning.

Our study contributes to several related literatures. First, we add to the literature that studies the design and implementation of transfer programs (Baird *et al.* , 2011; Barrera-Osorio *et al.* , 2011; Glewwe & Muralidharan, 2016). Consistent with the results in previous studies (Baird *et al.* , 2016; Araujo *et al.* , 2016; Baez & Camacho, 2011; Akresh *et al.* , 2013), we find that unconditional transfers do not lead to long term improvements in human capital outcomes. By studying the behavioral responses to an unconditional transfer up to 10 years after its inception, our paper adds to the recent literature that studies the transmission channels through which conditional transfers affect long run outcomes (Barham *et al.* , 2017, 2019). Previous work on PAL (Cunha *et al.* , 2015) had found that the transfer modality had no differential effect on goods prices. We show that this is not the case for the price of education services. Second, we add to the literature that studies how micronutrient consumption - or lack thereof - in early life contributes to learning (Almond *et al.* , 2011; Maluccio *et al.* , 2009; Feyrer *et al.* , 2017; Chong *et al.* , 2016), by showing that improvements in the quality of micronutrient intakes in the first years of life are not sufficient to improve learning in the medium term.

The remainder of this paper is organized as follows: section 2 describes the PAL program and institutional features of primary education in Mexico; section 3 discusses our data and sample; section 4 presents the empirical strategy and results; section 5 discusses possible mechanisms through which PAL might affect learning outcomes; and section 6 concludes.

1.2 Background on Education in Mexico and the PAL Program

1.2.1 Primary education in Mexico

Public primary schools in Mexico include grades 1 though 6 and most are governed by the Federal Secretary of Education *Secretaria de Educacion Publica*,SEP).[4] The remaining public primary schools are governed by CONAFE (*Consejo Nacional para el Fomento de la Educacion*, a decentralized agency responsible for providing educational services in rural and hard to reach communities with fewer than 2,500 inhabitants.

SEP schools (also known as *general* schools) typically have one classroom per grade and are staffed by teachers with open-ended contracts who have received post-secondary education (INEE, 2014). In contrast, CONAFE schools (also known as *community* schools*)* always have a single multigrade classroom, with a typical enrollment of 10-15 students per school. CONAFE instructors are generally young community residents between 15 and

[4] Less than 10% of all primary schools in Mexico are in the private sector (INEE, 2014).

29 years old who have completed upper secondary school yet do not have formal teacher training; they typically teach in the CONAFE school for only two years.[5]

Both SEP and CONAFE are required to offer non-Spanish speakers the option of attending schools which offer instruction in their indigenous language (known as *indigenous* schools*)*. The large majority (66 percent) of the indigenous schools are multigrade. There are 68 officially recognized indigenous languages in Mexico, and the quality of these schools is often low, partly stemming from a low supply of trained indigenous-language teachers.

In 2013, general, community, and indigenous schools enrolled 93%, 1%, and 6% of Mexican public school students, respectively. However, in the more rural parts of Mexico we study, community and indigenous schools are more prevalent.

1.2.2 ENLACE tests

Between 2007 and 2013, all Mexican students in grades 3 through 9 were required to take a standardized test, the ENLACE (*Evaluación Nacional de Logro Académico en Centros Escolares)*. The test was administered at the end of each academic year and it assessed student knowledge in three areas: math, Spanish, and, starting in 2008, a third subject which rotated between Science (in 2008 and 2011), Ethics/Civics (in 2009 and 2013), History (in 2010), and Geography (in 2011). In the first year of implementation, ENLACE tests were normalized by subject and grade with a mean score of 500 and a standard deviation of 100; subsequent years' tests were graded relative to the base year to allow for the comparison of results over time. Nationwide, take-up of ENLACE was close to 90 percent.

Originally, teachers had no stake in the results of their student's ELACE test scores, but in 2008, ENLACE scores became one of the key criteria to measure teacher performance in *Carrera Magisterial* (CM) program. The CM is a national teacher incentive program which offered salary bonuses for taking professional development courses and agreeing to be subject to yearly evaluations (Santibañez *et al.* , 2007). The use of ENLACE scores in the CM program possibly increased teacher effort, but as SEP required the use of external proctors, it is unlikely that teachers were able to directly manipulate student responses. Previous work has shown that ENLACE tests in primary education are correlated with later learning and labor market outcomes (Avitabile & de Hoyos, 2018; De Hoyos *et al.* , 2018).

The ENLACE was not offered in 2014, and 2015 it was replaced by a new test, the PLANEA *(Plan Nacional para la Evaluación de los Aprendizajes)*. Unlike the ENLACE, the PLANEA only tested a random subset of students in schools and so is not useful for our analysis.

[5]Only 2.6 percent of CONAFE teachers report having a college degree, while 19 percent report having only completed lower secondary education (INEE 2014). CONAFE teachers should receive between five and seven weeks of training, but more than half report four weeks of training or less (INEE 2014).

1.2.3 The PAL program and experiment

The *Programa de Apoyo Alimentario* began in 2004 with the aim of increasing the nutritional intake of poor families, with an emphasis on children and mothers. By 2009, it had expanded to operate in about 5,000 poor, rural villages throughout Mexico. Villages were eligible to receive PAL if they had fewer than 2,500 inhabitants, were classified as highly marginalized by the Census Bureau, and did not currently receive aid from either *Liconsa*, a subsidized milk program, or *Oportunidades*, a conditional cash transfer program. PAL villages were therefore typically poorer and more rural than the widely-studied *Oportunidades* villages.[6] Within eligible villages, households were eligible for the program if they fell below the threshold of a poverty index derived from observable characteristics of permanent income (Vazquez Mota, 2004).

PAL food transfer packages were chosen by nutritionists to provide a balanced diet of about 1,750 calories per day, per household (Campillo Garcia, 1998) and contained seven basic items (enriched corn flour, rice, beans, dried pasta soup, biscuits (cookies), fortified milk powder, and vegetable oil) and two to four supplementary items (including canned sardines, canned tuna fish, lentils, chocolate powder, packaged breakfast cereal, and corn starch). All the items were common Mexican brands and were by and large available in local stores. The transfer was not conditional on family size, it was delivered bimonthly (two food boxes at a time), resale of in-kind food transfers was not prohibited, and the wholesale cost to the government per box was about 150 pesos (approximately 15 U.S. dollars).

PAL experiment

Concurrent with the national roll-out of the program, a random sample of 208 villages in southern Mexico were chosen for inclusion in an experiment.[7] Villages were randomized into three treatment arms, in which eligible households received either a monthly in-kind food transfer (50 percent of villages), a 150 peso per month cash transfer (25 percent of villages), or nothing (the remaining 25 percent of villages). Approximately 89 percent of households in the in-kind and cash villages were eligible to receive transfers (and received them).

In addition to the randomization of transfer modality, the experiment also assigned all the cash villages and a randomly selected half of the in-kind villages to receive health, nutrition, and hygiene classes, which were designed to promote healthy eating and food preparation practices. In practice, few transfer recipients reported attending classes and - importantly - administrators confirmed that the conditionality of transfers on class attendance was never enforced; that is, no household was denied transfers for not attending

[6] Villages were not incorporated in *Oportunidades* if they did not have health facilities and/or secondary schools in close enough proximity, as needed to fulfill the conditionality of *Oportunidades* transfers.

[7] The experiment was implemented in eight states: Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatan.

classes.[8] Furthermore, qualitative research finds that the classes were held infrequently, were generally of low quality, and were not taken seriously by participants, suggesting that the classes did not likely impart new knowledge on program recipients that would impact their food consumption decisions (Rodriguez Herrero, 2005). As shown below, our main empirical results are robust to separating the in-kind villages into the group with classes and the group without.

Using data from the experimental sample, Cunha (2014) documents that both cash and in-kind transfers led to equally sized increases in total consumption (food plus non-food) and food consumption between in-kind and cash villages. However, several of the in-kind food items were extra-marginal, as evidenced by greater increases in consumption of those goods in in-kind villages compared to cash villages. Some of these extra-marginal foods were nutrient rich, such as fortified powdered milk and vitamin enhanced corn flour, and thus children in in-kind villages consumed more iron, zinc and vitamin C than children in cash villages.

1.3 Data and Sample

1.3.1 Data

Our data come from several sources: pre- and post-intervention surveys of individuals and households in PAL villages, student-level ENLACE test scores, student surveys from a subset of ENLACE test takers, and school-level data collected by the SEP.

PAL data

In each of the experimental PAL villages, approximately 33 households were selected to be surveyed pre- and post-intervention. The pre-intervention survey was administered in the last quarter of 2003 and the first quarter of 2004, and the post-intervention follow-up survey was conducted in the final quarter of 2005.[9]

The pre-intervention data allow us to confirm the randomization was successful, as well as segment the population by various socio-economic characteristics. From the post-intervention survey, we use data from a 24-hour food recall for children and a time allocation module for both children and adults. The food recall was completed by the survey respon-

[8] Based on household survey data, 76 percent of respondents attended a class in the in-kind villages assigned to receive classes and 69 percent attended a class in the in-kind villages assigned to not receive classes. In both cases, average attendance was roughly four classes over the course of the program. Furthermore, assignment to classes did not affect total food expenditure or the composition of food expenditure (results available from the authors).

[9] To ensure respondents would not wrongly conclude that responses could affect their eligibility for aid, surveys were administered by Mexico's National Institute of Public Health, a different agency than the one that administered PAL.

dent, usually the female household head, for all children aged 2 to 6 at the time of the follow-up survey and allows us to calculate the quantity of macro- and micro-nutrients consumed. For all children 12 and older, both surveys asked the primary and secondary activity in the week prior to the interview. Therefore we can identify whether children attended school and/or worked. For those who reported working, the survey asks the number of hours worked.

Of the original 208 experimental villages, eight are excluded from our analysis. Two villages could not be resurveyed due to concerns for enumerator safety; two villages were incorporated in PAL prior to the pre-treatment survey; two villages were deemed ineligible for the experiment because they were receiving the conditional cash transfer program, *Oportunidades*, contrary to PAL rules; and two villages are geographically contiguous and cannot be regarded as separate villages.[10] Observable characteristics of excluded villages are balanced across treatment arms (results available upon request). Of the remaining 200 villages, three received the wrong treatment (one in-kind village did not receive the program, one cash village received both in-kind and cash transfers, and one control village received in-kind transfers). We include these villages and interpret results as intent-to-treat estimates.

ENLACE data

Our data allow us to study grade 3 through 6 ENLACE test scores for children in experimental villages who were born between 1998 and 2004.[11] The ENLACE identifies students through a government-issued identifier, the *Clave Única de Registro Poblacional* (CURP), which is formed via an algorithm which combines first name, last name, date of birth, sex, state of birth, and two randomly generated digits. The PAL surveys do not contain the CURP, but do contain all of its constituent demographics from which we generated a quasi-CURP which only lacks the random digits. Our data form an unbalanced panel of seven cohorts spanning seven academic years.

Each year, 20 percent of exam takers are randomly selected to complete the *ENLACE de Contexto,* a multiple choice survey asking about child labor, child and parental sociodemographic characteristics, child and parent expectations, and student perceptions about their peers, teachers and parental involvement.

[10] The contiguous villages are named “Section 3 of Adalberto Tejada” and “Section 4 of Adalberto Tejada,” so they appear to be part of the same administrative unit.

[11] We observe ENLACE scores for a small number of children from PAL villages in grades 7 through 9, but exclude them from our analysis in order to focus on primary school outcomes.

School-level data

The Ministry of Education conducts two school censuses per year, known as Formato 911. These censuses identify the school type (*general*, *indigenous*, or *community*) and collect information on school characteristics, including the number of teachers, students, classrooms, and laboratories, whether a library is available, and the exact geocordinates of the school. The censuses also collect information on fees that students attending general schools pay for uniforms, materials and enrollment; this information is not reported for indigenous and community schools as materials and uniforms are provided by the government either in the form of in-kind or cash grants to the schools. Fees in general schools are by law not compulsory, but school principals often ask for payments in a manner which make parents perceive the contributions are compulsory.[12] ENLACE tests are matched to schools using a unique school identifier (*clave de centro de trabajo* CCT). The geocoordinates allow us to calculate the distance between a village and the school a student attended.

1.3.2 ENLACE take-up

Post-treatment, there were 5,444 children in PAL villages born between 1996 and 2005, and we match 69% of them write at least one ENLACE test. Our sample therefore includes 3,773 children from 200 villages for whom we observe a total of 11,006 ENLACE tests; Table 1.1 shows how these observations vary across academic grades and years.

There are several reasons why we may not observe a child from the PAL survey taking the ENLACE test. First, children's school attendance and taking of the ENLACE test could be differentially impacted by the PAL transfers; however, as we show in the results below, there is no evidence that this is the case. Second, the child's family could have migrated abroad before the child reached the end of the third grade.[13] Migration information is not available for the PAL sample, but surveys of participants in the 1997 experimental evaluation of the *Oportunidades* program, a similar social transfer program to PAL, reveal that 0.7 percent of control group households migrated to the U.S. within one year (Angelucci, 2015). Each household in our sample has on average one child between the ages of 0 and 6 and if we apply this migration rate in each of the 10 years between 2004 and 2013, we would expect to not observe ENLACE tests for around 6.8 percent of children.

Third, a child could have never enrolled in school or could have dropped out of school before reaching the end of third grade. We can estimate this potential source of attrition using the PAL follow-up survey in 2005: 7 percent of the children aged 8 to 12 (the typical

[12]In order to discourage this behavior, in 2018 the Ministry of Education officially forbade school principals to ask for financial contributions.

[13]We observe students whose families moved within Mexico as the ENLACE was applied nationwide.

age of children in grades 3 through 6) report not attending school (and there is no difference across treatment groups).[14]

Fourth, a student may have not been present on the day of the exam, or the exam was not offered at the school. We do not have data on absence rates in schools, but there is evidence that entire schools did not take the exam in certain years in areas where there is a strong representation of the National Educational Workers Syndicate (*Sindicato Nacional de Trabajadores de la Educación*), a trade union representing a large percentage of Mexican teachers. In fact, the union scheduled strikes and disruptions specifically on the days of the ENLACE test as a form of protest, and state-level variation in ENLACE take-up rates demonstrates their influence. For example, in Yucatan and Veracruz, where union membership is relatively low, take-up rates were constantly above 94 percent in all years between 2007 and 2013. In Chiapas, Guerrero and Oaxaca, however, where union membership is high, take-up varied considerably by year, with certain years having take-up rates of 60, 70, or 80 percent. Importantly, as these protests and disruptions are orthogonal to the original treatment assignment, including state and year fixed effects in our specifications will allow to account for potential biases induced by differences in the test take-up.

A final reason why we would not see a child's ENLACE test is that the merging algorithm was not accurate. However, we failed to merge only 2.8 percent students and there are no differences in merge rates across treatment groups. Further details about the merging algorithm and the possible reasons for attrition are provided in the Supplement 1 Tables.

1.3.3 Summary statistics

The first two panels in Table 1.2 present pre-intervention sample means, by treatment group, for child and household characteristics respectively. The sample of children does not include those born after the pre-intervention survey (as they do not have baseline data), and the sample of households reflects one observation per child that we observe in the followup survey even if they were born after the baseline survey. Consistent with the random assignment, we find that characteristics are balanced across the three groups, with three exceptions: in cash localities, the share of boys is lower than in both in-kind and control localities; households in control localities are more likely to have an unmarried head than those in cash localities; and control households are more likely to have running water at home than those in cash villages.

Individual consumption data was only collected for children aged 1 through 4 years. As the 24-hour food recall module could overstate or understate actual consumption, it

[14] Although students are in school they might not reach the third grade during our period of observation either because they start late or because they repeat the early grades more than once. According to the estimates provided by INEE (2013), 95 percent of the children who enrolled for the first time in primary education in 2010 complied with the statutory starting age, and repetition rates in early grade are remarkably low. Furthermore, late entry and repetition rates do not differ across experimental groups.

is difficult to draw definitive conclusions about child health, however, comparing caloric and micronutrient intake to Recommended Daily Allowances (RDAs) suggests that most children consume too few calories and that for many, those calories do not contain enough essential micronutrients. In particular, 89 percent of children consume fewer than the RDA of calories, and 32, 46, and 41 percent of children are not consuming the RDA of iron, vitamin C, and zinc, respectively. Households in our sample are large and poor, with a total per-capita expenditure of about 360 pesos per month, or $36 US dollars, and about 5.5. household members. Approximately 30 percent of households have at least one member who speaks an indigenous language. The household head has on average 7 years of education, about 40 percent of households have a dirt floor, and around 50 percent do not have running water.

The bottom panel in Table 1.2 presents the characteristics of the closest school to each PAL community. The closest school is typically a general school and on average it is located 1km from the center of the community. The average distance remains the same when the closest school is a community or an indigenous school. The student-teacher ratio is around 29, and repetition rates are around 9 percent in general schools and 18 percent in community and indigenous schools. On average, the yearly cost of the closest school is about 380 pesos, including contributions, materials' and uniforms' costs. As mentioned above, this cost is entirely driven by general schools, as indigenous and community schools are free of charge.

1.3.4 Post-experiment

The interpretation of our estimates depends in part on what benefits children received between the experiment and the observed ENLACE tests. Self-reports from the follow-up survey show that households in treatment villages (both in-kind and cash) reported receiving on average 12 months of PAL aid, however we do not have household-level information on the type of transfers received after the follow-up survey.

SEDESOL was able to provide us only with village-level administrative counts of the number of beneficiary households per year receiving PAL, *Oportunidades*, or *Liconsa* between 2005 to 2013. Figure 1.1 plots the average number of beneficiaries of PAL, *Oportunidades*, and *Liconsa* by year for the three experimental groups. While the average number of PAL beneficiaries fluctuated between 200 and 50 for in-kind villages and between 250 and 50 for cash villages, the control villages remained with an average number of beneficiaries below 50 during the entire period (left panel in Figure 1.1). The number of beneficiaries of *Oportunidades* and *Liconsa* steadily increased over time. Cash localities displayed on average a lower number of households that are beneficiaries of the *Oportunidades* transfer (middle panel in Figure 1.1). This difference, which is very small in size, [15] is likely to

[15]On average cash and in-kind localities have 12 and 4 recipients fewer than the control group, that on average has 62 households that are *Oportunidades* beneficiaries. Since the follow-up survey, on which the

partly reflect differences in population. In fact, both in 2005 and 2010 - years for which the population census is available - we find that the number of households in in-kind and cash localities is lower than in control ones (see columns 1 and 4 in Supplemental Table 1.15), possibly as a result of differential migration. The share of beneficiary households in neither of the treatment groups is statistically different from the one in the control group (columns 3 and 6 in Supplemental Table 1.15).

Overall, the evidence suggests that our estimates should be interpreted as the combined effect of differential exposure and differential take-up in treatment and control localities.If the receipt of other programs after the experiment is correlated with the treatment assignment to the villages and those programs affect student performance outcomes, then our results would reflect not just the impact of initial transfer modality, but also the dynamic response to that initial treatment.[16]

1.4 Empirical Strategy and Results

1.4.1 Empirical model

Our empirical framework leverages the randomization of transfer modality across villages, with the main models taking the following form, where $ENLACE_{ivgt}$ is the test score of child i, in village v, grade g, and school year t:

$$ENLACE_{ivgt} = \alpha + \beta_1 InKind_v + \beta_2 Cash_v + \gamma' X_i + \delta_t + \gamma_g + \varepsilon_{ivgt}$$

We normalize test scores within grade and year with respect to the mean and the standard deviation in the control group. In our preferred specification, X_i includes fixed effects for Mexican states, the child's age, and a set of individual, household and locality characteristics that showed imbalance at baseline, namely indicators for the child's gender, whether the head of household is married, whether the house has running water, and whether the closest school offers a morning shift. δ_t and γ_g are year and grade fixed effects. Standard errors are clustered at the village level, the unit of randomization. The parameters β_1 and β_2 represent the Intent to Treat (ITT) effects of living in in-kind and cash communities at

merge is based on, samples 33 households (about 20 percent of the total number of households), differences in the number of *Oportunidades* beneficiaries are likely to lead to very small differences in terms of children who have to comply with the attendance conditionality in our sample.

[16] Our estimated impacts on learning are robust to controlling for number of beneficiaries of *PAL* and *Oportunidades* in the village (available upon request).

the time of the follow-up survey. When testing the null hypothesis β_2-β_1= 0 we also present results based on Randomization Inference.[17]

We also consider several other outcomes as part of our investigation into the mechanisms behind the impact of PAL transfers on learning, and those models take the same form as the model for ENLACE described above.

1.4.2 Results

Table 1.3 contains our first set of results on the likelihood of a child taking the ENLACE test. Columns 1 and 2 show the impact on a student ever taking an ENLACE test (one observation per child), while columns 3 and 4 use a balanced panel of students in every year 2007 through 2013 (seven observations per child). All models use state fixed effect and the models using yearly observations use year fixed effects. The In-kind and Cash indicators are small and statistically insignificant in all models. Including covariates, the results in columns 2 and 4 show that older students, those with greater height-for-age, and those coming from households with a married head or a head with more years of education are more likely to take the ENLACE test. Furthermore, we do not observe any difference across groups in the probability that a child takes the test in a grade appropriate for her age (see Supplemental Table 1.16) nor do we see significant differences in the characteristics of children for whom we have and do not have ENLACE tests (see Supplemental Table 1.17). These results suggest that estimates of the effect of PAL on learning outcomes are not driven either by differential selection into taking the ENLACE test or the timing of taking the test.

Before presenting the impacts of PAL transfers on test scores, it is useful to see how test scores are correlated with observable characteristics. Table 1.4 contains estimates from regressions of test scores on child, household, and village characteristics, using only children from control villages. Several correlations stand out: having a general school in the village, as opposed to a community or indigenous school, is associated with approximately 0.5-0.6 s.d. higher test scores in all subjects; girls perform better than boys in all subjects, with the gap being largest in Spanish (0.23 s.d.); child height-for-age (a proxy for health status) is positively correlated with test scores; and having running water in the home (a proxy for household wealth) is associated with approximately 0.2 s.d. higher test scores. The fact that both an anthropometric measure and household wealth are positively correlated with learning outcomes is suggestive that transfers, whether in-kind or in-cash, could also have meaningful impacts on performance. State fixed effects reveal meaningful geographic variation in mean test scores, reflecting the heterogeneous nature of the population of southern Mexico.

[17] All other hypothesis testing results based on Randomization Inference are in line with those presented and are available upon request.

Table 1.5 contains our main results on the impact of cash and in-kind transfers on test scores. We show two specifications for each subject. First, the models in columns 1, 3, and 5 show mean differences between treatment and control localities only controlling for state fixed effects. In-kind transfers did not meaningfully impact test scores: all coefficients are negative, yet very small in magnitude and not statistically significant. Cash transfers, on the other hand, caused a large drop in test scores relative to students from control communities: -0.19, -0.14, and -0.17 s.d. for math, Spanish, and the 3rd subject, respectively, with math and the 3rd subject being significant at the 5 percent level. The differential impact of in-kind and cash transfers is also significant for all three subjects, both using classical asymptotic theory and Randomization Inference.[18]

Columns 2, 4, and 6 add pre-program child and household characteristics and year and grade fixed effects. These controls do not meaningfully impact our estimates and the differences between the two treatment types are statically significant at the 10 percent level in all subjects. In addition, these results are not driven by the outcomes in specific years (see Figure 1.2) or grades (see Figure 1.3), although the negative impacts of cash transfers are larger for students in higher grades (5th and 6th grades, compared 3rd and 4th grades).[19]

We next explore program impacts by the age at which children first received the program. A large literature has shown that nutrition interventions are the most impactful between conception and the second birthday (Pollitt *et al.* , 1995), and children in our sample ranged from in-utero to six years of age at the time that transfers began. Figure 1.4 plots the treatment effects by age at the follow-up survey. For children younger than two, we find positive point estimates, but correspondingly large confidence intervals that prevent us from rejecting the null hypothesis of no impact.

We also test whether there is any treatment heterogeneity along two dimensions that are particularly relevant for southern Mexico: household expenditure and ethnicity. Table 1.6 contains estimates from models that interact treatment indicators with an indicator for "poor" households, defined as those with below median expenditure per capita. While coefficients and comparisons between them are imprecise, the negative coefficients on the interaction terms suggest that poor households experience larger declines across the three subjects for both cash and in-kind transfers.

Interacting treatment with an indicator for an indigenous household (defined as at least one member speaking an indigenous language), we find the negative impacts of PAL are even greater among the indigenous population: Table 1.7 shows the impact of cash and

[18]Supplemental Tables 1.18 and 1.19 report main results separating villages into the three treatment arms verifying that results are robust. Similarly, when using the ENLACE test's official 4-item categorical classification of student performance (insufficient, sufficient, good, excellent), we find results fully consistent with those presented (Supplemental Table 1.21).

[19]Similarly, we find no evidence of our results being driven by selection into taking ENLACE test in specific years (see Figure 1.7).

in-kind transfers on test scores are between 0.14 to 0.29 standard deviations lower among the indigenous compared to the non-indigenous students. Among non-indigenous students, in-kind transfers have no impact on learning outcomes and cash transfers have a negative (albeit insignificant) impact.

Finally, we estimate treatment effects across the test score distribution with quantile regressions. Figure 1.5 shows that the negative impacts of the cash treatment relative to both the treatment and control treatments is concentrated in the middle ventiles (the 35th to the 75th percentile) and the highest ventile of the distribution.

1.5 Mechanisms

To help understand the mechanisms through which the PAL transfers affected student learning, we adopt a simple learning production function (e.g. Glewwe & Miguel, 2007).

Assume there are two time periods in a young child's life: period 1 begins with conception and ends when the child enters primary school, while period 2 covers the primary school years. Further suppose that a child's academic knowledge in period 2, Y_{i2}, is a function $(f\,(.))$ of several factors, in addition to an unobserved component μ_i: the stock of health prior to entering school (HS_{i1}); parental health investment in the child prior to primary school (HI_{i1}), such as nutritional intake and vaccinations; the effort the student devotes to school related activities (SE_{i2}); the quality of the primary school (Q_{i2}); parental investments in the child during school (PI_{i2}).

$$Y_{i2} \quad = \quad f(HS_{i1}, HI_{i1}, SE_{i2}, Q_{i2}, PI_{i2}, \mu_i) \tag{1.1}$$

For each of the inputs described in eq. 1.1 there is well established evidence on the relationship with learning. Due to the self-productivity of human capital skills (Heckman & Cunha, 2007), child health before entering school is an important determinant of the health stock later in life. Recent evidence shows that it has a significant impact on learning outcomes.[20]Parental health investments might compensate or reinforce gaps in children's endowments (Becker & Tomes, 1976), since parents might decide to invest more either on children who are in worse health in order to minimize the gap with other siblings or on those who are in better health in order to maximize the overall return. Zinc and iron supplementation in early years has been found to be beneficial for cognitive outcomes (e.g. Powell *et al.* , 2005; Feyrer *et al.* , 2017). Student effort, either in the form of class participation or

[20]Figlio *et al.* (2014), using a sample of siblings in Florida, find that birthweight has a positive constant effect on test scores throughout the entire academic life. Bharadwaj *et al.* (2012) exploit school and birth records from Chile and Norway to find that low-weight children who receive extra medical care at birth have higher test scores.

time spent on home assignments, can contribute to improved student performance. We do not have reliable measures of time spent by the students in those activities. We do, however, have measures of child labor, that has been shown to have detrimental effects on both education attainments and learning (Ravallion & Wodon, 2000; Beegle *et al.* , 2009; Heady, 2003) School quality - a broad term that refers, among others, to the quality of teachers (Rockoff, 2004; Chetty *et al.* , 2014) principals (Roland G. Fryer, 2017), peers (Duflo *et al.* , 2011), class size (Angrist & Lavy, 1999) - is a key determinant of learning outcomes. Finally, a growing body of literature shows that parental time investments play a key role in improving child cognitive and socioemotional skills in the early years (Attanasio *et al.* , 2018b; Agostinelli & Sorrenti, 2018).

Our data allow us to test the impact of cash and in-kind transfers on each of these inputs to academic knowledge. However, we note that the results presented below should be interpreted as the reduced form impacts of PAL transfers, as they combine both the direct and the indirect effect of the program. In particular, there are likely dynamic complementarities among inputs (Heckman & Cunha, 2007). For example, children of PAL recipients might attend better (and more expensive) schools (a higher Q_{i2}) because transfers increased the family budget, or, they may attend better schools because the returns to school quality are increasing in child health investments (HI_{i1}).

1.5.1 Health stock and health inputs

Similar to Cunha (2014), we use information from the evaluation follow-up survey in order to study whether PAL transfers impacted proxies for health status and parental health investment of children aged 0 to 6 at the baseline. We focus on four indicators: 1) the caregiver reported probability that the child was sick in the four weeks prior to the interview; 2) the child height per age, expressed as a z-score; 3) the child weight per age, expressed as a z-score; 4) whether the child was anemic or not, based on the analysis of a blood sample.[21]

Table 1.8 presents results for the whole sample and for only those children for whom we observe at least one ENLACE test score. There are no differences between treatment groups in either sample in terms of the probability of being sick, height-for-age, and weight-for-age. Children in both the in-kind and the cash group were about 3 percentage points less likely to be anemic than those in the control group; this is a large difference in relation to the prevalence of anemia (19 percent in the control group), but the effect is not statically different from zero.

Table 1.9 presents the impacts of PAL on consumption of calories, one macronutrient (protein), and five micronutrients (vitamin C, iron, zinc, calcium, and retinol). As with health outcomes, the results for the entire sample are very similar to results for the sample

[21] Anemia is a strong predictor of test scores, as evidenced by a 0.2 standard deviation difference in math and Spanish scores between control group students who had and did not have anemia as a young child.

of children taking the ENLACE, which is consistent with the fact that the program did not appear to impact the probability of taking the exam. Only in-kind children display an increase in the caloric consumption, but the effect is not statistically different from zero. Neither transfer type impacted the intake of protein. There was a large and statistically significant impact of both in-kind and cash on the intake of vitamin C. When looking at iron and zinc, two key nutrients for brain development, we find large and statistically significant impacts for the in-kind modality, but not for cash. For zinc, the difference between the two treatment types is statistically significant at conventional levels. Similarly, the in-kind transfer has a significantly larger impact than the cash transfer on the intake of calcium and retinol. To quantify an overall impact of transfers on nutritional intakes, we construct the first principle component of the six nutrients. In-kind transfers led to non-trivial 0.1 standard deviation larger impact on intake than did cash transfers (p-value = 0.09). Supplemental Table 1.22 shows that our conclusions do not change if we use as outcomes indicators of whether a child consumed at least the Recommended Daily Allowance of each nutrient.

In sum, despite having better nutritional intake, children who had been exposed to the PAL program for at least 18 months did not have better measured health.

1.5.2 Student effort

There is compelling evidence that conditional cash transfers reduced child labor (Fiszbein *et al.* , 2009), but the evidence on unconditional transfers is more mixed (Edmonds & Schady, 2012; Edmonds & Theoharides, 2019). We use two distinct data sources to study the impact of PAL on child labor.

First, information from the post-treatment survey identified attendance and labor outcomes of children aged 12 and 13 who were enrolled in primary education. These children would have been eligible to take the ENLACE in 2005, if the test had been in place at the time.[22] We study five outcomes: 1) only attending school (and not working); 2) attending school and working; 3) only working; 4) neither attending school nor working; 5) the number of hours of work (including zeros). Results are reported in columns 1 to 5 in Table 1.11. In the control group, 84 percent of the children attended school and did not work, 5 percent worked and did not attend school, 1 percent both worked and attended school, and 10 percent neither worked nor attended school. In cash localities we observe a 6.2 percentage point reduction in the share of students who reported only attending school in the week prior to the interview, as opposed to a null effect in the in-kind localities. The difference between the two treatment types is statically significant at 10 percent level. In

[22] Because of the almost universal enrollment in primary education and the lack of any effect of PAL on the probability of attending a grade that is appropriate for student age, the results are unlikely to be driven by a selection effect. While 12 is the standard age for completing primary school in Mexico, being "over-age" is quite common and we include 13 year-olds in order to improve statistical power. Results restricted to 12 year-olds are qualitatively similar.

cash localities, we observe an increase both in the share of students who report combining school and work, and in the share of those for whom work is the only activity. For the latter, the difference with the in-kind transfer localities is statistically significant at 5 percent level. Hours of work increased by 1.6 for children in the cash group, as opposed to a negative but very small effect in the in-kind group; the cash versus in-kind difference is large in terms of size - equivalent to the average number of hours in the control group - and is statistically significant (p-value=0.07).

Second, we use data from the *ENLACE de Contexto* to study students for whom we observe test scores.[23] Surveys between 2008 and 2013 ask students how many days on average they are involved in labor activities, and in addition surveys from 2011 to 2013 also ask how much time students dedicated to household chores in the week prior to the interview. Child labor hours are strongly correlated with test scores, for example, in the control group an additional day of work per week is associated with 0.10 s.d. reduction in the average ENLACE score of the three subjects.

The impacts of PAL on the labor outcomes of primary school children answering the *Enlace de contexto* are reported in columns 6 and 7 in Table 1.11. Students from both in-kind and cash localities increased the number of working day per week, with the effect being particularly large in cash villages (1.31 days per week). Similarly, there was a large increase in the share of children who report helping with household work, although the impacts are not statistically significant, owing in part to the limited sample size.

Basu *et al.* (2010) suggest that, in the presence of multiple factor market failures, the introduction or expansion of a productive asset could increase child labor. Looking at cross-sectional variation of the entire PAL sample pre-program, we see that the probability that a child age 12-13 exclusively attends school declines with the total number of animals owned by her family while the probability of working, either exclusively or in combination with school attendance, increases (see the top panel in Figure 1.6). In addition, the average number of hours that a child works steadily increases with the number of animals (see the bottom panel in Figure 1.6).

Using the experimental variation, and consistent with evidence from *Progresa* (or *Oportunidades*) transfers (Gertler *et al.* , 2012), we find that among households with at least one child aged 8-13 PAL transfers increased the number of animals owned, and the effect is statistically larger under cash and under in-kind transfers (see column 1 in upplemental Table 1.23). Households from both treatment groups are more likely to report being involved in agricultural activities post-program (column 2 in Table 1.23), with the effect being statistically larger among those from cash localities. We also find that households in the cash group are more likely to report a higher number of family members involved in

[23] We show in Supplemental Table 1.20 that the pre-intervention characteristics of students observed in the *ENLACE de Contexto* are balanced across treatment groups.

agricultural activities, and a higher number of hours farming, but in both cases differences are not statistically significant at conventional levels (columns 3 and 4 in in Table 1.23). Results presented in Skoufias *et al.* (2013) find that PAL did not lead to any change in the overall labor supply of adult males and females, but adult males substituted agricultural activities with non-agricultural ones. Also, Tagliati (2019) finds that among older children (age 15-16 at the baseline) both transfers increased child labor, with a stronger impact for cash transfers. Taken all together, the available evidence is consistent with the hypothesis that the transfer contributed to a partial reallocation of agricultural tasks from adults and older children to younger ones.

In summary, the results in this section show that the income effect generated by either type of unconditional transfer was not large enough to induce a reduction in child labor and an increase in school attendance. Less robust evidence suggests that in cash localities there was an increase in the return to labor of young children and a higher probability of combining work and school attendance, compared to in-kind localities.

1.5.3 School quality

Children in our sample have access to three types of schools that vary greatly in terms of quality and costs. We estimate the impact of PAL transfers on the quality of school a child attended in several ways. First, we estimate the differential likelihood that a child attended a typically higher quality general school (vis-à-vis an indigenous or a community one). Second, we look at several school-level indicators that impact the cost of schools: the distance from the community to the school and fees parents pay for materials, uniforms, and general attendance. Third, we look at student-teacher ratio as a proxy for resource congestion.

Results are reported in Table 1.12. The probability of attending a general school increased by 2 percentage points for children from in-kind localities, with respect to a baseline of 77 percent for children from control localities (column 1). For those from cash localities the probability of attending a general school decreased by 8.3 percentage points, although the effect is not statistically significant (column 1). The difference between in-kind and cash localities is statistically significant at 10 percent level (p-value=0.07). For both the in-kind and the cash groups, we observe a reduction in the probability of attending schools less than 5km from the community and between 5 and 10km away (columns 2 and 3 respectively). These reductions are compensated by an increase in the probability of attending schools between 10 and 30km, and those more than 30km away (column 4 and 5). However, for none of the distance outcomes are differences between in-kind and cash localities statistically significant. Results in column 5 rule out the possibility that the differential effect of in-kind and cash treatments on learning outcomes is driven by a differential effect of the transfer modalities on household internal migration.[24] There is a 10 percent reduction in the school fee paid by parents from cash localities, but the total school fees paid by parents are not statistically different across any treatment groups (column 6). Finally, column 7 shows that while the size of the increase is non trivial for students from cash localities, there is no significant difference in the student-teacher ratio of schools attended by students across treatment groups.

In principle, the income effect generated by the transfer should have led to an increase in the propensity to attend a more expensive (and possibly better quality) school option. We find a negligible positive effect on the propensity to attend a general school among students from in-kind localities, and a large negative effect for students from cash localities. In line with the evidence presented in the previous section, the income effect generated by the program does not seem large enough to alter medium-term investments in education by beneficiary households.

As a result of the income shock, school principals in general schools might ask for higher school fees in treatment than in control localities, pushing students towards alternative modalities, and the increase can potentially differ for in-kind and cash localities. In order to test this hypothesis, we use information on the fees charged during the period between 2007 and 2013 by the general schools that were closest to evaluation localities, based on the pre-program school roster in 2003. We present the results in Table 1.13. General schools located closest to cash localities display the highest values for all three types of fees (materials, uniforms, and enrollment). When we look at the total amount, general schools located closest to cash localities have fees that are 306 pesos higher than in general schools closest to in-kind localities, about 39 percent of the average fee in general schools closest to control schools.

In order to provide a quantitative assessment of how important school type might be in explaining the differential effect of in-kind and cash treatment on learning outcomes, we perform a causal mediation analysis (Conti *et al.* , 2016; Carneiro *et al.* , 2019). The bounding procedure is detailed in Supplement 1 and the results are presented in Table 1.14. For all three subjects, the differential treatment effect on the probability of attending a general school can explain a very large share of the differential treatment effect on learning and is always statistically significant. In fact, even after controlling for the quality of the micronutrient intake, the differential effect on school type can explain at least 40 percent of the differential treatment effect on math, 49 percent for Spanish and 32 percent for the third subject.

The results presented in this section provides clear evidence that, when distributed in cash, PAL induced children to enroll in lower quality school options, possibly as a result of

[24] McKenzie & Rapoport (2011) find that internal migration in Mexico can have negative impacts on the education attainment of children from rural areas.

the large increase in fees implemented in the closest general schools. This finding contrasts with that of De Hoop *et al.* (2017) who find that in the presence of a partial subsidy, children in the Philippines increase labor supply in order to cover education costs. However, in our setting, we can not rule out the possibility that for children who decided not to switch to a lower quality option, the increase in the number of working hours might be partly driven by the need to cover the large increase in fees.

1.5.4 Parental investment

There are different channels through which PAL might affect parental involvement in children's education. Depending on whether the income effect generated by the program is larger or smaller than the substitution effect driven by the potentially higher labor market returns, parents might work more or less. Changes in labor supply might alter the involvement in children's education, for instance through help and supervision of home assignments or increased participation in school activities or visits. All else equal, parents might reallocate the time they devote to a specific child, depending on whether they want to complement or compensate for differential investments in early nutrition.

In the *ENLACE de Contexto,* students are asked how often (never, rarely, sometimes, almost always, always) parents are involved in five activities that could impact learning: helping with homework, explaining topics that were not clear from school lectures, inviting them to review class material that was not clear, paying attention to student grades, and attending school meetings. We create an index of parental investment which is the sum of five binary variables indicating whether a student replied "always" or "almost always" to each question. We also construct the first principle component of the five indicator variables. Table 1.10 reports that the treatment effects on both summary indices are positive for both groups, but only significant for the cash group (at the 5 percent level). The difference between the two treatment groups is small and not statistically significant. These results suggest that the differential impact of the in-kind and cash transfers on learning is unlikely to be driven by differences in the amount of time parents invest in activities related to their children's learning.

1.6 Discussion and Conclusions

In this paper, we studied how unconditional cash and in-kind welfare transfers impacted the standardized test scores of primary school children from rural and marginalized areas in Mexico. We merged individual-level data from the randomized controlled trial of the *Programa de Apoyo Alimentario* with administrative panel data on standardized test scores taken 4 to 10 years after transfers began. Despite the fact that both transfer modalities increased household consumption, and that in-kind transfers induced children to consume more key micronutrients, we find that in-kind transfers did not impact student learning and cash transfers significantly reduced student learning.

There are several reasons why the improved micronutrient intake induced by in-kind transfers did not lead to improved learning in primary school. First, the improvement in micronutrients was not accompanied by a significant increase in the overall caloric intake. A high share of children were consuming below the recommended daily allowance of calories even after transfers and a sufficient caloric intake may be a necessary compliment to micronutrient intake. Second, most of the previous evidence on the beneficial effects of iron supplementation on cognitive outcomes is based on interventions that target individuals diagnosed with iron deficiency anemia (IDA) with high doses of iron. At the opposite, in our study all children from PAL beneficiary households are potentially exposed to improved intakes, but the doses do not seem sufficient to benefit those with IDA at the baseline. In fact, we did not observe any statistically significant reduction in anemia in the follow-up survey. Finally, previous evidence shows that zinc supplementation per se is not sufficient to improve cognitive development. Powell *et al.* (2005) find for a group of undernourished children in Jamaica that zinc supplementation increases cognitive development only when complemented by psychosocial stimulation. This result together with the evidence from other studies that analyze the combination of micronutrient supplementation and psychosocial stimulation (see Attanasio *et al.* , 2018a) is consistent with the hypothesis that nutrition interventions only improve the cognitive development of children who have adequate stimulation, which may not have been the case in the poor, rural villages we study.

The discussion above, however, cannot explain why children from cash villages had lower test scores. We find suggestive evidence that these lower scores are driven by students in cash villages attending lower quality schools and being more likely to work. Students attended lower quality community or indigenous schools, possibly because principals at the higher quality general schools increase school fees. Several pieces of evidence are consistent with the hypothesis that cash transfer increased the return to child labor and decreased the time and energy dedicated to learning, including the fact that the negative impacts of the cash modality on learning are more prominent among households with older children and those that are more likely to be credit constrained.

Overall, our results show that the PAL welfare transfers, even when they lead to increased nutritional intake in early childhood, were not sufficient to improve learning in primary school. One lesson for policy is clear: behavioral responses to government policies can be complex and counterintuitive, especially as they compound into the medium- and long-term horizons.

1.7 Tables

Table 1.1: Number of Observations in the Sample by Grade and Year

Academic year	Grade				Total
	3rd	4th	5th	6th	
2007	335	209	30	0	574
2008	557	442	280	37	1,316
2009	557	518	411	261	1,747
2010	531	544	514	403	1,992
2011	517	519	525	513	2,074
2012	420	408	441	426	1,695
2013	329	428	417	434	1,608
Total	3,246	3,068	2,618	2,074	11,006

Notes: Observations are at the student-year level.

Table 1.2: Balance of Main Variables at Baseline

	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	(1)	(2)	(3)		(4)	(5)	(6)
	Child level characteristics						
Male	0.51	0.51	0.47	4,405	0.87	0.04**	0.02***
	(0.02)	(0.01)	(0.01)				
Age	3.10	3.17	3.17	4,448	0.28	0.39	0.97
	(0.05)	(0.04)	(0.06)				
Caloric intake (kcal, daily)	831.67	805.68	817.99	2,347	0.47	0.73	0.72
	(29.40)	(21.06)	(27.00)				
Iron consumption (mg, daily)	5.25	5.07	5.19	2,392	0.47	0.82	0.67
	(0.19)	(0.15)	(0.22)				
Zinc consumption (mg, daily)	3.85	3.77	3.62	2,392	0.75	0.40	0.48
	(0.21)	(0.12)	(0.15)				
Vitamin C consumption (mg, daily)	31.76	29.63	34.92	2,392	0.54	0.51	0.20
	(3.00)	(1.80)	(3.73)				
Z-score height for age	-0.25	-0.21	-0.27	2,719	0.79	0.91	0.71
	(0.12)	(0.10)	(0.13)				
Math grade (1-10 scale, previous school year)	7.86	7.93	7.83	3,352	0.33	0.67	0.20
	(0.05)	(0.04)	(0.06)				
Spanish grade (1-10 scale, previous school year)	7.92	7.92	7.82	3,363	0.97	0.21	0.16
	(0.05)	(0.04)	(0.06)				
	Household level characteristics						
Indigenous household	0.33	0.26	0.27	5,444	0.48	0.57	0.93
	(0.09)	(0.05)	(0.07)				
Number of household members	5.93	5.66	5.74	5,444	0.31	0.54	0.77
	(0.21)	(0.16)	(0.23)				
Household head is married	0.87	0.91	0.92	5,444	0.13	0.04**	0.39
	(0.02)	(0.01)	(0.01)				
Years of education of household head	7.36	7.30	7.11	5,444	0.88	0.56	0.61
	(0.30)	(0.22)	(0.29)				
Dirt floor in the home	0.40	0.40	0.41	5,444	0.94	0.88	0.80
	(0.05)	(0.04)	(0.05)				
Running water in the home	0.65	0.52	0.42	5,444	0.08	0.01***	0.20
	(0.06)	(0.05)	(0.07)				
Monthly per capita total expenditure	382.52	362.34	356.23	5,444	0.47	0.38	0.81
	(22.46)	(16.54)	(20.07)				
	Village level characteristics						
Distance to closest primary school (km)	1.09	1.06	0.91	200	0.95	0.66	0.59
	(0.33)	(0.18)	(0.21)				
Closest school is a general school	0.81	0.87	0.80	200	0.30	0.96	0.26
	(0.06)	(0.03)	(0.05)				
Closest school is a community school	0.06	0.03	0.10	200	0.41	0.45	0.10
	(0.04)	(0.02)	(0.04)				
Closest school is a indigenous school	0.13	0.10	0.09	200	0.52	0.55	0.97
	(0.05)	(0.03)	(0.04)				
Student-teacher ratio in closest school	28.53	29.38	28.51	192	0.60	0.99	0.66
	(1.22)	(1.04)	(1.66)				
Repetition rate in closest school	0.09	0.09	0.12	200	0.70	0.20	0.10
	(0.01)	(0.01)	(0.02)				
Repetition rate in closest community or indigenous school	0.17	0.19	0.18	174	0.55	0.82	0.72
	(0.03)	(0.02)	(0.03)				
Morning shift closest school	0.85	0.91	0.94	200	0.27	0.09*	0.38
	(0.05)	(0.03)	(0.03)				
Yearly parental expenditure per child in closest general school (fees, uniform, books)	378.77	450.19	398.21	200	0.26	0.76	0.39
	(46.05)	(42.80)	(42.47)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos. (7) Parental school expenditure data was only collected for general schools, as government grants cover all costs for community and indigenous schools; it is in 2003 nominal pesos.

Table 1.3: The impact of PAL on taking ENLACE tests

Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE
	(1)	(2)	(3)	(4)
In-kind	0.009	-0.003	0.000	-0.004
	(0.019)	(0.017)	(0.011)	(0.010)
Cash	-0.001	0.004	-0.003	0.004
	(0.027)	(0.023)	(0.016)	(0.012)
Z-score height for age		0.022***		0.015***
		(0.005)		(0.002)
Closest school is a general school		0.029		0.021
		(0.025)		(0.014)
Male		0.000		-0.007
		(0.014)		(0.006)
Age		0.100***		0.073***
		(0.004)		(0.002)
ln(Monthly per capita total expenditure)		-0.017		-0.005
		(0.012)		(0.005)
Age of household head		-0.002***		-0.001***
		(0.001)		(0.000)
Years of education of household head		0.011***		0.006***
		(0.002)		(0.001)
Household head is married		0.070***		0.022**
		(0.025)		(0.011)
Running water in the home		0.005		0.009
		(0.017)		(0.008)
State fixed effects	YES	YES	YES	YES
Year fixed effects	NO	NO	YES	YES
Outcome mean in control group	0.65	0.65	0.27	0.27
Observations	5,444	3,817	38,108	26,719
***Effect size:* In-kind - Cash**	0.01	-0.01	0.00	-0.01
***H_0*: In-kind = Cash (p-value)**	0.69	0.74	0.83	0.48

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) The outcome in columns 1-3 are indicators for whether a student was observed to take any ENLACE test between 2007 and 2013, and regressions include one observation per child. The outcome in columns 4-6 vary by year, and regressions include one observation for each child in every year from 2007 to 2013.

Table 1.4: Explaining ENLACE test scores

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
General school	0.523***	0.666***	0.534***
	(0.121)	(0.143)	(0.100)
Male	-0.117*	-0.220***	-0.180***
	(0.063)	(0.053)	(0.049)
Z-score height for age	0.050**	0.049*	0.044**
	(0.025)	(0.025)	(0.020)
Age	-0.059	-0.080*	-0.037
	(0.048)	(0.046)	(0.042)
ln(Monthly per capita total expenditure)	-0.036	0.008	-0.069
	(0.065)	(0.069)	(0.061)
Age of household head	0.001	0.002	-0.000
	(0.003)	(0.002)	(0.002)
Years of education of household head	0.023**	0.020	0.020**
	(0.011)	(0.012)	(0.009)
Household head is married	-0.018	0.011	0.002
	(0.123)	(0.140)	(0.095)
Running water in the home	0.156**	0.201**	0.168**
	(0.075)	(0.089)	(0.072)
Guerrero	-0.334	-0.657**	-0.445**
	(0.225)	(0.271)	(0.196)
Oaxaca	0.500**	0.365	0.308
	(0.194)	(0.221)	(0.275)
Tabasco	-0.548***	-0.490***	-0.635***
	(0.135)	(0.146)	(0.126)
Veracruz	-0.603***	-0.677***	-0.747***
	(0.100)	(0.131)	(0.091)
Year and grade FE	YES	YES	YES
Observations	1,576	1,576	1,573
R-squared	0.109	0.150	0.128

*** p<0.01, ** p<0.05, * p<0.1.

(1) Standard errors are clustered at the village level.

(2) The ommited state is Chiapas.

(3) Sample includes only individuals from the control group.

Table 1.5: The impact of PAL on learning

Outcome =	Math (z-score)	Math (z-score)	Spanish (z-score)	Spanish (z-score)	3rd subject (z-score)	3rd subject (z-score)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.050	-0.049	-0.025	-0.026	-0.029	-0.029
	(0.078)	(0.073)	(0.086)	(0.081)	(0.075)	(0.071)
Cash	-0.192**	-0.182**	-0.158*	-0.156*	-0.161**	-0.156*
	(0.084)	(0.086)	(0.091)	(0.093)	(0.080)	(0.080)
State FE	YES	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES	NO	YES
Observations	11,006	11,006	11,006	11,006	10,432	10,432
***Effect size:* In-kind - Cash**	0.14	0.13	0.13	0.13	0.15	0.13
H_0: In-kind = Cash (p-value)	0.05*	0.06*	0.07*	0.07*	0.05*	0.05*
H_0: In-kind = Cash (Randomization Inference p-value)	0.08*	0.09*	0.11	0.1	0.08*	0.08*

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.
(3) The 3rd subject was not administered in 2007, and it covered Science in 2008 and 2012, Ethics and Civics in 2009 and 2013, History in 2010, and Geography 2011.

Table 1.6: Heterogeneous impact of PAL on learning by household expenditure

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
In-kind	-0.001	0.051	0.062
	(0.071)	(0.073)	(0.067)
Cash	-0.130	-0.079	-0.085
	(0.081)	(0.083)	(0.076)
In-kind x poor	-0.089	-0.151	-0.181**
	(0.091)	(0.105)	(0.090)
Cash x poor	-0.101	-0.156	-0.144
	(0.108)	(0.119)	(0.102)
Poor	-0.075	-0.056	0.004
	(0.079)	(0.096)	(0.079)
Observations	11,006	11,006	10,432
***Effect size:* In-kind - Cash**	0.13	0.13	0.15
***H_0:* In-kind = Cash (p-value)**	0.07*	0.07*	0.03**
***Effect size:* In-kind x poor - Cash x poor**	0.01	0.01	-0.04
***H_0:* In-kind x poor = Cash x poor (p-value)**	0.89	0.95	0.64

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) All models include state fixed effects, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) "Poor" is an indicator variable equal to one for households with expenditure per capita below median.

Table 1.7: Heterogeneous impact of PAL on learning by indigenous ethnicity

Outcome =	Math (z-score)	Spanish (z-score)	3rd subject (z-score)
	(1)	(2)	(3)
In-kind	0.009	0.021	0.043
	(0.069)	(0.070)	(0.065)
Cash	-0.118	-0.106	-0.086
	(0.075)	(0.078)	(0.073)
In-kind x Indigenous household	-0.245	-0.207	-0.288*
	(0.153)	(0.176)	(0.159)
Cash x Indigenous household	-0.206	-0.137	-0.221
	(0.192)	(0.195)	(0.185)
Indigenous household	-0.291**	-0.375**	-0.199
	(0.141)	(0.170)	(0.156)
Observations	11,006	11,006	10,432
***Effect size:* In-kind - Cash**	0.13	0.13	0.13
***H_0:* In-kind = Cash (p-value)**	0.06*	0.06*	0.05**
Effect size: In-kind x indigenous - Cash x indigenous	-0.04	-0.07	-0.07
***H_0:* In-kind x indigenous = Cash x indigenous (p-value)**	0.81	0.61	0.61

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) All models include state fixed effects, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table 1.8: The impact of PAL on health outcomes

Outcome =	Ever sick in last 4 weeks		z-score height for age		z-score weight for age		Anemia	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In-kind	-0.023	-0.015	0.025	0.031	0.026	0.020	-0.021	-0.020
	(0.027)	(0.027)	(0.107)	(0.118)	(0.085)	(0.094)	(0.029)	(0.031)
Cash	0.001	0.002	-0.109	-0.082	-0.005	-0.001	-0.024	-0.022
	(0.032)	(0.033)	(0.136)	(0.145)	(0.099)	(0.111)	(0.030)	(0.033)
Only those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	4,266	3,138	3,817	2,494	3,861	2,522	2,403	1,855
Outcome mean in control group	0.29	0.30	-0.32	-0.20	0.99	0.12	0.19	0.19
***Effect size:* In-kind - Cash**	-0.02	-0.02	0.13	0.11	0.03	0.02	0.00	0.00
H_0: In-kind = Cash (p-value)	0.38	0.55	0.23	0.32	0.69	0.80	0.91	0.93
H_0: In-kind = Cash (Randomization Inference p-value)	0.37	0.57	0.25	0.34	0.73	0.83	0.92	0.93

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) State fixed effects and the follosing pre-program controls included: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) Sample only includes individuals aged 6 or younger in 2003.

Table 1.9: The impact of PAL on nutrition

Outcome =	Energy (kcal)		Proteins		Vitamin C		Iron	
In-kind	48.13	38.5	1.76	1.37	25.22***	23.89***	1.06***	1.13***
	(41.53)	(46.07)	(1.76)	(1.93)	(5.21)	(5.62)	(0.39)	(0.42)
Cash	0.99	-5.47	1.93	1.73	25.26***	23.96***	0.55	0.54
	(48.65)	(52.74)	(1.99)	(2.24)	(7.63)	(8.68)	(0.42)	(0.45)
Only those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,381	1,856	2,419	1,880	2,419	1,880	2,419	1,880
Outcome mean in control group	967.5	980.1	32.48	33.5	31.89	32.37	6.81	6.79
***Effect size:* In-kind - Cash**	47.13	43.98	-0.17	-0.35	-0.04	-0.07	0.51	0.59
***H_0:* In-kind = Cash (p-value)**	0.21	0.26	0.92	0.84	1.00	0.99	0.20	0.14
***H_0:* In-kind = Cash (Randomization Inference p-value)**	0.22	0.28	0.92	0.85	1.00	0.99	0.21	0.17

Outcome =	Zinc		Calcium		Retinol		Principal component macro/micro	
In-kind	1.16***	1.10***	76.26***	80.73***	114.72**	107.96**	0.25***	0.25***
	(0.27)	(0.29)	(25.01)	(28.59)	(48.29)	(50.54)	(0.07)	(0.08)
Cash	0.52*	0.44	19.67	16.56	27.2	16.34	0.13*	0.12
	(0.27)	(0.30)	(30.68)	(34.20)	(54.69)	(56.47)	(0.08)	(0.08)
Only those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,419	1,880	2,419	1,880	2,419	1,880	2419	1,880
Outcome mean in control group	4.28	4.36	467.5	468.1	360.2	342.2	-0.32	-0.31
***Effect size:* In-kind - Cash**	0.64	0.65	56.60	64.17	87.52	91.61	0.12	0.13
***H_0:* In-kind = Cash (p-value)**	0.02**	0.02**	0.05*	0.04**	0.05*	0.04**	0.10*	0.09*
***H_0:* In-kind = Cash (Randomization Inference p-value)**	0.03**	0.04**	0.05*	0.04**	0.05*	0.05*	0.12	0.12

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) State fiexed effects and the following pre-program controls are included: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.
(3) Sample only includes individuals aged 6 or less in 2003.

Table 1.11: The impact of PAL on child labor

Outcome =	Works & attends school	Only works	Only attends school	Neither works nor attends	Hours of work	Average number of working days per week	At least 1 hour per day of help with domestic work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-kind	-0.001	-0.009	0.004	0.006	-0.653	0.683	0.176
	(0.009)	(0.017)	(0.040)	(0.029)	(0.863)	(0.420)	(0.166)
Cash	0.011	0.030	-0.062	0.021	1.555	1.313**	0.195
	(0.013)	(0.023)	(0.049)	(0.033)	(1.411)	(0.593)	(0.191)
Observations	986	986	986	986	988	310	113
Outcome mean in control group	0.01	0.05	0.84	0.1	2.06	1.12	0.21
***Effect size:* In-kind - Cash**	-0.01	-0.04	0.07	-0.02	-2.21	-0.63	-0.02
H_0: In-kind = Cash (p-value)	0.33	0.05*	0.09*	0.54	0.07*	0.30	0.91
H_0: In-kind = Cash (Randomization Inference p-value)	0.30	0.04**	0.10*	0.59	0.03**	0.37	0.96

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) All models include state, year, and grade fixed effects and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and the whether the closest school offers morning shift.
(3) The outcomes in columns (1) to (5) are based on the information collected in the 2005 follow-up survey and refer to the week prior to the survey; the sample includes children age 12 and 13 who are reported to be enrolled in primary school. Hours of work includes observations with zero hours. The outcomes in columns (6) and (7) are based on the Enlace de Contexto which asks information on the average number of working days for all years between 2008-13 and information on household chores for years between 2011-13.

Table 1.12: The impact of PAL on school characteristics

Outcome =	General school	Km from village to school = [0,5)	Km from village to school = [5,10)	Km from village to school = [10,30)	Km from village to school >= 30	Total parental contributions	Student-teacher ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-kind	0.022	-0.052	-0.037	0.031	0.058	-45.43	0.38
	(0.061)	(0.053)	(0.033)	(0.020)	(0.038)	-121.68	-1.49
Cash	-0.083	-0.074	-0.024	0.056	0.041	-30.83	2.16
	(0.075)	(0.067)	(0.034)	(0.038)	(0.047)	-154.31	-2.29
Observations	10,852	10,852	10,852	10,852	10,852	10,740	10,344
Outcome mean in control group	0.77	0.83	0.07	0.02	0.08	730.42	28.71
***Effect size:* In-kind - Cash**	0.10	0.02	-0.01	-0.03	0.02	-14.6	-1.77
H_0: In-kind = Cash (p-value)	0.07*	0.75	0.54	0.55	0.75	0.89	0.44
H_0: In-kind = Cash (Randomization Inference p-value)	0.07*	0.74	0.51	0.51	0.76	0.89	0.38

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) All models include state fixed effects, year fixed effects, and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and the whether the closest school offers morning shift.

(3) The outcome in column (1) is an indicator for a student attending a general school. The outcomes in column (2)-(5) are indicators for whether the driving distance from the center of the village to the school is within the specified range. The outcome in column (6) is the total cost that includes contributions to the school, materials and uniforms; this cost is by definition 0 in community and indigenous schools (see text). The outcome in column (7) is the average number of students per teacher in the school.

Table 1.13: The impact of PAL on general school fees

Outcome =	Fees for materials	Fees for uniforms	Fees for enrollment	Total fees
	(1)	(2)	(3)	(4)
In-kind	18.070	-7.158	-33.172	-22.261
	(50.691)	(50.162)	(96.926)	(192.011)
Cash	111.118*	81.511	91.201	283.830
	(58.950)	(58.335)	(112.718)	(223.295)
Observations	1,372	1,372	1,372	1,372
Outcome mean in control group	291.7	323.04	162.1	776.85
***Effect size:* In-kind - Cash**	-93.05	-88.67	-124.37	-306.09
H_0: In-kind = Cash (p-value)	0.06*	0.07*	0.19	0.10*
H_0: In-kind = Cash (Randomization Inference p-value)	0.15	0.14	0.22	0.14

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Regressions are at the village-year level.

(2) All models include state fixed effects, year fixed effects, and the following pre-program (2003) controls:distance of the village to the school, total enrollment, and an indicator for whether the school had a morning shift, and an indicator for whether the information on the shift was not available.

(3) Outcomes are from the Formato 911 for the years from 2007 to 2013 and are in nominal pesos.

Table 1.14: Mediation analysis on learning outcomes

Outcome =	Math			Spanish			3rd subject		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In-kind	-0.098	-0.104	-0.108	-0.047	-0.054	-0.062	-0.053	-0.058	-0.058
	(0.080)	(0.070)	(0.071)	(0.089)	(0.075)	(0.075)	(0.077)	(0.070)	(0.071)
Cash	-0.233**	-0.171**	-0.173**	-0.171*	-0.104	-0.108	-0.192**	-0.137*	-0.137*
	(0.091)	(0.078)	(0.078)	(0.100)	(0.084)	(0.083)	(0.087)	(0.078)	(0.078)
Attends a general school		0.621***	0.619***		0.661***	0.659***		0.552***	0.552***
		(0.081)	(0.081)		(0.081)	(0.080)		(0.084)	(0.084)
Principal component of nutrients			0.011			0.024			-0.002
			(0.016)			(0.016)			(0.015)
Observations	5,988	5,988	5,988	5,988	5,988	5,988	5,985	5985	5985
***Effect size:* In-kind - Cash**	0.14	0.07	0.07	0.12	0.05	0.05	0.14	0.0786	0.0790
***H_0*: In-kind = Cash (p-value)**	0.07*	0.32	0.33	0.12	0.49	0.53	0.06*	0.23	0.23
Indirect Differential Effect through School Type:									
Lower Bound		0.06	0.06		0.07	0.06		0.06	0.04
Upper Bound		0.08	0.08		0.09	0.09		0.08	0.08
Indirect Differential Effect through Micronutrients:									
Lower Bound			-0.01			0.00			-0.01
Upper Bound			0.01			0.01			0.01

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level. The lower bound and the upper bound are computed based on the results of 1,000 Montecarlo simulations.

(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) Sample is restricted only to children for which learning outcomes both intermediate outcomes are measured.

(4) Principal component includes proteins, iron, zinc, calcium and retinol.

Table 1.10: The impact of PAL on parental investment

Outcome =	Index of parental involvement in activities	Principal component of parental activities
	(1)	(2)
In-kind	0.459*	0.309*
	(0.266)	(0.183)
Cash	0.343	0.238
	(0.319)	(0.219)
Observations	283	283
Outcome mean in control group	3.19	-0.02
***Effect size:* In-kind - Cash**	0.12	0.07
H_0: In-kind = Cash (p-value)	0.68	0.71
H_0: In-kind = Cash (Randomization Inference p-value)	0.78	0.81

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

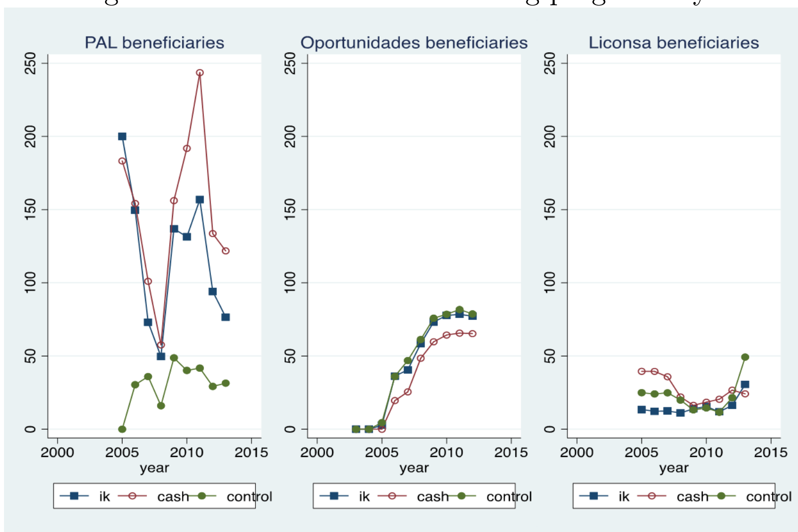
(2) All models include state, year, and grade fixed effects and the following pre-program controls: gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

(3) Results based on the Enlace de Contexto for all years between 2008-13

(4) The *Index of parental activities* equals the number of activities where parents are involved among the following: 1) helping with homework; 2) explaining topics that were not clear from the lecture; 3) inviting them to review class material that was not clear; 4) paying attention to student grades; 5) attending school meetings. The principal component of parental activities is the first component of the 5 indicators of parental activity.

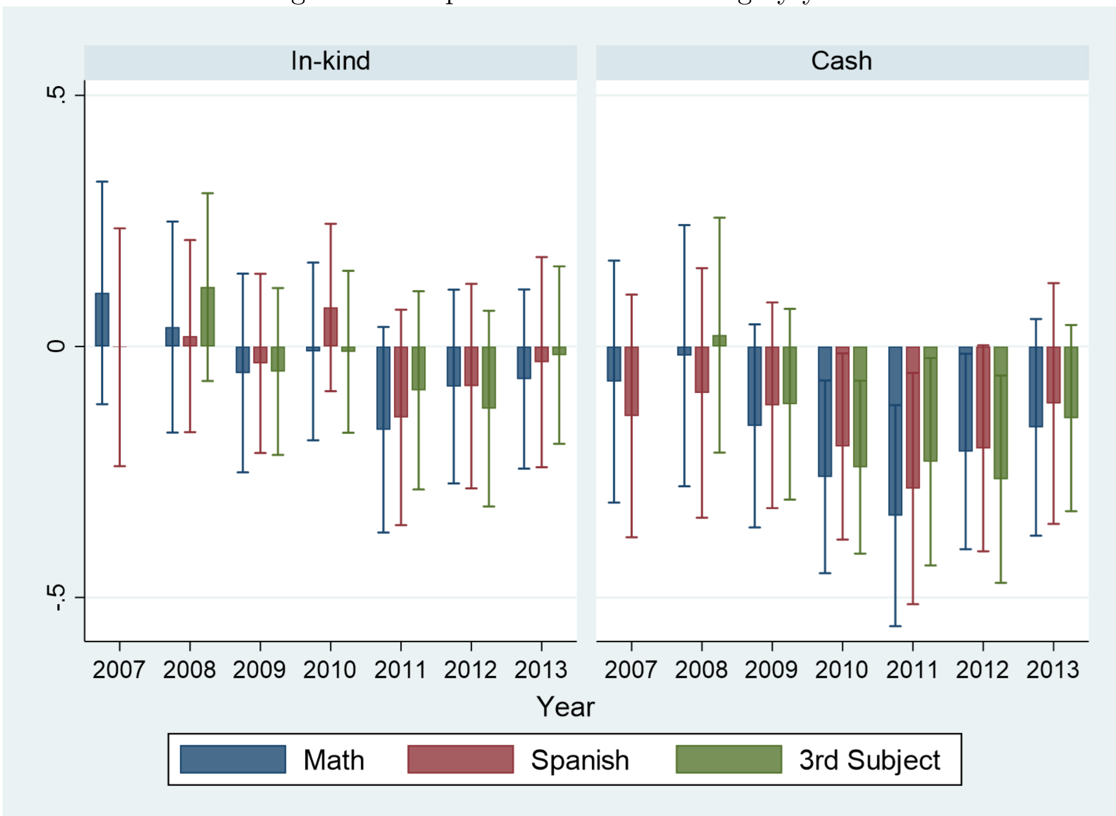
1.8 Figures

Figure 1.1: Average number of households receiving programs by treatment group



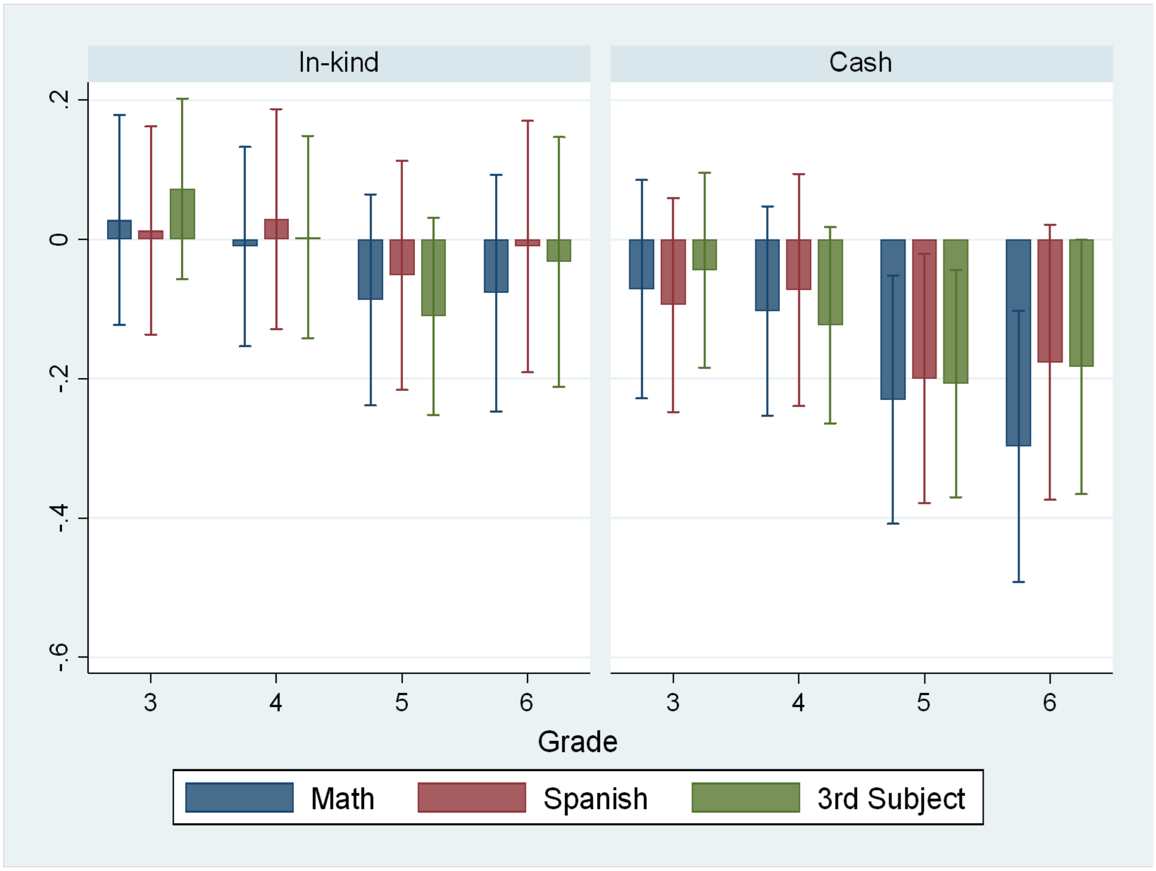
Notes: Year/group specific averages were obtained by averaging village level number of beneficiary households provided by SEDESOL.

Figure 1.2: Impact of PAL on learning by year



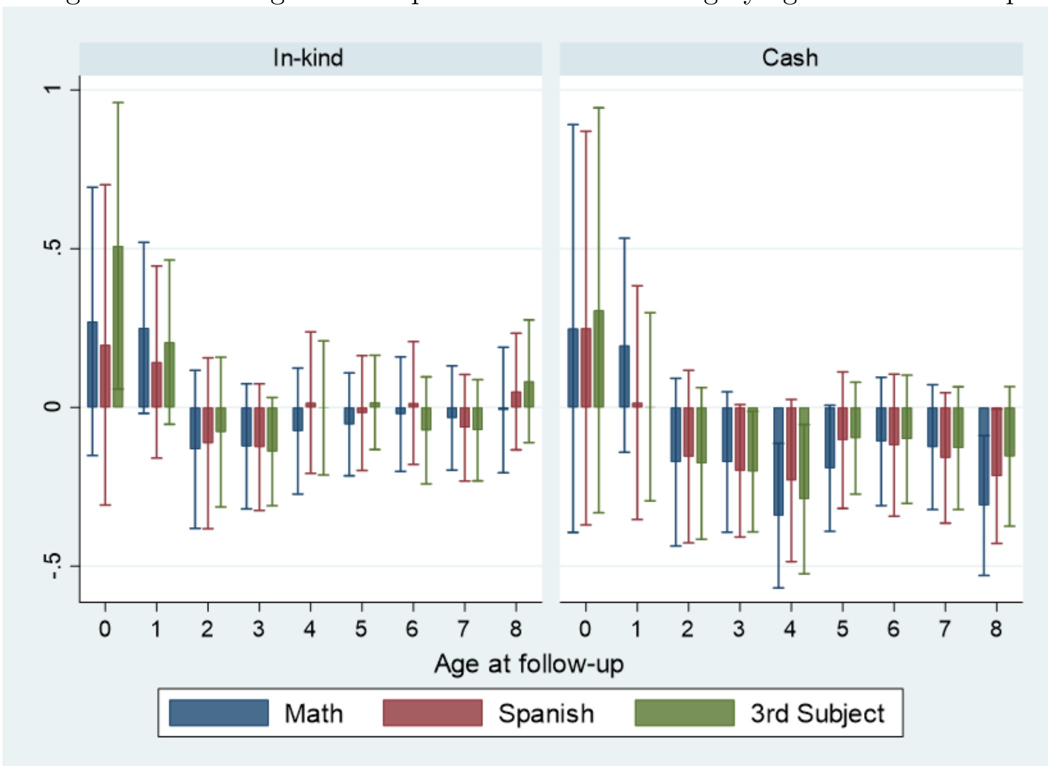
Notes: (1) Coefficients are from models that include pre-program controls, state and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 1.3: Heterogeneous impact of PAL on learning by grade



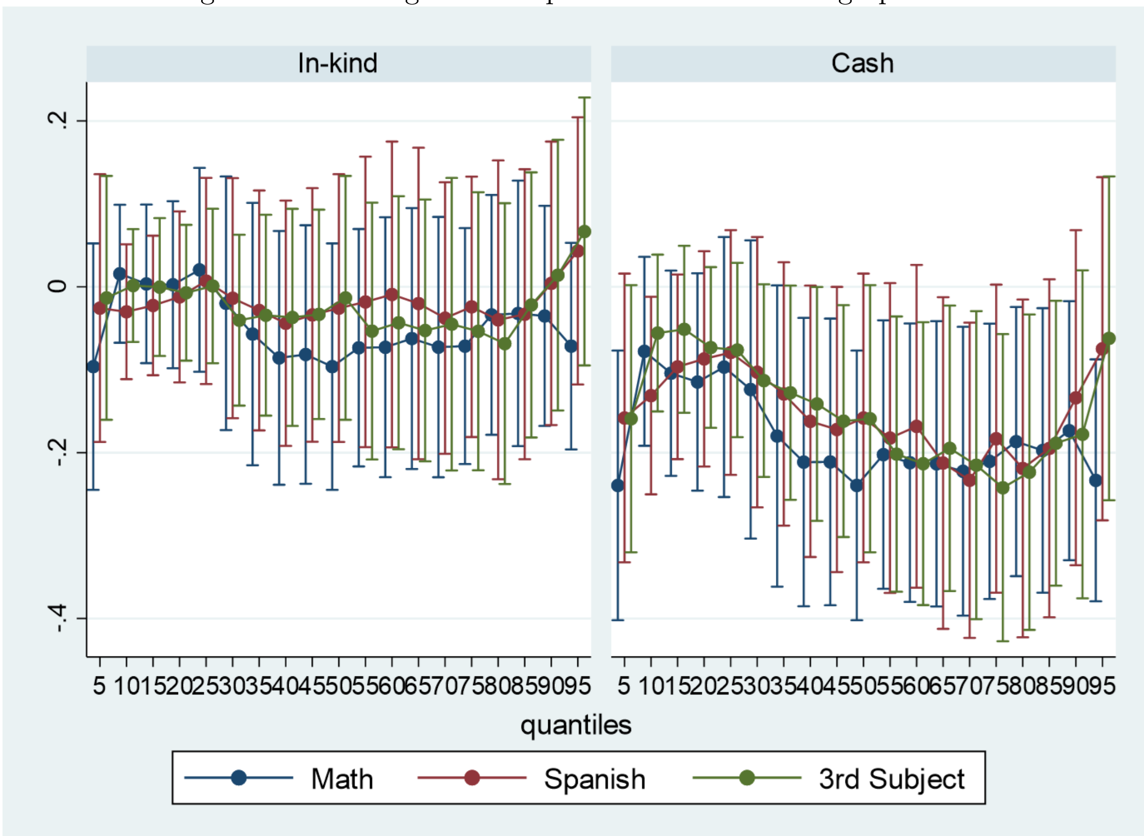
Notes: (1) Coefficients are from models that include pre-program controls, and state and year fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 1.4: Heterogeneous impact of PAL on learning by age at the follow-up



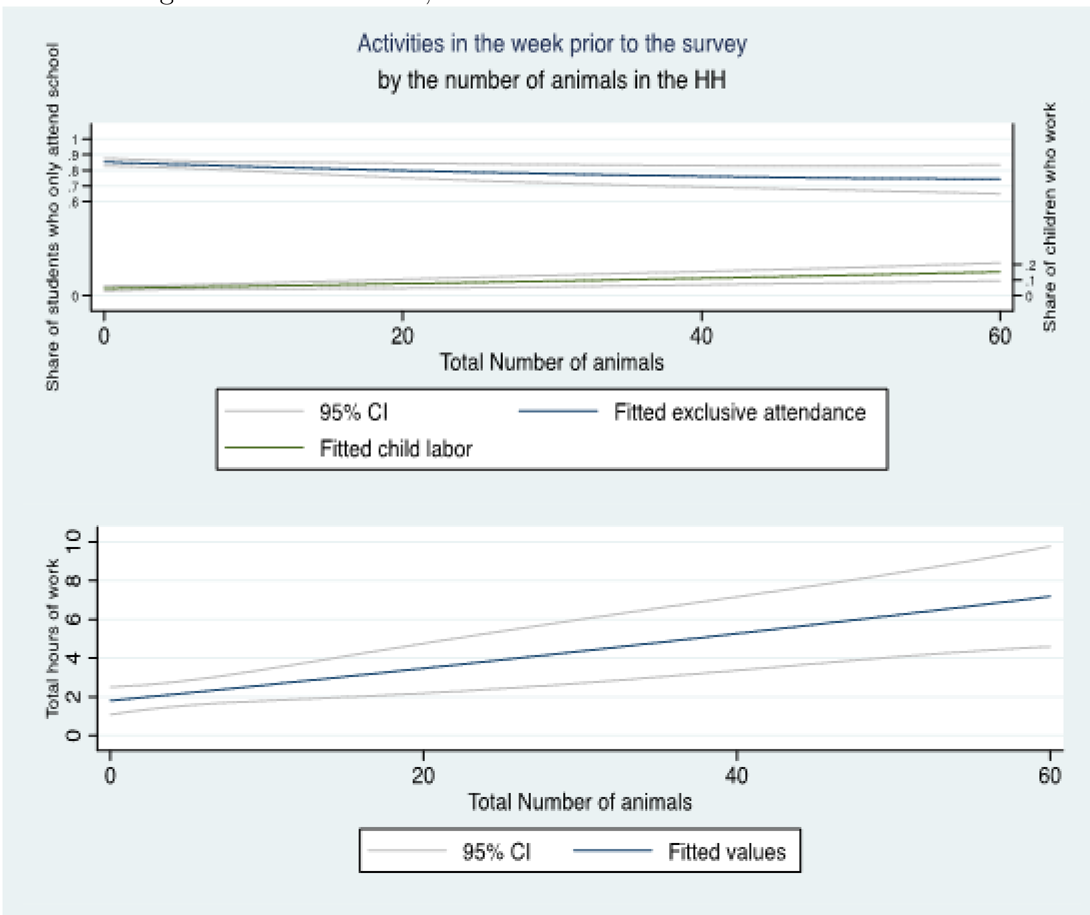
Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 1.5: Heterogeneous impact of PAL on learning: quantiles



Notes: (1) Coefficients are from models that include pre-program controls, and state, year, and grade fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

Figure 1.6: Child labor, school attendance and number of animals



Source: Baseline household survey. The sample is restricted to children age 12 and 13 who report being enrolled in primary school. The top panel plots the share of students who report attending school as the only activity and the share of those working (either exclusively or in combination with school attendance) vis a vis the total number of animals owned by the household. The bottom panel plots the number of hours of work (including 0s) vis-a-vis the total number of animals owned by the household.

1.9 Supplement 1

Data Merge

Mexican citizens have a unique personal identifier, known as *Clave Única de Registro Poblacional, CURP*, formed by an algorithm combining name, surname, date of birth, sex, state of birth, plus two randomly generated digits. Using individual personal information collected both during the baseline and follow-up survey we were able to generate a quasi-*CURP* that differs from the real one only in the lack of the last two randomly generated digits. With the quasi-*CURPs* in hand, we were able to merge the baseline survey with the micro data from the ENLACE 3rd to 6th grade for the period 2007-2013, and the *ENLACE de contexto*. There are two potential explanations for the partial attrition of the ENLACE scores: (1) the exam is voluntary and students enrolled in primary might have not taken it, and (2) matching issues arose either because we could not generate a quasi-*CURP* or there were multiple individuals with the same identifier.

1.9.1 Mediation Analysis

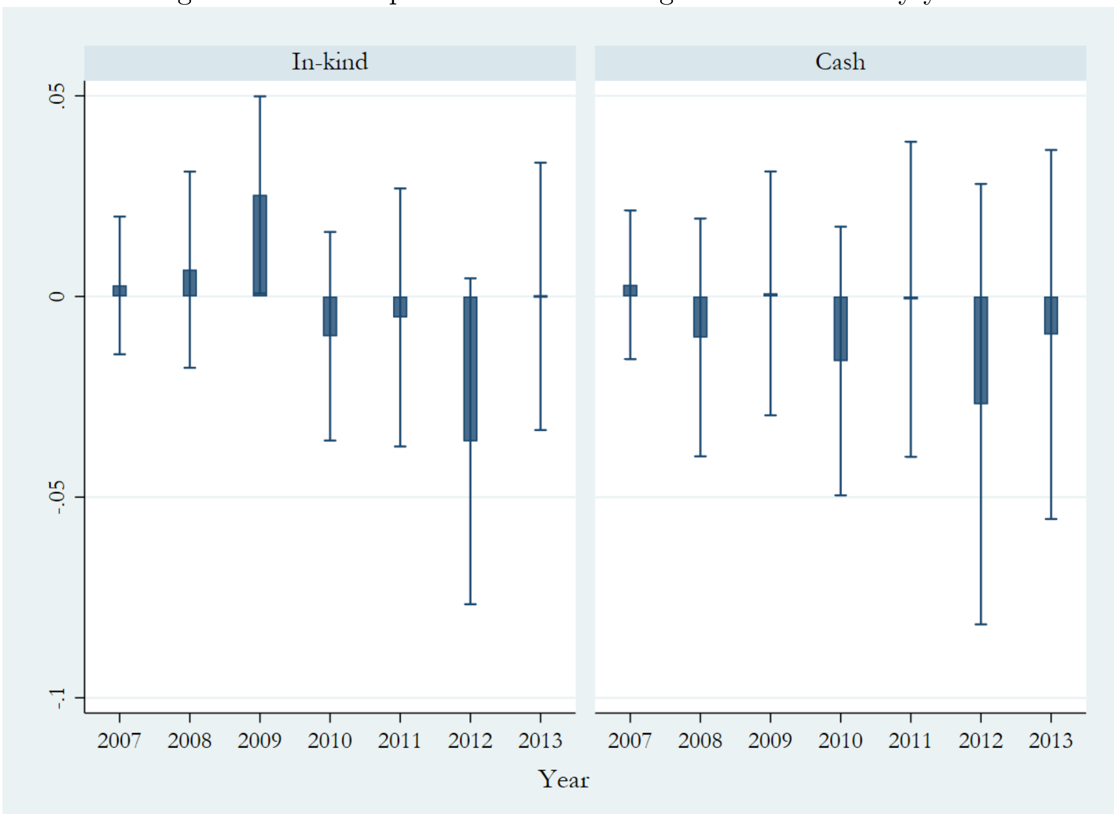
In this section we describe a standard mediation analysis to examine to what extent the differential impact of the in-kind and cash modalities on learning outcomes is driven by the differential impact on the type of school attended and the quality of micronutrient intakes. The assumptions under which one can decompose treatment effects estimates into different components are however very strong. This means that the results can only be interpreted as suggestive evidence of the importance of these mediators. In a standard mediation model where the outcome of interest is Y and the mediating factor (observed measured input) is M (it can be a vector of factors), the goal is to separately identify the interventions total indirect effect $((\gamma_{ik} - \gamma_{cash}) \cdot \delta)$ from the direct differential effect $(\beta_{ik} - \beta_{cash})$ from the following model:

$$
\begin{aligned}
Y_{ij} &= \alpha_0 + \beta_{ik} Ik_j + \beta_{cash} Cash_j + \delta M_{ij} + u_{ij} \\
M_{ij} &= \alpha_1 + \gamma_{ik} Ik_j + \gamma_{cash} Cash_j + e_{ij}
\end{aligned}
$$

where γ_{ik} and γ_{cash} are the ITT estimates of PAL on a particular mediator (type of school and quality of micronutrients), and δ the marginal effect of mediator on the learning outcomes. We estimate the model in steps using a Monte Carlo simulation approach. First, we estimate the coefficients by regressing the effect of in-kind and cash treatment assignments on each mediator. Second, we obtain estimates of δ from a regression of learning outcomes on treatment status (as in the ITT equation, controlling for the baseline regressors) and add one particular mediator at a time. We then compute the lower bound and upper bound of $((\gamma_{ik} - \gamma_{cash}) \cdot \delta)$ based on 1,000 Montecarlo repetitions. An interval that does not include zero indicates a significant indirect differential effect of that particular mediating variable on learning outcomes.

1.9.2 Supplemental Figures

Figure 1.7: The impact of PAL on taking ENLACE tests by year



Notes: (1) Coefficients are from models that include pre-program controls and state fixed effects. (2) The 90% confidence intervals were estimated with standard errors clustered at the village level.

1.9.3 Supplemental Tables

Table 1.15: Post-experiment

	Year 2005			Year 2010		
Outcome=	Number of Households	Households with Oportunidade	Share of Oportunidades recipients	Number of Households	Households with Oportunidade	Share of Oportunidades recipients
	(1)	(2)	(3)	(4)	(5)	(6)
In-Kind	-58.33*	-1.760	0.00407	-57.97*	-7.800	-0.0175
	(34.89)	(4.155)	(0.00620)	(34.81)	(13.36)	(0.0468)
Cash	-65.77*	-1.184	0.00274	-63.78*	-17.02	-0.0811
	(36.24)	(2.798)	(0.00418)	(36.18)	(15.37)	(0.0553)
Observations	197	197	197	197	197	190
Outcome mean in control group	244.64	4.62	0.01	245.28	83.55	0.42
***Effect size:* In-kind - Cash**	7.44	-0.58	0.00	5.81	9.22	0.06
H_0: In-kind = Cash (p-value)	0.78	0.67	0.51	0.83	0.49	0.19

Notes: *** p<0.01, ** p<0.05, * p<0.1.

(1) The number of households was obtained from Population Census. The number of households that receive Oportunidades was provided by SEDESOL. The share of households that receive Oportunidades is calculated as ratio of the former two.

(2) State fixed effects included.

Table 1.16: Impact of PAL on the probability of being in the right age for grade

Outcome =	Appropriate age for grade	At least one appropriate age for
	(1)	(2)
In-kind	0.000	-0.007
	(0.006)	(0.018)
Cash	0.003	-0.021
	(0.007)	(0.027)
State FE	YES	YES
Year FE	YES	NO
Grade FE	YES	NO
Pre-program controls	YES	YES
Observations	11,006	5,444
Outcome mean in control group	0.24	0.62
Effect size: **In-kind - Cash**	0.00	0.01
H_0: **In-kind = Cash (p-value)**	0.60	0.57

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) Pre-program controls include gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) The dependent variable in (1) is an indicator function that equals one when a student has the appropriate age for the grade he/she is observed. The appropriate ages are defined based on the number of completed years of age at Dec 31st of the year observed. The appropriate ages are between 8 and 10 for grade 3, between 9 and 11 for grade 4, between 10 and 12 for grade 5, and between 11 and 13 for grade 6.

(4) The dependent variable in (2) is an indicator variable that equals one if the student was observed at least once with the appropriate age for his/her grade.

Table 1.17: Balance across treatment arms at baseline amongst the non-attrited sample

	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	(1)	(2)	(3)		(4)	(5)	(6)
	Child level characteristics						
Male	0.53	0.53	0.47	3,773	0.91	0.04	0.01
	(0.02)	(0.01)	(0.02)				
Age	2.93	2.90	2.89	3,773	0.80	0.72	0.89
	(0.09)	(0.07)	(0.08)				
Caloric intake (kcal, daily)	826.78	814.55	846.26	1,840	0.75	0.66	0.40
	(31.19)	(21.93)	(30.54)				
Iron consumption (mg, daily)	5.13	5.09	5.26	1,875	0.89	0.70	0.56
	(0.22)	(0.16)	(0.24)				
Zinc consumption (mg, daily)	3.78	3.75	3.72	1,875	0.93	0.85	0.86
	(0.25)	(0.12)	(0.15)				
Vitamin C consumption (mg, daily)	33.19	30.88	35.01	1,875	0.59	0.74	0.38
	(3.68)	(2.12)	(4.15)				
Z-score height for age	-0.19	-0.17	-0.14	2,094	0.90	0.80	0.87
	(0.13)	(0.11)	(0.13)				
	Household level characteristics						
Indigenous household	0.30	0.25	0.24	3,773	0.63	0.59	0.92
	(0.08)	(0.05)	(0.07)				
Number of household members	5.80	5.63	5.68	3,773	0.54	0.71	0.86
	(0.22)	(0.16)	(0.22)				
Household head is married	0.90	0.91	0.92	3,773	0.43	0.16	0.41
	(0.02)	(0.01)	(0.01)				
Years of education of household head	7.67	7.43	7.43	3,773	0.51	0.55	0.99
	(0.30)	(0.23)	(0.28)				
Dirt floor in the home	0.35	0.38	0.36	3,773	0.67	0.89	0.75
	(0.05)	(0.04)	(0.04)				
Running water in the home	0.65	0.53	0.43	3,773	0.13	0.01	0.19
	(0.06)	(0.05)	(0.07)				
Monthly per capita total expenditure	386.97	360.12	362.98	3,773	0.42	0.49	0.92
	(27.69)	(18.23)	(21.54)				
	Village level characteristics						
Distance to closest primary school (km)	0.91	1.17	0.89	3,773	0.48	0.96	0.36
	(0.28)	(0.22)	(0.20)				
Closest school is a general school	0.76	0.79	0.73	3,773	0.76	0.76	0.51
	(0.08)	(0.05)	(0.07)				
Closest school is a community school	0.05	0.05	0.13	3,773	0.84	0.16	0.21
	(0.03)	(0.03)	(0.05)				
Closest school is a indigenous school	0.19	0.15	0.14	3,773	0.68	0.59	0.85
	(0.07)	(0.05)	(0.06)				
Student-teacher ratio in closest school	30.23	29.34	30.04	3,658	0.65	0.94	0.78
	(1.62)	(1.10)	(2.19)				
Repetition rate in closest school	0.10	0.09	0.13	3,773	0.91	0.27	0.20
	(0.01)	(0.01)	(0.03)				
Repetition rate in closest community or indigenous school	0.16	0.17	0.19	3,325	0.71	0.61	0.78
	(0.03)	(0.02)	(0.04)				
Morning shift closest school	0.78	0.85	0.93	3,773	0.41	0.07	0.20
	(0.07)	(0.05)	(0.04)				
Yearly parental expenditure per child in closest general school (fees, uniform, books)	338.23	406.01	377.42	3,773	0.32	0.58	0.68
	(49.20)	(47.47)	(50.63)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos. (7) Parental school expenditure data was only collected for general schools, as government grants cover all costs for community and indigenous schools; it is in 2003 nominal pesos.

Table 1.18: The impact of PAL on test taking - 3 treatment arms

Outcome =	Took at least one ENLACE	Took at least one ENLACE	Took ENLACE	Took ENLACE
	(1)	(2)	(3)	(4)
In-kind only	0.023	0.019	0.005	0.004
	(0.021)	(0.020)	(0.012)	(0.011)
In-kind & Education classes	-0.006	-0.017	-0.004	-0.009
	(0.022)	(0.019)	(0.012)	(0.011)
Cash	-0.001	-0.015	-0.003	-0.009
	(0.027)	(0.025)	(0.016)	(0.014)
State fixed effects	YES	YES	YES	YES
Year fixed effects	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES
Observations	5,444	5,444	38,108	38,108
Outcome mean in control group	0.65	0.65	0.27	0.27
***Effect size:* In-kind only - In-kind & Education classes**	-0.03	-0.04	-0.01	-0.01
***H_0:* In-kind only - In-kind & Education classes (p-value)**	0.15	0.07*	0.40	0.19

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) The outcome in columns 1-3 are indicators for whether a student was observed to take any ENLACE test between 2007 and 2013, and regressions include one observation per child. The outcome in columns 4-6 vary by year, and regressions include one observation for each child in every year from 2007 to 2013.
(3) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table 1.19: The impact of PAL on learning - 3 treatments arms

Outcome =	Math (z-score)	Math (z-score)	Spanish (z-score)	Spanish (z-score)	3rd subject (z-score)	3rd subject (z-score)
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind only	-0.069	-0.052	-0.038	-0.018	-0.048	-0.032
	(0.094)	(0.085)	(0.102)	-0.092	(0.088)	(0.080)
In-kind & Education classes	-0.031	-0.046	-0.012	-0.034	-0.009	-0.027
	(0.088)	(0.085)	(0.094)	(0.091)	(0.088)	(0.084)
Cash	-0.192**	-0.182**	-0.159*	-0.156*	-0.162**	-0.156*
	(0.084)	(0.086)	(0.091)	(0.093)	(0.080)	(0.080)
State FE	YES	YES	YES	YES	YES	YES
Year FE	NO	YES	NO	YES	NO	YES
Grade FE	NO	YES	NO	YES	NO	YES
Pre-program controls	NO	YES	NO	YES	NO	YES
Observations	11,006	11,006	11,006	11,006	10,432	10,432
***Effect size:* In-kind only - In-kind & Education classes**	0.04	0.01	0.03	-0.02	0.04	0.01
***H_0:* In-kind only - In-kind & Education classes (p-value)**	0.69	0.95	0.78	0.85	0.67	0.95

Notes: *** p<0.01, ** p<0.05, * p<0.1
(1) Standard errors are clustered at the village level.
(2) Pre-program controls include gender, age, and indicators for whether the household head is married, whether the house has running water, and whether the closest school offers morning shift.

Table 1.20: Balance of Main Variables at Baseline for Sample in *ENLACE de Contexto*

	(1)	(2)	(3)		(4)	(5)	(6)
	Control	In-kind	Cash	Obs.	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value
	Child level characteristics						
Male	0.51	0.50	0.41	461	0.91	0.24	0.17
	(0.503)	(0.501)	(0.495)				
Age at baseline	4.96	5.16	5.37	461	0.76	0.56	0.76
	(3.479)	(5.970)	(4.205)				
Caloric intake, kcal daily	1,055.00	827.16	890.91	196	0.09	0.23	0.56
	(615.177)	(507.731)	(397.841)				
Iron consumption, mg daily	5.67	5.58	6.82	199	0.90	0.42	0.36
	(4.748)	(3.692)	(6.158)				
Zinc consumption, mg daily	5.22	3.67	4.67	199	0.15	0.66	0.21
	(4.463)	(2.410)	(2.961)				
Vitamin C consumption, mg daily	51.86	25.66	55.16	199	0.03	0.87	0.11
	(63.703)	(37.474)	(81.977)				
Z score height for age	0.44	-0.37	0.22	203	0.04	0.59	0.11
	(1.553)	(1.346)	(1.493)				
	Household level characteristics						
Indigenous household	0.23	0.27	0.11	376	0.76	0.31	0.11
	(0.420)	(0.443)	(0.308)				
Number of household members	5.20	5.75	5.29	376	0.14	0.82	0.25
	(1.817)	(2.296)	(1.987)				
Married household head	0.88	0.93	0.91	376	0.10	0.38	0.31
	(0.333)	(0.250)	(0.292)				
Maximum years of education in HH	7.83	7.16	7.83	376	0.21	1.00	0.19
	(2.759)	(3.041)	(3.189)				
House has a dirt floor	0.28	0.32	0.24	376	0.66	0.80	0.37
	(0.449)	(0.469)	(0.432)				
House has plumbing	0.75	0.65	0.61	376	0.36	0.23	0.69
	(0.436)	(0.477)	(0.492)				
Total expenditure per capita in the household	434.44	344.28	401.61	376	0.10	0.55	0.20
	(248.866)	(231.506)	(236.781)				
	Village level characteristics						
Distance to closest primary school (km)	1.28	1.04	0.94	99	0.66	0.58	0.81
	(2.717)	(1.506)	(1.524)				
Closest school is a general school	0.81	0.78	0.75	99	0.71	0.63	0.83
	(0.389)	(0.416)	(0.444)				
Closest school is a community school	0.07	0.06	0.15	99	0.83	0.41	0.30
	(0.262)	(0.238)	(0.366)				
Closest school is a indigenous school	0.12	0.17	0.10	99	0.55	0.83	0.44
	(0.317)	(0.370)	(0.308)				
Student-teacher ratio in closest school	28.72	29.12	26.45	98	0.87	0.34	0.25
	(9.223)	(11.744)	(7.020)				
Repetition rate in closest school	0.10	0.10	0.11	99	0.88	0.65	0.68
	(0.086)	(0.067)	(0.155)				
Repetition rate in closest community or indigenous school	0.13	0.21	0.21	90	0.05*	0.18	0.94
	(0.134)	(0.195)	(0.212)				
Morning shift closest school	0.74	0.86	0.93	99	0.17	0.05*	0.38
	(0.417)	(0.317)	(0.245)				
Yearly expenditure per child in closest school (fees, uniform, books)	4,217.62	4,035.20	3,907.50	99	0.71	0.63	0.83
	(2,029.062)	(2,167.869)	(2,314.603)				

Notes: *** p<0.01, ** p<0.05, * p<0.1 (1) Standard errors in parentheses, clustered at the village level. (2) Data are from the pre-intervention PAL survey and the 2003 Formato 911 school databases. (3) Child consumption data was only collected for children aged 1 to 4 in the pre-program survey. (4) Math and Spanish grades are self-reported recalls of the student's most recent report card, sample includes students currently attending school. (5) A household is defined as indigenous if one or more members speak an indigenous language. (6) Expenditure is the value of non-durable items (food and non-food) consumed in the preceding month, measured in pesos.

Table 1.21: The impact of PAL on learning - categorical classification

Outcome =	Math Insuff.	Spanish Insuff.	3rd subject Insuff.	Math Excellent	Spanish Excellent	3rd subject Excellent	Levels Math	Levels Spanish	Levels 3rd subject
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In-kind	0.011	0.012	0.006	-0.017	-0.003	-0.010	-0.052	-0.022	-0.027
	(0.025)	(0.026)	(0.017)	(0.028)	(0.030)	(0.023)	(0.063)	(0.063)	(0.052)
Cash	0.051*	0.054*	0.018	-0.072**	-0.050	-0.054**	-0.162**	-0.120*	-0.114*
	(0.031)	(0.032)	(0.020)	(0.032)	(0.034)	(0.025)	(0.073)	(0.071)	(0.058)
Observations	11,006	11,006	11,006	11,006	11,006	11,006	11,006	11,006	8,737
***Effect size:* In-kind - Cash**	-0.04	-0.04	-0.01	0.06	0.05	0.04	0.11	0.10	0.09
H_0: In-kind = Cash (p-value)	0.14	0.11	0.48	0.03**	0.06*	0.03**	0.06*	0.06*	0.08*

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) State, year, and grade fixed effects and the following pre-program controls included: gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) The 3rd subject was not administered in 2007 and it covered Science in 2008 and 2012, Ethics and Civics in 2009 and 2013, History in 2010, and Geography 2011.

(4) All dependent variables were created using categorical classification of the ENLACE for each subject. There are 4 categories: Insufficient, Sufficient, Good, Excellent. The dependent variables in columns (1)-(3) are indicator variables equal to 1 for test scores being insufficient, 0 otherwise. The dependent variables in columns (4)-(6) are indicator variables equal to 1 for test scores being excellent. The dependent variables in columns (7)-(9) takes the value between 0 and 3, with 0 being Insufficient and 3 being Excellent.

Table 1.22: The impact of PAL on nutrition - RDA outcomes

Outcome =	RDA energy (kcal)	RDA energy (kcal)	RDA protein	RDA protein	RDA vitamin C	RDA vitamin C	RDA iron	RDA iron	RDA zinc	RDA zinc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
In-kind	0.045	0.041	0.048	0.038	0.171***	0.153***	0.066**	0.068**	0.107***	0.110**
	(0.031)	(0.033)	(0.030)	(0.033)	(0.038)	(0.043)	(0.027)	(0.030)	(0.039)	(0.044)
Cash	-0.001	-0.001	0.050	0.049	0.084*	0.053	0.040	0.023	0.059	0.050
	(0.036)	(0.036)	(0.037)	(0.040)	(0.048)	(0.054)	(0.031)	(0.035)	(0.045)	(0.050)
Only those with ENLACE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2,419	1,880	2,419	1,880	2,419	1,880	2,419	1,880	2,419	1,880
Outcome mean in control group	0.22	0.21	0.78	0.79	0.46	0.46	0.76	0.76	0.54	0.54
***Effect size:* In-kind - Cash**	0.05	0.04	0.00	-0.01	0.09	0.10	0.03	0.04	0.05	0.06
H_0: In-kind = Cash (p-value)	0.12	0.16	0.94	0.71	0.05**	0.03*	0.30	0.12	0.19	0.13

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level.

(2) State fixed effects and the following pre-program controls are included: gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) Sample only includes individuals aged 6 or less in 2003.

(4) Outcome variables are indicator variables equal to 1 if value of the macro/micro nutrients exceeds the RDA.

Table 1.23: The impact of PAL on proxies for the returns of child labor

Outcome =	Total number of animals	HH farmed or raised animals in past year	Number of HH members farming	Average number of hours farming
	(1)	(2)	(3)	(4)
In-kind	1.007	0.126***	0.108	3.162
	(1.253)	(0.044)	(0.070)	(3.428)
Cash	3.528**	0.196***	0.221**	7.965
	(1.546)	(0.050)	(0.091)	(5.211)
Observations	3,013	3,013	3,013	1,307
Outcome mean in control group	6.95	0.51	0.52	52.95
Effect size: **In-kind - Cash**	-2.52	-0.07	-0.11	-4.80
H_0: **In-kind = Cash (p-value)**	0.04**	0.09*	0.15	0.33
H_0: **In-kind = Cash (Randomization Inference p-value)**	0.04**	0.12	0.14	0.33

Notes: *** p<0.01, ** p<0.05, * p<0.1

(1) Standard errors are clustered at the village level. Sample restricted to those with ENLACE test scores.

(2) State fixed effects and the following pre-program controls included: gender, age, indicators for whether the household head is married, the house has running water and the closest school offers morning shift.

(3) The outcome in column (1) is the total number of the number of small and large animals owned by the household; the outcome in column (2) is an indicator for whether any household member was involved either in farming or raising animals, the outcome in column (3) is the total number of household members who reported spending time farming; and the outcome in column (4) is the average number of hours spent farming among household members.

Chapter 2

Sorting, peer effects and school effectiveness in private and public schools

[1]

2.1 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 better suit their individual needs and, more fundamentally, by leveraging market pressures to motivate school leaders to deliver effective programs (Friedman, 1962). Nevertheless, they remain one of the most contentious instruments of education policy. The ongoing controversy stems partly from the fact that evidence on how vouchers affect educational outcomes, of both the students who take them up and those who remain in the public system, remains incomplete and inconclusive (see Epple *et al.* (2017) for a comprehensive review). An extensive theoretical literature provides some guidance, but also makes clear that the general equilibrium effects of vouchers will depend heavily on the details of their design and implementation.[2] One prediction that emerges almost universally from these theoretical models – and adds to the controversy – is that voucher programs are likely to increase sorting and stratification on academic ability and income, as private schools "cream-skim" the most able or affluent students from the public system. Whether and how

[1] co-authored with Jane Friesen and Simon Woodcock.

[2] Friedman (1962) made the initial case for vouchers. Manski (1992) presents a seminal equilibrium model of an educational market with vouchers. More recent contributions include Epple & Romano (1998, 2008); Nechyba (1999, 2000, 2003); McMillan (2004); Ferreyra (2007); MacLeod & Urquiola (2009, 2013, 2015); Ferreyra (2007); Chakrabarti (2013); Neilson (2017).

this affects academic outcomes, however, will depend on the role of peers in those outcomes, and how public schools respond to increased competition from private schools.

We present novel empirical evidence that student sorting and private school quality have large effects on student achievement under a mature universal voucher program. Our estimates are based on longitudinal student-level data for the universe of grade 4-7 students in the Lower Mainland region of British Columbia (B.C.), Canada.[3] Most private schools in B.C. receive a per-student operating grant equal to 35 or 50 percent of the corresponding public school amount.[4] This emulates a universal voucher system, similar to Denmark's in key features and take-up rates (see Table 2.1). B.C. private schools are free to set any admissions criteria consistent with human rights laws and charge any amount of tuition, which makes school-level sorting and stratification by income and ability likely (see Epple *et al.* (2017)). They must also administer the same low-stakes centrally graded standardized tests in grades 4 and 7 as public schools, and these results are publicly disseminated and widely discussed (Federation of Private Schools Associations, British Columbia, 2015). B.C. private school students score very well on these tests: their average scores exceed public school students' by about one third of a standard deviation in our data. This raises a number of important questions, namely: does the strong performance of private school students reflect positive selection into private schools, more effective schools, or the influence of "better" peers? And if selection is important, do peer effects in academic outcomes imply that private school students' gains from cream-skimming come at the expense of students who remain in the public system?

Our data offer a rare opportunity to answer these questions. They allow us to make direct comparisons between outcomes in public and private schools, because we observe considerable student mobility between schools and sectors, and all students take the same standardized tests. We exploit these features of our data to estimate an empirical model of test scores that controls for the time-invariant observable and unobservable characteristics of students, schools, and peers, via fixed effects. Following Arcidiacono *et al.* (2012) and Burke & Sass (2013), we define peer ability in terms of the fixed effects of students' peers. This mitigates the measurement error that arises when lagged test scores are used as proxies for peer quality (Angrist, 2014; Feld & Zölitz, 2017), side-steps the reflection problem (Manski, 1993), and accounts for both observable and unobservable peer characteristics and behaviors that are associated with spillovers (Fruehwirth, 2014). Our specification allows us to distinguish between selection, school effectiveness, and peer quality as potential ex-

[3]The Lower Mainland consists of the city of Vancouver and 15 surrounding municipalities. Its population of about 2.5 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.

[4]We discuss eligibility requirements for the grants, which almost all B.C. private schools satisfy, in Section 2.2.

planations for the strong academic performance of private school students. Equivalently, it allows us to characterize the relative effectiveness of public and private schools, net of potentially confounding differences in the unobserved characteristics of their students, and in the presence of peer spillovers. Indeed, our paper is the first to directly distinguish between the contributions of peers versus other school inputs to private school effectiveness. This distinction has important policy implications because school effectiveness that is driven by interactions among highly selected students cannot be replicated system-wide.

Our approach also admits a natural characterization of selection into private schools in terms of student fixed effects. Previous studies of universal voucher programs in Chile, Sweden, and New Zealand have measured sorting between public and private schools in terms of socioeconomic status and achievement and generally find evidence that students sort on such characteristics (see Epple *et al.* (2017) for a review). While sorting on characteristics like socioeconomic status and race or ethnicity may be of direct interest to policy makers, such characteristics mainly serve as proxies for ability when assessing how sorting affects achievement in the presence of spillovers (Hoxby & Weingarth, 2005; Fruehwirth, 2014). Our estimates of student effects, on the other hand, capture persistent components of individual ability that are more general than those predicted by observed measures of socioeconomic status and less noisy than lagged test scores. They therefore provide a more accurate picture of the extent of sorting by ability and provide a stronger foundation for evaluating the effects of ability sorting.

We find strong evidence that students are positively selected into private schools by ability, and that selection is substantially stronger in reading than in numeracy. Of the 0.34 standard deviation difference between the average reading scores of private and public school students in our data, more than half is explained by differences in their time-invariant observed and unobserved characteristics. However, selection explains only 18% of a comparably-sized gap in average numeracy scores. In both cases, the remainder of the test score gap is attributable to differences in average peer ability and differences in other school inputs; we characterize the latter as differences in "school effectiveness" via fixed school effects. Peer effects are an important determinant of the test score gap in both skill areas, explaining 35% of the gap in average reading scores and 25% of the numeracy gap. Differences in school effectiveness explain relatively little of the test score gap in reading (12%). However, due to the lesser role of selection on ability, they are the primary determinant of the gap in numeracy scores, explaining 58% of the difference between average private and public school scores. Despite the relative importance of private school effectiveness, we show that B.C.'s private schools are a diverse and heterogeneous group, with students at academically-focused "prep schools" having much better test score outcomes than students at faith-based schools. This is explained by both stronger selection into prep schools, and more effective school-specific unmeasured inputs to test scores at those schools.

The credibility of our analysis rests on the validity of our estimates of individual student and school effects and of the spillover parameter. The key to our strategy for identifying these separate effects is student mobility between schools. Our estimator is robust to mobility due to the time-invariant unobserved characteristics of students, schools, or peers, due to changes in peer composition, or due to time-varying observables. Our primary identifying assumption is that mobility is exogenous with respect to any transitory student- or school-specific shocks, and with respect to unobserved student-by-school match-specific factors that affect observed test scores. We carefully consider scenarios that could threaten the validity of our estimates, and present evidence that the data are consistent with our estimator's identifying assumptions via a collection of specification tests.

Our evidence of substantial selection into private schools, coupled with meaningful peer effects in test scores, raises serious concerns about whether "cream-skimming" harms B.C.'s public school students. In the spirit of Altonji *et al.* (2015), we use our estimates to quantify the effect of cream-skimming by simulating the effects of counterfactual voucher policies in the presence of spillovers. An important difference between our approach and theirs, however, stems from the fact that they study school choice behavior in the absence of large-scale voucher funding, while we observe choice under a universal voucher program. As a consequence, their simulations consider only the effects of counterfactual voucher policies that would *increase* private school enrollment. In contrast, it is meaningful in our environment to consider counterfactual policies that would *increase or reduce* private school enrolment, and this is the approach we take. In the former instance, we follow previous work and simulate the effect of policies that would reallocate large numbers of public school students to private schools, and measure how these outflows would affect students that remain in the public system. In the latter instance, we go farther and make a novel contribution. We begin by estimating an analogous cream-skimming effect, but where we measure the effect of student outflows from *private* schools on students that remain enrolled in private schools. Then, under the plausible assumption that private school students would attend their attendance zone public school under the counterfactual, we extend previous work and estimate the effect of the resulting student inflows on public school students as well as the effects on movers themselves. In this way, we are able to estimate the full distributional consequences of changes to voucher policies that would reduce private school enrolment.[5]

[5]To the extent that competition from private schools causes public schools to improve, this effect will be embedded in our estimated school fixed effects and hence not included in our measures of the overall impact of private schools. Elsewhere in Canada, Card *et al.* (2010) find that competition with Catholic schools increases achievement in Ontario public schools. Evidence of the effect of private school competition on public school performance in the U.S. tends to find small but positive effects on public school test scores (see Figlio *et al.* (2020) for a recent example and references) while evidence for Chile is more mixed (see Urquiola (2016a) for a review; Feigenberg *et al.* (2019), Murnane *et al.* (2017), Navarro-Palau (2017), and Neilson (2017) provide more recent evidence), as is evidence of the effects from charter school entry (see Ridley & Terrier (2018), Cordes (2018), and Gilraine *et al.* (2019) for recent examples).

Despite the fact that we find substantial spillovers in test scores, our simulations indicate that the effects of cream-skimming by B.C. private schools are surprisingly small. Voucher policies that would increase private school enrolment have very small effects on the average public school student in all but the most pessimistic of our counterfactual scenarios. Likewise, reducing private school enrolment produces only small changes in the mean test scores of students that remain enrolled in private schools, and has little impact on students of the public schools that receive them. These effects are dominated by more substantial reductions in the mean test scores of students who leave private schools under the counterfactual, because they enroll in public schools with relatively small school effects and weaker peers on average.

Several factors may explain the relatively small cream-skimming effects that we find in our data. One is that B.C.'s private school sector is relatively small, enrolling only about 13% of students (see Table 2.2). This limits the potential scope for detrimental effects on public school students. A second is that despite strong evidence that B.C.'s private school students have higher mean ability than public school students, both groups are in fact very diverse. Indeed, dispersion in estimated student ability is slightly greater in the population of private school students than in the public system (see Suplemental Figure 2.6). In addition, B.C.'s private schools face competition from a highly successful public school system that offers substantial choice through both inter-district open enrolment and popular and academically challenging magnet programs. [6] This too, limits the scope for negative cream-skimming effects.

Our result that attending a B.C. private school has a substantial positive effect on average test scores stands in contrast to most previous studies of voucher programs in other countries. For example, previous studies have found that attending a private Catholic school has little effect on test scores relative to attending a public school in the U.S. (Altonji *et al.* , 2005; Carbonaro, 2006; Elder & Jepsen, 2014; Jepsen, 2003; Lubienski *et al.* , 2008; Reardon *et al.* , 2009). Similar studies of the relative effectiveness of publicly funded private schools in Australia (Nghiem *et al.* , 2015) and Sweden (Hinnerich & Vlachos, 2017) yield similar results. Studies of small-scale and targeted U.S. voucher programs have found that attending a voucher school has little causal effect (see Epple *et al.* (2017) for a review) or in some cases even a negative causal effect (Abdulkadirolu & Sonmez, 2003; Figlio & Karbownik, 2016; Mills & Wolf, 2016; Waddington & Berends, 2018) on the test scores of students who take up the voucher.[7] In contrast, two previous Canadian studies have found that private

[6] 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, 2016).

[7] Results for high school graduation, college attendance and earnings are more positive (Altonji *et al.* , 2005; Evans & Schwab, 1995; Neal, 1997), and evidence of the relative performance of private versus public schools in developing and middle income countries is generally favorable (e.g. Andrabi *et al.* (2015);

schools outperform public schools under universal voucher programs: Lefebvre *et al.* (2011) find that private high schools in Quebec outperform their public counterparts; and Azimi *et al.* (2018) find a private school advantage of 0.15 standard deviations in reading and numeracy in B.C. using a value-added model of test scores.

We speculate that several specific institutional features may help to shape the capacity and motivation of B.C. private schools to deliver academic quality, and this likely explains their positive effect on average test scores. Importantly, unlike their Swedish and American voucher counterparts, B.C.'s private schools are permitted to apply selective admissions criteria. If private schools' selection criteria shape the composition of the student body in ways that are not captured by our measure of mean peer ability (e.g., via the exclusion of disruptive peers), then the associated spillovers will be captured by our estimated school effects and this might contribute to our relatively positive results.[8] Unfortunately, our data do not allow us to investigate this hypothesis. Again, unlike their Swedish and American voucher counterparts, B.C.'s private schools are permitted to charge tuition top-ups. If this allows them to attract better teachers via higher pay, increase instructional hours, or add other instructional supports, then this too could explain their greater effectiveness. However, B.C. private schools' capacity to do so is limited by a feature of the funding formula that reduces the size of the public grant when per student operating costs exceed those in the public system. We show that a relatively small number of private schools receive the reduced grant associated with higher operating costs, and these schools do not drive our overall results.

Important institutional features of B.C.'s public school system also likely shape our results. As noted above, B.C.'s public schools are highly successful overall, and students are afforded considerable school choice within the public system. Additionally, because B.C. public schools are funded out of provincial general revenues, they are more homogeneous in the quality of educational inputs that they provide than in jurisdictions where public schools are funded out of local property taxes. As a consequence, there are fewer opportunities for low-quality private schools to attract students by locating in areas where they compete with below-average public schools.[9] Such a strategy has been posited as a mechanism to explain why urban minority students in the U.S. tend to benefit from attending a private school, while other students do not (Evans & Schwab, 1995; Neal, 1997; Grogger *et al.* , 2000;

Alderman *et al.* (2001); Angrist *et al.* (2002); Feigenberg *et al.* (2019); Muralidharan & Sundararaman (2015); Neilson (2017); Rau *et al.* (2019); Sánchez (2017); Singh (2015).

[8] The literature on the effects of disruptive peers is mixed (see Ruijs (2017) for a relatively recent review). Friesen *et al.* (2010) find no effect of students diagnosed with behavioral disorders on their peers' test scores in B.C. public schools.

[9] Martinez-Mora (2006) formalizes the idea that private schools that are low-quality relative to the average public school can survive in equilibrium by locating in areas where they compete with below-average public schools.

Figlio & Stone, 2001; Altonji *et al.* , 2005). In contrast, we find that students who attend faith schools on average reside in the attendance zones of average public schools, while prep school students reside in the attendance zones of schools that are well above average with respect to both school and peer quality.

Finally, our evidence of peers' contributions to academic outcomes at selective private schools can also shed light on the potential for selective public schools to promote student achievement. Previous evidence of the overall effectiveness of selective public schools is mixed (Abdulkadiroğlu *et al.* , 2014; Clark, 2010; Jackson, 2013; Pop-Eleches & Urquiola, 2013),[10] and none of these studies provides direct evidence of the role of peers in the effectiveness of selective schools. Abdulkadiroğlu *et al.* (2014) argue that, since peer quality is likely to be positively correlated with other school inputs, estimates of overall school effectiveness provide an upper bound on the spillover parameter. Jackson (2013) supplements his main estimates of school effectiveness in Trinidad and Tobago with direct estimates of peer effects; combining these results, he concludes that peers account for about 10% of the overall selective school effect. Our work contributes to this literature by jointly estimating peer effects and school effects for a group of (private) schools that have the authority to selectively admit students.

2.2 Institutional Context

2.2.1 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 residential geography. Throughout, we call this a student's attendance zone public school. Before July 2002, enrolment in a public school outside the student's 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 a public school outside their attendance zone as long as space and facilities are available after students who reside in the attendance zone have enrolled. School transportation is not provided. When attendance zone schools are over-subscribed, school boards must give priority to within-district students and may elect to give priority to siblings of current students. Within these categories, principals of attendance zone schools have discretion over which students to enroll.

[10] Estimates of selective school effects on longer-run impacts are similarly mixed. Clark (2010) finds very large positive effects on high school course-taking and university enrolment. Jackson (2010) finds that attending a more selective school in Trinidad and Tobago has positive effects on the number of exams passed and high-school graduation. Clark & Del Bono (2016) find that elite school attendance in the UK had large effects on completed education, but only small and statistically insignificant positive (for women) or negative (for men) effects on income, employment and wages. Dobbie & Fryer Jr (2013) find no impact of admission offers at New York exam schools on college enrollment or quality.

B.C. parents may also enroll their children in a public magnet program. The most popular of these is French Immersion, which enrolls about 10 percent of Kindergarten students in the province (BC Ministry of Education, 2011). Entry into "early" French Immersion programs 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.

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

2.2.2 Private school choice and funding

Since 1977, British Columbia has provided universal vouchers to private schools that conform to provincial curriculum standards and meet various provincial administrative requirements, and where fewer than 50 percent of students enrolled are "international" (i.e. whose parents are neither citizens of permanent residents of Canada and are not normally resident in B.C.) (Federation of Private Schools Associations, British Columbia, 2015). The total amount of funding is not limited, and publicly funded private schools are not constrained in their selection of students. However, to be eligible for funding, private schools must operate as not-for-profits, offer the provincial curriculum, hire qualified B.C. teachers and participate in standardized testing programs. Unlike public schools, publicly funded 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. In particular, their teachers are not covered by the collective agreement that applies to public school teachers.

B.C. private schools are classified into one of four funding groups. Group 1 private schools, whose operating costs are no higher than the average public school in the same school district, receive 50 percent of the per student public school grant.[11] Group 2 schools, whose operating costs exceed the district public school average, receive only 35 percent of the public school grant (BC Ministry of Education, 2005). Group 3 schools choose not

[11]In 2005, the supplemental grant for special education students in private schools was increased from 50% to 100% of the corresponding supplemental grant paid to public schools.

to meet provincial education requirements and are not provincially certified or funded. Group 4 schools meet provincial education requirements and are provincially certified but are ineligible for public funding because more than half of their enrolment is comprised of international students.

Private schools in B.C.'s Lower Mainland are diverse. They serve a variety of faith communities, including Catholics, Protestants, Sikhs, Jews and Muslims. Many are secular, including academically-focused "prep schools," schools that offer Montessori or Waldorf programs, and schools with specialized programs for students with special learning needs. Tuition fees range widely, from less than a thousand dollars at some faith schools to $20,000 or more at top-ranked prep schools. Private schools are also supported through donations from individuals and supporting foundations and organizations. In the case of Catholic schools, for example, initial building costs are subsidized by both the Diocese and the local parish, with the parish contributing funds towards other capital and operating costs (Catholic Private Schools Vancouver Diocese, 2017).

2.2.3 Testing and accountability

All public and provincially certified private schools in B.C. must administer annual standardized tests in reading, writing and numeracy to students in grades 4 and 7, called the Foundation Skills Assessment (FSA). Centralized grading ensures that a consistent standard is applied across schools.[12] FSA scores do not contribute to students' academic records, 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 FSA scores on their website in 2001 (BC 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 & Easton, 2003) that include school scores and rankings based on FSA scores. From the outset, the school report cards received widespread media coverage in the province's print, radio and television media.

2.3 Data

Our analysis is based on administrative data provided to us by the B.C. Ministry of Education. Our extract includes all grade 4 students who were enrolled in a public or private school located within the geographic boundaries of the fourteen school districts in the Lower Mainland of B.C. between the 1999/2000 school year and 2003/2004, and follows them for

[12] Hinnerich & 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.

four years. Students remain in our data so long as they remain enrolled in the public or private school system in this geographic region.

Our analysis relies primarily on data extracted from an enrolment database that records the school at which each student is enrolled on September 30 of each year. Enrolment records also include indicators for student characteristics (home language, gender, self-identified Aboriginal, postal code), program type (ESL, gifted, disabled, French Immersion) and school name and type (public or private) and funding category. We categorize private schools into secular and faith categories by matching schools to information from various websites and reports by name, and by making telephone calls to schools when necessary. We match student postal codes to Census neighborhoods (enumeration areas) to augment the enrolment records with neighborhood characteristics including average family income, the proportion of immigrant families, and the distribution of educational attainment.[13] A detailed description of our procedures for locating residential postal codes within enumeration areas is provided in the supplement.

We merge the enrolment records with records from a test score database via a unique student identifier. The test score database provides student-level data on participation and scores on the FSA exams administered in grades 4 and 7 for the 1999/2000 through 2006/2007 school years. Valid test scores in reading and numeracy are normalized to have a province-wide mean of zero and standard deviation of one in each year.[14] This enrolment-based restriction removes a small number of private Montessori and Waldorf schools and private schools that offer services exclusively to students with special needs. The effects of these exclusions on our sample size is reported in Supplemental Table 2.9.

The upper panel of Table 2.2 presents selected school characteristics by school type. The students in our sample attend 676 different schools, of which 559 are public and 117 are private. Of the private schools, 38 are Catholic, 41 are associated with another Christian denomination, 10 are associated with other faiths, 15 are secular prep schools with an academic focus, and 13 are secular schools that serve particular groups (e.g. gifted, special needs) or offer a specialized program (e.g. Waldorf or Montessori). Most private schools are in funding group 1, qualifying for a per student subsidy equal to half of the public school subsidy; only 18 private schools are in funding group 2, qualifying for a per student subsidy equal to 35% of the public school subsidy. Thirteen private schools do not receive public funding, including nine schools that do not comply with provincial educational standards

[13] An enumeration area is the smallest geographic area for which public-use Census data are produced, and typically comprises several hundred households.

[14] A small number of francophone students attend schools operated by the public francophone school board; we exclude these students from our sample. Over 7 percent of students in our sample attend a public French Immersion program that offers instruction in French to non-francophone students. We include these students in our sample. When English and French Immersion tracks are offered in the same school, we do not distinguish between these programs when estimating school effects.

(funding group 3) and four schools that primarily serve international students (funding group 4). Most primary schools in B.C. offer Kindergarten through grade 7. In our sample, seven private schools and 102 public schools offer grade 4 but not grade 7, and nine private schools and 37 public schools offer grade 7 but not grade 4.

The lower panel of Table 2.2 presents descriptive statistics for the students in our data. Over 11 percent of students attend a private school. This share is roughly comparable to private school enrolment rates in Denmark, New Zealand and Sweden, which also offer universal vouchers that cover some but not all of school operating costs (see Table 2.1). Private school students' average test scores are 0.39 standard deviations above the provincial mean in reading, and 0.42 standard deviations above the mean in numeracy. In comparison, public school students in our sample score 0.03 (reading) and 0.10 (numeracy) standard deviations above the provincial mean. Private school students are less than half as likely as public school students to report a missing test score. Mean achievement varies substantially across private school types. Prep school students excel: their average grade 7 test scores are more than 0.80 standard deviations above the mean in both reading and numeracy. Catholic school students average 0.34 in reading and 0.39 in numeracy, and students at private schools associated with other Christian denominations average 0.27 in reading and 0.23 in numeracy. Students at private schools associated with other faiths average 0.23 in reading and 0.49 in numeracy. In the group of "other" secular schools, i.e. those that do not follow the prep school model of focusing on high academic achievement, the average school size is very small, students score below average in numeracy, and a much higher share of students do not have valid test scores.

The remaining rows of Table 2.2 demonstrate other differences in student characteristics by school type. Prep school students typically come from high SES neighborhoods, and they are least likely to self-identify as Aboriginal or speak Punjabi at home (these are characteristics associated with lower test scores in B.C., see Friesen & Krauth (2011)). Students attending Catholic and other Christian schools are also less likely to have these characteristics but reside in neighborhoods similar to the overall average. A disproportionate number of students attending other faith schools speak Punjabi at home, revealing the dominance of Sikh schools in this category, but they are otherwise drawn from neighborhoods with fairly typical SES profiles. Other secular students are disproportionately male and English-speaking and are drawn from relatively high SES neighborhoods.

The final two columns of Table 2.2 show that almost 40% of students change schools between grades 4 and 7. The mean socioeconomic and demographic characteristics of these students, who play a key role in our identification strategy, are very similar to the full population. While their average test scores are slightly lower than students who don't switch schools and they are slightly more likely to have missing test scores, these differences are small.

Figure 2.1 plots the estimated distribution of school-average test scores for public and private schools. School-average numeracy scores are substantially more dispersed than reading scores, especially in private schools. While private schools have higher average test scores than public schools in both reading and numeracy, the distribution of their average test scores is also more dispersed and has a notably longer right tail.

2.4 Methodology

2.4.1 Specification

Our analysis is based on a model of student test scores that controls for the time-invariant observed and unobserved characteristics of students, schools, and peers. Our baseline model for the test score of student i in grade g is:

$$y_{ig} = X'_{ig}\beta + \alpha_i + \psi_s + \eta\bar{\alpha}_{\sim i,gst} + \epsilon_{ig} \text{ for } g = 4, 7 \tag{2.1}$$

where X_{ig} is a vector of time-varying characteristics of individual i in grade g;[15] α_i is a student fixed effect, ψ_s is a school fixed effect for the school $s = S(i, g)$ at which student i is enrolled in grade g; $\bar{\alpha}_{\sim i,gst}$ is the average student effect of i's peers, where we define "peers" to be all other students in grade g at school s in the year $t = T(i, g)$ that student i is enrolled in grade g; and ε_{ig} is a mean-zero error term. The student effect α_i captures the effect of time-invariant student characteristics, both observed and unobserved, on test scores. While we recognize that α_i will capture the effect of a broad array of individual characteristics, including innate intellectual ability, motivation, and family inputs, we use the shorthand "ability" throughout to refer to this effect. The spillover parameter η, meanwhile, estimates the effect of peers' average ability on test scores. Similarly, the school effect ψ_s captures the effect of observed and unobserved school characteristics on students' test scores. Again, this will capture the effect of an array of school-specific characteristics, including administrative and teacher quality, facilities, etc., which we refer to collectively as "school effectiveness" throughout. Moreover, we note that if different schools face different levels of competition from private schools and this causes between-school differences in the quality of education that they provide, this will be reflected in our school effects.

Our primary interest is to characterize how attending a private vs. public school affects students' FSA scores. Whether a school is public or private is a time-invariant characteristic of a school in our data. The average effect of attending a private school, holding student and average peer ability constant, is therefore embodied in our school effects, ψ_s, and can

[15]Time-varying covariates X_{ig} in our baseline specification include the neighborhood share of immigrant households; neighborhood shares of household heads with a high school diploma, a post-secondary certificate, or a bachelor's degree; mean neighborhood family income; and a fixed effect for the year in which the student is enrolled in grade 7.

be recovered from the difference between the average school effect in each sector, e.g., $\bar{\psi}^{priv} - \bar{\psi}^{pub}$. Indeed this measure is equivalent to including a private school dummy variable in eq. (1) and normalizing the school effects to have mean zero in each sector. Moreover, eq. (1) allows us to characterize how differences between the mean ability of public and private school students affects average test scores in the two sectors, both via students' own ability, e.g., $\bar{\alpha}^{priv} - \bar{\alpha}^{pub}$, and the influence of peers,$\eta\left(\bar{\bar{\alpha}}^{priv} - \bar{\bar{\alpha}}^{pub}\right)$. We return to these ideas with a formal decomposition in Section 5.4.

We estimate eq. 2.1 using an algorithm adapted from Arcidiacono *et al.* (2012) and Battisti (2017), which iteratively toggles between estimating the spillover parameter and the student and school effects; see the supplement for details. We restrict our estimation sample to students with non-missing test scores, and to the largest connected set of schools that are linked by student mobility (see Abowd *et al.* (2002)).[16] This includes all but a handful of students and two schools for each of the FSA tests. We normalize our estimated student and school effects to have zero mean in the largest connected set and estimate standard errors via the wild bootstrap.

Our baseline specification has several notable features. First, it assumes that peers influence test scores through their time-invariant ability α_i, rather than through their contemporaneous test scores. This avoids the simultaneity problem that would otherwise arise. Second, following Arcidiacono *et al.* (2012) and Burke & Sass (2013), it assumes that the peer effect is linear in mean peer ability and proportional to the effect of own ability on test scores. Altonji *et al.* (2015) formally justify this restriction in the context of a model with endogenous peer effects in the sense of Manski (1993). Our estimated peer effects should be interpreted as reduced form coefficients that capture both direct spillovers associated with peer ability and any spillovers associated with behavior that is predicted by peer ability.

Third, our specification will estimate the average effect of changes in the composition of students' same-grade peers within schools. We do not observe classroom assignments in our data, so it is not possible to estimate the effects of same-classroom peers. We expect same-grade peers to have smaller effects on academic outcomes than same-classroom peers for several reasons. On the one hand, if peers have a larger effect on achievement when they are in the same classroom, then our specification effectively measures peer groups with error and our estimated peer effects will be attenuated. On the other hand, streaming students into classrooms on the basis of ability will produce a spurious positive correlation between own and peer ability within classrooms, so specifying peer groups at the grade level may avoid some upward bias in estimated peer effects.

Fourth, our specification does not include a lagged test score, as is common in models where learning is cumulative and lagged inputs aren't observed (Todd & Wolpin, 2003). The

[16]This yields slightly different estimation samples for reading and numeracy test scores, because some students have missing scores for only one of the tests.

peer effect in eq. 2.1 can be interpreted as the effect of the mean time-invariant component of peer achievement on a student's own achievement. Including a lagged test score would change this interpretation: the peer effect would measure the effect of gains in the mean time-invariant component of peer achievement on a student's own gains. Peer effects in levels and gains will be similar if achievement gains and levels are themselves highly correlated. Burke & Sass (2013) show this not to be the case in their Florida data and estimate a levels specification analogous to eq. 2.1. With only two test scores for each student, we cannot include both lagged a test score and student fixed effects in our model. We opt for the latter since it allows us to characterize students, peers, and sorting in terms of ability, and allows us to estimate school effects that are net of unobserved student characteristics. In the context of a cumulative model of learning, however, this imposes the restriction that the effect of ability on achievement does not vary with age. In our case, this means that ability has the same effect on achievement at (approximately) ages 9 and 12.

Finally, eq. 2.1 assumes that peer effects are homogeneous, and do not vary with a student's own achievement. Some previous researchers have estimated richer models that allow for heterogeneous peer effects that vary with own achievement and/or quantiles of the distribution of peer achievement (e.g. Arcidiacono *et al.* (2012); Burke & Sass (2013); Ding & Lehrer (2007); Hoxby & Weingarth (2005); Lavy *et al.* (2012)). The complex patterns of results that emerge are not easily summarized, but all find statistically significant peer effects that range from very small to substantial. To determine whether peer effects are heterogeneous in our data, we also estimate quantile specifications analogous to eq. 2.1 at deciles of the achievement distribution. This is somewhat complicated by the fact that our specification includes large numbers of fixed student and school effects.[17] We therefore implement an extension of Canay (2011) two-step quantile regression estimator, which assumes that student and school effects shift the location of the achievement distribution but not its scale or other moments. That is, we define $y^*_{ig} = y_{ig} - \hat{\alpha}_i - \hat{\psi}_s$, where $\hat{\alpha}_i$ and $\hat{\psi}_s$ are estimated via eq. 2.1, and then estimate the τ^{th} conditional quantile regression:

$$\Pr\left(y^*_{ig} \leq \; X'_{ig}\beta^\tau + \eta^\tau \bar{\hat{\alpha}}_{\sim i,gst}\right) = \tau \;\text{ for } g = 4,7 \tag{2.2}$$

where $\bar{\hat{\alpha}}_{\; i,gst}$ is the estimated average ability of student i's peers. The spillover parameter η^τ in eq. 2.2 measures the effect of same-grade peers' average ability on the τ^{th} quantile of the distribution of test scores.

[17] See Koenker (2004) and Canay (2011) for a discussion of issues that arise in the context of quantile regression with fixed effects.

2.4.2 Identification

Our key identifying assumption is that transitory unobserved factors in eq. 2.1 that affect student i's test score in grade g have zero mean conditional on observable time-varying student characteristics, time-invariant student and school heterogeneity, and peer composition:

$$E[\varepsilon_{ig} \,|X_{ig},\ \alpha_i, \psi_s,\ \bar{\alpha}_{\sim i,gst}] = 0 \ \text{ for } g = 4, 7 \tag{2.3}$$

This is simply the standard OLS exogeneity assumption. However, it is important to discuss the implications of eq. 2.3 and potential circumstances that could violate it and bias our estimates.

One such possibility is non-random selection into test-taking. As noted in Section 2.2.3, a private think tank publishes annual rankings of B.C. schools that are partly based on average FSA test scores. Friesen *et al.* (2012) show that parents' enrolment decisions are highly responsive to these rankings. A potential concern, therefore, is that some schools might seek to improve their ranking by selectively excluding students from the FSA test if they are expected to perform poorly. To understand the potential consequences of such a policy, suppose that each student's test score is only observed if it exceeds a threshold, y_{min}. The expected value of observed test scores is:

$$E\left[y_{ig}|y_{ig} > y_{min}, X_{ig}, \alpha_i, \psi_s, \bar{\alpha}_{\sim i,gst}\right] = \omega\left(i,g,s,t\right) + E\left[\varepsilon_{ig}|\varepsilon_{ig} > y_{min} - \omega(i,g,s,t)\right] \quad \text{for } g = 4, 7 \tag{2.4}$$

where $\omega\left(i,g,s,t\right) = X'_{ig}\beta + \alpha_i + \psi_s + \eta\bar{\alpha}_{\sim i,gst}$ is the conditional mean of test scores in the absence of selection into test-taking. The expected error in observed test scores is a decreasing function of the school effect, ψ_s. This will tend to inflate observed test scores at schools with smaller values of ψ_s relative to schools where greater school effectiveness ensures that students are more likely to meet the threshold y_{min}. As a consequence, non-random selection into test-taking could attenuate variation in our estimated school effects. In specification checks reported in Section 2.5, we find no systematic relationship between school-average test scores and the school-average conditional probability that a student takes the FSA exam, which alleviates concerns about systematic selection into test-taking.

Endogenous student mobility between schools is another potential source of concern, since we rely on mobility to separately identify student and school effects. Our identifying assumption eq. 2.3 allows inter-school mobility to depend on many things: time-invariant observed and unobserved student, school, and peer heterogeneity (α_i, ψ_s, and $\bar{\alpha}_{\sim i,gst}$); and observable student characteristics (X_{ig}). If more students move from low quality schools to high quality schools than vice versa, for example, this would not violate eq. 2.3 because our estimator conditions on the actual sequence of schools at which each student is observed. Our estimator is valid even if school mobility rates differ among high and low ability students, or if high ability students are more likely to move to high quality schools or to schools with high quality peers. School mobility may also depend on fixed or time-varying non-academic characteristics of schools.

To better understand what kinds of mobility might violate eq. 2.3, we decompose the error term in eq. 2.1 as:

$$\varepsilon_{ig} \ = \phi_{is} + \pi_{ig} + \sigma_{st} + \mu_{ig} \tag{2.5}$$

where ϕ_{is} is a match-specific effect between student i and the school $s = S(i,g)$ that he/she attends in grade g; π_{ig} is a student-by-grade effect; σ_{st} is a school-by-year effect; and μ_{ig} captures transitory shocks to student achievement. Match-specific effects ϕ_{is} arise if students differ in unobserved ways that affect their achievement in specific school environments. Student-by-grade effects π_{ig} arise if students learn at heterogeneous rates, which we represent via a student-specific effect ς_i in test score *gains*:

$$\pi_{i7} = \varsigma_i + \pi_{i4}. \tag{2.6}$$

Similarly, σ_{st} includes a school-specific trend ρ_s in school quality:

$$\sigma_{st} = \rho_s + \sigma_{st-1} + \chi_{st} \tag{2.7}$$

where χ_{st} is an orthogonal mean zero error term. We likewise assume that ϕ_{is} and μ_{ig} have mean zero for every student, grade, and school in our sample.

Correlation between inter-school mobility and any one of these error components would violate our identifying assumption eq. 2.3. For example, if students systematically sort on the idiosyncratic match component of test scores, ϕ_{is}, then positive match effects would shrink the change in test scores of students who move to lower quality schools and inflate the change in test scores of students who move to higher quality schools. This would have the potential to bias our estimated school fixed effects. However, in specification checks we find that test score gains and losses are roughly symmetric for students moving between schools in different quartiles of the distribution of average test scores, which suggests that there is no systematic test score gain for movers, and alleviates concerns about sorting on match effects in test scores.

Similarly, our estimates may be biased if student effects in test scores gains, ς_i, are conditionally correlated with the change in school quality among school movers. For example, if this correlation is positive, so that movers whose test scores are growing more rapidly due to their own ability also tend to move to more effective schools, then our estimates will overestimate the importance of school and/or peer effects. If the positive correlation arises from mobility related to time-invariant *between-school* differences in school quality (school fixed effects or mean peer ability), then it will bias our estimated school effects and cause us to overestimate the value of schools. On the other hand, if it arises from mobility patterns related to time-varying *within-school* variation in peer quality across cohorts, then it will bias our estimate of the spillover parameter upwards and cause us to overestimate the value of peers. Either way, specification checks reported in Section 2.5.2 give us little cause for concern about systematic mobility related to heterogeneity in test score gains.

Likewise, if schools that are improving in quality also attract high quality movers, the change in average peer quality will be correlated with the improvement in school quality. Our estimator will attribute the effect of increasing school quality to the improvement in peer quality, causing us to overestimate peer effects. And finally, if students change schools due to temporary shocks that affect grade 4 test scores, μ_{i4}, then our estimator will attribute the effect of such shocks to the grade 4 school. For example, if parents' divorce, illness, or job loss depresses grade 4 test scores and increases the probability that the student moves to a lower quality school for grade 7, this kind of "Ashenfelter's dip" would lead us to underestimate the difference in quality of the two schools, attenuating the estimated importance of school effects.[18] Again, we investigate these potential sources of bias in section 2.5.2 below, and find little evidence of systematic endogenous mobility.

2.5 Results

2.5.1 Baseline estimates

We report estimates of some key parameters of our baseline model, eq. 2.1, in Table 2.3; see Supplemental Table 2.10 for additional coefficient estimates for this specification. Our specification explains the vast majority of observed variation in test scores: about 84 percent of both variation in both reading and numeracy scores. The lion's share of this – 81 percent of the total variation in reading scores, and 76 percent of variation in numeracy – is accounted for by variation in student ability, $\hat{\alpha}_i$.[19] Variation in school effectiveness, $\hat{\psi}_s$, accounts for a further 4.7 percent of the variation in reading scores and 6.4 percent in numeracy. Variation in average peer ability ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$) explains 2.7 percent of the overall variation in reading scores and 4.1 percent of numeracy scores, while observed covariates account for less than 2 percent of test score variation.

[18]Mobility of this type seems less likely to bias estimated peer effects, since this would require students who experience transitory grade 4 shocks to systematically move to schools that experience transitory negative shocks to peer quality in their grade 7 year, as opposed to schools that had peers with lower average ability in all grades and years.

[19]We characterize the relative importance of student, school and peer effects in explaining the overall variation of student test scores via the simple decomposition:

$$var\left(y_i\right) = var\left(X_i\hat{\beta}\right) + var\left(\hat{\alpha}_i\right) + var\left(\hat{\eta}\bar{\hat{\alpha}}_s\right) + var\left(\hat{\psi}_s\right) + var\left(\hat{\varepsilon}_i\right) + covariance\,terms \tag{2.8}$$

The relatively small portion accounted for by peers reflects the fact that between-school variation in mean student ability ($\bar{\hat{\alpha}}_{\sim i,gst}$) is small relative to the overall variance of achievement, and belies the importance of peers as a determinant of test scores. Our estimates imply that a one standard deviation increase in mean peer quality is associated with a 0.165 standard deviation increase in reading test scores, and a 0.201 standard deviation increase in numeracy test scores.[20] These estimates fall towards the larger end of the range of estimates of ability peer effects from similar specifications for elementary and middle schools students. They are smaller than the homogeneous linear-in-means estimates of ability peer effects for middle school students reported by Hoxby & Weingarth (2005), but more than twice as large as those reported by Hanushek *et al.* (2003) and at least 2-3 times as large as those reported by Burke & Sass (2013) for elementary school students.[21]

2.5.2 Heterogeneous peer effects

Before moving on to additional specification checks and our primary analysis, we assess the validity of one of our baseline specification's key assumptions: that peers have a homogeneous effect on test scores throughout the achievement distribution. To do so, we estimate quantile specifications analogous to eq. 2.1. The estimated quantile spillover parameters, $\hat{\eta}^{\tau}$, are reported in Suplemental Table 2.11 and do not vary much across deciles of achievement. In the case of reading, the spillover parameter increases monotonically from 0.627 at the 10th percentile to 0.659 at the 90th percentile, but the differences are not statistically significant below the 60th percentile.[22] As illustrated in Figure 2.2, this implies that a one standard deviation improvement in average peer ability increases reading scores by 0.161 standard deviations at the 10th percentile, versus 0.169 standard deviations at the 90th percentile.[23] For numeracy, the spillover parameter increases monotonically from 1.09 at

[20]As reported in Table 2.3, the estimated coefficient on mean peer ability in our baseline specification, $\hat{\eta}$, is 0.645 for reading and 1.145 for numeracy. The standard deviation of mean peer ability in reading test scores is 0.233, and the standard deviation of reading test scores is 0.908 in our data. Thus a one standard deviation increase in mean peer ability increases reading scores by $0.645 \times \left(\frac{0.233}{0.908}\right) = 0.165$ standard deviations. Likewise, the standard deviation of peer ability in numeracy scores is 0.165, and the standard deviation of numeracy tests scores in our data is 0.938, so that a one standard deviation increase in mean peer ability increases numeracy test scores by $1.145 \times \left(\frac{0.165}{0.938}\right) = 0.201$ standard deviations.

[21]Evidence of peer effects in ability for secondary school students is similarly mixed. Gibbons & Telhaj (2016) and Lavy *et al.* (2012) find little or no evidence of spillovers in the linear-in-means model in U.K. secondary schools. Ding & Lehrer (2007) find substantial spillovers among Chinese high school students, as does Jackson (2013) for Trinidad and Tobago. To our knowledge, Burke & Sass (2013) is the only paper that estimates ability peer effects separately for elementary, middle, and high school students. They find that spillovers are largest among elementary school students, although much smaller than our estimates.

[22]Quantile spillover parameters are statistically significant at the .001 level at all deciles for both reading and numeracy. Complete estimation results for the quantile specifications are available on request.

[23]These values are obtained by multiplying the estimated spillover parameters by $\sigma_{\eta\bar{\theta}}/\sigma_y$, where $\sigma_{\eta\bar{\theta}}$ is the estimated standard deviation of peer quality and σ_y is the standard deviation of test scores in our data. In the case of reading, $\sigma_{\eta\bar{\theta}} = 0.233$ and $\sigma_y = 0.908$; for numeracy, $\sigma_{\eta\bar{\theta}} = 0.165$ and $\sigma_y = 0.938$.

the 10th percentile to 1.21 at the 90th, which implies that a one standard deviation improvement in average peer ability increases numeracy scores by 0.191 standard deviations at the 10th percentile, versus 0.212 at the 90th.[24]

Since we find very little variation in estimated peer effects across deciles of achievement, we focus our attention on the specification with homogeneous peer effects, eq. 2.1, in the remainder. We have, however, replicated our analysis using estimates from the quantile specifications. This produces results that are very similar to those reported in the text, and are available on request.

2.5.3 Specification checks

Before moving on to our primary analysis, we conduct a series of specification tests to assess the threats to identification described in Section 2.4.2. We begin by investigating the potential for non-random selection into test-taking because this has the potential to bias our estimates of differences between public and private school outcomes. Our primary concern is that some schools might attempt to artificially inflate school-average test scores by systematically excluding some students (e.g., those expected to perform poorly) from taking the test, and that private schools might face stronger incentives to do so. To assess this threat, we estimate the following model of test participation:

$$T_{ig} = X'_{ig}\lambda + \upsilon_i + \theta_s + \gamma_{ig} \text{ for } g = 4, 7 \tag{2.9}$$

where T_{ig} is an indicator that equals one if student i has a valid non-missing test score in grade g, and υ_i and θ_s are fixed student and school effects, respectively. The school effects θ_s capture systematic differences between schools in test participation rates conditional on observables and unobserved heterogeneity in students' propensity to participate. If θ_s varied systematically with school-average test scores, it would raise concerns about selection bias in our test score model. To assess this, we plot the estimated school effects from eq. 2.9 against school-average test scores in Figure 2.3.[25] The relationship between them is essentially flat in both reading and numeracy, which alleviates our concerns about selection into test-taking.

Endogenous mobility or sorting associated with omitted match effects (ϕ_{is}) also has the potential to bias estimates based on eq. 2.1.[26] Suppose, for example, that test scores

[24] Burke & Sass (2013) find that peer effects for top quintile elementary students in Florida are approximately the same as for middle quintile students. Unlike us, however, they find that good peers have smaller (or even negative) effects on low quintile students. Hanushek *et al.* (2003) find little variation in peer effects across quartiles of the distribution of lagged achievement.

[25] See Supplemental Table 2.12 for estimates of coefficients in eq. 2.9.

[26] See Woodcock (2015) for an in-depth discussion of bias due to omitted match effects in the labor market context.

depend on additively-separable match effects as in eq. 2.3 and students sort across schools to maximize their own expected score. Then students enrolled at more effective schools will have worse match effects than students enrolled at less effective schools, on average, because additively-separable school and match effects are perfect substitutes in the production of test scores.[27] This would induce a negative correlation between ψ_s and ϕ_{is}, which could bias our estimated school effects toward zero.

To assess the likelihood of bias due to omitted match effects, we follow an approach pioneered by Card *et al.* (2013) in labor market data. The idea is simple. If student mobility and test scores depend on match effects, then students who move in opposite directions between pairs of schools will experience asymmetric gains and losses in test scores. To see this, suppose that school A is more effective than school B, i.e., $\psi_A - \psi_B = \delta > 0$. If students change schools in response to match effects, then a student who moves from A to B will experience a test score loss of less than δ (ignoring any change in peer quality) because the loss of school effectiveness is offset by an improved match. A student who moved from B to A, on the other hand, will gain more than δ because the improvement in school effectiveness is reinforced by an improved match. We therefore examine the change in test scores among students that change schools between grade 4 and grade 7. For each student in each year, we begin by calculating the average test score of their peers. Then for each individual that changes school, we classify their grade 4 and 7 schools based on the quartile of peer-average test scores. Next, we assign individuals to sixteen cells based on the quartile of peer-average test scores at the grade 4 and 7 schools, and calculate the average grade 4 and 7 test scores in each cell. Figure 2.4 shows the change in average test scores between grades 4 and grade 7 for students who leave schools in the top or bottom quartile of peer-average test scores. Supplemental Figure 2.9 presents the same information for students who leave schools in the two middle quartiles of peer-average test scores. The two figures show that the average gain of students who move from a school in a lower quartile to a higher quartile is about the same as the average loss of those who move in the opposite direction, consistent with our model of additive student, school, and peer effects. Students who move between schools in the same quartile do experience small average gains in test scores at upper quantiles of the peer-average test score distribution, but not in lower quartiles. These small within-quartile gains could reflect within-quartile sorting into more effective schools, or sorting on match effects in test scores. Either way, the average gains are reassuringly small and this limits the scope for bias.

[27]Proof: Consider the simplest case where $y_{is} = \psi_s + \phi_{is}$, match effects are *iid* random variables, and students choose the school s that maximizes their test score y_{is}. Consider any two schools A and B such that $\psi_A - \psi_B = \delta > 0$. Then students will choose school A if and only if $\phi_{iA} > \phi_{iB} - \delta$, and will choose B if and only if $\phi_{iB} > \phi_{iA} + \delta$. The average match quality of students choosing school A is $M_A = E[\phi_{iA}|\phi_{iA} > \phi_{iB} - \delta]$ and the average match quality of students choosing B is $M_B = E[\phi_{iB}|\phi_{iB} > \phi_{iA} + \delta]$. It follows immediately that $M_A = E[\phi_{iA}|\phi_{iA} > \phi_{iB} - \delta] < E[\phi_{iA}|\phi_{iA} > \phi_{iB} + \delta] = E[\phi_{iB}|\phi_{iB} > \phi_{iA} + \delta] = M_B$ because $\delta > 0$ and ϕ_{iA} and ϕ_{iB} are *iid*.

Endogenous mobility associated with student-specific gains in test scores (ς_i) has a similar potential to bias estimates based on eq. 2.1. If students whose test scores are growing rapidly tend to move to more effective schools, for example, then ς_i will be positively correlated with school effects and this would bias our estimated school effects upward in absolute value. As in the case of omitted match effects, omitted student-specific gains in test scores would cause asymmetric gains and losses for students who move in opposite directions between pairs of schools. To see this, assume as above that $\psi_A - \psi_B = \delta > 0$. Then a student who moves from A to B would lose more than δ because the loss of school effectiveness is reinforced by lower student-specific gains, while a student who moves from B to A would gain more than δ because the improvement in school effectiveness is reinforced by larger student-specific gains. As we've already seen in Figure 2.4, there is little evidence of such asymmetric gains and losses in our data.

Finally, our model assumes that school effects are homogeneous for all students in a school. If school effects varied systematically with student effects instead, then we might expect the mean value of test score residuals to vary across different groupings of students and schools (see Card *et al.* (2013) for a discussion in the labor market context). For example, if high ability students gain more from attending highly effective schools than the average student, then we would expect our additively separable model to yield positive mean residuals for high ability students attending highly effective schools. To determine if our baseline model neglects important interactions between student and school effects such as this, we divide the estimated student and school effects into deciles and compute the mean residual in each of the 100 cells defined by the cross-classification of student and school effect deciles. Figure 2.5 plots the mean residual in each cell. These are reassuringly close to zero for all groups, and we observe no systematic patterns that would suggest heterogeneous school effects.

Overall, our specification tests reassure us that our baseline model with additively separable and homogenous student, school, and peer effects does a good job of capturing the main sources of variation in test scores in our data.

2.5.4 Stratification, peer quality and the test score gap

We now turn to our main analysis. We begin by using our estimates to explain the difference between average public and private school test scores. As shown in column (1) of Table 2.6, the average test score of private school students is 0.34 standard deviations above public school students in reading, and 0.30 standard deviations above public school students in numeracy. In the context of our baseline eq. 2.1, this test score gap could be a consequence of differences in the observed characteristics of public and private school students, differences in their underlying ability (α_i), differences in the ability of their peers, differences in the effectiveness of public and private schools (ψ_s), or transitory factors. We measure the relative contribution of each of these components using the decomposition identity:

$$\bar{y}^{priv} - \bar{y}^{pub} = \left(\bar{X}^{priv} - \bar{X}^{pub}\right)' \hat{\beta} + \left(\bar{\hat{\alpha}}^{priv} - \bar{\hat{\alpha}}^{pub}\right) + \hat{\eta}\left(\bar{\bar{\hat{\alpha}}}^{priv} - \bar{\bar{\hat{\alpha}}}^{pub}\right) + (\bar{\hat{\psi}}^{priv} - \bar{\hat{\psi}}^{pub}) + (\bar{\hat{\varepsilon}}^{priv} - \bar{\hat{\varepsilon}}^{pub}) \quad (2.10)$$

where hats indicate estimated parameters and overbars with the superscript *pub* or *priv* indicate conditional averages for public and private school students, respectively. The estimated components of this decomposition are presented in Table 2.6.

Perhaps unsurprisingly, the decomposition reveals strong positive selection into private schools. Differences between the average ability of public and private school students account for more than half of the gap in reading scores (0.181 standard deviations, or 53 percent) and about 18 percent of the gap in numeracy scores (0.056 standard deviations). The observable time-varying covariates in our data (neighborhood income, education, and immigrant share; and grade $\times$ year effects) account for a negligible fraction of this, and in fact serve to slightly close the overall gap. Students' time-invariant observed and unobserved characteristics, $\hat{\alpha}_i$, are thus the primary dimension along which students in our data select into private schools. This is visible in Supplemental Figure 2.6, where we plot the distributions of $\hat{\alpha}_i$ among public and private school students. The private school mean of $\hat{\alpha}_i$ exceeds the public school mean in both reading and numeracy, and the variance of student effects (especially in reading) is also slightly smaller among private school students, which reflects stronger positive selection on characteristics that are positively associated with high reading scores. This pattern holds across all of the private school types shown in Table 2.6 but is especially strong among prep school students.

The decomposition 2.10 also opens the black box of how schools influence test scores by distinguishing between peer effects and other school inputs. After controlling for differences between the observed and unobserved characteristics of their students, we find that private schools improve test scores relative to public schools by 0.159 standard deviations on average in reading and 0.252 standard deviations on average in numeracy. This is substantial. In the case of reading, where positive selection is strongest, more able peers account for roughly three quarters of private schools' superior performance (0.118 standard deviations), while school effectiveness $\hat{\psi}_s$ accounts for the remaining quarter (0.041 standard deviations). The situation is reversed for numeracy: better peers account for only 30 percent (0.077 standard deviations) of the public-private test score gap, while greater school effectiveness explains the remaining 70 percent (0.175 standard deviations).

These overall differences in means mask considerable heterogeneity among private schools. As illustrated in Supplemental Figures 2.7 and 2.8, the means of peer ability and school effects are larger at private schools than public schools, but so are their variances. Prep schools improve average student test scores substantially more than faith schools in both reading and numeracy. Indeed, their overall effect on test scores is more than twice the private school average (0.334 standard deviations in reading, and 0.581 standard deviations in numeracy; see Table 2.6). Peers are a very important component of prep schools' overall performance, responsible for 85 percent (0.28 standard deviations) of the test score gap in reading and over 40 percent (0.24 standard deviations) in numeracy. Prep school effectiveness accounts for the remaining 0.05 standard deviations in reading and 0.34 standard deviations in numeracy. Catholic schools are also highly effective, with an overall effect on test scores similar to the private school average. However, peer effects play a smaller role in Catholic schools than in prep schools, contributing only 0.09 standard deviations to reading scores and 0.03 to numeracy. Catholic school effectiveness, on the other hand, accounts for a more substantial 0.07 standard deviations in reading and 0.23 in numeracy. Finally, other Christian schools are the least effective group, though they still outperform public schools by roughly 0.04 standard deviations in both reading and numeracy. All of this advantage comes about via peer effects, and the average school effect in this sector is smaller than that of the average public school.[28]

What determines the performance of private schools? In Table 2.7 we consider the role of public funding and apply our decomposition 2.10 by funding group. Recall that group 1 schools receive 50% of the public school grant per student; group 2 schools receive a reduced grant (35%) because their operating expenses exceed the public school average; and group 3 and 4 schools do not receive public funding because they do not comply with provincial requirements (group 3) or because more than half of their students are international (group 4). The estimates in Table 2.7 reveal a strong relationship between public funding and academic performance among prep schools, but not among faith schools. Group 2 prep schools have the highest average test scores – nearly a full standard deviation above the public school mean in both reading and numeracy. Once again, these schools outperform in reading primarily because of more able students and peers, while their strong numeracy performance is primarily due to greater school effectiveness. However, group 2 prep schools also have larger average school effects than faith schools or prep schools in other funding categories. This suggests that private prep schools that emphasize academic quality use the additional operating funds that they raise from tuition or donations to improve learning outcomes, and in doing so attract stronger students. Among faith schools, on the other hand, there is little systematic relationship between funding group, average test scores, or the components of test scores, which suggests that these schools may use additional operating funds to pursue other goals.

We have already seen that the average private school improves test scores relative to the average public school. Do private schools also improve test scores relative to the public schools with which they compete most directly? In Table 2.4, we compare the outcomes of

[28] Given the small size and variable focus of the schools in our "other secular" category, we don't report decompositions for that group.

private school students with average outcomes at their attendance zone school. The first column replicates the mean test score gap between private and public school students from column (1) of Table 2.6. In the second column, we report the average difference between the test scores of private school students and the mean test score at their attendance zone public school. Overall, the two test score gaps are almost identical, indicating that average test scores in the attendance zones from which private school students are drawn are no different than the overall average. However, this equality does not hold when we disaggregate by private school type. The gap between prep school students' test scores and average scores at their attendance zone public school is only about two thirds as large (0.513 standard deviations in reading and 0.459 in numeracy) as the gap measured against the average public school. This indicates that prep school students are drawn from attendance zones served by public schools where students achieve well above the overall average. Catholic and Other Christian school students, on the other hand, are drawn from attendance zones where public school students have test scores slightly below the overall average, so that the test score gap measured against their attendance zone school exceeds the average gap.

The remaining columns of Table 2.4 report similar gaps between private school students' estimated test score components ($\hat{\alpha}_i$, $\hat{\psi}_s$, and $\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$) and the average value of those components at their attendance zone public school.[29] Prep school students face attendance zone gaps that are smaller than the gap measured against the average public school. This is true for all three components of test scores, indicating that prep school students are drawn from neighborhoods served by public schools that exceed the public school average in student ability, school effectiveness, and peer ability. Prep school students' attendance zone gap in ability is large and positive (0.286 standard deviations in reading and 0.163 in numeracy). On average, therefore, students enrolled at prep schools substantially exceed the average ability of students at their attendance zone public school, which is further evidence of positive selection, or cream-skimming. The prep school attendance zone gaps in school effects and peer effects are also positive, indicating that prep schools offer their students greater school effectiveness and access to better peers than is available at their attendance zone public school.

Estimates for Catholic schools generally mirror those of prep schools, but with attenuated magnitudes. The notable exception is the attendance zone gap in school effects for reading (0.096), which exceeds the difference between the average school effect of Catholic schools and public schools (0.068). This indicates that Catholic school students are drawn,

[29]Column (4), for example, reports the mean value of $\hat{\alpha}_i - \bar{\hat{\alpha}}^{az(i)}$ among private school students, where $\bar{\hat{\alpha}}^{az(i)}$ is the average estimated student effect at student i's attendance zone public school. Column (6) reports the mean difference between private school students' estimated school effect ($\hat{\psi}_s$) and the estimated school effect of their attendance zone public school; and column (8) reports the mean difference between private school students' estimated peer effect ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$) and the estimated average peer effect at their attendance zone public school.

on average, from neighborhoods served by public schools that are less effective than the average public school in reading.

Students attending Other Christian schools face attendance zone gaps that slightly exceed the gap measured against the average public school. This is true for all three components of test scores, indicating that students attending these schools live in the attendance zones of public schools that are slightly below the public school average in student ability, school effectiveness, and peer ability. The attendance zone gaps in student and peer ability are quite large, especially in reading (0.194 and 0.125 standard deviations, respectively), which is in line with our earlier evidence of positive selection. However, the attendance zone gap in reading school effects is notably negative (-0.064 standard deviations), indicating that Other Christian schools are substantially less effective at fostering achievement in reading than the public schools with which they compete for students.

Overall, our estimates indicate that B.C.'s private schools serve students who have very good public school enrolment opportunities, and yet they are more effective overall than these local public school alternatives. While this insight does not tell us how these schools succeed, strong public school alternatives do provide motivation for them to work hard to do so. It also helps to reconcile our results with findings in other jurisdictions: if private schools in other jurisdictions are located in areas where they compete locally with public schools at the lower end of the quality distribution, this could explain why others have found private schools to be similar or less effective than the average public school.

2.5.5 Cream-skimming effects on public school students

Our estimates provide strong evidence that students in B.C.'s Lower Mainland are positively selected into private schools on the basis of ability. This raises concerns about a potential cream-skimming effect, i.e., that high-ability students who opt out of their local public school might negatively affect the achievement of students who remain enrolled in public schools ("stayers") via peer effects. To quantify the potential magnitude of the cream-skimming effect, we undertake several policy simulations in which we consider counterfactual policies that would reallocate a large number of students between public and private schools.

Consider a counterfactual policy δ that affects students' decision to attend a public or private school. The cream-skimming effect for a student i who is enrolled in the same public school under both the status quo and the counterfactual δ, $\pi_i(\delta)$, is the difference between her predicted achievement under δ, $y_i(\delta)$, and her observed achievement under the status quo, $y_i(0)$:

$$\begin{aligned}\pi_i(\delta) &= y_i(\delta) - y_i(0) && (2.11)\\ &= \eta\left(\bar{\alpha}(\delta)_{\sim i,gst} - \bar{\alpha}(0)_{\sim i,gst}\right) && (2.12)\end{aligned}$$

where $\bar{\alpha}(\delta)_{\sim i,gst}$ and $\bar{\alpha}(0)_{\sim i,gst}$ are the average ability of her peers under policy δ and the status quo, respectively. The aggregate cream-skimming effect equals the expected value of the individual cream-skimming effect, taken over all public school stayers:

$$\pi(\delta) = E\left[\eta(\bar{\alpha}(\delta)_{\sim i,gst} - \bar{\alpha}(0)_{\ \sim i,gst})|\ P_i(\delta) = P_i(0) = 1\right] \tag{2.13}$$

where $P_i(0)$ and $P_i(\delta)$ are indicators that equal one if i is observed attending a public school under the status quo and policy δ, respectively.[30]

To quantify the cream-skimming effect we must define which students attend public and private schools under the counterfactual. This defines the population of interest (those who remain enrolled in a public school under the counterfactual, i.e., stayers) and allows us to simulate the change in peer quality at each public school. To this end, we estimate a simple public/private school choice model and use our estimates to rank public school students according to their predicted probability of attending a private school.[31] We present estimates of the choice model in Supplemental Table 2.13. In Counterfactual One, we reassign the 10% of public school students who are most likely to attend a private school to a private school, calculate the mean peer ability of those who remain in public schools, and predict the counterfactual test scores of stayers with these new peer groups using our Table 2.3 coefficient estimates. The first row of Table 2.5 shows that the counterfactual test scores of the remaining 90% of public school students are virtually identical to their actual test scores in both reading or numeracy. In Counterfactuals Two and Three, we double the share of students residing in each attendance zone who attend a private school. In Counterfactual Two, we do so by reassigning public school students with the highest within-zone probability of attending a private school. Again, we find that the counterfactual test scores of stayers are essentially identical to their actual test scores. In Counterfactual Three, we reassign the public school students with the highest within-zone ability, $\hat{\alpha}_i$. This highly unrealistic counterfactual provides an upper bound on the potential negative effect of doubling private school enrolment. In this case, the simulated cream-skimming effect sizes are large, lowering the average reading score of stayers by about 0.1 standard deviations in reading and 0.2 in numeracy. Together, these results show that while the potential cream-skimming effects of increasing private school enrolment are large, non-negligible effect sizes are very unlikely.

[30]Our simulations assume that students are randomly assigned to classrooms within school/grades, both under the status quo and the counterfactual, and that changes in private school enrolment levels have no effect on public school quality.

[31]The choice model is a simple binary probit where the dependent variable is an indicator that equals one if a student attends a private school. Explanatory variables are the neighborhood share of immigrant households; neighborhood shares of household heads with a high school diploma, a post-secondary certificate, or a bachelor's degree; mean neighborhood family income; indicators for language spoken at home, enrolment in an English as a Second Language program, Aboriginal self-identity, and gender; and cohort fixed effects. See Supplemental Table 2.13 for coefficient estimates

2.5.6 Effects of reducing private school enrolment

To estimate the full distributional consequences of a policy that increases private school enrolment, we would need to estimate its effect on students counterfactually reassigned to private schools, and on those enrolled in private schools under the status quo. Doing so would require us to counterfactually assign movers to specific private schools. Unfortunately, we have little basis on which to predict these specific school choice decisions. Under B.C.'s existing voucher policy, however, it is equally of interest to evaluate the effects of policies that would *reduce* private school enrolment. In this case, we can estimate the full distributional consequences by counterfactually reassigning private school students to their attendance zone school.[32] Table 2.8 presents the simulated effect of reducing private school enrollment under four additional counterfactuals of this type. In Counterfactual Four, we rank all private school students according to their predicted probability of attending a public school (again, using the estimated public-private school choice model reported in Supplemental Table 2.13 and reassign the 50% most likely to do so. In Counterfactual Five, we rank private school students within each attendance zone according to their predicted probability of enrolling in a public school and reassign the 50% most likely to do so. This reduces the share of private school enrolment in each attendance zone by half. In Counterfactual Six, we rank private school students by $\hat{\alpha}_i$ *within each attendance zone* and reassign the 50% with the highest ability to their attendance zone public school. Finally, in Counterfactual Seven, we reassign all private school students to their attendance zone public school.

The first column of Table 2.8 shows that none of these counterfactual reassignments would have a large effect on public school students. The largest positive effect arises in Counterfactual Six, in which the highest ability private school students from each attendance zone are reassigned to their local public school, but the effect remains very small: a 0.011 standard deviation increase in average reading test scores; 0.017 in numeracy. The negative effects on those who remain enrolled in private schools are larger under every counterfactual, e.g., a 0.082 standard deviation decline in reading scores and a 0.122 standard deviation decline in numeracy due to the loss of high-ability peers under Counterfactual Six. Those who are counterfactually reassigned to their attendance zone public school also experience substantial negative effects on average. Under Counterfactual Four, for example, private school movers experience a 0.133 standard deviation average decline in reading scores, of which roughly 70% is due to the loss of above-average peers and 30% due to a reduction

[32] About 65% of elementary school students attended their attendance zone public school during the period of study (see Friesen *et al.* (2019a)). Of the remaining 35%, 13% were enrolled in private schools, and the remainder were enrolled in public magnet programs or out-of-zone public schools. Our simulations may overestimate the effect of reducing private school enrolment if private schools students are more likely to enroll in such public school alternatives, and if those alternatives are more similar to a private school than their attendance zone public school is. Our simulations also assume that residential choice is unaffected by voucher generosity, which is unlikely to hold in practice.

of school effectiveness; and a 0.223 standard deviation average decline in numeracy scores, primarily due to lower school effectiveness. Private school movers experience smaller, but still substantial, negative effects under the other three counterfactuals.

2.6 Conclusion

Private school vouchers remain an actively debated and contentious area of education policy. In Chile, wide-spread opposition to a voucher program has policy makers discussing proposals to eliminate tuition top-ups and reduce the scope for selective admissions (Epple *et al.* , 2017). In the U.S., voucher proponents continue to actively pursue new and expanded voucher programs (e.g. DeVos (2017). Despite a rich theoretical literature, empirical evidence on how large-scale school voucher programs affect student outcomes remains limited. B.C.'s voucher system offers a unique opportunity to observe the outcomes of both public and private school attendees, and yet has received relatively little attention from researchers – despite having several key features espoused by voucher advocates, including selective admissions policies, tuition top-ups, and faith-based (or other) supplements to the standard curriculum. Our estimates provide novel empirical evidence about how vouchers affect student outcomes that can inform the wider policy debate.

Our results show that students who attend B.C.'s private voucher schools experience substantial academic benefits, at little cost in terms of peer quality to those who remain enrolled in public schools. From one perspective, it is unsurprising that these schools are so effective: they can selectively admit students, charge tuition top ups, selectively hire and fire teachers, supplement the curriculum, etc., all of which affords them opportunities to shape a high quality learning environment that may not be available to regular public schools. However, it is more surprising when viewed in the context of previous research that has found little evidence that private schools offer any advantage in primary school achievement in other advanced economies. Understanding why B.C.'s private schools are successful is therefore a key question for research going forward. Our results indicate that differences in mean peer quality are not the primary underlying mechanism. It remains possible, however, that our private school effects reflect some kind of peer spillover that is not captured by our linear-in-means specification, such as the exclusion of disruptive peers. If so, then B.C. private schools' recipe for success could not be implemented system-wide. Similarly, if B.C. private schools succeed because they are poaching the best teachers from the public system, this also could not be replicated system-wide.[33] Our data do not allow us to investigate either of these hypotheses.

[33] 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.

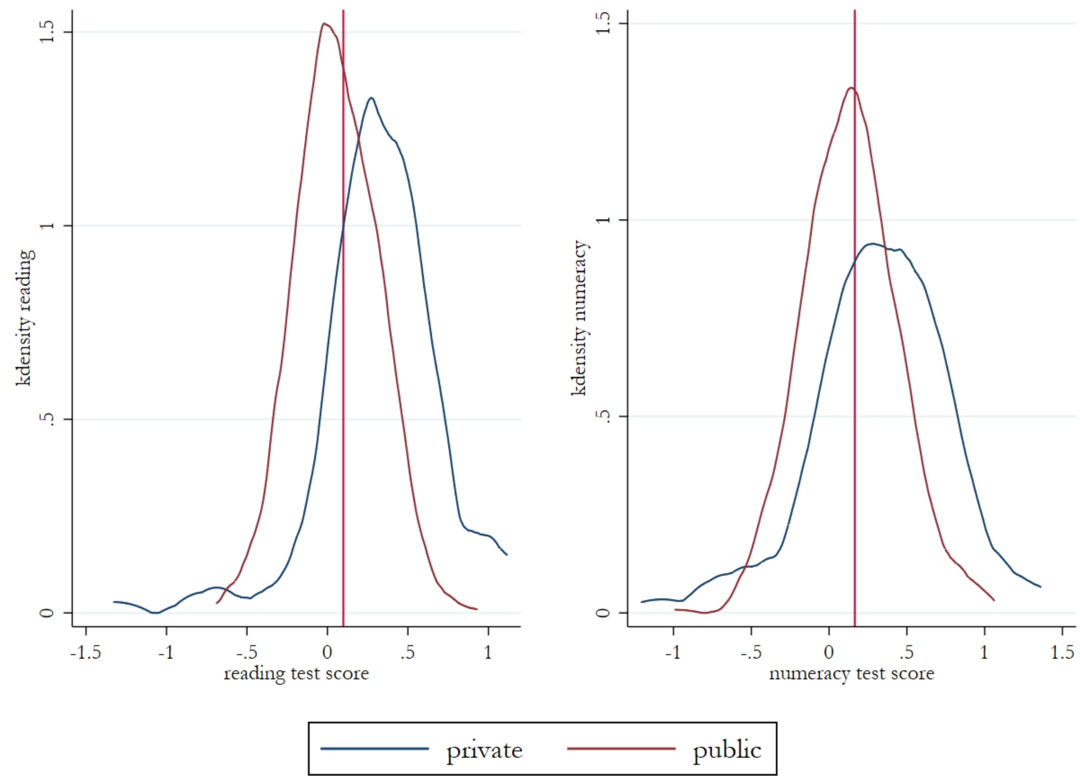
While public schools could, in principle, replicate features of private schools' funding mechanism, our results do not point to additional funding as a key factor in B.C. private schools' success. Recent evidence of the success of so-called *No Excuses* charter schools in the United States, which employ similar approaches as the stereotypical private school (e.g., school uniforms, high expectations for student conduct, and an emphasis on clear and frequent communication with parents), suggests one set of potential mechanisms that could be emulated by public schools.[34] However, evidence from other jurisdictions that Catholic schools that employ similar approaches are not especially effective casts doubt on this as an important factor in the success of B.C.'s private schools (e.g. Altonji *et al.* (2005); Carbonaro (2006); Elder & Jepsen (2014); Jepsen (2003); Lubienski *et al.* (2008); Reardon *et al.* (2009)).

A notable feature of the B.C. context is the high achievement levels of public school students and the wide range of choice available within the public school system. Students who attend B.C. private schools have high-quality public school alternatives available to them. This may not be true in all other jurisdictions, particularly in urban settings in the U.S. Whether and how school location and local competition from public schools influence the effectiveness of private schools remains an open question for future research. Stiff competition from high-quality public schools may be a key ingredient in B.C. private schools' success.

[34]See, for example, Dobbie & Fryer Jr (2011) and Angrist *et al.* (2013), although evidence from Chabrier *et al.* (2016) is less positive.

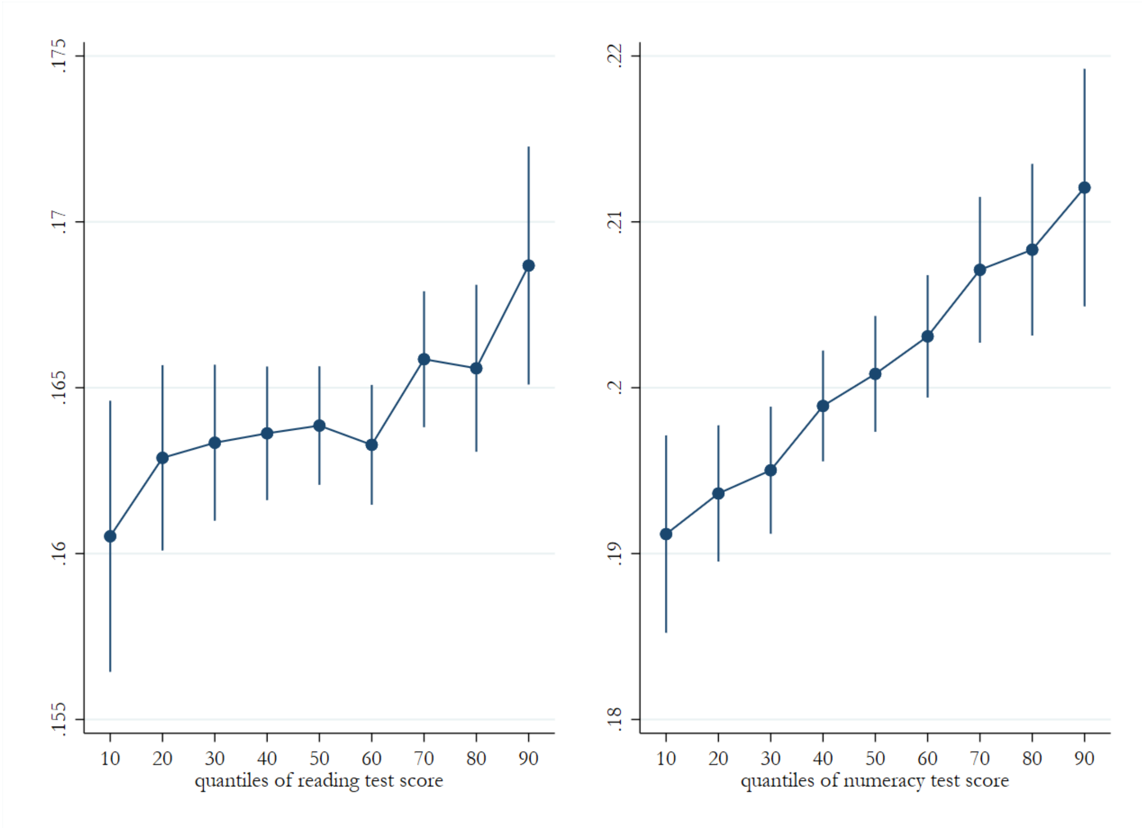
2.7 Figures

Figure 2.1: Kernel density estimates of the distribution of school mean test scores $\bar{y}_s$, by public and private schools



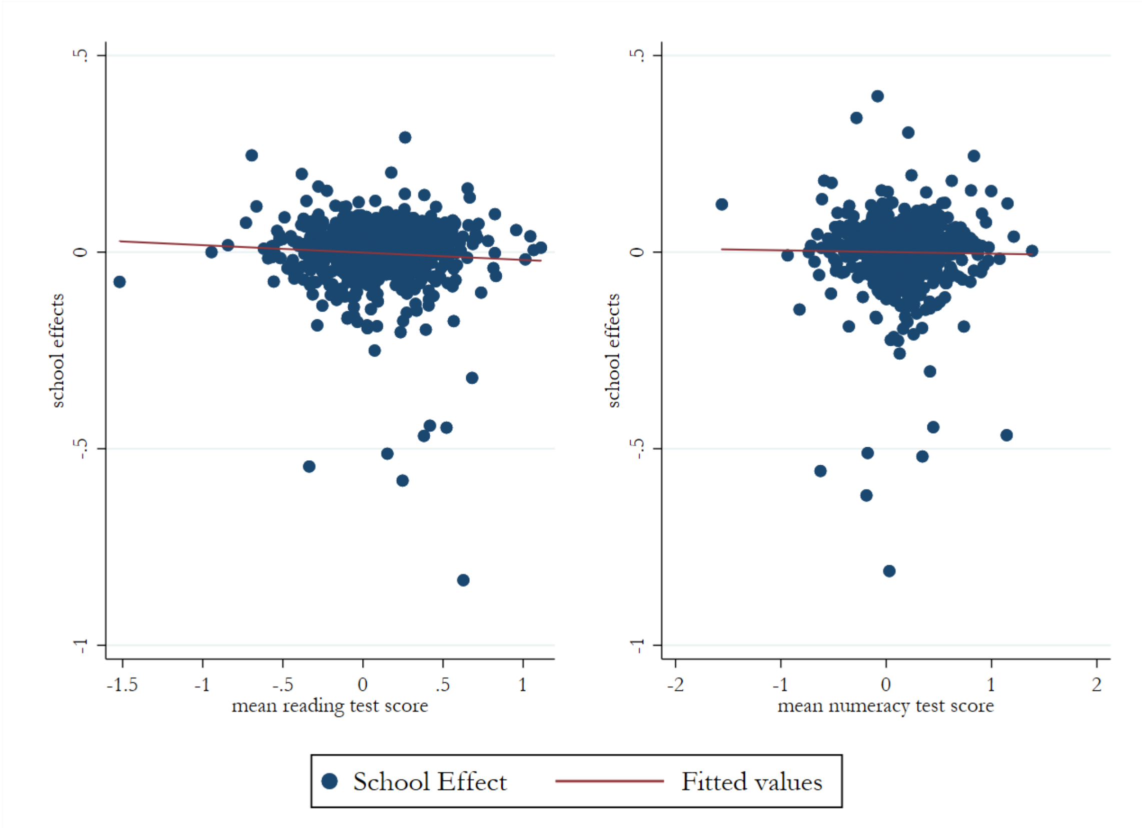
Notes: The figure plots the distribution of school-average reading and numeracy FSA exam scores in 559 public and 117 private schools in the Lower Mainland of B.C. The vertical red line in each panel is the overall mean in our sample.

Figure 2.2: Estimated peer ability effect at deciles of the distribution of achievement



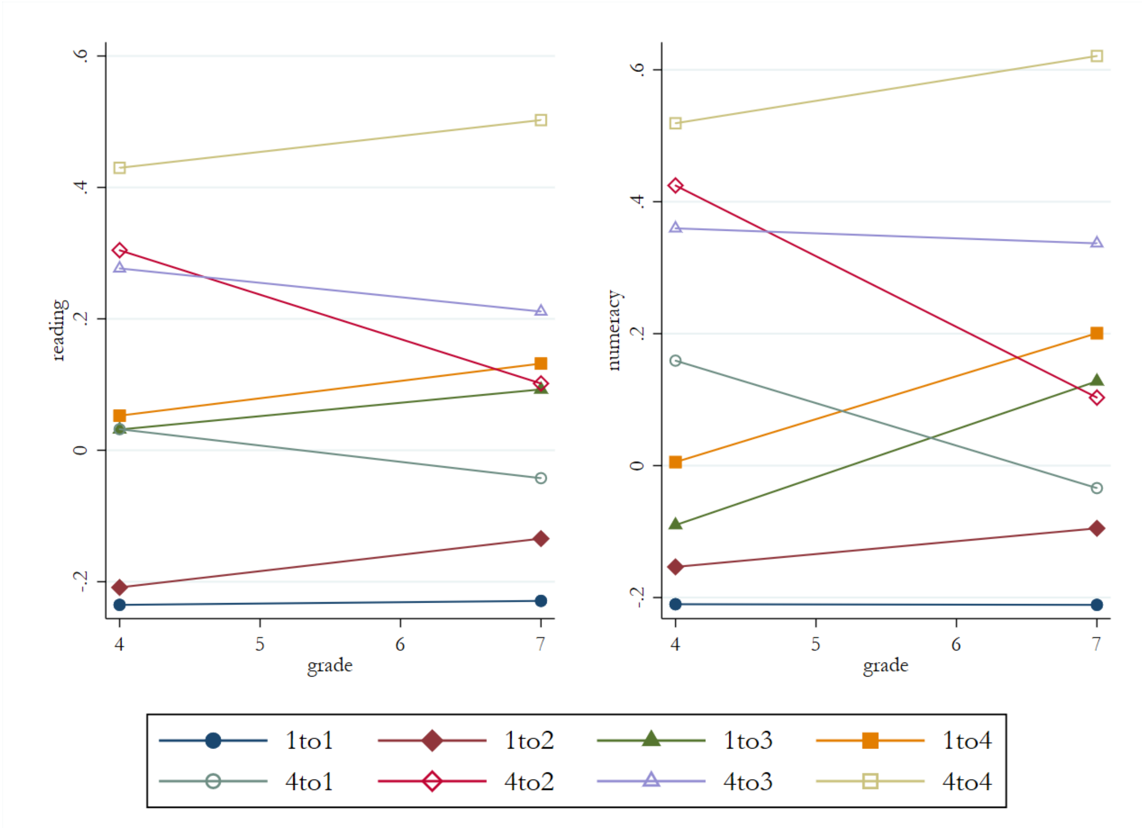
Notes: The figures are based on estimates of the quantile specification, eq. 2.2, at deciles of the distribution of test scores estimated on the same sample as our main estimates in Table 2.3. Dots indicate the estimated peer quality effect, and vertical bars provide a 95% confidence interval. Plotted values are obtained by rescaling the estimated spillover parameters (and confidence interval endpoints) by $\sigma_{\eta\bar{\theta}}/\sigma_y$, where $\sigma_{\eta\bar{\theta}}$ is the estimated standard deviation of peer quality and σ_y is the standard deviation of test scores in our data. The estimated quantile spillover parameters, $\hat{\eta}^{\tau}$, and their standard errors are reported in Supplemental Table 2.11. Covariates include: neighborhood share of immigrants; neighborhood shares of parents with high school, post-secondary certificate, and bachelor's degree; mean neighborhood family income; and grade $\times$ year effects.

Figure 2.3: The relationship between school-average test scores $\bar{y}_s$ and estimated school effects in test participation



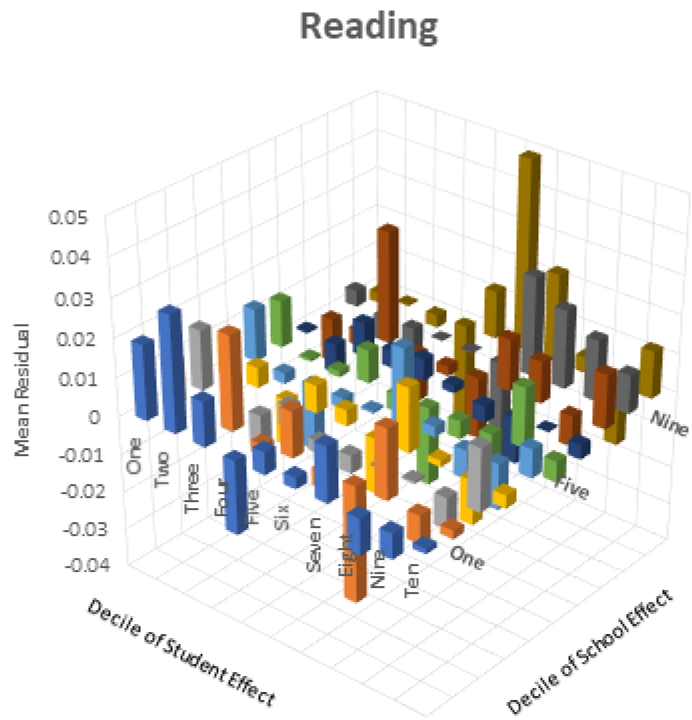
Notes: The figure plots school-average test scores (on the horizontal axis) vs. estimated fixed school effects in test score participation, $\hat{\theta}_s$ from equation 2.9. Coefficient estimates for this specification are reported in Supplemental Table 2.12. Covariates include: neighborhood share of immigrants; neighborhood shares of parents with high school, post-secondary certificate, and bachelor's degree; mean neighborhood family income; and grade × year effects. Estimation sample is the same as our main estimates in Table 2.3.

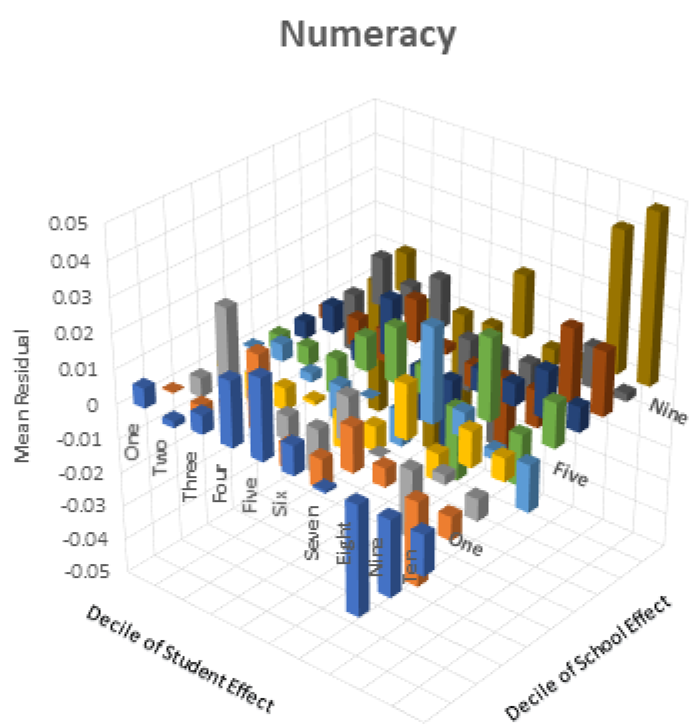
Figure 2.4: Mean test scores of movers, by quartile of peer-average test scores



Notes: The figure plots the average change in test scores of students who change school between grades 4 and 7, by quartile of peer-average test scores at the grade 4 and 7 schools. This figure shows the change in average test scores for students who leave schools in the top or bottom quartile of peer-average test scores; Supplemental Figure 2.9 presents a comparable plot for students that leave schools in the second or third quartile of peer-average test scores.

Figure 2.5: Mean test score residuals by decile of student and school effects





Notes: The figure plots the mean residual in 100 cells defined by deciles of student and school effects. The estimated student effects, school effects, and residuals are based on the specification and sample reported in Table 2.3.

2.8 Tables

Table 2.1: Characteristics and program features of some universal voucher programs

	Scope and history		2*Share of enrolment	2*For-profit	Allowed features		
Jurisdiction	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, Canada	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] Source: Federation of Private Schools Associations, British Columbia (2015). [f] Source: BC Ministry of Education (2005). [g] Source: Federation of Private Schools Associations, British Columbia (2015).

Table 2.2: Selected school and student characteristics, by school type

School type	Public Schools	Private Schools						Public & Private	
		All	Prep	Catholic	Christian	Other Faith	Other Secular	All	Movers only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					School Characteristics				
Number of schools: All	559	117	15	38	41	10	13	676	674
Funding group 1	0	83	9	32	30	5	7	83	82
Funding group 2	0	18	5	2	9	2	0	18	16
Funding group 3	0	9	1	4	2	1	1	9	9
Funding group 4	0	4	0	0	0	2	2	4	4
Offers grade 4 only	102	7	1	0	3	0	3	109	109
Offers grade 7 only	37	9	1	0	2	2	4	46	46
Offers grades 4 and 7	420	101	13	38	36	8	6	521	519
					Student Characteristics				
No. of observations	188608	24326	4137	9773	7959	2095	362	212934	82944
% of the sample	88.6	11.4	1.9	4.6	3.7	1.0	0.20	100.0	39.0
Reading score	0.03	0.39	0.81	0.34	0.27	0.23	0.09	0.07	0.01
Numeracy score	0.10	0.42	0.84	0.39	0.23	0.49	-0.16	0.14	0.06
Missing reading	0.12	0.05	0.04	0.04	0.06	0.07	0.29	0.11	0.13
Missing numeracy	0.14	0.06	0.05	0.05	0.07	0.08	0.31	0.13	0.15
Number of peers	53.1	37.2	49.2	28.2	40.4	48.5	6.7	51.3	66.8
French Immersion	0.07	0.00	0.03	0.03	0.00	0.00	0.00	0.06	0.08
Home language: English	0.67	0.71	0.75	0.76	0.74	0.26	0.87	0.67	0.68
Chinese	0.12	0.07	0.1	0.08	0.08	0.00	0.01	0.12	0.10
Punjabi	0.07	0.06	0.02	0.00	0.01	0.56	0.05	0.07	0.07
Other	0.14	0.16	0.14	0.17	0.17	0.19	0.07	0.14	0.16
Aboriginal	0.06	0.01	0.00	0.01	0.01	0.00	0.02	0.05	0.07
Female	0.49	0.50	0.51	0.51	0.47	0.52	0.44	0.49	0.49
Neighborhood: Share immigrant	0.08	0.08	0.08	0.09	0.06	0.13	0.06	0.08	0.08
Share without high school	0.29	0.27	0.19	0.28	0.30	0.33	0.21	0.29	0.29
Share high school	0.25	0.25	0.23	0.25	0.26	0.25	0.24	0.25	0.25
Share some college	0.29	0.28	0.25	0.28	0.30	0.24	0.29	0.29	0.30
Share Bachelor’s degree or higher	0.16	0.2	0.33	0.19	0.14	0.19	0.25	0.17	0.15
Mean family income	6.53	7.47	11.39	6.74	6.6	6.22	8.58	6.63	6.35

Table 2.3: Selected estimates of our baseline student test score model

	Reading	Numeracy
	(1)	(2)
Spillover parameter ($\hat{\eta}$)	0.645***	1.145***
	(0.012)	(0.016)
Peer quality effect	0.165	0.201
Standard deviation of:		
Test Scores (y_{ig})	0.908	0.938
Student effect ($\hat{\alpha}_i$)	0.817	0.818
School effect ($\hat{\psi}_s$)	0.197	0.237
Peer ability ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$)	0.150	0.189
Correlation between:		
Student and school effects ($\hat{\alpha}_i$, $\hat{\psi}_s$)	-0.136	-0.008
Student and peer effects ($\hat{\alpha}_i$, $\bar{\hat{\alpha}}\ _{i,gst}$)	0.175	0.037
Share of test score variance accounted for by:		
Student effects ($\hat{\alpha}_i$)	0.810	0.761
School effects ($\hat{\psi}_s$)	0.047	0.064
Peer ability ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$)	0.027	0.041
Covariates ($X'_{ig}\hat{\beta}$)	0.008	0.017
Residuals ($\hat{\varepsilon}_{ig}$)	0.156	0.157
Covariances	-0.048	-0.040
R^2	0.844	0.843
N	188,690	185,636

Notes: Reported estimates are based on equation 2.1; variance decomposition is based on equation 2.8 in footnote 19. Peer quality is defined as the average estimated student effect of same-grade peers, $\bar{\alpha}_{\sim i,gst}$, and the peer quality effect measures the effect of a one standard deviation increase in peer quality on test scores (in standard deviations); see footnote 20 for details. Covariates include: neighborhood share of immigrants; neighborhood shares of parents with high school, post-secondary certificate, and bachelor's degree; mean neighborhood family income; and grade \times year effects. Estimation sample comprises all public and private school students in the Lower Mainland of B.C. who were enrolled in Grade 4 between 1999/2000 and 2004/2005 and advanced one grade in each of the following three years, at a school that met our enrollment restriction, with non-missing values of all key variables. See Section 2.3 for additional details about sample construction and composition. Standard errors are estimated via the wild bootstrap and are reported in parentheses, *** indicates statistical significance at the 0.1% level.

Table 2.4: Private school outcomes vs. attendance zone school outcomes

	Test score gap		Student effects gap		School effects gap		Peer effects gap	
	Average	Attendance zone	Average	Attendance zone	Average	Attendance zone	Average	Attendance zone
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Reading				
All Private	0.340	0.338	0.183	0.173	0.041	0.041	0.118	0.112
	(0.002)	(0.004)	(0.011)	(0.010)	(0.016)	(0.016)	(0.006)	(0.006)
Prep	0.766	0.513	0.440	0.286	0.050	0.017	0.284	0.185
	(0.006)	(0.009)	(0.022)	(0.019)	(0.034)	(0.031)	(0.012)	(0.011)
Catholic	0.297	0.344	0.142	0.131	0.068	0.096	0.092	0.085
	(0.004)	(0.005)	(0.016)	(0.017	(0.026)	(0.027)	(0.010)	(0.010)
Other Christian	0.221	0.235	0.171	0.194	-0.066	-0.064	0.110	0.125
	(0.004)	(0.006)	(0.016)	(0.016)	(0.026)	(0.025)	(0.009)	(0.009)
				Numeracy				
All Private	0.304	0.307	0.067	0.072	0.175	0.143	0.077	0.082
	(0.003)	(0.004)	(0.005)	(0.005)	(0.010)	(0.010)	(0.005)	(0.005)
Prep	0.726	0.459	0.210	0.163	0.341	0.105	0.240	0.187
	(0.006)	(0.009)	(0.012)	0.012	(0.022)	(0.024)	(0.010)	(0.010)
Catholic	0.274	0.320	0.027	0.025	0.231	0.228	0.030	0.029
	(0.004)	(0.006)	(0.008)	(0.009)	(0.016)	(0.016)	(0.007)	(0.007)
Other Christian	0.120	0.147	0.068	0.082	-0.039	0.010	0.078	0.094
	(0.004)	(0.006)	(0.009)	(0.009)	(0.017)	(0.017)	(0.008)	(0.008)

Notes: Columns (1), (3), (5), and (7) reproduce estimates from columns (1), (2), (5), and (6) of Table 2.6, respectively. Column (2) reports the average difference between private school students' test score and the average test score at their attendance school. Column (4) reports the average difference between private school students' estimated student effect ($\hat{\alpha}_i$) and the average student effect at their attendance zone public school. Column (6) reports the average difference between private school students' estimated school effect ($\hat{\psi}_s$) and the estimated school effect of their attendance zone public school. Column (8) reports the average difference between private school students' estimated peer effect ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,gst}$) and the estimated peer effect at their attendance zone public school. Specification and sample are the same as Table 2.3; standard errors in parentheses.

Table 2.6: Decomposition of mean test score gap between private and public schools

	Mean test score gap	Contributions of student characteristics			Contributions of school characteristics			
		Student effects	Covariates	Total student	School effects	Peer effects	Total school	Residuals
	(1)	(2)	(3)	(4) = (2) + (3)	(5)	(6)	(7) = (5) + (6)	(8)
				Reading				
		Fixed effects	Covariates	Total student	Fixed effects	Peer effects	Total school	
All Private	0.340	0.183	-0.002	0.181	0.041	0.118	0.159	0.000
	(0.002)	(0.011)	(0.001)		(0.016)	(0.006)		(0.000)
Prep	0.766	0.440	-0.007	0.433	0.050	0.284	0.334	-0.001
	(0.006)	(0.022)	(0.005)		(0.034)	(0.012)		(0.000)
Catholic	0.297	0.142	-0.004	0.138	0.068	0.092	0.160	0.000
	(0.004)	(0.016)	(0.001)		(0.026)	(0.010)		(0.000)
Other Christian	0.221	0.171	0.006	0.177	-0.066	0.110	0.044	0.000
	(0.004)	(0.016)	(0.001)		(0.026)	(0.009)		(0.000)
				Numeracy				
All Private	0.304	0.067	-0.011	0.056	0.175	0.077	0.252	-0.003
	(0.003)	(0.005)	(0.001)		(0.010)	(0.005)		(0.000)
Prep	0.726	0.210	-0.055	0.155	0.341	0.240	0.581	-0.010
	(0.006)	(0.012)	(0.007)		(0.022)	(0.010)		(0.000)
Catholic	0.274	0.027	-0.012	0.015	0.231	0.030	0.261	-0.001
	(0.004)	(0.008)	(0.001)		(0.016)	(0.007)		(0.000)
Other Christian	0.120	0.068	0.014	0.082	-0.039	0.078	0.039	-0.002
	(0.004)	(0.009)	(0.001)		(0.017)	(0.008)		(0.000)

Notes: Authors' calculations based on the specification and sample reported in Table 2.3, using the decomposition identity. Column (1) reports the mean test score gap between private and public school students, $\bar{y}^{priv} - \bar{y}^{pub}$. Column (2) reports the difference between the average student effect of private and public school students, $\bar{\hat{\alpha}}^{priv} - \bar{\hat{\alpha}}^{pub}$. Column (3) reports the difference in average characteristics of private and public school students, $\left(\bar{X}^{priv} - \bar{X}^{pub}\right)' \hat{\beta}$. Column (4) is the sum of columns (2) and (3). Column (5) reports the difference between the average school effects of private and public schools, $\bar{\bar{\hat{\psi}}}^{priv} - \bar{\bar{\hat{\psi}}}^{pub}$. Column (6) reports the difference between the average peer effects of private and public school students, $\hat{\eta}\left(\bar{\bar{\hat{\alpha}}}^{priv} - \bar{\bar{\hat{\alpha}}}^{pub}\right)$. Column (7) is the sum of columns (5) and (6), and column (8) is the difference between the mean residuals of private and public school students, $\bar{\hat{\varepsilon}}^{priv} - \bar{\hat{\varepsilon}}^{pub}$. Standard errors are calculated using a wild bootstrap procedure and reported in parentheses.

Table 2.7: Decomposition of mean test score gap between private and public schools, by funding group

	2*Funding group	2*# of schools	2*Mean test score gap	Contributions of student characteristics			Contributions of school characteristics			2*Residuals
				Student effects	Covariates	Total student	School effects	Peer effects	Total school	
	(1)	(2)	(3)	(4)	(5)	(6) = (4) + (5)	(7)	(8)	(9) = (7) + (8)	(10)
						Reading				
All Private	1	82	0.325	0.181	-0.001	0.180	0.029	0.117	0.146	-0.000
	2	17	0.524	0.312	-0.005	0.307	0.016	0.201	0.217	-0.001
	3	9	0.304	0.204	-0.001	0.203	-0.030	0.132	0.102	-0.001
	4	3	0.156	-0.194	0.000	-0.194	0.475	-0.125	0.350	0.000
Prep	1	9	0.697	0.413	-0.010	0.403	0.028	0.267	0.295	-0.001
	2	5	0.924	0.495	-0.003	0.492	0.114	0.319	0.433	-0.001
	3	1	0.655	0.417	0.001	0.418	-0.032	0.269	0.237	0.001
Faith	1	67	0.271	0.145	0.000	0.145	0.032	0.094	0.126	0.000
	2	12	0.210	0.169	-0.007	0.162	-0.061	0.109	0.048	-0.000
	3	7	0.217	0.152	-0.001	0.151	-0.032	0.098	0.066	-0.000
						Numeracy				
All Private	1	82	0.269	0.059	-0.010	0.049	0.155	0.068	0.223	-0.003
	2	17	0.500	0.149	-0.026	0.123	0.214	0.170	0.384	-0.007
	3	9	0.242	0.055	-0.007	0.048	0.133	0.063	0.196	-0.003
	4	3	0.504	-0.004	-0.001	-0.005	0.514	-0.005	0.509	-0.001
Prep	1	9	0.641	0.181	-0.066	0.115	0.328	0.208	0.536	-0.010
	2	5	0.910	0.272	-0.045	0.227	0.384	0.311	0.695	-0.013
	3	1	0.617	0.173	-0.015	0.158	0.267	0.198	0.465	-0.015
Faith	1	67	0.218	0.043	-0.001	0.042	0.129	0.049	0.178	-0.001
	2	12	0.177	0.052	-0.011	0.041	0.079	0.060	0.139	-0.002
	3	7	0.149	0.027	-0.004	0.023	0.097	0.031	0.128	-0.002

Notes: Authors' calculations based on the specification and sample reported in Table 2.3, using the decomposition identity 2.10. Column definitions are the same as Table 2.6. Group 1 schools receive 50% of the public school per student grant; group 2 schools receive 35% of the grant because their operating costs per student exceed the district-level public school average; group 3 schools receive no public funding because they do not comply with the requirements for funding stipulated under the Independent Schools Act; group 4 schools meet provincial requirements and are provincially certified but are ineligible for public funding because more than half of their enrolment is comprised of international students.

Table 2.5: Simulated cream-skimming effect of increasing private school enrolment

	Reading		Numeracy	
	Cream skimming $\hat{\pi}(\delta)$	Number reassigned	Cream skimming $\hat{\pi}(\delta)$	Number reassigned
	(1)	(2)	(3)	(4)
One	0.001	216,240	0.003	215,943
(reassign 10% most likely)	(0.000)		(0.000)	
Two	0.000	232,267	0.008	231,995
(double private enrolment by reassigning most likely in each attendance zone)	(0.000)		(0.000)	
Three	-0.101	232,696	-0.201	232,992
(double private enrolment by reassigning highest ability in each attendance zone)	(0.002)		(0.003)	

Notes: Authors' calculations based on estimates from the sample and specification reported in Table 2.3. $\hat{\pi}(\delta)$ reported in columns (1) and (3) is the sample analog of equation 2.13, which is the average difference between actual and counterfactual test scores among public school students who remain enrolled in a public school under the counterfactual. Columns (2) and (4) report the number of public school students reassigned to a private school under each counterfactual. In Counterfactual One, we reassign the 10% of public school students most likely to enroll in a private school to a private school. In Counterfactual Two we double the share of students in each attendance zone who are currently enrolled in a private school by reassigning the public school students most likely to choose a private school to a private school. In Counterfactual Three we double the share of students in each attendance zone who are currently enrolled in a private school by reassigning the public school students with the highest ability, $\hat{\alpha}_i$, to a private school. Public school students' probability of attending a private school is estimated using the public-private choice model reported in Supplemental Table 2.13. Standard errors are calculated using a wild bootstrap procedure and reported in parentheses.

Table 2.8: Simulated effect of reducing private school enrolment

	Public school students	Private school students		
	All	Stayers	Movers	
			Peer effects	School effects
	(1)	(2)	(3)	(4)
		Reading		
Four	0.006	-0.041	-0.094	-0.039
(reassign 50% most likely overall)	(0.000)	(0.003)	(0.005)	(0.017)
Five	0.001	-0.004	-0.065	-0.006
(reassign 50% most likely in each attendance zone)	(0.000)	(0.000)	(0.004)	(0.017)
Six	0.011	-0.082	-0.074	0.053
(reassign 50% highest ability in each attendance zone)	(0.000)	(0.002)	(0.005)	(0.017)
Seven	0.011	-0.082	-0.081	-0.040
(reassign all private school students)	(0.001)	(0.005)	(0.004)	(0.016)
		Numeracy		
Four	0.004	-0.026	-0.054	-0.169
(reassign 50% most likely)	(0.000)	(0.002)	(0.005)	(0.012)
Five	0.001	-0.007	-0.040	-0.102
(reassign 50% most likely in each attendance zone)	(0.000)	(0.001)	(0.005)	(0.011)
Six	0.017	-0.122	-0.011	-0.070
(reassign 50% highest ability in each attendance zone)	(0.000)	(0.002)	(0.005)	(0.011)
Seven	0.006	-0.045	-0.046	-0.136
(reassign all private school students)	(0.001)	(0.004)	(0.004)	(0.010)

Notes: Authors' calculations based on estimates from the sample and specification reported in Table 2.3. Column (1) reports the sample analog of equation (11), which is the average difference between actual and counterfactual test scores among public school students who remain enrolled in a public school under the counterfactual. Column (2) reports the sample analog of $E\left[\eta(\bar{\alpha}(\delta)_{\sim i,gst} - \bar{\alpha}(0)_{\sim i,gst}) |\ P_i(\delta) = P_i(0) = 0\right]$, which is the average difference between actual and counterfactual test scores among private school students who remain enrolled in their private school under the counterfactual. Column (3) reports the sample analog of $E\left[\eta(\bar{\alpha}(\delta)_{\sim i,g\widetilde{s}t} - \bar{\alpha}(0)_{\sim i,gst}) |\ P_i(\delta) = 1, P_i(0) = 0\right]$, which is the average difference between the actual and counterfactual peer effect among private school students who are reassigned to their attendance zone public school, $\widetilde{s}$, under the counterfactual. Column (4) reports the sample analog of $E\left[(\psi(\delta)_{\widetilde{s}} - \psi(0)_s) |\ P_i(\delta) = 1, P_i(0) = 0\right]$, which is the average difference between the actual and counterfactual school effect ($\psi(0)_s$ and $\psi(\delta)_{\widetilde{s}}$, respectively) among private school students who are reassigned to their attendance zone public school, $\widetilde{s}$, under the counterfactual. In Counterfactual Four, we reassign the 50% of private school students most likely to enroll in a public school to their attendance zone public school. In Counterfactual Five, we halve the share of students in each attendance zone who are enrolled in a private school by reassigning the private school students most likely to choose a public school to their attendance zone public school. In Counterfactual Six, we halve the share of students in each attendance zone who are enrolled in a private school by reassigning the private school students with the highest estimated ability, $\hat{\alpha}_i$, to their attendance zone public school. In Counterfactual Seven, we reassign all private school students to their attendance zone public school. Private school students' probability of attending a public school is estimated using the public-private choice model reported in Supplemental Table 2.13. Standard errors are calculated using a wild bootstrap procedure and reported in parentheses.

2.9 Supplement 2

2.9.1 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.

2.9.2 Details of the estimation procedure

We rewrite our empirical model of test scores as:

$$y_{ig} = X_i'\beta + \alpha_i + \psi_s + \frac{\eta}{M_{gst}} \sum_{j \in \mathcal{M}_{gst\sim i}} \alpha_j + \varepsilon_{ig}$$

where $\mathcal{M}_{gst\sim i}$ denotes the set of students (numbering M_{gst}) enrolled in grade g at school s in year t with student i removed.

Define $\widetilde{y_{ig}} = y_{ig} - X_i'\beta - \psi_s$, so that the nonlinear least square problem we seek to solve is:

$$\min_{\beta,\eta,\alpha,\psi} \sum_g \sum_i \left(\widetilde{y_{ig}} - \alpha_i - \frac{\eta}{M_{gst}} \sum_{j \in \mathcal{M}_{gst\sim i}} \alpha_j \right)^2 . \tag{A1}$$

We solve the least squares problem (A1) by iteratively minimizing the sum of squared residuals. Following Arcidiacono *et al.* (2012) and Battisti (2017), we toggle between estimating the spillover parameter and β by OLS and the fixed effects in each iteration. In each iteration, we update the α's using the first order condition from the least squares problem:

$$\sum_{g}\left[\left(\widetilde{y_{ig}}-\alpha_i-\frac{\eta}{M_{gst}}\sum_{j\in\mathcal{M}_{gst\sim i}}\alpha_j\right)+\sum_{j\in\mathcal{M}_{gst\sim i}}\frac{\eta}{M_{gst}}\left(\widetilde{y_{jg}}-\alpha_j-\frac{\eta}{M_{gst}}\sum_{k\in\mathcal{M}_{gst\sim j}}\alpha_k\right)\right]=0. \tag{A2}$$

Solving for α_i yields the updating formula for the student effects:

$$\alpha_i=\frac{\sum_{g}\left[\left(\widetilde{y_{ig}}-\frac{\eta}{M_{gst}}\sum_{j\in\mathcal{M}_{gst\sim i}}\alpha_j\right)+\sum_{j\in\mathcal{M}_{gst\sim i}}\frac{\eta}{M_{gst}}\left(\widetilde{y_{jg}}-\alpha_j-\frac{\eta}{M_{gst}}\sum_{k\in\mathcal{M}_{gst\sim i\sim j}}\alpha_k\right)\right]}{\left(G+\sum_{g}\frac{\eta^2}{M_{gst}}\right)} \tag{A3}$$

where G is the number of grades. Theorem 2 in Arcidiacono et al. (2012) establishes that (A3) is a contraction mapping, so that repeatedly updating the α's via this equation yields a fixed point and estimates converge to the nonlinear least squares solution.

Similarly, we update the school effects using the first order condition with respect to ψ_s:

$$\psi_s=N_s^{-1}\sum_{i\in\mathcal{N}_s}\left[y_{ig}-X_i'\beta-\alpha_i-\frac{\eta}{M_{gst}}\sum_{j\in\mathcal{M}_{gst\sim i}}\alpha_j\right] \tag{A4}$$

where $\mathcal{N}_s$ is the set of student-grade observations (numbering N_s) at school s in our data.

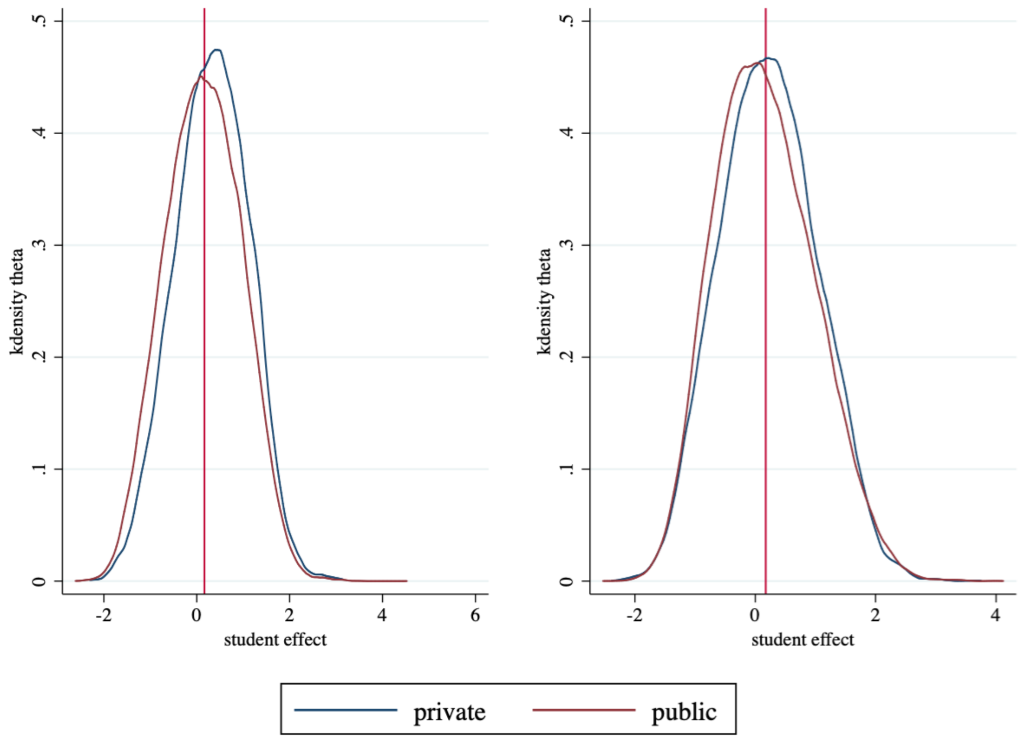
Our specific iterative algorithm is as follows. We intialize the values of β, the α's, and ψ's in the first iteration with estimates from a model without spillovers ($\eta=0$). Then in each subsequent iteration q, we apply the following steps:

1. Conditional on α^{q-1} and ψ^{q-1}, we estimate β^q and η^q by OLS.
2. Conditional on $\alpha^{q-1}, \psi^{q-1}, \beta^q$, and η^q, we update α^q using eq. (A3).
3. Conditional on α^q, β^q and η^q, we update ψ^q using eq. (A4).

We continue iterating until parameters converge and the sum of squared residuals does not change between iterations (change is smaller than 10^{-10}). Because the sum of squared residuals decreases at each step, the estimates eventually converge to the parameter values that minimize the least squares problem in (A1). Under assumptions discussed in that paper, Theorem 1 of Arcidiacono *et al.* (2012) ensures that the nonlinear least squares estimator of η is consistent and asymptotically normal.

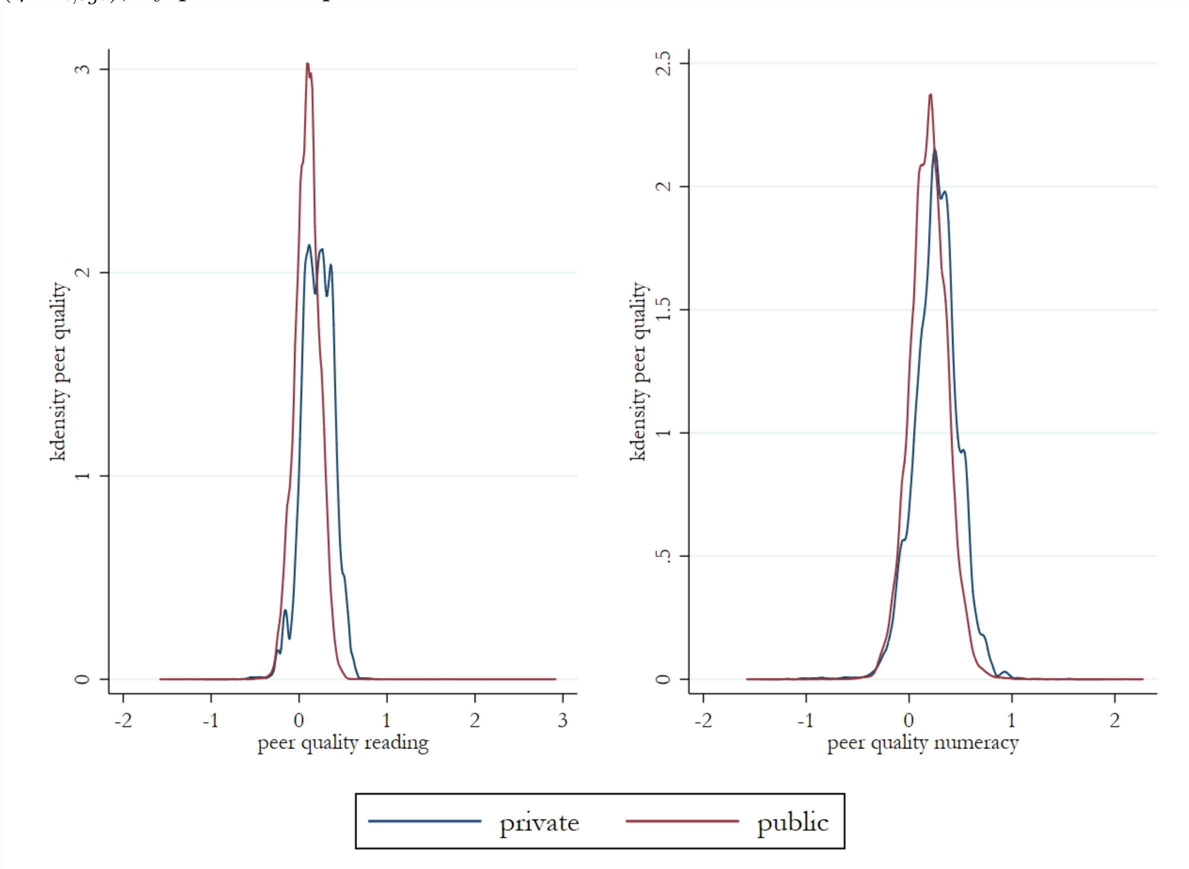
2.9.3 Supplemental Figures

Figure 2.6: Kernel density estimates of distributions of estimated student fixed effects ($\hat{\alpha}_i$), public and private schools



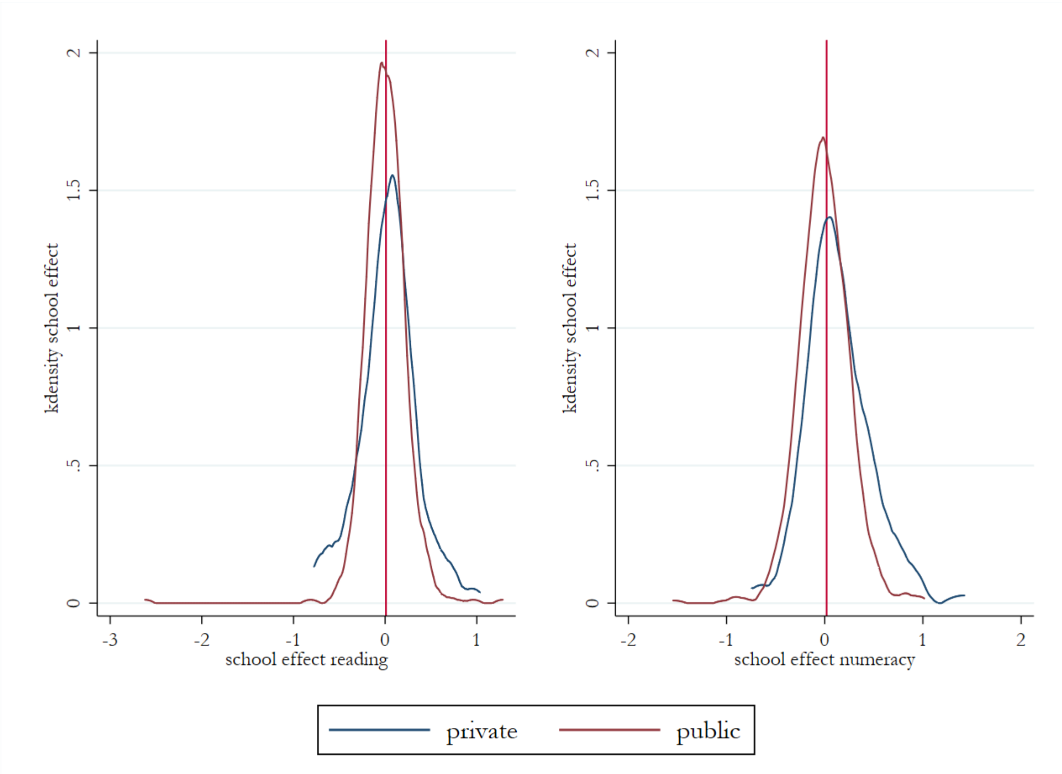
Notes: The figures plot the distribution of estimated student effects, $\hat{\alpha}_i$, in our sample. Estimates are based on our baseline specification 2.1; the sample and specification are the same as our main estimates in Table 2.3. The unit of observation for the figure is a student.

Figure 2.7: Kernel density estimates of the distribution of estimated peer quality effects ($\hat{\eta}\bar{\hat{\alpha}}_{\sim i,sgt}$), by public and private schools



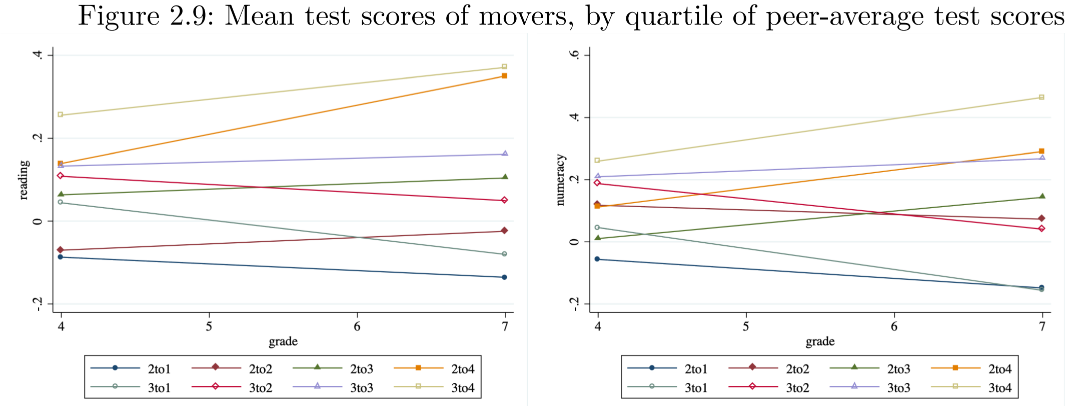
Notes: The figures plot the distribution of estimated peer quality effects, $\hat{\eta}\bar{\hat{\alpha}}_{\sim i,sgt}$, in our sample. Estimates are based on our baseline specification 2.1; the sample and specification are the same as our main estimates in Table 2.3. The unit of observation for the figure is a student.

Figure 2.8: Kernel density estimates of the distribution of estimated school effects ($\hat{\psi}_s$), by public and private schools



Notes: The figures plot the distribution of estimated school effects, $\hat{\psi}_s$, in our sample. Estimates are based on our baseline specification 2.1; the sample and specification are the same as our main estimates in Table 2.3. The unit of observation for the figure is a school.

Figure 2.9: Mean test scores of movers, by quartile of peer-average test scores



Notes: The figure plots the change in test scores of students who change school between grades 4 and 7, by quartile of peer-average test scores at the grade 4 and 7 schools. This figure shows the change in average test scores for students who leave schools in the second or third quartile of peer-average test scores; Figure 2.4 presents a comparable plot for students that leave schools in the first or fourth quartile of peer-average test scores.

2.9.4 Supplemental Tables

Table 2.9: Sample exclusions

Number of student/test score observations	Number excluded	Reason for exclusion
524,678	1,749	excludes observations with missing values for all variables
522,929	555	excludes duplicates or observations without postal code
522,374	2,482	non-match with census geography
519,892	259,095	keeps grade 4 and grade 7 only
260,797	48,967	reading: excludes observations from schools with less than 5 students, with irregular progress, not in Lower Mainland in grade 4 and 7, with missing values for ID, ethnicity, gender, school type and Census characteristics, and test scores.
260,797	50,878	numeracy: excludes observations from schools with less than 5 students, with irregular progress, not in Lower Mainland in grade 4 and 7, with missing values for ID, ethnicity, gender, school type and Census characteristics, and test scores.
211,830	23,104	reading: excludes students observed only in one grade (either 4 or 7).
209,919	24,241	numeracy: excludes students observed only in one grade (either 4 or 7).
188,726	36	reading: excludes observations with zero observed students in the peer group
185,678	42	numeracy: excludes observations with zero observed students in the peer group
188,690	*Final sample reading*	
185,636	*Final sample numeracy*	

Table 2.10: Estimated coefficients in baseline student test score model (1)

	Reading (1)	Numeracy (2)
Mean peer quality (spillover parameter, $\hat{\eta}$)	0.645***	1.145***
	(0.012)	(0.016)
Neighborhood share immigrant	-0.225***	-0.344***
	(0.024)	(0.027)
Neighborhood share with high school diploma	-0.319***	-0.287***
	(0.034)	(0.037)
Neighborhood share with trade certificate	-0.300***	-0.272***
	(0.034)	(0.044)
Neighborhood share with bachelors degree	-0.221***	-0.368***
	(0.029)	(0.034)
Neighborhood mean family income /1000	0.001	-0.005***
	(0.001)	(0.001)
School fixed effects	YES	YES
Student fixed effects	YES	YES
Grade × year effects	YES	YES
R^2	0.844	0.843
N	188,690	185,636

Notes: Reported estimates are based on equation 2.1, for the same sample and specification reported in Table 2.3 of the main text. Peer quality is defined as the average estimated student effect of same-grade peers, $\bar{\alpha}_{\sim i,sgt}$. Estimation sample is the same as Table 2.3 and comprises all public and private school students in the Lower Mainland of B.C. who were enrolled in Grade 4 between 1999/2000 and 2003/2004 and advanced one grade in each of the following three years, at a school that met our enrollment restriction, with non-missing values of all key variables. See Section 3 for additional details about sample construction and composition. Standard errors are estimated via the wild bootstrap and are reported in parentheses, *** indicates statistical significance at the 0.1% level.

Table 2.11: Estimated Quantile Spillover Parameters ($\hat{\eta}^{\tau}$)

	Reading	Numeracy
	(1)	(2)
Spillover parameter ($\hat{\eta}^{\tau}$)		
$\tau = 0.10$	0.627***	1.09***
	(0.010)	(0.021)
$\tau = 0.20$	0.636***	1.10***
	(0.007)	(0.014)
$\tau = 0.30$	0.638***	1.11***
	(0.005)	(0.013)
$\tau = 0.40$	0.639***	1.13***
	(0.005)	(0.012)
$\tau = 0.50$	0.639***	1.14***
	(0.004)	(0.012)
$\tau = 0.60$	0.638***	1.16***
	(0.004)	(0.013)
$\tau = 0.70$	0.648***	1.18***
	(0.005)	(0.015)
$\tau = 0.80$	0.647***	1.18***
	(0.006)	(0.018)
$\tau = 0.90$	0.659***	1.21***
	(0.008)	(0.025)
School fixed effects	YES	YES
Student fixed effects	YES	YES
Grade $\times$ year effects	YES	YES
N	188,690	185,636

Notes: Reported estimates are based on equation 2.2. Covariates include: neighborhood share of immigrants; neighborhood shares of parents with high school, post-secondary certificate, and bachelor's degree; mean neighborhood family income; and grade \times year effects. Estimation sample is the same as Table 2.3; see Section 2.3 for additional details about sample construction and composition. Standard errors are clustered by school and are reported in parentheses, *** indicates statistical significance at the 0.1% level.

Table 2.12: Coefficient estimates in a model of test participation

	(1) Numeracy	(2) Reading
Neighborhood share immigrant	-0.012	-0.014
	(0.033)	(0.026)
Neighborhood share immigrant with high school diploma	-0.074	-0.075
	(0.034)	(0.039)
Neighborhood share immigrant with trade certificate	-0.067*	-0.051
	(0.024)	(0.034)
Neighborhood share immigrant with bachelors degree	-0.030	-0.034
	(0.036)	(0.052)
Neighborhood mean family income /1000	0.001	0.001
	(0.001)	(0.001)
School fixed effects	YES	YES
Student fixed effects	YES	YES
Grade $\times$ year effects	YES	YES
r2	0.666	0.674
N	220661	220661

Notes: Reported estimates are based on equation 2.9, and dependent variable is an indicator that a student has a non-missing value for numeracy and reading scores respectively. Estimation sample comprises all public and private school students in the Lower Mainland of B.C. who were enrolled in Grade 4 between 1999/2000 and 2004/2005 and advanced one grade in each of the following three years, at a school that met our enrollment restriction, with non-missing values of all key variables except test scores. See Section 2.3 for additional details about sample construction and composition. Standard errors are clustered by school and year and reported in parentheses. * indicates statistical significance at the 5% level.

Table 2.13: Estimates from public/private school choice model (used to reassign students under cream-skimming counterfactuals)

	Numeracy	Reading
	(1)	(2)
Neighborhood share immigrant	0.100	0.101
	(0.330)	(0.330)
Neighborhood share with high school diploma	-0.472	-0.489
	(0.268)	(0.270)
Neighborhood share with trade certificate	-1.48***	-1.50***
	(0.392)	(0.393)
Neighborhood share with bachelors degree	0.505	0.487
	(0.430)	(0.431)
Neighborhood mean family income /1000	0.031**	0.030**
	(0.012)	(0.012)
English as a Second Language program	0.318	0.305
	(0.409)	(0.406)
Chinese home language	-0.360**	-0.369**
	(0.140)	(0.140)
Punjabi home language	-0.201	-0.200
	(0.291)	(0.291)
Other home language (not English)	0.046	0.044
	(0.135)	(0.136)
Aboriginal self-identity	-0.921***	-0.922***
	(0.060)	(0.063)
Female	0.011	0.011
	(0.037)	(0.037)
Intercept	-0.897***	-0.872***
	(0.232)	(0.233)
Cohort fixed effects	YES	YES
R^2	0.034	0.034
N	188,690	185,636

Notes: Dependent variable is indicator the student attends a private school in a given grade. We use estimates of this model to reassign students between public and private schools under the counterfactual policy experiments described in Sections 2.5.5 and 2.5.6. Standard errors are clustered by school and reported in parentheses. * indicates statistical significance at the 0.05 level, ** indicates statistical significance at the 0.01 level and *** indicates statistical significance at the 0.001 level.

Chapter 3

Effects of Public School Choice on Private Schools: Evidence from Open Enrollment Reform

3.1 Introduction

School choice remains an important topic of debate in education research and policy. Proponents argue that school choice can improve outcomes of families via two main channels: by allowing families to enroll their children in better schools or schools more suited to their children's needs; and by providing market incentives for schools to produce better outcomes as they compete for students. While numerous empirical and theoretical studies have investigated how competition from private school affects public school outcomes,[1] very little attention has been paid to the ways in which private schools respond to increased public school choice, possibly due to lack of data.

This paper addresses this gap and provides the first evidence of how competition from public school open enrollment affects private school outcomes. Open enrollment is a very common policy that aims to increase choice by allowing students to attend public schools outside their neighborhood attendance zone.[2] I investigate how the increased public school competition arising from the introduction of open enrollment affects the number of students enrolled in private schools, tuition prices and expenditures.

The idea that private school choice incentivizes public schools to produce better educational outcomes relies on the premises that families (i) value school quality when making school choice decisions and (ii) view private and public schools as close substitutes. I test the latter premise by studying how the expansion of public school choice affects private school

[1]See Urquiola (2016b) for a summary of those theoretical and empirical results.

[2]In 2019, all but three states in the US have policies addressing intra or interdistrict open enrollment policy (Wixom, 2017). Among many jurisdictions, Chile in 1981, Sweden in 1992, England in 1998, and British Columbia, Canada in 2002 have also adopted some form of open enrollment policy.

enrollment. I answer whether private schools show active competitive behavior, namely whether schools adjust prices and spending in response to a negative demand shock. The private school responses also help to interpret the effects on enrollment. For example, with an upward-sloping supply curve, enrollment reduction is a result of a demand shock and a price decrease.

If increased public school choice results in reduction in private school enrollment, then the goal of open enrollment of making education provision more equitable can be undermined. Private school students may migrate from private schools to take available spots in high-quality public schools that would otherwise go to disadvantaged students residing in an out-of-catchment neighborhood. Furthermore, as the private sector often accounts for a growing share of total enrollment,[3] policies that affect private school can impact the school system overall. For example, as funding for public schools is tied to student enrollment, changes in private enrollment can affect government budgets to fund students.

Increased public school choice may not raise competitive pressure on private schools, for few reasons. First, families might view public schools as imperfect substitutes to private schools given their differentiated curricula.[4] That is, private schools might differentiate themselves along dimensions of faith, academic focus, or an alternative learning environment. Second, if families have preference over peers, public school students who experience or anticipate a reduction in peer or school quality under open enrollment might decide to attend a private school instead. [5] Third, the emotional cost of switching schools for the child might be too high.

I frame my analysis using a simple stylized model of school choice in an environment where spatially differentiated private schools choose price and quality to maximize profits. I assume families have preferences over school characteristics, such as proximity, quality of instruction, and price. Under this model, private schools that charge higher prices and are located in areas with more high-quality proximate public schools experience stronger

[3]Private schools account for 13 percent in British Columbia, 47 percent in Chile, 12 percent in Denmark, 70 percent in Holland, 15 percent in New Zealand, 10 percent in Sweden of K-12 enrollment (Friesen *et al.* , 2019b). In the US, 32 percent of parents reported considering both private and public schools, according to the 2007 National Household Education Survey. World Bank (2011) highlights the growing importance of nonstate provision of education. The share of private sector enrollment for primary and secondary school is highest in South Asia (around 30 and 50 percent, respectively) and in Latin America and the Caribbean (around 15 and 18 percent, respectively).

[4]MacLeod & Urquiola (2013) discuss how schools strategically offer differentiated products to exploit the fact that families do not have unanimous preferences. Gilraine *et al.* (2019) provide evidence that charter school entry increases test scores of exposed students, but when the charter schools are horizontally differentiated (e.g: by choosing an alternative curriculum to the one used in traditional public schools), then there is no effect on student achievement. This result suggests that competitive effects may be muted if product differentiation makes schools less substitutable.

[5]In a theoretical model of public school choice, Barseghyan *et al.* (2019) show that if peer preference is strong, open enrollment can reduce the quality of public schools in affluent neighborhoods when the benefits of increased school effort are offset by the cost of inferior peer groups.

negative demand shocks when open enrollment is introduced. To maximize profits, private schools choose tuition price in response to their local market power, which in turn depends on their market share and its sensitivity to changes in price. The choice of quality also depends on market power, but it can be constrained by government funding rules. If higher competition from public schools decreases demand for private schools, the model predicts that private schools will reduce tuition and provide higher school quality.

I empirically evaluate the impact of open enrollment policy on private school outcomes using a natural experiment created by the introduction of an open enrollment policy that relaxed restrictions on enrollment in public schools out of the neighborhood attendance zone. My identification strategy relies on the variation in the intensity of exposure to local public school competition among private schools before and after the policy. For private schools located in areas where public schools are very distant from one another, students are less likely to opt out of private schools following the introduction of open enrollment because open enrollment creates fewer new local alternatives and travel is costly. In contrast, private schools located in areas where public schools are spatially dense experience a greater increase in competition under open enrollment. In those areas, I hypothesize that students who would otherwise attend private school are more likely to attend an out of catchment public school under the new policy.

Using a new data set constructed from nonprofit information returns and administrative school data for the population of private schools in British Columbia (BC), Canada, I compare pre- and post-treatment enrollment and school budget outcomes for K-12 private schools that were differentially exposed to increased public school competition, depending on the concentration of nearby public schools. My empirical specification differences out any unobserved time-invariant factors at the school and grade level that influence outcomes and are correlated with the spatial concentration of public schools. I investigate how the estimated impacts are mediated by the underlying school market structure. In particular, I assess heterogeneity of the effects across school characteristics, including curriculum/school type, funding level, tuition price,[6] and expenditure per student. This paper is the first to use nonprofit information returns in Canada to study private school budgets.[7]

I find that increased public school choice reduces private school annual enrollment by 0.9 student per grade for a private school with the median number of nearby public schools. This effect correspond to about 2.6 percent of the average number of students per grade in private school. The initial effect is small but grows in magnitude over time. After five years, the effect on a private school facing median competition level is -1.4 students per grade. Effects are concentrated in Catholic schools. In contrast, other Christian and other

[6] I use revenue per student net of government grants and donations as a proxy for tuition price.

[7] Hungerman & Rinz (2016) use data of similar nature to investigate the effect of large-scale subsidies on private school enrollment and revenue in the US.

faith schools experience no reduction in enrollment under increased public school choice. This result indicates that public schools are better substitutes for Catholic schools than for other faith schools, potentially due to similarity in curricula. This result echoes the finding in Gilraine *et al.* (2019) that demand for horizontally differentiated charter schools (by different curriculum choice) is unresponsive to public school quality. The enrollment effects I find are also concentrated in private schools that offer secondary education. Since students have to change schools when they start high school, I conjecture that greater competition from public schools was more salient to students in these grades. These effects are robust to several checks including various measures of competition and model specifications.

On the supply side, I find no empirical evidence that private schools respond to increased competition by adjusting per student revenue or expenditure. The competition created from open enrollment causes a small negative shock to private school demand, but it is not strong enough to affect private school spending and pricing choices.

My enrollment results are related to the literature on the effects of charter school entry on private school enrollment in Michigan. Using school fixed effects and instrumental variables to address endogenous location of new charter schools in their estimation, (Chakrabarti & Roy, 2016) find no effect of charter school entry on enrollment in either secular or faith private schools. (Toma *et al.* , 2006) find that private schools lose around one student for every three students gained by charter schools, but their analysis was limited to county-level data for a narrower period and does not account for pre-policy trends or endogenous charter location. Unlike these two papers, the environment I study eliminates concerns about treatment endogeneity arising from school location. Also, I include in my analysis private schools that offer kindergarten to grade 12, in contrast with the papers that assess the impact of charter school entry on private school enrollment exclusively on elementary grades. Furthermore, I go beyond enrollment outcomes and investigate whether private schools respond by adjusting expenditures and tuition prices. Consequently, I can rule out the hypothesis that enrollment changes (or lack thereof) were a result of price changes and learn whether private schools show active competitive behavior.

This paper also contributes to the literature that investigates how private schools and their students respond to large scale government programs. Dinerstein *et al.* (2015) find that increasing subsidies for New York public schools increased the likelihood of private school closure. Hsieh & Urquiola (2006) study the effect of private school entry in response to the introduction of Chile's voucher system on test scores, repetition rates, and years of schooling of students. They find no effect of choice on educational outcomes, but they observe an increase in sorting due to cream-skimming. Estevan (2015) show that increase in public school expenditure in Brazil leads to a decrease in private school enrollment in grade 1. Menezes-Filho *et al.* (2014) investigate the effect of *Bolsa-Familia* cash transfer program in Brazil on private school entry. Their results show that towns where the skill distribution of students widened due to the expansion of *Bolsa-Familia* program saw higher rates of private school entry.

A third related literature to my paper studies the effects of public school open enrollment on student achievement. Friesen *et al.* (2019a) assess the effect of the same open enrollment reform that I study here on student achievement. Unlike this paper, they use student-level data restricted to BC Lower Mainland students in grade 4 and 7 and focus on public school students. Their evidence that many more parents enrolled their children in out-of-catchment public schools following the reform shows that the policy had a positive impact on increasing public school choice. However, they do not investigate whether out-of-catchment students came exclusively from in-catchment public schools or whether they had previously been enrolled in private schools. Exploiting variation in the intensity of competition from nearby schools, they find small positive effects of open enrollment on student achievement. Lavy (2010) evaluates a public school choice reform implemented in Tel Aviv, Israel. He finds that the reform significantly improved student attainment as reflected in reduced drop-out rates, higher test scores, and better behavioral outcomes. For the UK, Gibbons *et al.* (2008) exploit geographical variation in choice and use instrumental variable strategy to account for the potential endogeneity of residential sorting. They find little evidence that choice affects student achievement but found a positive effect of competition on school performance. Also in the UK, Bradley & Taylor (2002) find stronger productivity gains using a difference-in-differences approach.

The remainder of the chapter is organized as follows. Section 3.2 reports the institutional context of the school system in BC and the details of the open enrollment policy. Section 3.3 sets up a simple theoretical framework. Section 3.4 describes the data and section 3.5 presents the empirical strategy. In section 3.6, I present the results. I conclude in section 3.7.

3.2 Institutional Context and Open Enrollment

I study the introduction of open enrollment legislation in BC during the 2002/2003 school year. Before 2002, public school choice was fairly restricted in BC and the prevailing assumption was that a student would attend his or her neighborhood school. Transfers across school catchments required approvals from the sending and receiving school principals.[8] Since 2002, parents were granted the right to enroll their children in any public school in the province that has space available after students who reside in the catchment area have

[8]Given that the school funding moves with the student, school principals had low incentives to approve sending students to other public schools. Conversely, principals in receiving schools could also have low incentives to accept students from other public schools for fear of them not being good enough for their sending schools.

enrolled.[9] When public schools are over-subscribed, school boards give priority to students who reside within the district. Boards may elect to give priority to siblings of children who are already enrolled. Within these enrollment criteria, public school principals have discretion over which students to enroll.

In addition to regular public schools, parents have the choice to send their children to independent (non-public) schools that charge tuition (private schools henceforth)[10], public magnet programs, or they may opt for home schooling. French Immersion is the most popular public magnet program, accounting for 9.3 percent[11] of total enrollment in the public sector in 2017/2018 school year. Spaces in French Immersion programs are often allocated by lottery.

Operating and capital funding from the BC Ministry of Education goes directly to public school districts. Districts receive operating funds in proportion to enrollment, with supplementary funding for Aboriginal students, gifted students, students with disabilities and students who qualify for English as a Second Language (ESL) instruction. Public districts and schools are not permitted to raise any additional revenue and are required to teach the provincial curriculum. Hiring, firing and remuneration of teachers is determined by a collective agreement between the government and the BC teachers' union. Teachers are usually hired on permanent contracts and are difficult to fire.

Since 1977, private schools that conform to provincial curriculum standards and meet various provincial administrative requirements receive subsidies. BC provides 50 percent of the per student public school grant to private schools whose operating costs are no higher than in the public system (funding group 1), and 35 percent to those whose operating costs are higher (funding group 2). The BC Ministry of Education does not limit the total subsidy to each school.

To be eligible for the subsidies, private schools must operate on a not-for-profit basis, offer the provincial curriculum, hire qualified BC teachers and participate in standardized testing programs. Unlike public schools, private schools may provide a faith-based learning environment and offer religious instruction. They may charge any amount of tuition and they have discretion to admit students as long as it does not violate the Canadian Charter of Rights and Freedoms or the provincial Human Rights Code. Private schools have autonomy to hire, fire and remunerate teachers subject only to provincial labor standards legislation. Private faith schools in BC serve a variety of religious communities, including Catholic,

[9]In Canada, while all provinces permit interdistrict transfers, only three have legislation of intradistrict transfers, and BC is the only province that does not requires educators to approve the transfers of students (Brown, 2004). Wixom (2017) reports that, in 2016, nearly all states in US (46 states plus District of Columbia) have policies addressing open enrollment.

[10]"Independent school" in US usually means a private school that are not part of chain. In contrast, the independent schools in BC may or may not be part of chain.

[11]Based author's calculations from administrative data from the BC Ministry of Education.

Protestant, Sikh, Jewish and Muslim. Secular schools include "prep schools" that are focused on academic excellence and university preparation. A smaller group of private schools offer Montessori or Waldorf programs, or specialized education for students with special learning needs. Tuition fees range widely, from several thousand dollars per year at some faith schools to $25,000 or more at top-ranked prep schools. Private schools are also supported through donations (gifts) from individuals and from supporting foundations and organizations.

All public and provincially funded private schools in BC are required to administer standardized tests to students in grades 4 and 7 in reading and numeracy each year. A centralized grading system ensures that a consistent standard is applied across schools. These tests are low stakes as their 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. The Fraser Institute, an independent research and educational organization, began issuing annual "report cards" on BC's elementary schools in June 2003 (Cowley *et al.* , 2003). These reports include school scores and rankings based on test scores. From the outset, the school report cards have received widespread media coverage in the province's print, radio and television media.

BC is an ideal setting to study the competitive pressure of the public school system on private schools because there is a narrower quality gap[12] than in other jurisdictions and the cost of private school education is more accessible to families compared to school markets where private schools are not subsidized and are for profit. Compared to other countries, BC is ranked among the top 10 in reading and top 20 in mathematics in the PISA 2018.[13]

3.3 Theoretical Framework

The simplified theoretical framework that follows characterizes the ways families trade off different school characteristics when making their choices. Spatially differentiated private schools choose quality and prices to maximize profits.[14] The objective of this model is to provide intuition for the empirical analysis that estimates the impact of increased public school choice on private school outcomes. After open enrollment, families might find that the schools added to their choice set yield larger utility than their current choice depending on their characteristics, such as distance, quality and price. Private schools experience increased

[12] Friesen *et al.* (2019b) document a large overlap in the distributions of school quality, peer quality and student ability of private and public schools.

[13] See for example O'Grady *et al.* (2019).

[14] The setup of my model parallels Card *et al.* (2010) and Friesen *et al.* (2019a) on the demand side, and Neilson (2017) on the supply side.

competition from public schools by adjusting prices and/or investments in quality in order to maximize profits and adapt to the new environment.

3.3.1 Demand side

I develop a simple model of school choice, conditional on residential choice. The family of student i living in neighborhood k chooses a school j from a set of C_k schools and obtains utility:

$$U_{ij} = \beta q_j + \lambda p_j + \gamma d_{jk} + \epsilon_{ij}$$

where q_j is the quality of school $j \in C_k$, p_j is the tuition price of j, d_{jk} is the travel distance to school j for residents of neighborhood k. The parameters $\beta > 0$, $\lambda < 0$ and $\gamma < 0$ represent taste for school quality, tuition price and travel distance, respectively. Lastly, ϵ_{ij} is an independent and identically distributed random shock.

After evaluating utility from schools in the choice set, families choose school j when $U_{ij} \geq U_{ir}$ $\forall\, r = 1, 2, ..., n_k$. That is,

$$\beta \Delta q_{jr} + \lambda \Delta p_{jr} + \gamma \Delta d_{jr,k} \geq \epsilon_{ir} - \epsilon_{ij}$$

$$\forall\, r = 1, 2, ..., n_k$$

where $\Delta q_{jr} = q_j - q_r$, $\Delta p_{jr} = p_j - p_r$, and $\Delta d_{jr,k} = d_{jk} - d_{rk}$.

Consider now the families that maximize their utility by choosing a private school. Denote this private school $j \in C_k$ that maximizes the utility of family i as j^*. Open enrollment expands families' choice sets to include additional public schools. Let the new choice set be C'_k. The probability that family i prefers a public school $r \neq j^*$ added to the choice set is:

$$1 - F\left[\beta \Delta q_{j^*r} + \lambda p_{j^*} + \gamma \Delta d_{j^*r,k}\right] \tag{3.1}$$

where F is the distribution function of the random variable $\epsilon_{ir} - \epsilon_{ij^*}$, normalised to have mean zero. p is zero for public schools added to the choice set, so only p_{j^*} is in eq. 3.1.

The partial derivatives of eq. 3.1 with respect to school quality q_r, price of private school p_{j^*}, and travel distance to school d_{rk} are:

$\frac{\partial(1-F)}{\partial q_r} = \beta f(\beta \Delta q_{j^*r} + \lambda p_{j^*} + \gamma \Delta d_{j^*r,k}) > 0$ (since $\beta > 0$)

$\frac{\partial(1-F)}{\partial p_{j^*}} = -\lambda f(\beta \Delta q_{j^*r} + \lambda p_{j^*} + \gamma \Delta d_{j^*r,k}) > 0$ (since $\lambda < 0$)

$\frac{\partial(1-F)}{\partial d_{rk}} = \gamma f(\beta \Delta q_{j^*r} + \lambda p_{j^*} + \gamma \Delta d_{j^*r,k}) < 0$ (since $\gamma < 0$)

where f is the density function of $\epsilon_{ir} - \epsilon_{ij^*}$. All else equal, the probability that a family will choose a school different than j^* is *increasing* in the quality of the new public schools available relative to private school j^*, in the price of j^*, and in the number of schools added to the choice set. It is *decreasing* in the travel distance to the additional schools relative to school j^*.

Assuming the area is partitioned in neighborhoods $k = 1, 2, ..., K$, and that all homes in neighborhood k have the same relative distance to each school, the market share of school j is given by:

$$s_j(Q, P, D) = \sum_{k}^{K} \sum_{i \in A_k} \prod_{r \neq j} F\left[\beta \Delta q_{jr} + \lambda p_j + \gamma \Delta d_{jr,k}\right] \tag{3.2}$$

where A_k is the set of students that live in neighborhood k and Q, P, D are vectors of relative qualities, prices, and relative distances of all schools in the market. In words, the market share of school j is the product of the probabilities of school j being preferred to each other school for each student, summed over all students in all neighborhoods.

3.3.2 Supply side

Spatially differentiated private schools maximize profits[15] by choosing quality and price in a market with N students:

$$\underset{p_j, q_j}{max}\ \pi_j(P, Q) = N s_j(P, Q)(p_j - MC(q_j)) - FC_j \tag{3.3}$$

where j indexes schools, s is market share, p is the tuition price, MC is the marginal cost, q is school quality and FC is the fixed cost. The marginal cost of an extra student is assumed to be constant and an increasing function of school quality delivered to students. Market share s_j is determined by demand for school j, which is a function of vectors of prices, relative qualities and relative distances of the schools in the local market (P, Q, and D).

The first order condition with respect to price is:

$$\frac{\partial \pi_j(P, Q)}{\partial p_j} = N \frac{\partial s_j(P, Q)}{\partial p_j}(p_j - MC(q_j)) + N s_j(P, Q) = 0$$

$$p_j^* = MC(q_j) + s_j(P, Q)\left[-\frac{\partial s_j(P, Q)}{\partial p_j}\right]^{-1} \tag{3.4}$$

The first term in the right-hand side corresponds to the competitive price for school j ($= MC(q_j)$) and the second term is a price mark-up.

The first order condition with respect to quality is:

[15] Unlike private businesses, nonprofit schools cannot distribute profits to its shareholders, but they can use the surplus (difference between revenue and costs) to improve working conditions or pursue social goals valued by staff. Even though the incentives of nonprofit schools to maximize surplus are not as strong as they would be had they been able to distribute cash, they can be modeled similarly, as in Hoxby (2003)

$$\frac{\partial \pi_j(P,Q)}{\partial q_j} = N\frac{\partial s_j(P,Q)}{\partial q_j}(p_j - MC(q_j)) - Ns_j(P,Q)\frac{\partial MC(q_j)}{\partial q_j} = 0$$

Assume $MC(q_j) = c_0 + c_1 q_j$ [16], then:

$$q_j^* = \frac{p_j - c_0}{c_1} - s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial q_j}\right]^{-1} \tag{3.5}$$

The first term in the right-hand side corresponds to the competitive quality for school j $(= \frac{p_j - c_0}{c_1})$[17] and the second term is a quality mark-down, which also measures market power.

According to this model, private schools respond to a negative demand shock that reduces their market share by adjusting revenue and quality. If market shares respond negatively to price increase and positively to quality increase, a new equilibrium would consist of higher private school quality and lower tuition serving a smaller number of students. The degree to which the optimal prices and qualities will be affected by open enrollment depends on the sensitivity of demand to price and quality changes, the size of the market share, and how much this market share is reduced if at all. From the demand side, the market share of school j, in turn, will decrease the closer in distance the additional public schools available are to the families, the higher quality they are relative to j, and the higher the price of j.

The impacts of the policy on enrollment depends on whether private and public schools are operating at full capacity. For a private school not operating in full capacity, the demand shock facing each private school will depend on the extent to which the surrounding public schools face binding capacity constraints. That is, if most public schools around a private school are operating at full capacity with students of their corresponding catchment area before the introduction of open enrollment, then there should be a negligible effect of the policy on enrollment for that private school. Conversely, if surrounding schools are not operating at full capacity, a private school with binding capacity constraint and a wait list of students before the policy would not experience any effect on its enrollment, but it will likely experience a change in the quality of students from which it can select.

Families residing in the more densely populated areas are more likely to experience choice as there are more private and public schools around them. If capacity constraints in public and private school are more likely to bind in those areas, the enrollment response to open enrollment reform will be limited. Thus, it is not clear that the theoretical predictions derived here will materialize in practice.

[16] I also assume c_0 and $c_1 > 0$ are constants.

[17] With perfect competition $p_j^* = MC(q_j)$. By plugging in marginal cost, we get $q_j^* = \frac{p_j - c_0}{c_1}$.

Extension of the supply side model

Private schools in BC receive either 50 percent or 35 percent of the per student public school grant if operating costs are lower or higher than in the public system, respectively. As a result, for the low-cost private schools in funding group 1 (those receiving the higher grant per student), their ability to increase quality (via spending) is constrained by the rule that their operating cost is below the public school one. I incorporate this institutional feature in the model and the profit maximizing problem becomes:

$$\begin{aligned} \underset{p_j,q_j}{max}\ \pi_j(P,Q) =& N s_j(P,Q)(p_j + 0.5G^{pub}\mathbb{1}\{MC(q_j) \leq G^{pub}\} \\ &+ 0.35G^{pub}\mathbb{1}\{MC(q_j) > G^{pub}\} - MC(q_j)) - FC_j \end{aligned}$$

where G^{pub} is the per student public school grant. The condition to receive the higher grant ($0.5G^{pub}$) is that the marginal cost per student is less than or equal to the per student public school grant, otherwise the school receives the lower grant ($0.35G^{pub}$).[18]

For low-cost private schools facing high competition or serving demand for higher school quality, the $MC(q_j) = G^{pub}$ and thus quality will be fixed ($q_j = MC^{-1}(0.5G^{pub})$).[19] In this case, quality q_j would not be a choice variable anymore and the optimal price is:

$$p_j^* = 0.5G^{pub} + s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial p_j}\right]^{-1} \tag{3.6}$$

Low-cost private schools might not be able to compete by adjusting quality while remaining eligible for the 50 percent government grant. Higher public school competition would then have no effect on quality of those schools, only reduction of the price markup.

For the purposes of assessing the comparative statics in the case of a negative demand shock, the interpretations of the optimal price and quality for low-cost private schools (with non-binding grant constraint) and high-cost private schools remain similar to the ones from the model without grants, expressed in eq. 3.3.[20]

[18] Since marginal cost is constant for each extra student, this constraint implicitly assumes that the per student grant public schools receive corresponds to the per student operating cost in public schools.

[19] While I do not observe operating costs per students, I observe a clear mode of total expenditure per student for schools in funding group 1 category. This suggests that schools are bunching just below the operating cost limit and thus the modeled constraint seems to bind for a large share of low-cost schools.

[20] For low-cost private schools, with non-binding grant constraint, optimal price and quality are: $p_j^* = MC(q_j) - 0.5G^{pub} + s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial p_j}\right]^{-1}$ and $q_j^* = \frac{p_j - c_0 + 0.5G^{pub}}{c_1} - s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial q_j}\right]^{-1}$. For high-cost schools, $p_j^* = MC(q_j) - 0.35G^{pub} + s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial p_j}\right]^{-1}$ and $q_j^* = \frac{p_j - c_0 + 0.35G^{pub}}{c_1} - s_j(P,Q)\left[-\frac{\partial s_j(P,Q)}{\partial q_j}\right]^{-1}$.

3.4 Data

The data used in this study come from several sources. The primary data used are enrollment records, private school's nonprofit information returns, and neighborhood level Census data. The enrollment data is publicly available from the BC Ministry of Education. The financial information returns of private schools' nonprofit organizations come from Canada Revenue Agency (CRA).

The enrollment data contains school-grade level records for the population of students of private and public schools they are enrolled on September 30 of each year. It also contains information on the share of enrolled student who are female, who are Aboriginal, and whether the school offers full-day kindergarten. I used the school postal codes to link mean Census neighborhood (enumeration or dissemination area, depending on year) characteristics to schools in each year.[21]

The nonprofit information returns data contain the list of all nonprofit registered charities[22] in the province each year and their reported expenses and revenues, including government grants and donations. Private schools in BC need to operate as nonprofits in order to be eligible to receive the government grants. Organizations with registered charity status can issue tax receipts for donations (unlike other nonprofit organizations), need to meet various regulatory requirements defined by the CRA, and must submit an annual information return (T3010) to CRA that reports its activities, revenues, and expenditures. In addition to revenues and expenses broken into subcategories, the data also include the date of registration of each organization. I link the nonprofit organizations' legal name to private schools using the authority name reported in the school database. In some cases, this is not a one-to-one mapping as one charity is associated with multiple different schools.[23]

3.4.1 Stylized Facts

Before describing the sample I use for the empirical exercise, I highlight some stylized facts to contextualize the analysis.

Private school share of total enrollment has been increasing since 1999. Figure 3.1 shows that the share of enrollment has been declining for public schools and increasing for private schools, such that private school enrollment went from 9 percent in 1999 to 12 percent in 2017. Figure 3.2 shows that enrollment in special programs also increased quickly, particularly for French Immersion (early and late) in the public sector and English Language

[21]The data supplement includes a detailed description of my procedures to link Census neighborhood characteristics via postal code.

[22]This data set does not include other nonprofits without registered charity status.

[23]Fifty nine percent of schools with financial information are each linked to a unique nonprofit. Each school of the remaining 40 percent of sample is linked to a nonprofit, together with one or more schools.

Learning in the private sector. Altogether, these figures indicate the growing interest in greater school choice among families in BC.

In the figures described above, there is no clear structural break in enrollment trends after the implementation of open enrollment policy. That is not surprising as out-of-catchment enrollment increased gradually over the years that followed the fall of 2002[24] and each private school was affected differently depending on the level of public school competition they faced, their student capacity constraints and the demand for private schools in each location. In contrast, there is clear increase in private school share of total enrollment in the year after the public teachers' strike that occurred from April to September 2014. In this case, all public schools were affected similarly as teachers were on strike together until after the scheduled start of classes in the fall. The change in enrollment shares after the public teachers' strike suggests that private schools can be substitutes for public school for some students if the incentives are strong enough.

Families are more likely to choose a different school when a cycle is about to start as it is less disruptive to the students, and easier to find available spots. At those moments, students often have to move to a different school anyways, because their school does not offer later grades. Figure 3.4 shows the number of students who moved ("movers") from private to public school and vice versa over the years. The number of movers in both direction is particularly high in grade 8, at the start of high school.[25]

Regarding the finances of private schools, it is important to distinguish schools in funding group 1 and in funding group 2[26] as they face very different incentives. Figure 3.5 shows that schools in funding group 1 have lower revenue and expenditure, but higher provincial grants and gifts than schools in funding group 2. This figure illustrates that the lower levels of expenditure and other revenue of schools in funding group 1, in comparison to schools in funding group 2, are related to the funding rules that determine the value of the provincial grant. As the operating cost per student is capped by the operating cost per student in public schools and those schools are required to operate at a nonprofit basis, the expenditure per student and other revenue per student (from tuition) are constrained from reaching higher levels for schools in funding group 1. Most of the differences between government grants received across the school categories can be explained by the variation in value of the provincial grant according to the school district, but also by differences in specific grants from federal or municipal governments that the registered charities can

[24]This is documented in Friesen *et al.* (2019a).

[25]Figure 3.4 uses confidential administrative data from (BC Ministry of Education, 2019), but for the rest of the analysis I use similar public use enrollemernt data. While my plan initially was to conduct the analysis using only the administrative data, only a small share of the data requested was provided by the data steward in time for the completion of this paper.

[26]Funding group 1 private schools receive 50 percent of the per student public school grant and funding group 2 private schools 35 percent.

receive. In the fall of 2005, the supplementary funding for special education students in private schools changed from half the amount per student in special education in public schools to the full amount. The number of special education students after 2005 may help to explain the divergence of other secular and Waldorf/Montessori schools after the change in the funding formula.

3.4.2 Sample

I combine the data sets with enrollment, neighborhood characteristics, and school financial information to create a panel of school-grades for the years from 2000 to 2007. I restrict my sample to schools that offer any grade between Kindergarten to grade 12, that were operating in 2001, and have non-missing values for all relevant variables in the analysis. The sample does not include alternative schools, distance education schools, and private schools in the funding group 4.[27] The sample includes 147 private schools, of which 92 were matched to nonprofit returns data.[28] Not all schools could be matched with the financial data for two reasons. First, several schools had missing information for the name of their corresponding nonprofit organization[29] in the data from the Ministry of Education of BC. Second, most of the unmatched schools had information on the name of their nonprofit organization, but those organizations were not in the list of charities that reported information to the CRA.[30] Table 3.1 reports descriptive statistics for schools in the sample. An average private school has about 35 students per grade, has 12 public schools and 4 private schools within 5 km of travel distance. Forty two percent of schools offer at least one grade in the range kindergarten to grade 3, 38 percent in the range from grade 4 to 7, and 20 percent in the range from grade 8 to grade 12. Only 13 percent of private schools are in funding group 2, all of which are either Waldorf/Montessori or other secular. The remaining schools are in

[27] Most students in funding group 4 schools are not eligible to public schools. Group 4 schools mainly provide services to Canadian students from outside British Columbia, or to international students.

[28] Supplemental Table 3.9 show descriptive statistics of the sample of schools that are matched vs. unmatched with CRA data. On average, in comparison with matched schools, unmatched schools: have smaller enrollment per grade; are located in areas with lower concentration of public and private schools; have a lower share of funding group 2 schools; and are in neighbourhoods with less educated population with lower average family income.

[29] Specifically, they had missing information in the "authority name" variable. The nonprofit organizations that fund each private school are labeled in the data from the Ministry of Education of BC as school authorities.

[30] Nonprofits that do not have charity status are not required to file information returns with the CRA and therefore are not in the CRA data set. While all registered charity organizations are required to be nonprofits in order to issue tax receipts for donations, not all nonprofits need to have the registered charity status. Thus, it is likely that the nonprofits assigned to schools in the BC Ministry of Education data, that are not matched to a charity in the CRA dataset, are nonprofit organizations without the status of registered charity.

funding group 1 with representation in all school categories.[31] Other secular private schools are located in areas with more educated population, higher average income per family, higher density of dwellings and population. In contrast, other Christian schools are located in neighborhoods with lower education levels, lower income per family and lower share of immigrants. The schools in the other categories seem to fall between these two groups of schools in terms of the neighborhood characteristics.

Also for the period from 2000 to 2007, I create a panel of schools that could be matched with nonprofit financial data. I use enrollment per school-year as a denominator to create financial variables per student so that budget variables of small and large schools could be compared. Since there are some nonprofit organizations that fund multiple private schools in the sample[32] and their financial reports do not specify how much each of their schools generates of revenue or spends, I aggregate the enrollment of all schools funded by an organization so that I can create per student variables. The number of questions and the granularity of the information required in the form T3010 (which charities are required to submit to CRA) changed over the years of the sample. Consequently, the choice of financial variables used was constrained to variables that were consistently reported in the years of the sample. Table 3.1 shows in the lower rows the mean budget outcomes at the school-year level. Average revenue and expenditure per student are C$9,406 and C$9,272, respectively, for the private schools in the sample. The lowest average values are in Catholic schools, below C$5,000. Not surprisingly, schools categorized as Waldorf or Montessori and other secular have the highest spending and revenue per students as a substantial share of those schools are in the funding group 2, which have operating costs per student above those of public school.[33] Some of the Montessori schools offer early education in addition to Kindergarten and elementary school grades. Since I do not have information on enrollment in early education programs, the average budget variables per student are overestimated for Montessori schools, which also helps to explain their disproportionately high mean revenues and expenditures. Gifts/donations account for a larger share of total revenue in religious non-Catholic schools which likely come from their respective congregation members, in addition to gifts from students' parents. I use other revenue per student (revenue net of gifts and grants) as a proxy for tuition price that parents pay. Except from the artificially

[31] Around 3 percent of the schools are either in other funding groups that do not receive funding or have missing information on the funding group they belong.

[32] As reported in Table 3.1, 53 nonprofit organizations fund 92 schools in the sample. The most extreme case of one charity funding many schools in the sample is the case of the registered charity Catholic Independent Schools of Vancouver Archdiocese, which funds all the 44 catholic schools in Vancouver and their financial returns report only the aggregate figures of all schools each year.

[33] While private schools have to operate at nonprofit basis to be eligible to the provincial grants, the average revenue does not equal the average expenditure per student. If schools have positive surplus (revenue minus expenditure) in a given year, it can be saved as retained earnings to be used in the future for capital expenditure or for other specific purposes.

inflated mean in Montessori/Waldorf schools, other revenue varies from close to C$11,000 in other secular schools to C$1,732 in Catholic schools. While this measure does not exactly match the tuition listed for each school for the period of analysis, its variation reveals how per student revenue in private schools change due to changes in tuition prices and in the provision of discounts to families.

3.5 Empirical strategy

3.5.1 Empirical model

Students who live in areas that are served by a larger number of proximate public schools experience a greater increase in meaningful school choice options under open enrollment than those who live in sparsely populated areas where public schools are widely dispersed. Consequently, open enrollment also leads to a greater increase in competition between schools in areas where schools are more spatially dense. My identification strategy exploits this variation in the intensity of treatment under open enrollment to identify the effects of interest. I build up from the insights of the demand side model to create measures of local competition. This empirical approach follows insight from the literature that quantifies school competition using geographically based school competition indicators to explore implicit variation in the level of choice available to families in different markets.[34]

My empirical strategy follows a difference-in-differences design that leverages the natural experiment created by some private schools being more exposed to competition from public schools than others. I compare changes in outcomes before and after open enrollment in private schools that faced different levels of competition from local public schools. Under reasonable identifying assumptions, differential changes in outcomes are attributed to differential exposure to open enrollment. I consider several outcomes: per grade enrollment, per student log revenue, and per student log spending.

Private schools are spatially spread across the province with different levels of public school competition. Figure 3.6 shows that schools offering K-12 education have higher concentration in the more densely populated areas of Metro Vancouver and Greater Victoria. For example, Figure 3.7 illustrates that within Metro Vancouver, there is significant spread of private schools with varying numbers of nearby public schools.

Let Y_{jtg} denote enrollment level of private school j, in year t, and in grade g using the model below:

$$Y_{jtg} = X'_{jt}\beta_X + \eta\, Comp_{jg} + \theta\, OE_t * Comp_{jg} + \tau_t + \gamma_g + \delta_j + \epsilon_{jtg} \tag{3.7}$$

[34] See for example Gibbons *et al.* (2008), Friesen *et al.* (2019a), Hoxby (2000), Rothstein (2007)

where X_{jt} is a vector of school, grade and Census neighborhood characteristics. The variable $Comp_{jg}$ is an indicator of exposure to local competition and OE_t is a dummy variable that equals one for the years when open enrollment reform was effective, 2003 onward. The vector β_X and the scalars η and θ are parameters to be estimated. The parameters τ_t, γ_g, and δ_j are year, grade and school fixed effects, and ϵ is the idiosyncratic error term. Standard errors are clustered at the school level.[35] The coefficient of interest θ represents the effect of public school competition on private school outcomes. It measures the net difference in the outcome variable between pre- and post-treatment for private schools whose locations face larger number of public school competitors, compared to schools whose locations face less competition.

Based on the insights from the demand side model, I measure local competition $Comp_{jg}$ in two ways: i) number of public schools that offer grade g and are located within a defined travel distance of a private school j in the period prior to open enrollment reform, ii) indicator weighting each public school-grade by the inverse of their distance from school j and/or the inverse of a measure of student capacity. I report results using 5 km maximum travel distance[36], but I experimented with radii[37] from 2 to 8 km and results are qualitatively similar. For each additional public school within the radius, open enrollment changes private school outcome in θ units. For example, if a private school has 10 public schools nearby, then the effect of open enrollment correspond to $10 \times \theta$ units of the outcome.

The weighted competition indicator is calculated according to the equation:

$$Comp_{jg}^{weighted} = \sum_{k \in M_{jg}} (d_{kj} \times c_k)^{-1} \mathbb{1}_k$$

In words, $Comp_{jg}^{weighted}$ is a weighted sum of all public schools k within 5 km of private school j that offer grade g, that combined make the set M_{jg}. The weights are defined as the product of the inverse of the distance d_{kj} and the inverse of a measure for student capacity

[35] I confirm the presence of serial correlation of the mean school-year residuals from a regression of enrollment on school, grade and year fixed effect (as in Bertrand *et al.* (2004)). I cluster standard error at the school level to allow for serial correlation and heteroskedasticity (arising from schools being of different sizes).

[36] Five km travel distance corresponds to the 90th percentile of the distance between private school students' residence and their respective private school. The implicit assumption is that schools located closer to more public schools face greater competition after open enrollment reform, as families located in these areas would have more public school choice. While commuting times can differ for similar travel distances in different locations in BC, using travel time to define the sets of public school competitors requires implicitly assuming the mean of transportation used by students.

[37] I use maximum travel distance and radius interchangeably, but they are not exactly the same. Since I use driving distance between schools (instead of geodetic distance), the number of schools within a maximum distance to a private school would not perfectly correspond to having a perfect circle around each school.

constraint c_k of each public school k.[38] The student capacity constrain c_k is defined as the ratio of per grade enrollment in year 2001 over maximum per grade enrollment from 1997 to 2001.[39]

Covariates in the vector X include school and neighborhood characteristics. In the former, I include: share of female students, share of Aboriginal student, whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. For neighborhood characteristics, I include share of population with trade or diploma, with college, with some university, with university degree or higher, who are recent immigrants; average family income; number of dwellings; population size; and population size in the age ranges 0 to 4, 5 to 9 10 to 14, and 15 to 19.

Differential number of nearby public school competitors across grades

It is implicit in the empirical model that the intensity of the treatment, captured by $OE_t * Comp_{jg}$: i) is linear in the number of public schools nearby, and ii) is the same for private schools with the same number of public schools nearby, despite that they might have very different enrollment sizes. The main estimation results are based on a panel of private school-grades including Kindergarten until grade 12. By combining all school-grades in one estimation, the statistical power increases and the estimates provide the effect of having an additional public school nearby for the average private school-grade. However, the market structure is quite different for elementary and middle schools, in comparison to high schools. In particular, the enrollment per grade is larger in public high schools and, in turn, there are fewer of them than public elementary and middle schools.[40] To make the intensity of the public school competition comparable across grades, I normalize the indicators of exposure to local competition, $Comp_{jg}$ and $Comp_{jg}^{weighted}$, so that their values are rescaled to range from 0 to 1 as follows: $normComp_{jg} = \frac{Comp_{jg} - min(Comp_g)}{max(Comp_g) - min(Comp_g)}$. The distributions of the four different indicators of local competition ($Comp_{jg}$, $Comp_{jg}^{weighted}$, $normComp_{jg}$, and $normComp_{jg}^{weighted}$ are plotted in Figure 3.13. Each indicator has its advantages and disad-

[38]The BC Ministry of Education does not collect data on school enrollment capacity, thus I had created a measure based on past enrollment data in each school/grade.

[39]For brevity, I only report results for these two measures of local competition. Qualitatively similar results with an indicator that only uses inverse distance as weight are available upon request. Based on the demand side model, I would also include to the competition indicator weights for relative quality based on school test scores and relative tuition prices. While data on school quality was requested to the Popdata BC in the start of this project, its access was not granted to date. The use of weights including relative price would require excluding all private schools without financial information in my data set, which would reduce the sample substantially.

[40]Students from multiple elementary schools typically go to the same public high school as they transition. Analogously, the school attendance zone for a public high school covers greater area than the attendance zones of elementary and middle schools in a given district. For example, the maximum number of public schools within 5 km of private schools is 30 from kindergarten to grade 3 and it is 10 for grades 10, 11, 12.

vantages in capturing the intensity of exposure to local public school competition. Choosing the competition indicator ($Comp_{jg}$) presumes the number of nearby public schools is the most relevant feature to measure competition, regardless of their size, relative distance or potential capacity constraint. The choice of the weighted indicator ($Comp_{jg}^{weighted}$) takes into account the relative distance of each public school (within the pre-determined maximum travel distance) and the capacity constraint of public school competitors, but does it imperfectly as the choice of weights is somewhat arbitrary and the measure of student capacity constraint might suffer from unneglectable measurement error. On the other hand, the normalised indicators implicitly trade off the enrollment size and the number of the public school competitors in each grade. For example, with the nomalised indicators, the intensity of the exposure to competition of three small public elementary schools is assumed to be comparable to one large high school. The interpretation of θ using each indicator also changes with the normalised indicators as it reports the effect of being fully treated, that is, having the maximum number of public school nearby in a given grade, in comparison to having none, the minimum.

Differential trends prior to Open Enrollment

The specification described above does not control for pre-existing trends in private schools outcomes. Using insight from Hoxby (2003), I follow the two-stage procedure used in Chakrabarti & Roy (2016) to control for pre-existing trends. First, I estimate linear time trend for each private school-grade using only its pre-policy outcome data, then I extrapolate the predicted values for the entire period including the post-policy years. In addition to eq. (6), I estimate the model:

$$Y_{jtg} = X_{jt}'\beta_X + \eta\, Comp_{jg} + \theta\, OE_t * Comp_{jg} + \alpha\, T\hat{rend}_{jtg} + \tau_t + \gamma_g + \delta_j + \epsilon_{jtg} \tag{3.8}$$

where $T\hat{rend}_{jtg}$ controls for pre-policy differences in outcome trends across individual school-grades.

Dynamic effects over time

After open enrollment, information about out-of-catchment public schools can take time to disseminate to families. With more information, parents may decide whether to enroll their children in those schools. As a result, private schools may experience changing levels of competitive pressure in the period after open enrollment reform. If the effects are dynamic over time, then an average effect post-policy can underestimate the effect of public school competition. In order to account for dynamic effects, I also estimate the model:

$$Y_{jtg} = X'_{jt}\beta_X + \eta\, Comp_{jg} + \sum_{k=2003}^{2007} \theta_k\, \mathbb{1}_{t=k} * Comp_{jg} + \tau_t + \gamma_g + \delta_{c(j)} + \epsilon_{jtg} \tag{3.9}$$

Addressing identifying assumptions

Identification of the causal impact requires the outcomes of schools with different levels of local competition to follow similar trends in the absence of open enrollment. While this assumption cannot be directly tested, I can test empirically for the pre-policy trends by rewriting the estimation model above including individual year interactions in the pre-policy period:

$$Y_{jtg} = X'_{jt}\beta_X + \sum_{r=2000}^{2002} \theta_r\, \mathbb{1}_{t=r} * Comp_{jg} + \sum_{k=2003}^{2007} \theta_k\, \mathbb{1}_{t=k} * Comp_{jg} + \tau_t + \gamma_g + \delta_{c(j)} + \epsilon_{jtg} \tag{3.10}$$

If schools facing different levels of public school competition $Comp_{jg}$ followed parallel trends before open enrollment, then $\theta_{2000} = \theta_{2001} = \theta_{2002}$. The identifying assumption would also be violated if another shock occurred in the private school market after 2002 and is correlated with my competition indicator. I include time-varying neighborhood and school controls that deal with this potential issue but ultimately this violation cannot be tested empirically. I undertake various robustness checks to address identification concerns.

Allowing for heterogeneous effects

I investigate the heterogeneity in the effects of open enrollment by estimating the following model:

$$\begin{aligned} Y_{jtg} = X'_{jt}\beta_X + \eta\, Comp_{jg} + \theta\, OE_t * Comp_{jg} + \theta_h\, OE_t * Comp_{jg} * Z_j \\ + \tau_t + \gamma_g + \delta_j + \epsilon_{jtg} \end{aligned} \tag{3.11}$$

where Z_j is a pre-policy dummy variable for school j such as private school type, funding level, tuition price (proxied as other revenue per student). The parameter θ_h captures the differential effect for schools with $Z = 1$, in comparison to schools with $Z = 0$.

I estimate eq. 3.7 - 3.11 on per grade enrollment for a sample of 147 school for the period from 2000 to 2007, totaling 6,746 school/grade/year observations.

3.5.2 Private school budget outcomes

I estimate slightly different specifications to measure the effect of public school competition on school budget outcomes using school/year level data. Specifically, I estimate specifications without grade fixed effects and the measures of public school competition are averaged over all grades offered by each private school. Equation 3.7 using data at the school-year level becomes:

$$Y_{jt} = X'_{jt}\beta_X + \eta\, Comp_j + \theta\, OE_t * Comp_j + \tau_t + \delta_j + \epsilon_{jt} \tag{3.12}$$

The school budget dependent variables are defined using the reported financial information divided by the total enrollment in each school-year. Since most of the variation in this variable come from the the numerator, I cluster the standard error at the level of the nonprofit organization and account for serial correlation of observations funded by the same nonprofit organization.

3.6 Results

3.6.1 Enrollment

Table 3.2 reports the estimated $\hat{\theta}$s from eq. 3.7 and 3.8 with private school enrollment per grade as dependent variable. In each row, public school competition is measured differently using: the number of nearby public schools ($Comp_{jg}$), the normalised number of nearby public schools ($normComp_{jg}$), the weighted count of nearby public schools ($Comp_{jg}^{weighted}$), or the normalised weighted count of nearby public schools ($normComp_{jg}^{weighted}$).[41] These different indicators are calculated using driving travel distances of 3, 5 and 7km of a private school. The estimates in the even numbered columns control for school-grade specific pre-policy trends. Apart from the specifications with $normComp_{jg}$, all other estimated $\hat{\theta}$s show statistically significant negative effects on enrollment for private schools facing more competition from public schools after open enrollment was in effect. The estimates are slightly attenuated once pre-policy trends are included in the estimation. According to specification in the first row of column 4, using $Comp_{jg}$ to measure public school competition, for a median private school with 10 public schools within 5 km, open enrollment caused a net loss of almost 1 student per grade ($0.9 = 10 \times -0.09$). I focus on the 5 km travel distance for the measures of public competition for brevity purposes, but estimates using other radii are qualitatively similar.[42]

Exploring the dynamics of the effects over the years after open enrollment was adopted, Figure 3.8 plots the estimated θ_k's from eq. 3.9, using the same four measures of public

[41]In the text and tables, these are also referred to as competition indicator, normalized competition indicator, weighted competition indicator, normalized weighted competition indicator.

[42]Supplemental Figure 3.14 plots the impact estimates for number of public schools within radius from 2 to 8 km. Not surprisingly, for smaller radii the effect of open enrollment is stronger, as the presence of the same number of public schools within a smaller radius create more opportunities for families to transfer out of private school.

schools as in Table 3.2.[43] While the estimated θ_k's are negative for the entire period after open enrollment, the impact on reducing private school enrollment becomes larger after 2004 in all specifications. This indicates that it took some time for the policy to affect the private school market as the effect is growing in magnitude over time. This effect gradually changing is in line with the evidence that out-of-catchment enrollment in public schools also had a gradual increase after 2002.[44] By 2007, the net change of grade-level enrollment for a median school with 10 public schools nearby was -1.4 students. Once again, when $normComp_{jg}$ is used, the estimated θ_k's are very small and not statistically significant, in line with the results in Table 3.2.

Table 3.4 reports estimated θ_k's for all years before and after the policy took place, based on eq. 3.10. The common trends assumption seems to be reasonably satisfied as the estimated θ's for the pre-policy years are at a similar level. After 2002, the value of θ decreases more each year, in comparison to the pre-policy period. I also performed a Wald test on the joint equality of the pre-policy $\hat{\theta}s$. The estimated p-values are higher than 10 percent for the specifications that use the number of nearby public schools and its normalised counterpart as a measure of competition, thus I fail to reject the null of the joint equality of pre-policy $\hat{\theta}s$ in those specifications. This result supports the common trends assumption my specification requires for identification of the causal effect of open enrollment. In contrast, when the weighted competition indicators are used, the test suggests rejecting the null in both cases. Therefore, my preferred specifications are the ones using non-weighted competition indicators.

The estimated effect coefficients can be hard to interpret, particularly when normalised or weighted competition indicators are used. In order to make the effects of public competition more concrete, Figure 3.9 plots the counterfactual of how the mean private school enrollment per grade would have been in the absence of open enrollment reform, according to estimates based on eq. 3.9. Even in the specification with largest impact, the reduction in mean enrollment per grade is no more than two students, about 5 percent of the average private school enrollment per grade.

The estimates in Table 3.2 are based on a conditional differences-in-differences model with non-binary treatment. Table 3.3 compares the estimates of the unconditional model ("base")[45] with the estimates of the conditional model ("full") reported in first and second rows of column 4 of Table 3.2, using number of nearby public schools and its normalised counterpart to measure competition. The base model reported in column 1 and 3 indicates that the unconditional effect on enrollment is negative in both specifications, but it is

[43] See Table 3.10 for the estimated coefficients and corresponding standard errors plotted in this figure.

[44] This is documented for the Lower Mainland, the more densely populated area of BC, in Friesen *et al.* (2019a).

[45] Based on the following specification: $Y_{jtg} = \eta \, Comp_{jg} + \theta \, OE_t * Comp_{jg} + \tau_t + \epsilon_{jtg}$.

statistically significant only when the non-normalised indicator is used. Once I include time-varying covariates, pre-policy trend control, school and grade fixed effects, then $\hat{\theta}$ is attenuated in both cases, but remains significant at 5 percent level for the specification with the non-normalised competition indicator, as shown in column 2. This change from estimated impact in base model $\widehat{\theta}^{base}$ to full model $\widehat{\theta}^{full}$ is a consequence of neighborhood and school characteristics, which helps to explain variation in enrollment, being correlated with the measure of public school competition. Following the decomposition proposed in Gelbach (2016)[46], Table 3.5 shows how much each group of covariates/fixed effects accounts for in the difference in $\hat{\theta}$ between the full and base specifications of Table 3.3. Table 3.5 indicates that the variation in school fixed effects explains 97 percent of the coefficient gap and their component is precisely estimated when the specification uses number of nearby public schools. For the specification with the normalised competition indicator, school fixed effects accounts for 158 percent of the gap but is statistically significant at 10 percent level. The remaining components reported for the other covariates account for a smaller share and are not precisely estimated. As school fixed effects captures the time-invariant enrollment size of schools and are negatively correlated with the number of public schools nearby, the addition of school fixed effects contribute to attenuate the estimated effects of public competition.

Altogether, there is evidence of modest negative impacts on grade-level private enrollment using different measures of public school competition, but this result is not robust to specifications using the normalised count of nearby public schools. While the specifications using weighted competition indicators yield significant effects of open enrollment, their specification does not pass the test of pre-policy parallel trends.

Heterogeneity of effects

I explore the heterogeneity of impacts of open enrollment across subgroups by estimating eq. 3.11. Table 3.6 reports $\hat{\theta}$ and $\hat{\theta}_h$. Estimates from column 1 show that other secular, Catholic, and Waldorf/Montessori schools lost more students compared to other Christian schools. Column 5 reports a qualitatively similar result for Catholic and Waldorf/Montessori schools, but only statistically significant at 10 percent level for the former. This evidence suggests that, apart from Catholic schools, the demand for all other private school types are not very responsive to public school competition. In other words, parents whose children attend non-Catholic private schools view public schools as weaker substitutes than parents whose children attend the Catholic private schools.

[46]This decomposition accounts for the role that covariates and fixed effects have in changing the value of $\hat{\theta}$ from the unconditional difference-in-differences specification to the full conditional specification, using the omitted variables bias formula. Estimates in column 1 and 3 of Table 3.5 correspond to $\widehat{\delta} = \widehat{\theta}^{full} - \widehat{\theta}^{base}$.

Estimates in columns 2, 3, 6, and 7 indicate that enrollment effects for schools in different funding groups or different levels of expenditure per student are not statistically significant in either specification. For all the specifications in columns 2, 3, 6, and 7, effects vary but their differences are not significantly significant when comparing schools in funding groups 1 and 2, and schools above and below median expenditure per student. In column 8, schools reporting above median price (measured with other revenue per student) experience increase in enrollment as a result of increased competition, compared to schools reporting lower price. One possible explanation for this result is that public school students, who anticipated potential decline of peer or school quality under open enrollment, decided to attend less accessible higher price private schools.

Table 3.6 indicates that the most important school characteristic to explain heterogeneity of effects of public school competition on private school enrollment is the choice of curriculum. Families that have their children in schools with differentiated curricula likely have larger random taste parameters associated with them (represented by ϵ in the demand side model). For example, ϵ is high for families sharing the same religious beliefs that those schools teach in their curricula. If families perceive that private schools supply differentiated products and school choice is based on idiosyncratic taste for a particular school[47], then private schools might be imperfect substitutes for public schools. From Table 3.6, public schools seem to be unlikely substitutes particularly for Christian and other faith schools, but more likely for Catholic schools. While private schools in BC are required to offer the same curriculum as public schools (if recipient of provincial grants), they also differentiate by supplementing curriculum with religious education or an alternative learning environment.

To assess the heterogeneity of effects by grade, I split the sample based on grades offered and estimate eq. 3.8 for each subsample. Figure 3.10 plots $\hat{\theta}$ for each subsample indicating that the negative impact on enrollment is concentrated for private schools that offer grades 8, 9 and 10. Not surprisingly, the pattern of effects by grade looks similar for both specifications with the competition indicator and with the normalised indicator (the scale differs by construction). Splitting the sample by grades accomplishes the same as normalising the competition indicator by grade as it accounts for private high schools having larger cohort sizes and fewer public school competitors than elementary private schools. The result shown in Figure 3.10 is in line with the stylized fact that the number of movers from public school to private school is disproportionately larger in high school.[48] At the transition to high school, as most students are moving to different schools anyways, they are more susceptible to move from private to public schools if they have enough incentives. As high schools students are older and more likely independent travelers, the opportunity

[47]For charter schools, Walters (2018) shows that students do not choose schools based on school-specific match effect in academic achievement.

[48]See Figure 3.4.

to attend schools out of their neighborhoods seem to be more appealing than students in earlier grades. This result suggests that preferences for schools are not homogeneous across all grades, specifically the disutility of longer travel distance (γ in the stylized demand side model) seems to be smaller in high school than in earlier grades. Another important result from Figure 3.10 is the positive effect on enrollment in the first grades, from kindergarten to grade 3. While these effects are very small in magnitude, they are more precisely estimated than the ones for the high school grades.

3.6.2 Private School Budget

Table 3.7 reports $\hat{\theta}$s from estimation based on eq. 3.12 for specifications using the following logarithmic per student school outcomes as dependent variables: gifts, grants, total revenue, other revenue (net of grants and gifts), and total expenditure.[49] Similar to the analysis with enrollment as dependent variable, public school competition is measured using: the number of nearby public schools ($Comp_j$), the normalised number of nearby public schools ($normComp_j$), the weighted count of nearby public schools ($Comp_j^{weighted}$), or the normalised weighted count of nearby public schools ($normComp_j^{weighted}$). All these school level competition indicators are calculated by averaging the school-grade level competition indicators across grades offered by each private school. The specifications reported control for school specific pre-policy trends. The estimates reported in this section use school-year data combining schools that offer different grades, from kindergarten to grade 12.

Considering all measures of public school competitions and all specifications shown in Table 3.7, results indicate that private schools with higher exposure to public school competition did not experience significantly different changes in per student: gifts, grants, total revenue, or total expenditure. Effects of open enrollment on other revenue were positive and statistically significant for most specifications using the competition indicator and its normalised version. This result is not robust to using the weighted competition indicators as statistically significant effects vanish when these specifications are used.[50]

Figures 3.11 plot the pre-policy $\hat{\theta}_r$ and post-policy $\hat{\theta}_k$ based on estimates of eq. 3.10 for specifications using the normalised number of nearby public schools ($normComp_j$), and the normalised weighted count of nearby public schools ($normComp_j^{weighted}$). The year before open enrollment was effective, 2002, was normalized to zero in the figure. All the coefficients were also normalized to $\hat{\theta}_{2002}$.[51] The estimates do not follow a clear pattern before and after

[49] Gifts are the sum of donations with and without a tax receipt. Grants include federal, provincial, and municipal government grants. Other revenue is the total revenue net of gifts and grants.

[50] The results reported in this paragraph look qualitatively similar when other distances to private school are considered. Supplemental Figures 3.15 to 3.19 plots the effect estimates for number of public schools within radius from 2 to 8 km.

[51] I subtracted $\hat{\theta}_{2002}$ from all coefficients, so that the effect coefficient in 2002 is zero.

open enrollment as they go up and down over the years. In the case of log other revenue per student, the only outcome that changed due to open enrollment (as reported in Table 3.7), there is a gradual increase in the effect of public school competition after 2002, but this trend starts before open enrollment, year 0 in the horizontal axis.[52] Overall, the estimated effects per year, even when statistically significant, do not follow a pattern that suggests that private school responded to public school competition by adjusting their budget outcomes.

In Table 3.8, I explore the heterogeneity of impacts of open enrollment on private school revenues and expeditures per student estimating eq. 3.11 (adapted to the school-year data). The coefficients reported are $\hat{\theta}$ and $\hat{\theta}_h$ using the normalised number of nearby public schools to measure competition. For most outcomes, effects do not seem to differ significantly for schools in different subgroups based on funding group, private school type, expenditure and price level. The notable exception are schools with expenditure per student above median which had an increase in total revenue per student and other revenue per students, as a result of open enrollment reform.[53] This indicates that the source of increase in total revenue per student is other revenue (such as tuition) and not gifts or grants. As I show below, this finding seems to be associated with elementary schools, which experienced no enrollment effects as a result of open enrollment reform.

Figure 3.12 plots $\hat{\theta}$ for subsample of schools that offer each grade from kindergarten to grade 12. As schools typically offer the same subset of grades, many of these subsamples are similar.[54] Even though many estimates are not statistically significant, results indicate that the responses of elementary schools follow different patterns than those offering high school grades. Elementary schools, whose enrollment were not impacted by open enrollment, increased their other revenue per student (plausibly via tuition increases). In contrast, schools offering high school grades had no impact on other revenue. This finding relates to the negative enrollment effect of private high schools, because those schools facing greater competitive pressure likely had to use strategies different than raising tuition to maintain their revenues unchanged. One likely response strategy was increasing fundraising efforts to increase revenue via gifts and donations as suggested in Figure 3.12.

Altogether, evidence presented here indicates that private schools did not make substantial adjustments in their budget as a result of open enrollment. The estimated effects on budget outcomes were not robust to different measures of local public competition. The

[52]The p-value of the Wald test on the joint equality of the pre-policy $\hat{\theta}s$ in this case is 0.05 for the specification using the normalised weighted competition indicator and 0.75 the specification with the normalised competition indicator.

[53]This result is qualitatively similar when the normalised weighted count of nearby public schools is used to measure exposure to public school competition. See Supplemental Table 3.11.

[54]The subsample of schools offering grade 2 is similar to the one offering grades 3, 4, 5, 6. Analogously, subsample of schools offering grade 8 is very similar to the subsamples of schools offering grades 9, 10, 11, 12.

pattern of effects clearly differ for elementary and high school. The positive effects on other revenue per student were concentrated in elementary schools, which experienced no enrollment drops as a result of public school competition. This lack of greater response of private high schools in terms of revenues or expenditures is not surprising when placed in context. First, the funding incentives from the government constrain private school expenditure per student for most private schools, which are in funding group 1. Second, the estimated effect of increased public school choice on enrollment are negative but modest[55], even for the most affected schools. Finally, demand for a large share of private schools (non-Catholic schools) were unresponsive to open enrollment.

3.7 Conclusion

This paper examines the effects of increased public school choice on private school outcomes. After the introduction of open enrollment reform, students could exert greater public school choice and enroll in out-of-catchment schools that had availability. Using the insights of a stylized model and exploiting spatial variation in public school competition, I estimate effects of open enrollment using a difference-in-differences research design with non-binary treatment. Access to novel data set with information on private school revenues and expenditure permits examining whether enrollment effects are a result of a demand shock or simply a change in quantity demanded and prices along the private school demand curve.

Estimation results reveal significant but modest negative effects of higher public school competition on private school enrollment per grade. Those effects were concentrated in Catholic schools and in high school grades. In my causal analysis, I see no evidence of private schools responding to increased competition by adjusting per student revenue or expenditure. When the voucher system has a sharp threshold criterion to determine funding, private schools can be constrained to adjust spending when facing increased competition. These results combined indicate that open enrollment caused a small negative shock on private school demand.

It is worth noting two limitations of my analysis. First, my analysis is at the school level which limits my scope, particularly in accessing the heterogeneity of effects. Second, it is possible that local public competition would be more precisely captured by including a measure of relative quality between private and nearby public schools. By having measures of school quality, the analysis could be expanded to investigate whether demand for private school is sensitive to public school quality. These limitations can be opportunities for future research.

[55]This result also holds when considering the smaller sample of schools matched to nonprofit organization data. See Supplemental Table 3.12.

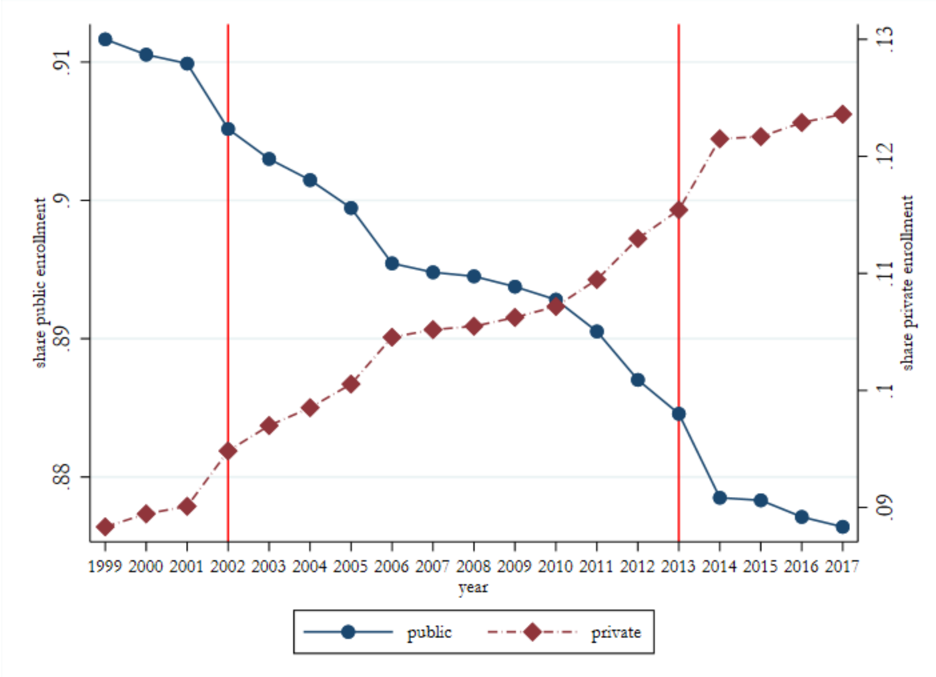
This paper provides evidence that a sizable part of the demand for private education is unresponsive to higher public school choice, even in BC where the public school system offers a wide range of high-quality alternatives. This suggests that not all private schools represent competitive incentives for public schools to improve outcomes.

The findings in this paper are of general interest beyond the Canadian context. When introducing increased public school choice,[56] policymakers will want to know to which extent it can crowd out private schools. Furthermore, knowing what type of private schools generate competitive incentives to public schools can help design voucher programs to targeted schools that could generate greater externality.

[56]In addition to open enrollment, other potential sources of public school competition to private schools are market-designed approaches to public school choice, charter school and magnet school expansion.

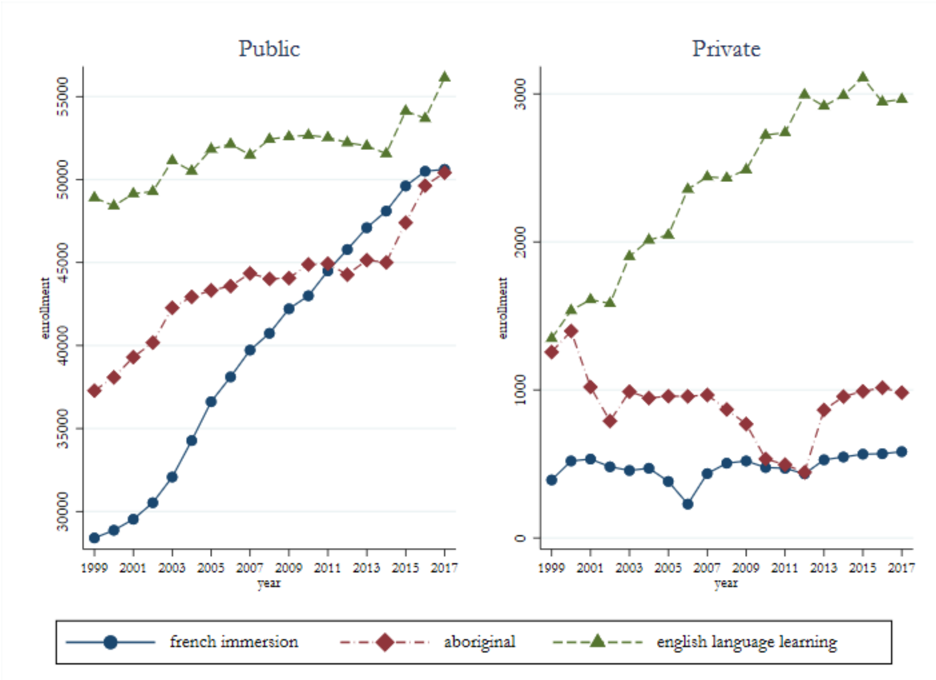
3.8 Figures

Figure 3.1: K-12 enrollment in public and private sectors in BC, 1999-2017



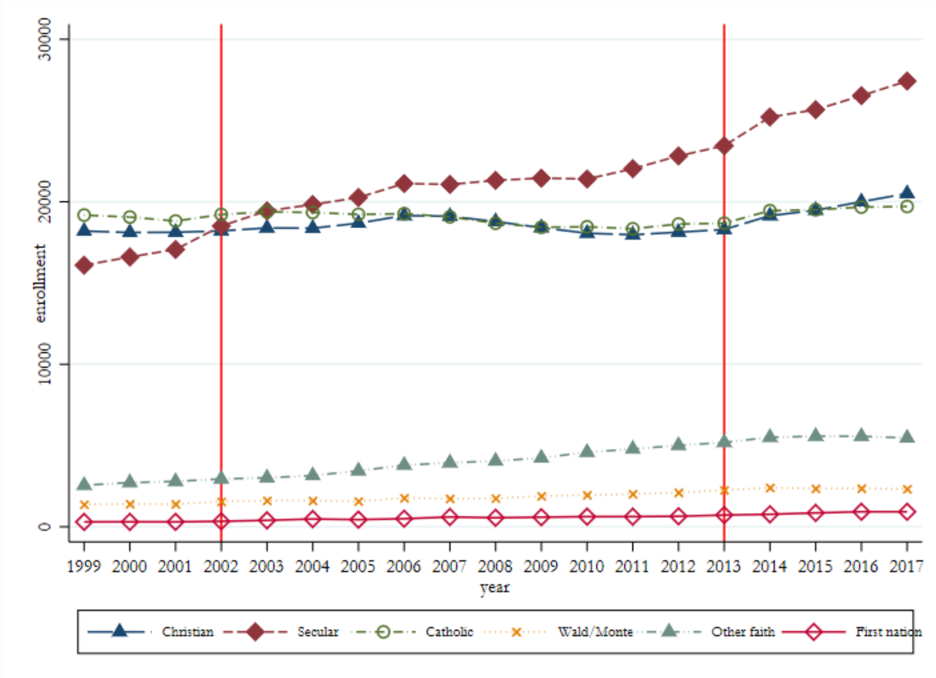
Notes: Public and private school enrollment measured on the left and right axes, respectively. In school-year 2003-2004, the open enrollment policy was effective. Teacher strikes lasted from spring to fall of 2014. Source: author's calculations using administrative data from BC Ministry of Education (2019). Disclaimer: all inferences, opinions, and conclusions drawn in this figure are those of the author, and do not reflect the opinions or policies of the Data Steward.

Figure 3.2: K-12 enrollment in special programs in public and private schools in BC, 1999-2017



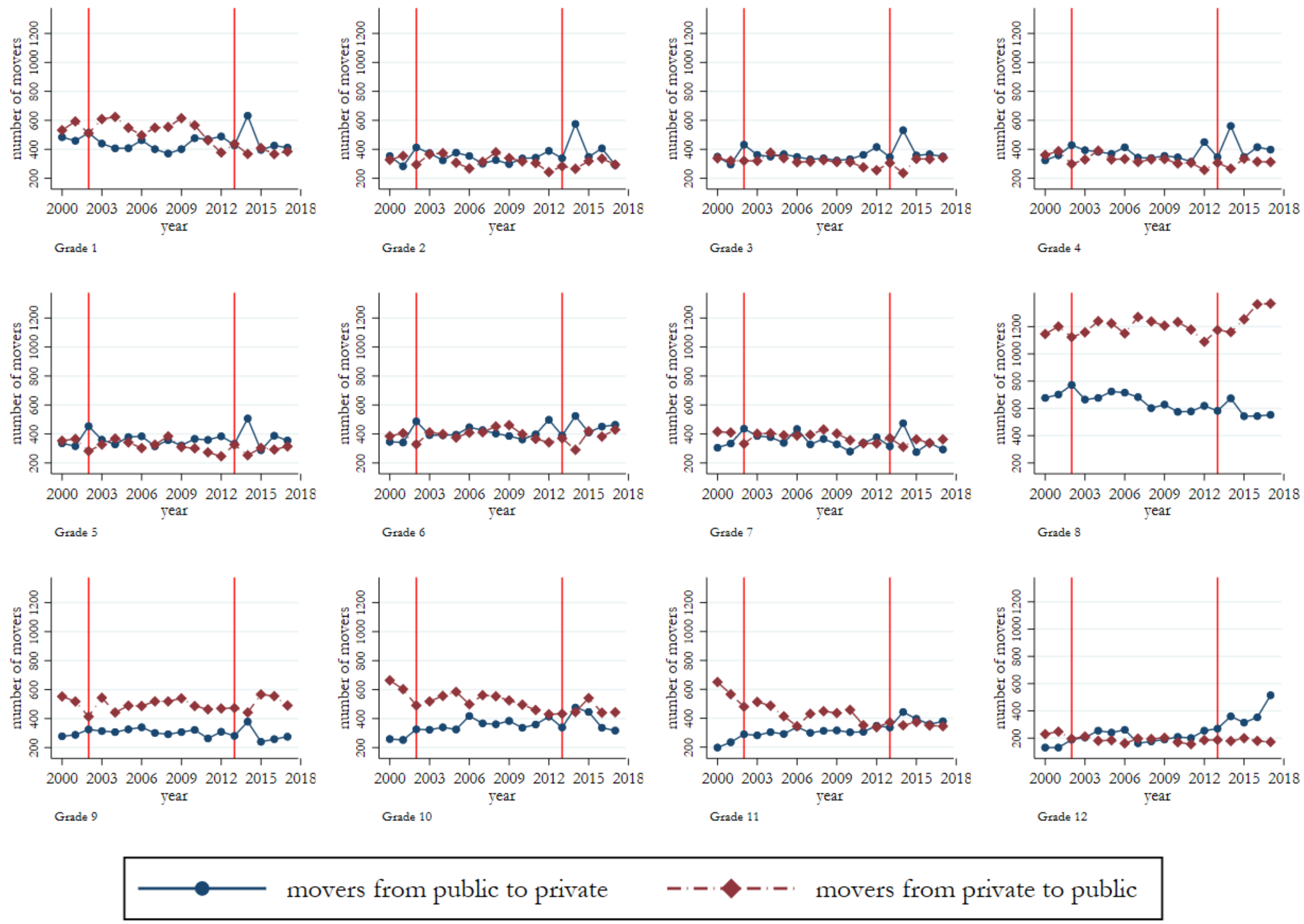
Notes: Enrollment in french immersion includes both early and late french immersion programs. Enrollment in Aboriginal program correspond to the sum of enrollment of "Aboriginal language and culture", "Aboriginal support services", and "other Aboriginal program". Source: author's calculations using administrative data from BC Ministry of Education (2019). Disclaimer: all inferences, opinions, and conclusions drawn in this figure are those of the author, and do not reflect the opinions or policies of the Data Steward.

Figure 3.3: Private school K-12 enrollment in BC by school type, 1999-2017



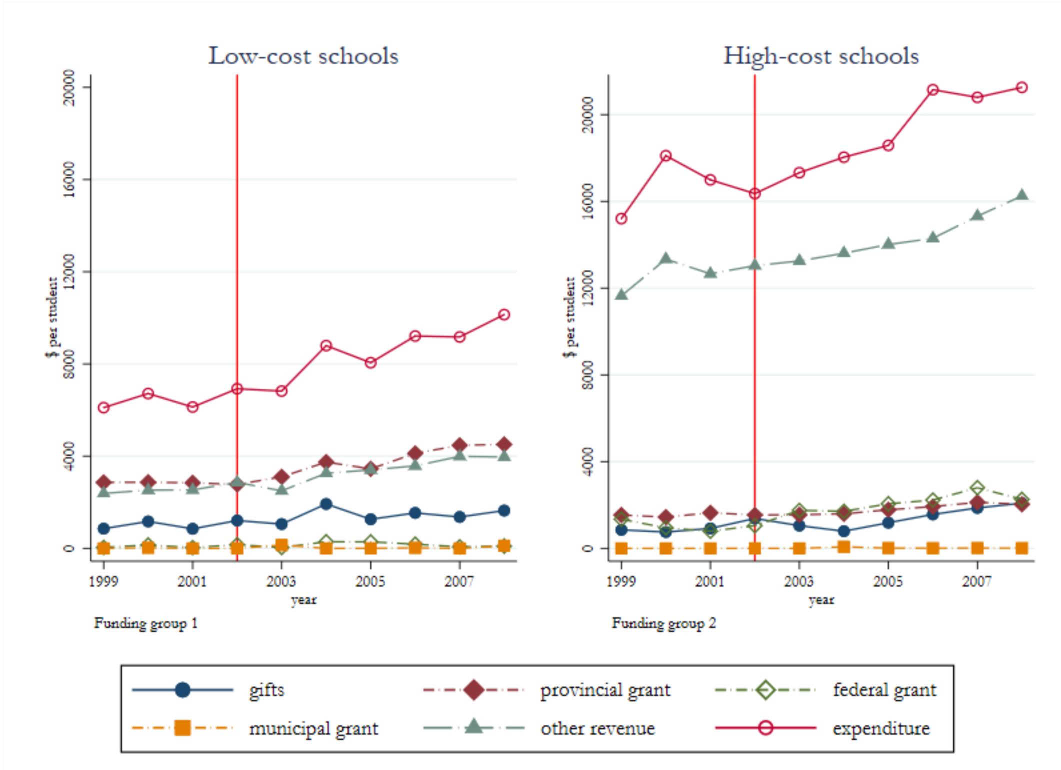
Source: author's calculations using administrative data from BC Ministry of Education (2019). Disclaimer: all inferences, opinions, and conclusions drawn in this figure are those of the author, and do not reflect the opinions or policies of the Data Steward.

Figure 3.4: K-12 movers between private and public schools in BC by grade, 2000-17



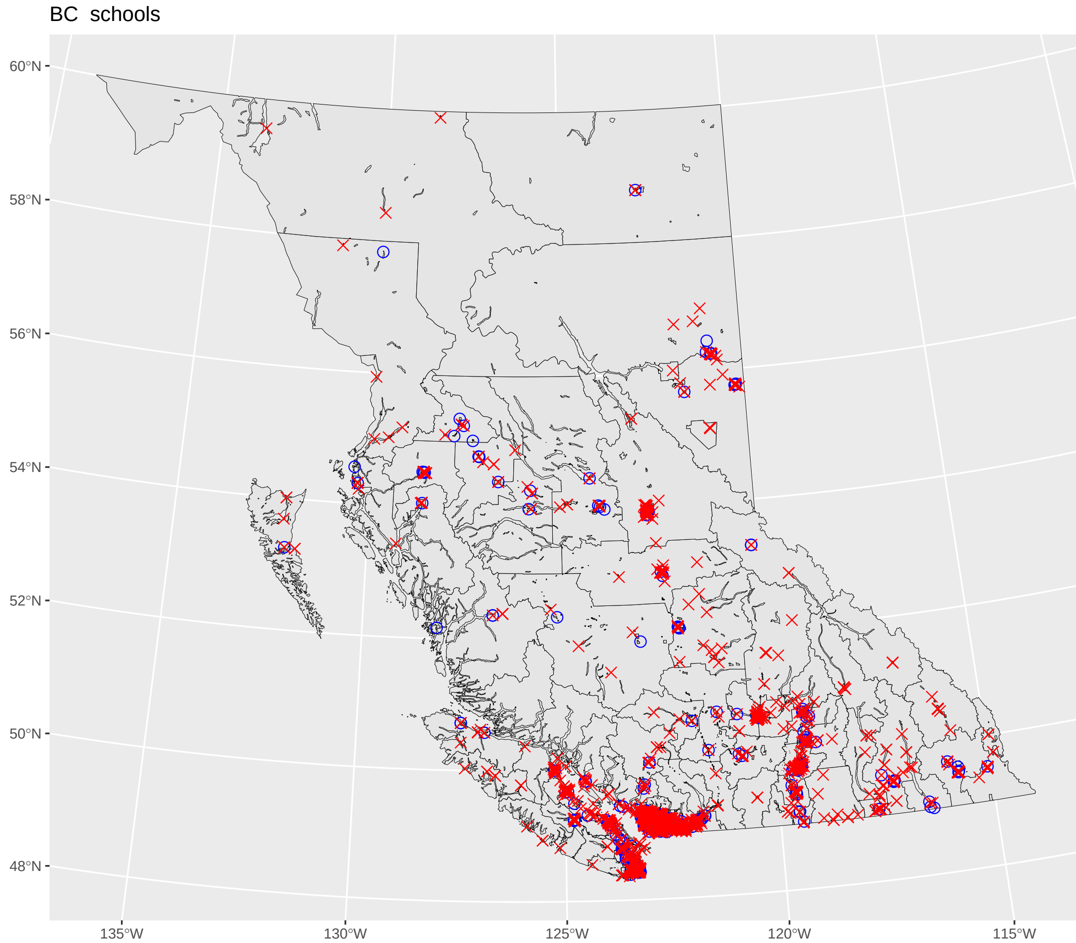
Source: author's calculations using administrative data from BC Ministry of Education (2019). Disclaimer: all inferences, opinions, and conclusions drawn in this figure are those of the author, and do not reflect the opinions or policies of the Data Steward.

Figure 3.5: Mean per student private school revenues and expenditure by government funding category, 1998-2018



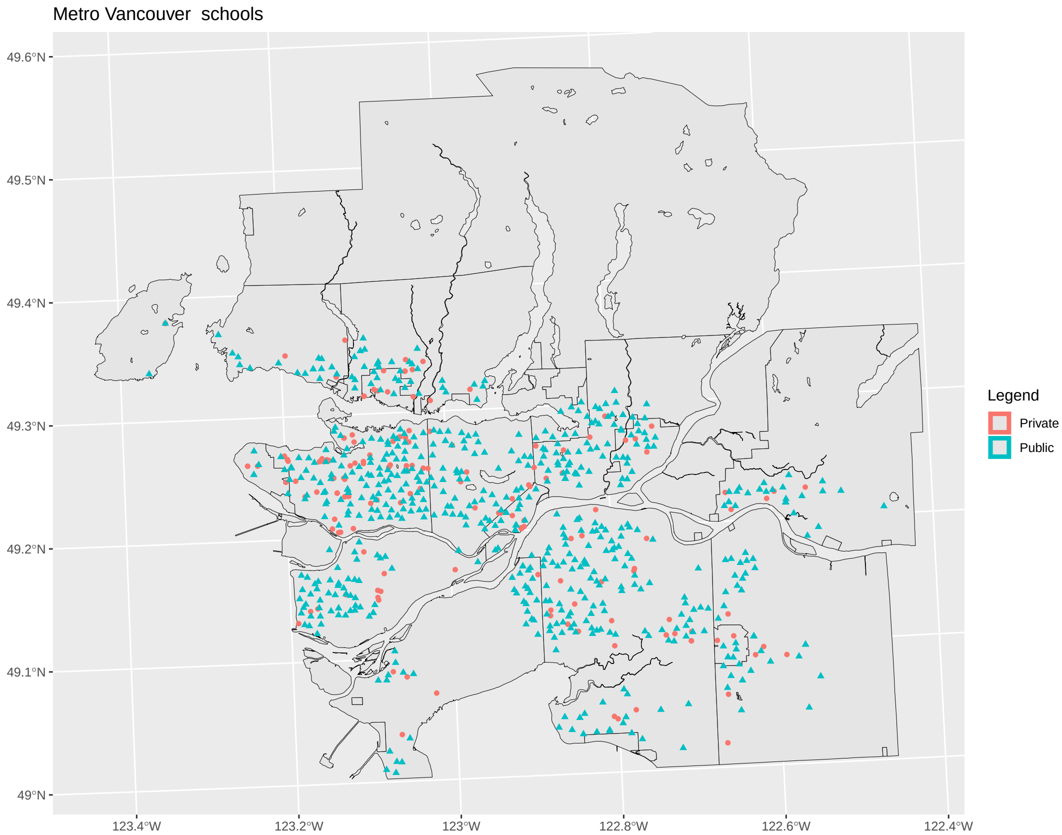
Notes: Funding group 1 and 2 receive 50 percent and 35 percent of the provincial grant that public schools receive per student, respectively. Gifts correspond to donations with or without a tax receipt. Provincial grants correspond to grants provided by the provincial government. Other revenue corresponds to total revenue net of gifts and grants. Expenditure correspond to total expenditure. All values are expressed in nominal terms. Source: author's calculations from CRA data.

Figure 3.6: Private and Public Schools in BC



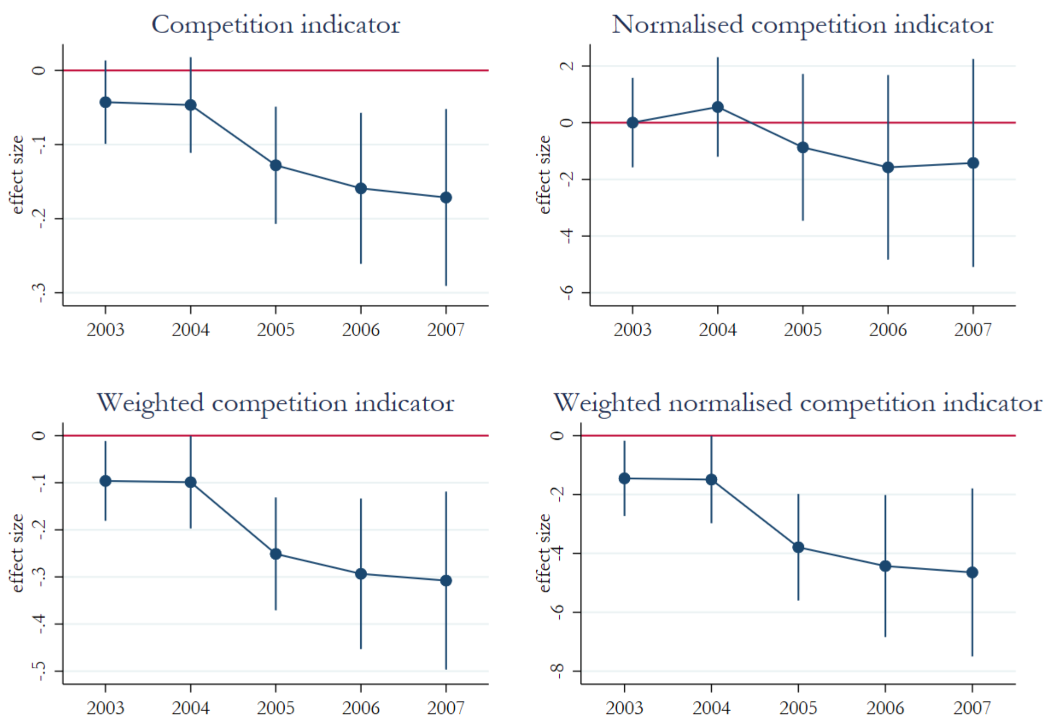
Legend: × represent public schools and ○ represent private schools.Source: author's calculations.

Figure 3.7: Private and Public Schools in Metro Vancouver



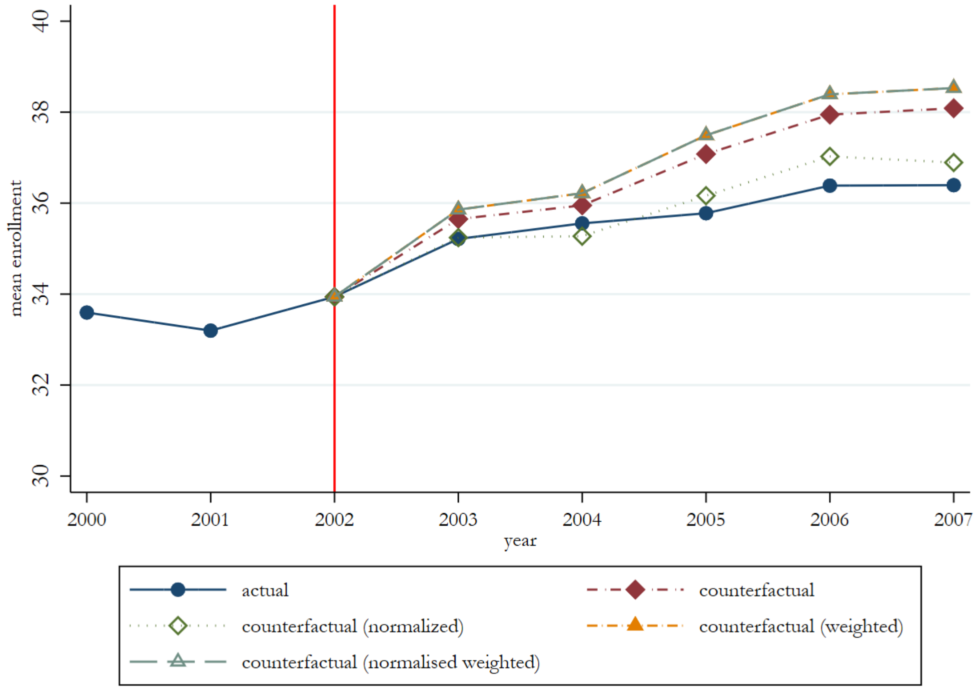
Source: author's calculations.

Figure 3.8: Estimated effects of public school competition on private school enrollment per grade, 2003-07



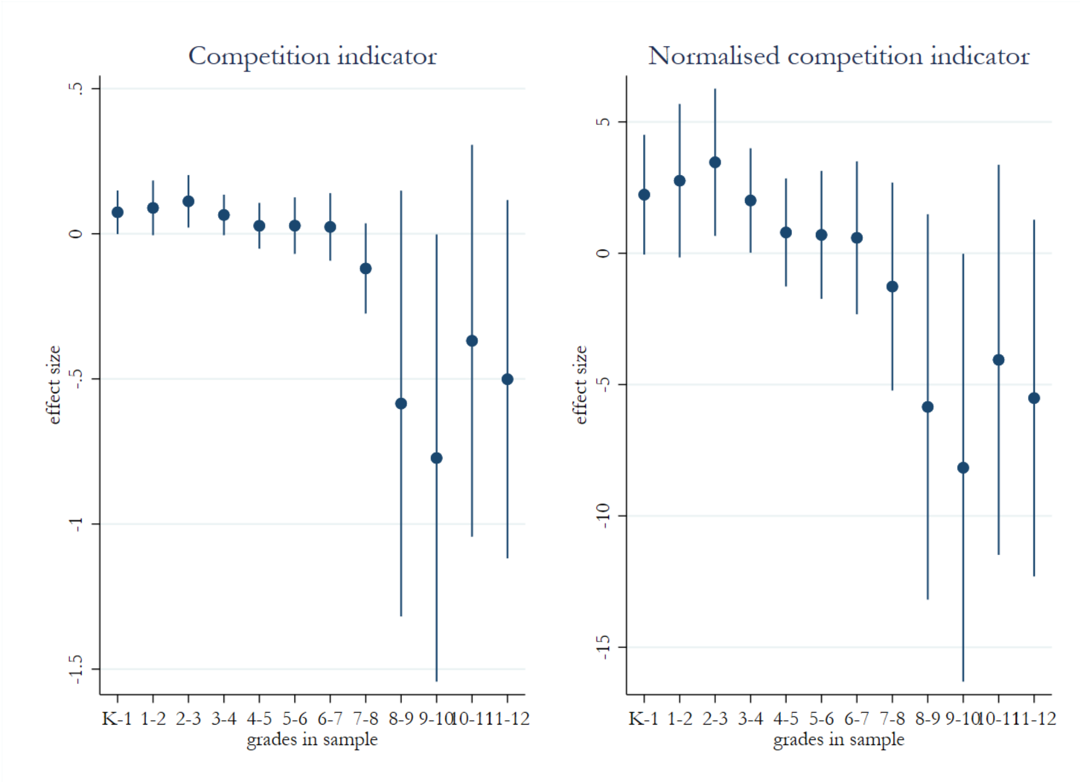
Notes: Figure plots θ for each year as defined in eq. 3.8. In (a) competition is measured using number of nearby public schools and in (b) the weighted competition indicator. 90 percent confidence intervals are estimated with clustered standard errors at the school level. Specification includes school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.9: Actual and counterfactual private school mean enrollment per grade, 2003-07



Notes: Figure plots actual and counterfactual mean private school enrollment per grade, based on specifications using different measures of public school competition: number of nearby public schools, normalised number of nearby public schools, weighted count of nearby public schools, normalised weighted count of nearby public schools. Counterfactual was calculated using estimates from specification of eq. 3.9. Specification includes school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools, and pre-policy trend), neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19), school, grade, year fixed effects.

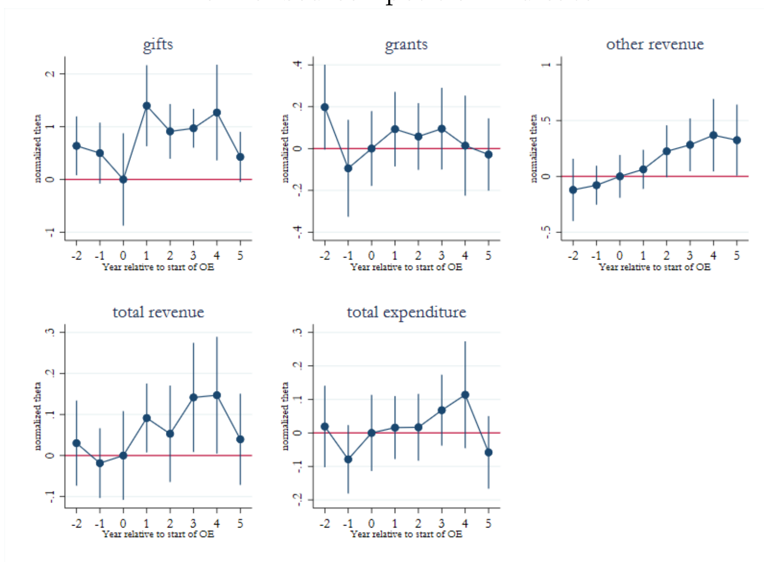
Figure 3.10: Heterogeneous effects of public school competition on private school enrollment by grade



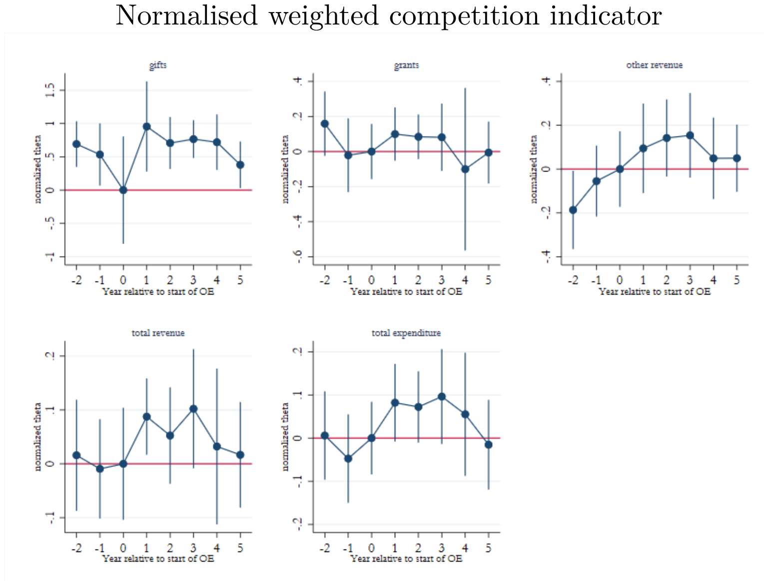
Notes: Figure plots θ as defined in eq. 3.8 for each sample of schools offering two grades. Competition is measured using number of public schools within 5 km of a private school on the graph in the right and the normalised number of public schools within 5 km of a private school on the left. 90 percent confidence intervals are estimated with clustered standard errors at the school level. Specification includes school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.11: Effects of public school competition on private school budget outcomes per student, 2000-07

Normalised competition indicator

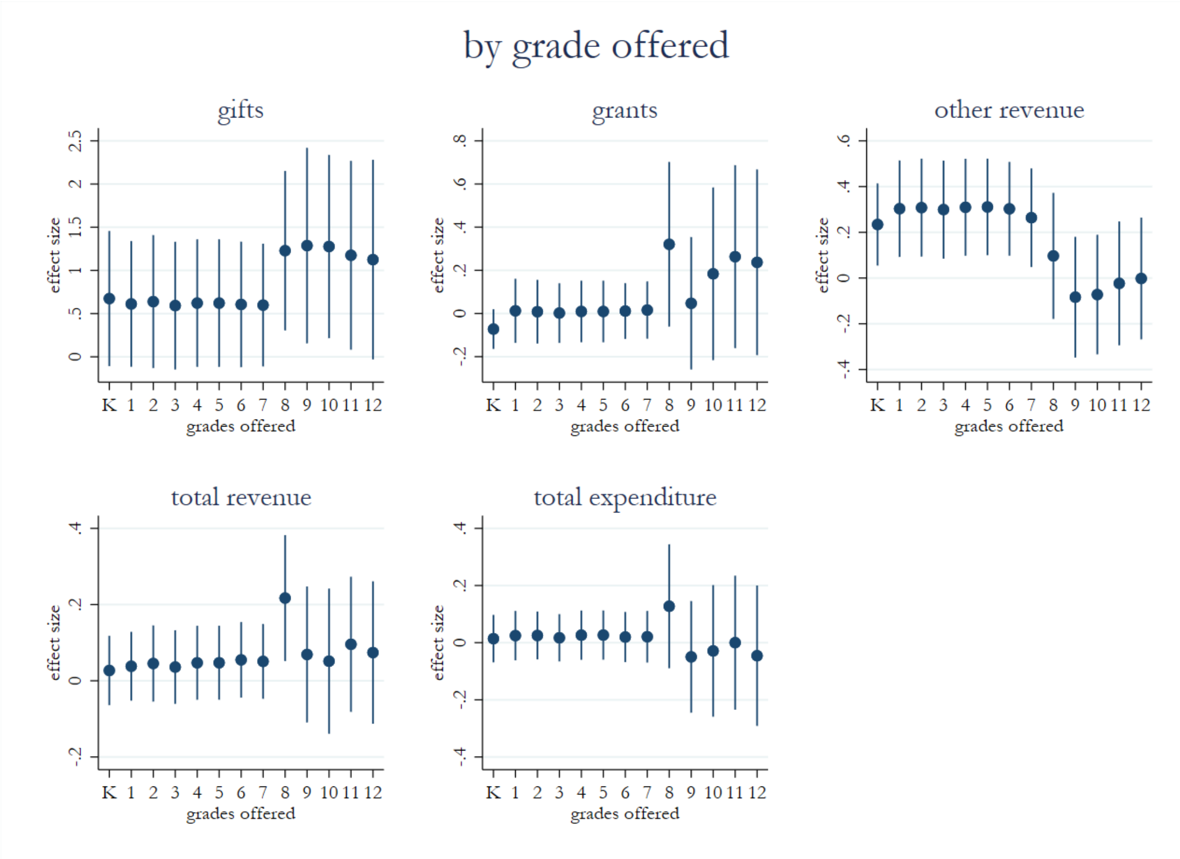


Normalised weighted competition indicator



Notes: 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Reported coefficients of interaction of competition indicator with OE are estimated in model with year FE, school FE, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance) and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19). Outcome variables are expressed in logarithmic per student. Grants include federal, provincial, and municipal government grants. Other revenue corresponds to total revenue net of gifts and grants. Expenditure correspond to total expenditure.

Figure 3.12: Heterogeneous effects of public school competition on private school revenues and expenditure per student by grade offered, normalised competition indicator



Notes: 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Reported coefficients of interaction of the normalised competition indicator with OE are estimated in model with year FE, school FE, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance) and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19). Outcome variables are expressed in natural logarithm per student. Grants include federal, provincial, and municipal government grants. Other revenue corresponds to total revenue net of gifts and grants. Expenditure correspond to total expenditure.

3.9 Tables

Table 3.1: Sample means, 2000 - 2007

	(1)	(2)	(3)	(4)	(5)	(6)
	All private	Other Christian	Other secular	Catholic	Waldorf/ Montessori	Other Faith
school-grade-year level						
Enrollment per grade	34.99	30.03	45.46	35.64	18.76	27.1
Number of public schools within 5km	11.79	8.17	9.22	14.59	10.63	16.72
Number of public schools within 5km, weighted by distance and capacity ratio	6.75	4.45	5.04	8.56	5.9	10.13
Offers Kindergarten - grade3	0.42	0.37	0.32	0.46	0.71	0.52
Offers grades 4 - 7	0.38	0.35	0.27	0.45	0.15	0.38
Offers grades 9 - 12	0.2	0.29	0.41	0.08	0.15	0.09
Funding Group 1	0.83	0.92	0.26	1	0.84	1
Funding Group 2	0.13	0	0.66	0	0.09	0
Number of private schools within 5km	4.17	2.46	5.32	4.49	4.15	6.05
Neighbourhood characteristics						
Share with bachelor or higher	0.24	0.14	0.39	0.23	0.27	0.26
Average income per family	77,936	67,313	118,665	67,607	84,230	67,002
Population	727	753	903	658	604	620
Share of recent immigrants	0.05	0.03	0.06	0.05	0.05	0.07
Observations	6,650	1,793	1,281	3,010	241	325
Number of schools	142	32	35	51	17	7
School-year level						
Total revenue per student $	9,406	6,547	15,876	4,951	23,464	8,477
Total Expenditure per student $	9,272	6,216	15,592	4,883	23,649	8,285
Gifts/donations per student $	1,072	1,696	1,893	337	1,027	4,156
Government grants per student $	3,236	2,773	3,213	2,882	6,228	2,241
Other revenue (net of grants/gifts) per student $	5,098	2,078	10,770	1,732	16,208	2,080
Observations	674	107	142	338	65	22
Number of schools	92	17	20	43	9	3
Number of non-profits	53	15	19	7	9	3

Notes: Neighborhood are defined according to Census enumeration or dissemination area where school is located. Nonprofits are defined as charities that filed T3010 form with information returns to Canada Revenue Agency and could be matched with private schools in the period from 2000 to 2007.

Table 3.2: Effect of public school competition on private school per grade enrollment, 2000-2007

	3km		5km		7km	
	(1)	(2)	(3)	(4)	(5)	(6)
OE * Number of nearby public schools	-0.18**	-0.14**	-0.11**	-0.09**	-0.13***	-0.10**
	(0.08)	(0.07)	(0.04)	(0.04)	(0.05)	(0.04)
OE * Normalised number of nearby public schools	-1.07	-0.61	-0.59	-0.47	-0.69	-0.53
	(1.59)	(1.33)	(1.38)	(1.22)	(1.72)	(1.34)
OE * Weighted count of nearby public schools	-0.35***	-0.29***	-0.25***	-0.20***	-0.26***	-0.22***
	(0.10)	(0.09)	(0.08)	(0.07)	(0.08)	(0.07)
OE * Normalised weighted count of nearby public schools	-3.79***	-3.29***	-3.72***	-3.09***	-3.72***	-3.09***
	(1.09)	(0.99)	(1.14)	(1.02)	(1.22)	(1.09)
Observations in each specification: 6,650						
Control for pre-policy trends	NO	YES	NO	YES	NO	YES

Notes: coefficients correspond to θ from eq. 3.7 and eq. 3.8. The competition indicator is measured as the number of public schools within either 3, 5 or 7 km of a private school, depending on the column. The weighted competition indicator weighs each nearby public school by the inverse distance and the inverse student capacity ratio. The normalization rescales the indicators for each grade to range from 0 to 1. Dependent variable is enrollment per grade. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the school level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.3: Effect of public school competition on private school per grade enrollment, unconditional and conditional difference-in-differences specification, 2000-2007

	Base	Full	Base	Full
	(1)	(2)	(3)	(4)
OE x Number of nearby public schools	-0.15***	-0.09**		
	(0.06)	(0.04)		
Number of nearby public schools	-0.42*	0.27		
	(0.23)	(0.26)		
OE x Normalised number of nearby public schools			-1.31	-0.47
			(1.63)	(1.22)
Normalised number of nearby public schools			3.55	5.72
			(7.22)	(5.28)
share of female students		0.63		0.59
		(1.22)		(1.22)
share of aboriginal students		-5.66**		-5.38**
		(2.60)		(2.58)
offer full-day kindergarten		0.41		0.33
		(0.65)		(0.68)
Number of nearby private schools		0.54		0.72
		(0.68)		(0.69)
share with trade or diploma		0.72		0.98
		(4.53)		(4.56)
share with college degree		0.28		0.22
		(4.35)		(4.31)
share with some university		5.04		4.84
		(4.06)		(4.05)
share with bachelors degree or higher		6.43*		6.07
		(3.67)		(3.68)
ln average family income		-0.17		-0.16
		(0.11)		(0.11)
ln number of dwellings		-1.05		-0.73
		(1.71)		(1.70)
ln population		2.66		2.21
		(2.58)		(2.58)
share of recent immigrants		-1.10		-0.37
		(4.24)		(4.25)
ln population with age 0 to 4 years		-1.06		-1.04
		(0.69)		(0.70)
ln population with age 5 to 9 years		0.27		0.30
		(0.76)		(0.76)
ln population with age 10 to 14 years		-0.20		-0.24
		(0.62)		(0.62)
ln population with age 15 to 19 years		0.48		0.50
		(0.76)		(0.76)
Observations	6,650	6,650	6,650	6,650
R-squared	0.03	0.92	0.00	0.92
Year FE	YES	YES	YES	YES
School and grade FE	NO	YES	NO	YES
School controls	NO	YES	NO	YES
Neighbourhood controls	NO	YES	NO	YES
Control for pre-policy trends	NO	YES	NO	YES

Notes: competition is measured with the number public school within 5 km. Weighted indexes weight each nearby public school by the inverse distance, the inverse student capacity ratio, or both. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the school level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.4: Effect of public school competition on private school per grade enrollment per year, 2000-2007

Year =	2000	2001	2002	2003	2004	2005	2006	2007	Ho: $\theta_{2000} = \theta_{2001} = \theta_{2003}$ (P-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1{Year} * Number of nearby public schools	0.24	0.27	0.23	0.20	0.20	0.11	0.10	0.10	0.13
	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.26)	(0.26)	(0.27)	
1{Year} * Normalised number of nearby public schools	2.98	3.96	3.33	3.53	4.00	2.90	2.77	2.76	0.22
	(5.88)	(5.75)	(5.77)	(5.80)	(5.75)	(5.76)	(5.77)	(5.93)	
1{Year} * Weighted count of nearby public schools	0.41	0.47	0.37	0.30	0.30	0.11	0.08	0.07	0.02
	(0.41)	(0.41)	(0.41)	(0.41)	(0.41)	(0.42)	(0.42)	(0.44)	
1{Year} * Normalised weighted count of nearby public schools	5.94	6.79	5.43	4.40	4.43	1.92	1.44	1.44	0.04
	(5.76)	(5.70)	(5.69)	(5.75)	(5.77)	(5.89)	(5.94)	(6.09)	
Observations in each specification: 6,746									

Notes: competition is measured with the number public school within 5 km. Weighted indexes weight each nearby public school by the inverse distance, the inverse student capacity ratio, or both. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Decomposing the effect of public competition on per grade enrollment into components for each group of covariates

Covariates	explained (1)	share explained (2)	explained (3)	share explained (4)
neighbourhood SES	0.007	-11%	0.295	-35%
	(0.006)		(0.251)	
neighbourhood density	0.005	-8%	0.153	-18%
	(0.006)		(0.221)	
school characteristics	-0.019	29%	-0.066	8%
	(0.014)		(0.373)	
number of nearby private schools	0.002	-3%	0.128	-15%
	(0.003)		(0.159)	
grade FE	0.002	-3%	-0.017	2%
	(0.004)		(0.092)	
school FE	-0.063**	97%	-1.336*	158%
	(0.027)		(0.791)	
Difference in coefficient between full and base specifications: θ^{base} - θ^{full}	-0.065*	100%	-0.843	100%
	(0.038)		(1.080)	
Observations	6,650		6,650	

Notes: numbers reported correspond to decomposition described in Gelbach (2016) to account for coefficient change in the coefficient θ from the base to the full specification. The base specification is reported in Table 3.3 and includes OE * (normalised) number of nearby public schools, (normalised) number of nearby public schools, and year effects. In addition to the variables included in base model, the full model, also reported in reported in Table 3.3, includes school controls, neighbourhood controls, pre-policy trends, grade fixed effects, and school fixed effects. Neighbourhood SES includes: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; and average family income. Neighbourhood density include: number of dwellings, population size, and population by age groups 0-4, 5-9, 10-14, 15-19. School characteristics include: share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, pre-policy trend, and the number of private schools within 5 km of travel distance. Clustered standard errors at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Heterogeneous effects of public school competition on private school per grade enrollment

	Competition indicator				Normalised competition indicator			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OE * (normalised) number of nearby public schools	0.10	-0.08*	-0.18	-0.09**	2.59	-0.73	-4.89	-1.23
	(0.09)	(0.04)	(0.11)	(0.05)	(2.85)	(1.21)	(3.85)	(1.34)
other secular * OE * (normalised) number of nearby public schools	-0.29**				-0.96			
	(0.14)				(2.81)			
Catholic * OE * (normalised) number of nearby public schools	-0.20**				-4.08*			
	(0.08)				(2.46)			
Waldorf/Montessori * OE * (normalised) number of nearby public schools	-0.21*				-4.66			
	(0.11)				(2.98)			
other faith * OE * (normalised) number of nearby public schools	-0.05				0.54			
	(0.13)				(4.03)			
funding group 2 * OE * (normalised) number of nearby public schools		-0.17				3.99		
		(0.16)				(2.82)		
above median expenditure * OE * (normalised) number of nearby public schools			0.09				4.14	
			(0.09)				(3.37)	
above median price * OE * (normalised) number of nearby public schools				0.05				3.34**
				(0.07)				(1.46)
Observations	6,650	6,650	4,774	4,774	6,650	6,650	4,774	4,774
R-squared	0.92	0.92	0.93	0.93	0.92	0.92	0.93	0.93
Year, school and grade FE	YES	YES	YES	YES	YES	YES	YES	YES
School controls	YES	YES	YES	YES	YES	YES	YES	YES
Neighbourhood controls	YES	YES	YES	YES	YES	YES	YES	YES
Control for pre-policy trends	YES	YES	YES	YES	YES	YES	YES	YES

Notes: coefficients reported correspond to θ and θ_h from eq.3.11. Competition is measured with number of public schools within 5 km of a private school in columns (1)-(4) and with the normalised number of public schools within 5 km in columns (5)-(8). Dependent variable is enrollment per grade. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income, number of dwellings, population size, population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Effects of public school competition on private school revenues and expenditure per student, 2000-2007

	Gifts			Grants			Other revenue			Total revenue			Total expediture		
	3km	5km	7km	3km	5km	7km	3km	5km	7km	3km	5km	7km	3km	5km	7km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(14)	(15)	(16)
OE * Number of nearby public schools	0.03	0.02	0.01	-0.00	-0.00	0.00	0.01**	0.01**	0.01	0.00	0.00	0.00	0.01	0.00	0.00
	(0.03)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
OE * Normalised number of nearby public schools	0.84	0.65	0.41	-0.01	0.02	0.06	0.26**	0.31***	0.23*	0.09	0.09	0.07	0.07	0.05	0.07
	(0.56)	(0.44)	(0.37)	(0.09)	(0.08)	(0.14)	(0.12)	(0.11)	(0.13)	(0.08)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)
OE * Weighted count of nearby public schools	0.02	0.02	0.02	0.01**	0.01	-0.00	0.00	0.01	0.00	0.01**	0.01*	0.00	0.00	0.01	0.00
	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
OE * Normalised weighted count of nearby public schools	0.43	0.33	0.32	0.01	-0.00	0.00	0.09	0.17*	0.12	0.05	0.06	0.05	0.08	0.07	0.07
	(0.36)	(0.32)	(0.30)	(0.08)	(0.08)	(0.10)	(0.13)	(0.10)	(0.12)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)
Observations in each specification: 6,746															

Notes: Coefficients reported correspond to θ from eq 3.12. The competition indicator is measured as the number of public schools within either 3, 5 or 7 km of a private school, depending on the column. The weighted competition indicator weighs each nearby public school by the inverse distance and the inverse student capacity ratio. The normalization rescales the indicators for each grade to range from 0 to 1. Dependent variables are in natural logarithm per student. Gifts correspond to the sum of donations with or without a tax receipt. Grants include federal, provincial, and municipal government grants. Other revenue corresponds to total revenue net of gifts and grants. School control include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income, number of dwellings, population size, population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the nonprofit level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.8: Heterogeneous effects of public school competition on private school revenues and expenditures per student, 2000-2007

Panel A

	Gifts				Grants				Other revenue			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OE * normalised number of nearby public schools	0.33	0.64	0.20	0.92**	-0.05	0.02	-0.05	-0.09	-0.04	0.31***	-0.30	0.09
	(0.58)	(0.43)	(0.52)	(0.40)	(0.13)	(0.09)	(0.28)	(0.13)	(0.41)	(0.11)	(0.23)	(0.19)
other secular * OE * normalised number of nearby public schools	1.11*				-0.07				0.24			
	(0.57)				(0.16)				(0.40)			
Catholic * OE * normalised number of nearby public schools	0.19				0.08				0.38			
	(0.52)				(0.09)				(0.42)			
Waldorf/Montessori * OE * normalised number of nearby public schools	0.49				0.81				0.05			
	(1.65)				(1.23)				(0.57)			
other faith * OE * normalised number of nearby public schools	0.38				0.08				0.42			
	(0.85)				(0.29)				(0.42)			
funding group 2 * OE * normalised number of nearby public schools		0.20				-0.67				-0.16		
		(0.71)				(0.49)				(0.19)		
above median expenditure * OE * normalised number of nearby public schools			0.48				0.05				0.61***	
			(0.57)				(0.28)				(0.20)	
above median price * OE * normalised number of nearby public schools				-0.26				0.10				0.26
				(0.24)				(0.13)				(0.17)
Observations	666	666	649	649	643	643	627	627	674	674	657	657
R-squared	0.82	0.81	0.83	0.83	0.68	0.68	0.64	0.64	0.93	0.93	0.93	0.93

Panel B

	Total Revenue				Total Expenditure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OE * normalised number of nearby public schools	0.08	0.09	-0.13	0.03	0.17	0.05	-0.09	-0.15
	(0.15)	(0.08)	(0.11)	(0.11)	(0.13)	(0.07)	(0.10)	(0.15)
other secular * OE * normalised number of nearby public schools	0.12				-0.25			
	(0.15)				(0.17)			
Catholic * OE * normalised number of nearby public schools	-0.01				-0.10			
	(0.12)				(0.10)			
Waldorf/Montessori * OE * normalised number of nearby public schools	0.02				0.32*			
	(0.30)				(0.18)			
other faith * OE * normalised number of nearby public schools	-0.05				-0.19			
	(0.30)				(0.29)			
funding group 2 * OE * normalised number of nearby public schools		-0.03				0.00		
		(0.15)				(0.13)		
above median expenditure * OE * normalised number of nearby public schools			0.21***				0.12	
			(0.08)				(0.07)	
above median price * OE * normalised number of nearby public schools				0.06				0.19
				(0.08)				(0.12)
Observations	674	674	657	657	674	674	657	657
R-squared	0.96	0.96	0.96	0.96	0.96	0.96	0.96	0.96

Notes: coefficients reported correspond to θ and θ_h from specification for using eq.3.11 adapted to school-year data. Competition is measured with the normalised number of public schools within 5 km of a private school. Dependent variables are in natural logarithm per student. Gifts correspond to the sum of donations with or without a tax receipt. Grants include federal, provincial, and municipal government grants. Other revenue corresponds to total revenue net of gifts and grants. School control include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the nonprofit level in parentheses. *** p<0.01, ** p<0.05, * p<\$0.1.

3.10 Supplement 3

3.10.1 Control variables

As additional control variables for unobserved student background characteristics, I use mean characteristics from Census Enumeration or Dissemination Area (EA or DA, respectively) where each school is located. Postal code level controls include the number of proximate private schools. Details of the construction of these variables are provided below. School level controls include the proportion of peers who speak Chinese, Punjabi or other non-English home languages, who are Aboriginal and who are female. Details of the construction of these variables are provided below.

3.10.2 Coding of Neighborhood Characteristics

To proxy for the socioeconomic status of the student body of each private school, we match the school postal code to the most recent public-use estimates of neighborhood average income, share of recent immigrants, share with high school, share with some college, share with bachelors or more, from the 1996, 2001, and 2006 Census long-form. Statistics Canada publishes these variables 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 Areas 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. For the years between the Census years, I linearly interpolated the values of the neighborhood variables.

I 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). Postal codes are smaller than EAs/DAs, although they sometimes straddle multiple EAs or DAs. In these cases, I follow the PCCF+ methodology which uses population-weighted random allocation for many postal codes that link to more than one geographic area. The PCCF+ also includes the latitude and longitude of the postal code's centroid, which I use to compute the distance between each student's residence and nearby schools.

3.10.3 Coding of Proximate School Alternatives

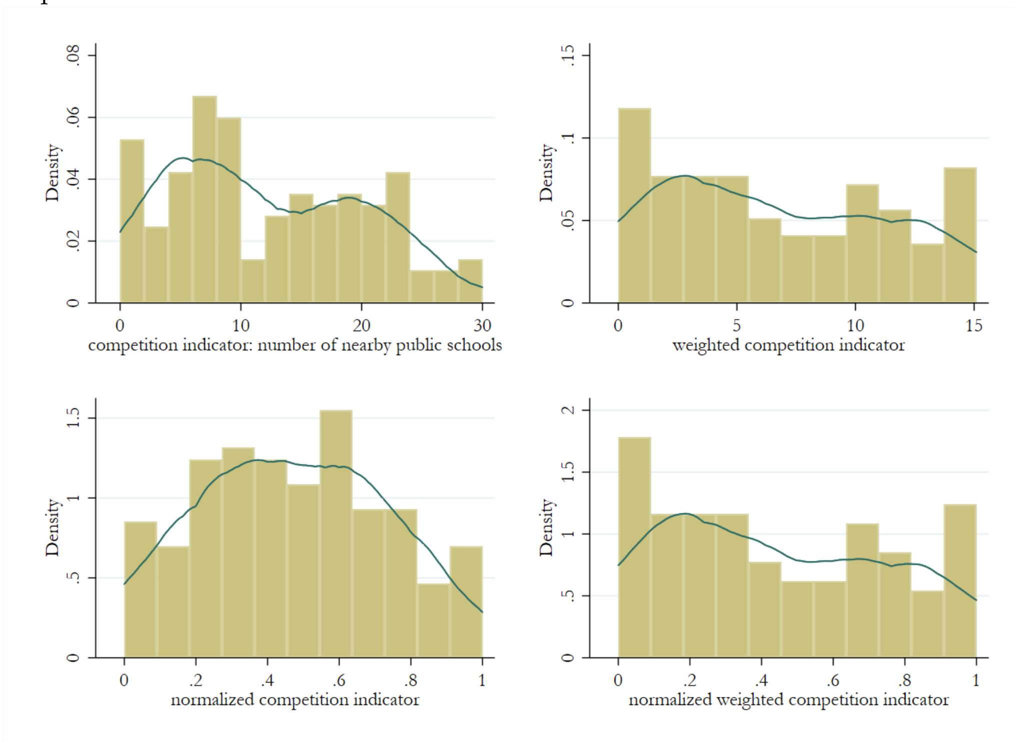
I obtained the address for all schools in BC from public sources and geocoded their locations using Here Maps API Hess (2015) and the PCCF+ to assign a latitude and longitude to each postal code in each year. Then I calculated the distance (in km) between the schools's location and all schools in our data set. These distances were calculated as driving distance according to Google Maps. For each private school postal code in each year, I then calculated the number of active public and private schools within a defined maximum travel distance.

3.10.4 Categorizing Private Schools

I categorized private schools into secular and faith categories according to the names of the school and the nonprofit organization. For example, schools categorized as "Other Christian" contained in their name any of the following words: Christian, Gospel, church, Lutheran, Mennonite.

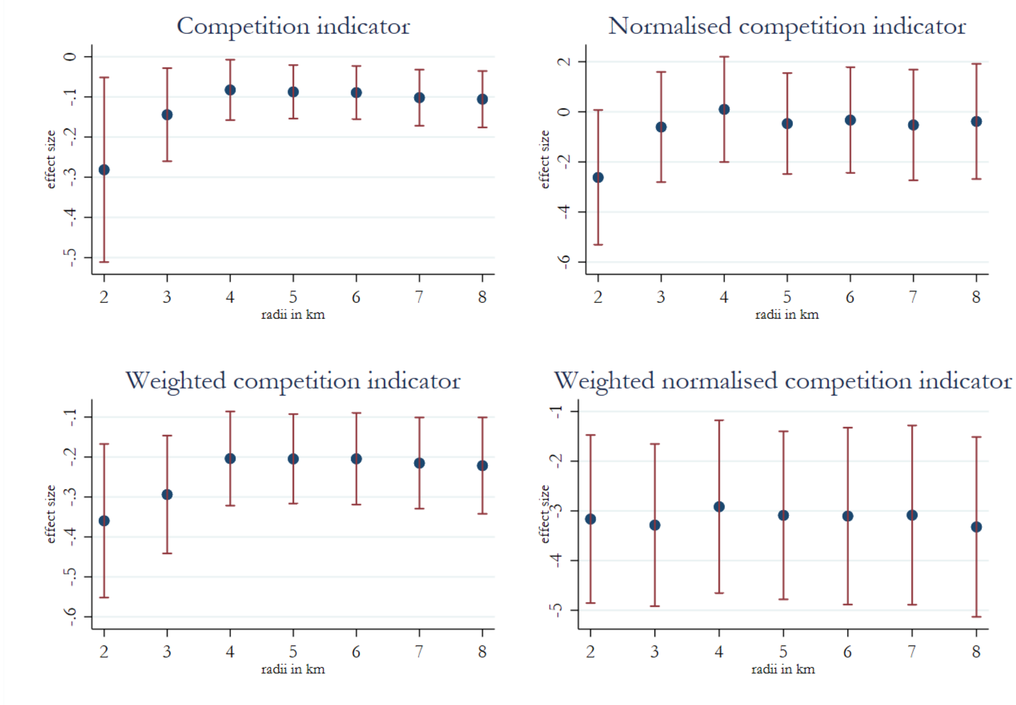
3.10.5 Supplemental Figures

Figure 3.13: Distribution of competition indicators using number of public schools within 5 km of a private school



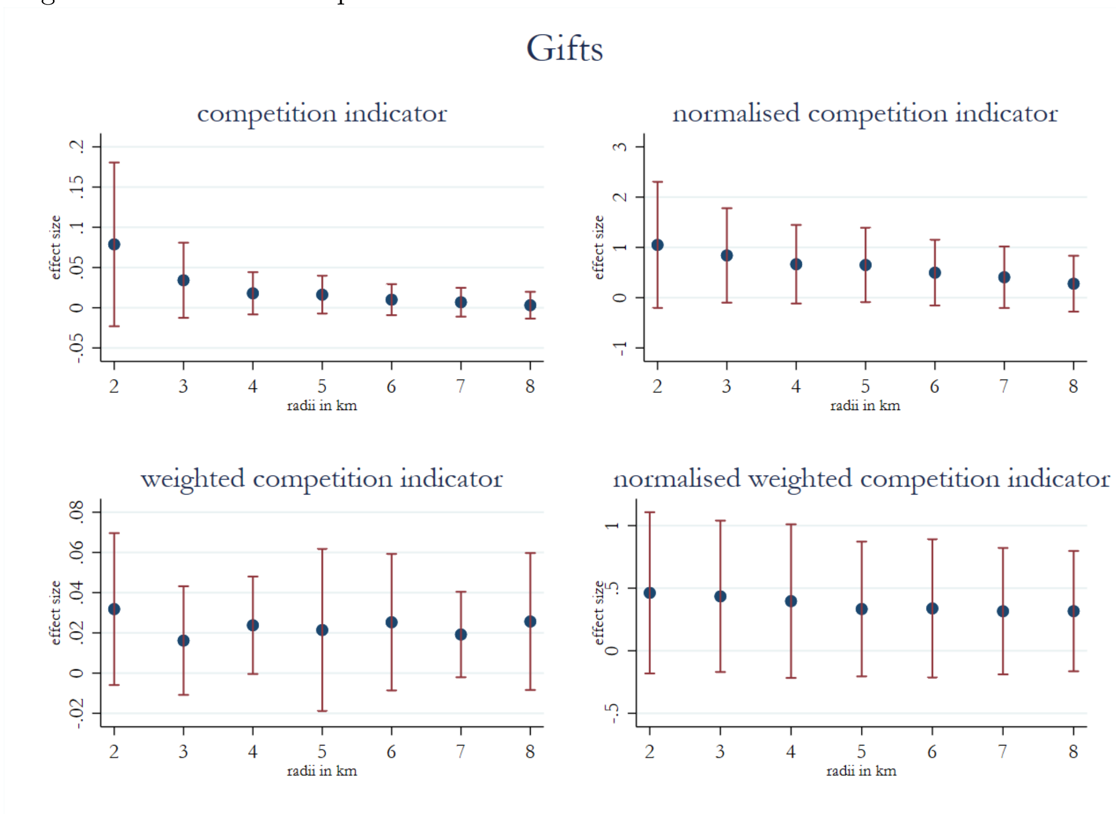
Notes: The mean number of public schools within 5 km is 11.9, the 25th percentile is 6, the median is 10, and the 75th percentile is 19. The weighted competition indicator is the number of public schools with 5 km weighted by inverse distance and inverse capacity. Its mean is 6.7, the 25th percentile is 2.5, the median is 5.8, and the 75th percentile is 10.7.

Figure 3.14: Estimated effects of public school competition on private school enrollment per grade using several radii



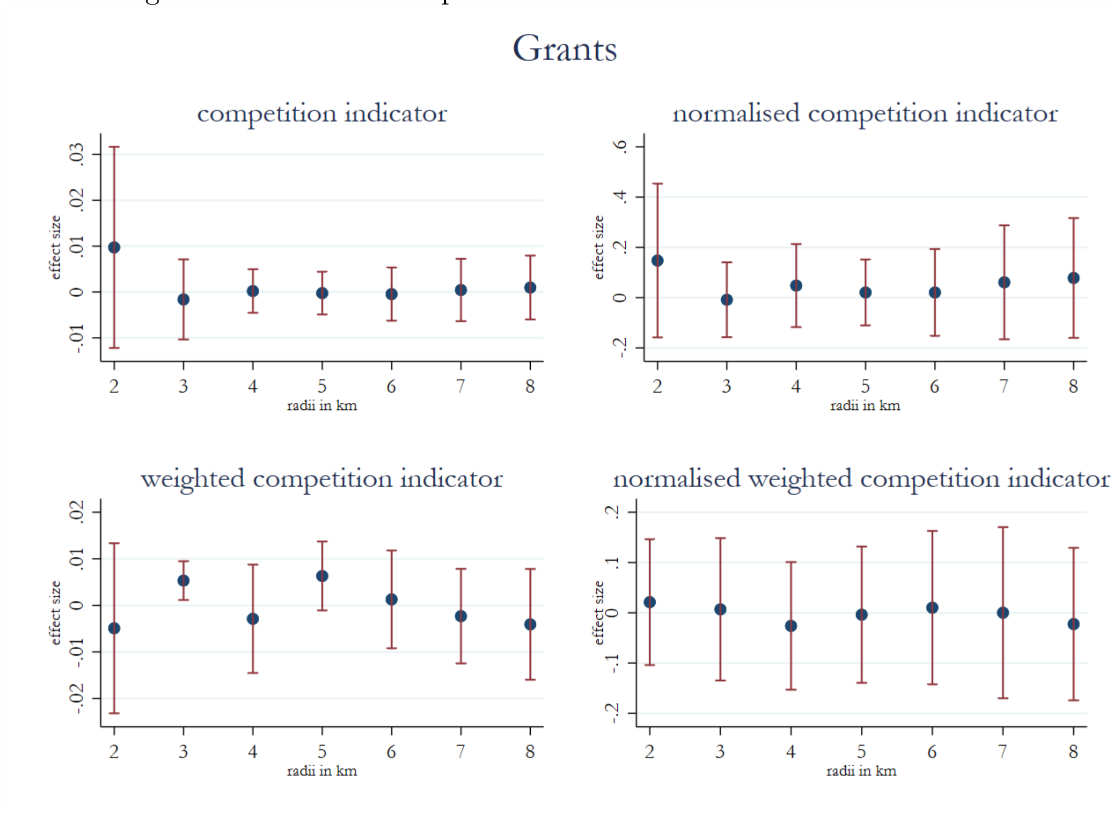
Notes: This figure shows the sensitivity of the effects on enrollment to the definition of the circle that determines the number of public schools near a private school. The figure plots θ based on eq. 3.8 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the school level. Specification includes school, grade, and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.15: Estimated effects of public school competition on private school gifts per student using several radii and competition indicators



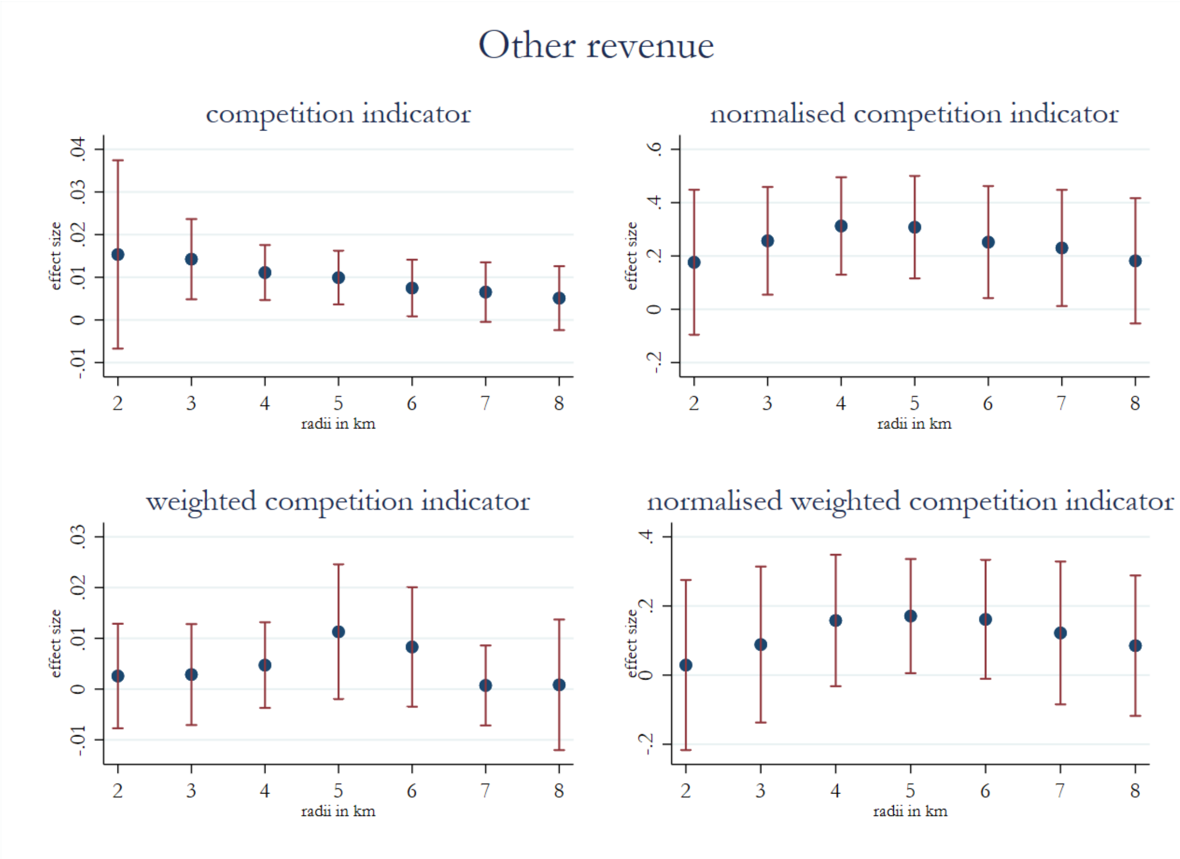
Notes: This figure shows the sensitivity of the effects on ln gifts per student to the definition of the circle that determines the number of public schools near a private school and to the competition indicator. The figure plots θ based on eq. 3.12 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Specification includes school and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.16: Estimated effects of public school competition on private school grants per student using several radii and competition indicators



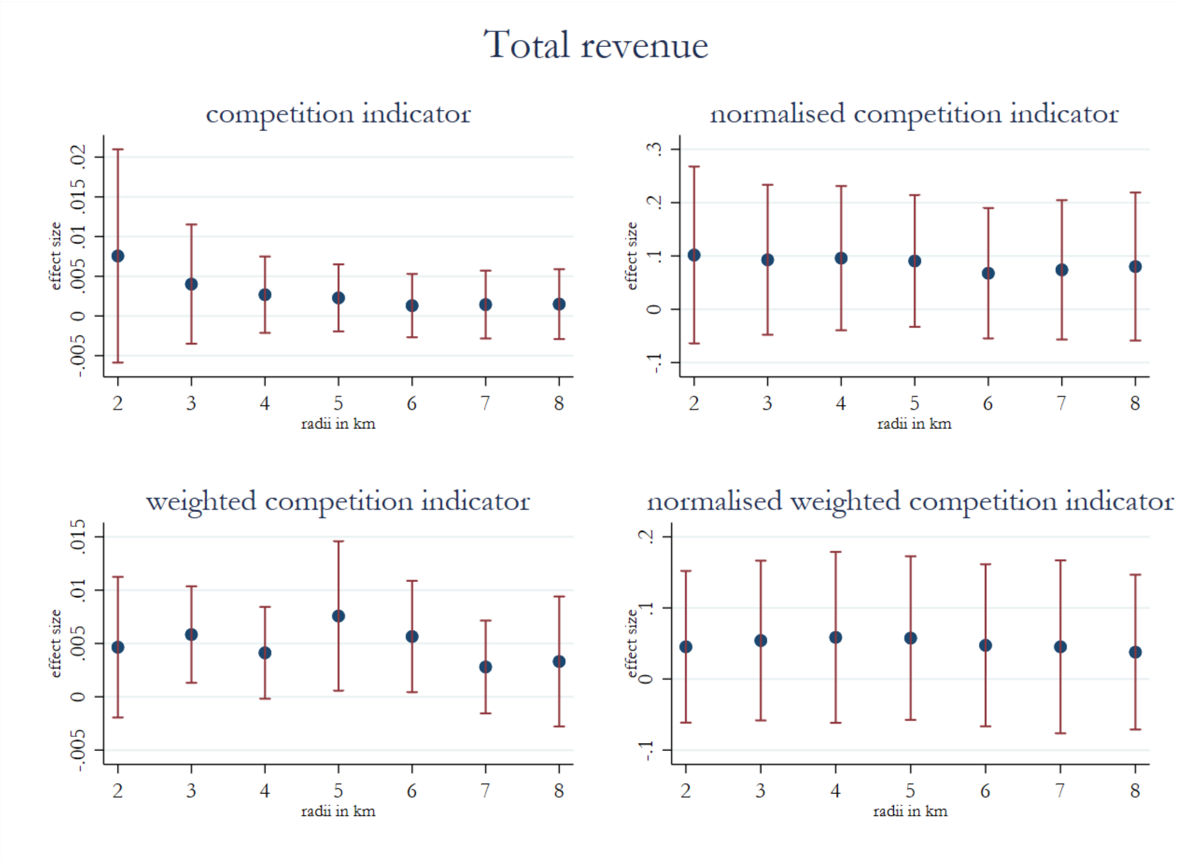
Notes: This figure shows the sensitivity of the effects on ln grants per student to the definition of the circle that determines the number of public schools near a private school and to the competition indicator. The figure plots θ based on eq. 3.12 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Specification includes school and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.17: Estimated effects of public school competition on private school other revenue per student using several radii and competition indicators



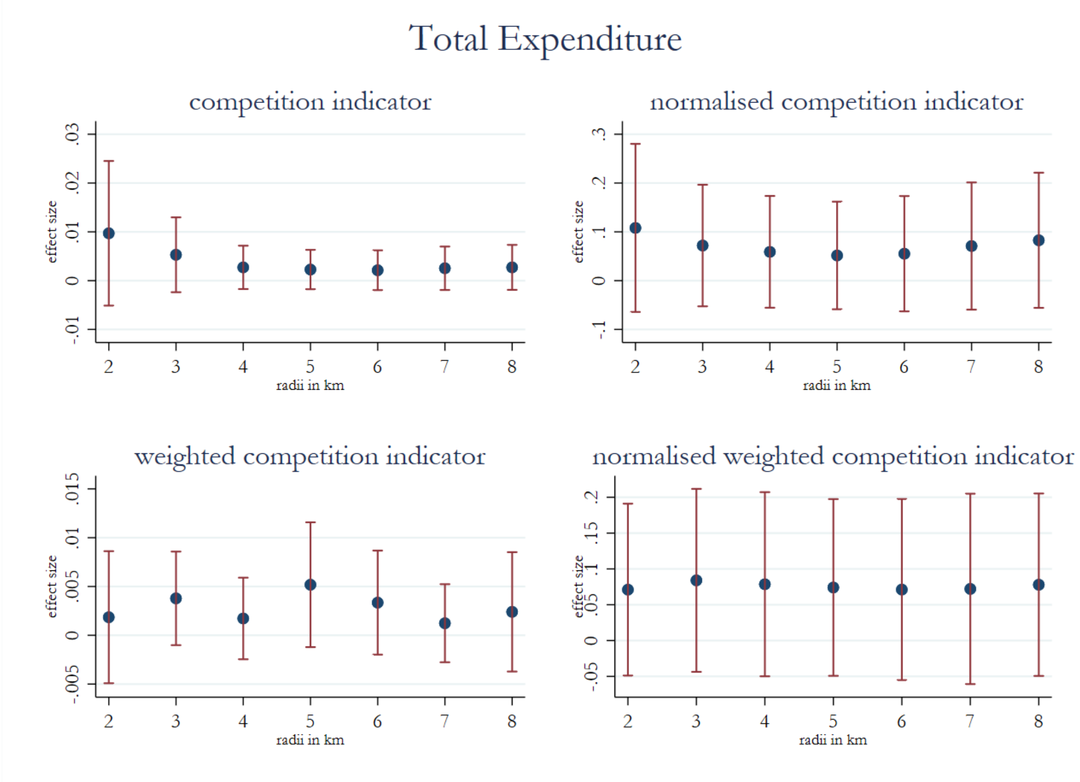
Notes: This figure shows the sensitivity of the effects on ln other revenue per student to the definition of the circle that determines the number of public schools near a private school and to the competition indicator. The figure plots θ based on eq. 3.12 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Specification includes school and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.18: Estimated effects of public school competition on private school total revenue per student using several radii and competition indicators



Notes: This figure shows the sensitivity of the effects on ln total revenue per student to the definition of the circle that determines the number of public schools near a private school and to the competition indicator. The figure plots θ based on eq. 3.12 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Specification includes school and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

Figure 3.19: Estimated effects of public school competition on private school total expenditure per student using several radii and competition indicators



Notes: This figure shows the sensitivity of the effects on ln total expenditure per student to the definition of the circle that determines the number of public schools near a private school and to the competition indicator. The figure plots θ based on eq. 3.12 for several radii (in km) of the circle around each private school. 90 percent confidence intervals are estimated with clustered standard errors at the nonprofit organization level. Specification includes school and year FE, pre-policy trend, school controls (share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of nearby private schools), and neighborhood controls (share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19).

3.10.6 Supplemental Tables

Table 3.9: Sample means of schools that are matched and unmatched with nonprofits in CRA data, 2000 - 2007

	(1) Matched	(2) Unmatched
school-grade-year level		
Enrollment per grade	37.76	27.8
Number of public schools within 5km	12.4	10.19
Number of public schools within 5km, weighted by distance and capacity ratio	7.03	6.02
Offers Kindergarten - grade3	0.41	0.44
Offers grades 4 - 7	0.39	0.34
Offers grades 8 - 12	0.2	0.22
Number of private schools within 5km	4.61	3.03
Funding Group 1	0.83	0.83
Funding Group 2	0.17	0.03
Share of Other Christian schools	0.19	0.47
Share of Other Secular schools	0.21	0.16
Share of Catholic schools	0.53	0.24
Share of Waldorf/Montessori schools	0.04	0.04
Share of Other Faith schools	0.03	0.09
Neighbourhood characteristics		
Share with high school or less	0.32	0.4
Share with trade or diploma	0.09	0.13
share with college degree	0.21	0.23
Share with bachelor or higher	0.27	0.15
Average income per family	81,730	68,091
Population	733	711
Share of recent immigrants	0.05	0.04
Observations	4,800	1,850

Notes: Neighborhood are defined according to Census enumeration or dissemination area where school is located. Matched and unmatched schools refer to schools that are matched or not to the date set with of registered charities that filed T3010 form with information returns to the CRA in the period from 2000 to 2007.

Table 3.10: Effect of public school competition on private school per grade enrollment by year

Year =	2003 (1)	2004 (2)	2005 (3)	2006 (4)	2007 (5)
1{Year} * Number of nearby public schools	-0.04	-0.03	-0.11**	-0.13**	-0.14**
	(0.03)	(0.04)	(0.04)	(0.06)	(0.07)
1{Year} * Normalised number of nearby public schools	-0.06	0.60	-0.82	-1.36	-1.04
	(0.87)	(0.96)	(1.44)	(1.85)	(2.08)
1{Year} * Weighted count of nearby public schools	-0.10*	-0.10*	-0.25***	-0.29***	-0.31***
	(0.05)	(0.06)	(0.07)	(0.10)	(0.11)
1{Year} * Normalised weighted count of nearby public schools	-1.45*	-1.49*	-3.79***	-4.43***	-4.65***
	(0.77)	(0.90)	(1.09)	(1.46)	(1.72)
Observations in each specification: 6,650					
Control for pre-policy trends	YES	YES	YES	YES	YES

Notes: coefficients correspond to $\theta_k$from eq. 3.9. Dependent variable is enrollment per grade. The competition indicator is measured as the number of public schools within 5 km of a private school. The weighted competition indicator weighs each nearby public school by the inverse distance and the inverse student capacity ratio. The normalization rescales the indicators for each grade to range from 0 to 1. Dependent variable is enrollment per grade. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.11: Heterogeneous effects of public school competition on private school revenues and expenditures per student, 2000-2007

Panel A

	Gifts				Grants				Other revenue			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OE * normalised weighted count of nearby public schools	-0.42	0.33	0.17	0.85**	-0.04	0.00	-0.16	-0.12	-0.07	0.18*	-0.68**	-0.17
	(0.93)	(0.32)	(0.54)	(0.39)	(0.16)	(0.09)	(0.27)	(0.15)	(0.50)	(0.10)	(0.33)	(0.29)
other secular * OE * normalised weighted count of nearby public schools					-0.07				0.10			
	(0.95)				(0.22)				(0.50)			
Catholic * OE * normalised weighted count of nearby public schools					0.03				0.25			
	(0.87)				(0.12)				(0.52)			
Waldorf/Montessori * OE * normalised weighted count of nearby public schools					0.51				-0.23			
	(1.78)				(1.38)				(0.63)			
other faith * OE * normalised weighted count of nearby public schools					0.06				0.26			
	(0.92)				(0.20)				(0.48)			
funding group 2 * OE * normalised weighted count of nearby public schools		0.02				-0.86				-0.25		
		(0.96)				(0.61)				(0.24)		
above median expenditure * OE * normalised weighted count of nearby public schools			0.24				0.15				0.87***	
			(0.55)				(0.26)				(0.28)	
above median price * OE * normalised weighted count of nearby public schools				-0.47*				0.12				0.39
				(0.27)				(0.14)				(0.25)
Observations	666	666	649	649	643	643	627	627	674	674	657	657
R-squared	0.82	0.81	0.82	0.82	0.68	0.68	0.64	0.64	0.93	0.93	0.93	0.93

Panel B

	Total Revenue				Total Expenditure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OE * normalised weighted count of nearby public schools	0.13	0.06	-0.17	0.01	0.27	0.07	-0.08	-0.06
	(0.18)	(0.07)	(0.10)	(0.11)	(0.17)	(0.07)	(0.12)	(0.14)
other secular * OE * normalised weighted count of nearby public schools					-0.35			
					(0.22)			
Catholic * OE * normalised weighted count of nearby public schools					-0.19			
	(0.15)				(0.13)			
Waldorf/Montessori * OE * normalised weighted count of nearby public schools					0.22			
	(0.29)				(0.18)			
other faith * OE * normalised weighted count of nearby public schools					-0.18			
	(0.20)				(0.19)			
funding group 2 * OE * normalised weighted count of nearby public schools		-0.09				-0.02		
		(0.18)				(0.17)		
above median expenditure * OE * normalised weighted count of nearby public schools			0.24***				0.16*	
			(0.06)				(0.08)	
above median price * OE * normalised weighted count of nearby public schools				0.05				0.14
				(0.08)				(0.11)
Observations	674	674	657	657	674	674	657	657
R-squared	0.96	0.96	0.96	0.96	0.96	0.96	0.96	0.96

Notes: coefficients reported correspond to θ and θ_h from specification for using eq.3.11 adapted to school-year data. Competition is measured with the normalised weighted count of public schools within 5 km of a private school. Dependent variables are in natural logarithm per student. Gifts correspond to the sum of donations with or without a tax receipt. Grants include federal, provincial, and municipal government grants. Other revenue corresponds to total revenue net of gifts and grants. School control include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, , with university without degree, who are recent immigrants; average family income; number of dwellings; population size; population by age groups 0-4, 5-9, 10-14, 15-19. Clustered standard errors at the nonprofit level in parentheses. *** p<0.01, ** p<0.05, * p<$0.1.

Table 3.12: Effect of public school competition on private school per grade enrollment by year, sample of schools matched with CRA data, 2000-07

Year =	2003 (1)	2004 (2)	2005 (3)	2006 (4)	2007 (5)
1{Year} * Number of nearby public schools	-0.01	-0.02	-0.12**	-0.17**	-0.14*
	(0.04)	(0.04)	(0.05)	(0.07)	(0.08)
1{Year} * Normalised number of nearby public schools	0.13	0.45	-1.08	-2.34	-1.45
	(1.09)	(1.22)	(1.64)	(2.05)	(2.30)
1{Year} * Weighted count of nearby public schools	-0.06	-0.08	-0.25***	-0.33***	-0.27**
	(0.06)	(0.07)	(0.08)	(0.11)	(0.13)
1{Year} * Normalised weighted count of nearby public schools	-0.86	-1.16	-3.81***	-5.06***	-4.15**
	(0.95)	(1.13)	(1.27)	(1.65)	(1.92)
Observations in each specification: 4,800					
Control for pre-policy trends	YES	YES	YES	YES	YES

Notes: Competition is measured in column (1) with number of public schools within 5 km of a private school. Competition indicator in columns (2) weights each public school within 5 km by the inverse distance and the inverse student capacity ratio. Dependent variable is enrollment per grade. School controls include share of Aboriginal students, share of female students, indicator for whether the school offers full-day kindergarten, and the number of private schools within 5 km of travel distance. Neighbourhood controls include: share of population with trade or diploma, with college degree, with bachelor's degree or higher, with university without degree, who are recent immigrants; average family income; number of dwellings; population size; and population by age groups 0-4, 5-9, 10-14, 15-19.Clustered standard errors at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (8) The sample used for those estimates include only schools that were matched to nonprofit information returns data.

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