LONG-TERM CONTEXTUAL EFFECTS IN EDUCATION: SCHOOLS AND NEIGHBORHOODS

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Abstract

To what extent do differences in educational outcomes across neighborhoods reflect discrepancies in local school quality? This paper decomposes total childhood exposure effects – the causal effect of growing up in a better area – into separate school and neighborhood components. To do so, it brings together two research designs, combining variation from one instrument that shifts school quality alone, and another that shifts both schools and neighborhoods. First, I implement a spatial regression-discontinuity design based on institutional rules that assign different default schools to students of different linguistic backgrounds to estimate school effects. Second, I study students who move across neighborhoods in Montreal during childhood to estimate total exposure effects by exploiting variation in the timing of moves. I focus on long-term educational attainment outcomes such as university enrollment, years of education, and timely secondary school graduation. I find that total exposure effects are large, and that between 50 and 70% of the long-term benefits of moving to a better area are actually due to access to better schools rather than to the neighborhoods themselves. *JEL* Codes: I24, J24, R23.

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I Introduction

Improving graduation rates and college attendance are high-priority objectives shared by community leaders, researchers and policymakers. Educational outcomes, however, vary greatly across regions, neighborhoods and schools. Given the sizable economic and nonpecuniary benefits to education, disparities in educational attainment can translate into persistent socio-economic inequality in adulthood.[1] Multiple policy interventions target neighborhoods directly or incentivize families to relocate to low-poverty areas, motivated by the belief that social context significantly influences students' aspirations and learning. Schools are key institutions of local communities and thereby plausibly constitute a pivotal mechanism fueling spatial inequalities. Yet, empirical evidence on the relative importance of schools and of neighborhoods for educational attainment remains scarce, despite the important implications of such information for the allocation of public resources towards policies directed at either schools or neighborhoods. For instance, in many jurisdictions, school enrollment is strictly residence-based, which makes these two dimensions observationally indistinguishable. Disentangling neighborhood and school effects is further complicated by sorting of families; identifying separate causal effects for schools and neighborhoods requires two sources of exogenous variation.

This paper examines the separate effects of schools and neighborhoods on long-term educational outcomes. In particular, I evaluate the long-term impact of growing up in a better area and calculate the fraction of the benefits that are driven by school quality. To do so, I combine unique student-level administrative data with several key institutional features of Quebec's education system to overcome the stringent data and institutional requirements that have hindered analyses of the separate contribution of schools and neighborhoods.[2] The large longitudinal database used here follows students who grew up in the region of Montreal throughout their entire educational career and tracks them on a yearly basis as they switch schools, move across neighborhoods, and make higher education investments.

My empirical framework brings together two research designs to develop a new approach that allows me to decompose *total exposure effects* – the combined effect of an additional year of exposure to a given neighborhood and to its schools – into school and neighborhood components. The analyses incorporate variation from one instrument shifting school quality alone (holding neighborhood quality constant) with another shifting both schools and neighborhoods simultaneously. First, institutional rules that assign different default schools to students based on their linguistic background are used to calculate the effect of attending a better school. Second, I adapt methods developed by Chetty and Hendren (2018*a*) to conduct a series of within-city across-neighborhood quasi-experiments that vary both school and neighborhood quality. More precisely, I compare students who made the same move – both from and to the same places – at different ages to pin down total exposure effects on a variety of measures of educational attainment, including university enrollment, graduating from secondary school on time, and number of years of education. Together, these research designs allow me to isolate the fraction of the benefits of moving to a better area that is due to access to schools of greater quality. The results indicate that these benefits are large and mostly driven by schools rather than by other neighborhoods characteristics.

[1] On the economic returns to education see Oreopoulos and Petronijevic (2013), Card (2001), and Angrist and Krueger (1991), and, specifically for Canada, Boudarbat, Lemieux and Riddell (2010). On non-pecuniary benefits, see Oreopoulos and Salvanes (2011) and Heckman, Humphries and Veramendi (2017).

[2] Identification of exposure effects requires longitudinal data, and separating the two contexts necessitates that both residence and school attended be observed in the data, and that the two dimensions don't perfectly overlap.

Quebec is particularly well suited for investigating the role of schools separately from neighborhoods. The province operates two parallel public school systems – one French and one English – thereby allocating neighbors to different default neighborhood schools on the basis of their mother tongue. Importantly, parents are allowed to opt out of these default options, breaking the deterministic link between schools and neighborhoods within language groups.[3] I exploit these assignment rules along with hand-collected geocoded data of primary school catchment areas to instrument for school quality in a spatial regression-discontinuity framework (RD-IV), leveraging the fineness of the spatial information in the administrative files. Here, I document an important role for schools independently of neighborhoods: students growing up on the side of a French primary school boundary associated with a relatively higher-quality default option are 3 percentage points more likely to enroll in university than their immediate neighbors on the opposite side of the boundary. Crucially, the catchment areas of English and French default schools are not same. This feature allows me to implement placebo tests confirming that the relevant boundaries do not coincide with discontinuous changes in non-school unobserved attributes.[4] The educational outcomes of students attending English schools exhibit no discontinuity around *French* primary school boundaries.

Next, I estimate the magnitude of total causal exposure effects by focusing on movers. To address the endogeneity of location decisions, I exploit variation in the *timing* of moves across families and focus on within-city moves to examine the role of schools.[5] Intuitively, if social context matters, the educational outcomes of movers should converge towards those of the permanent residents of their destination (children who always resided in the same area) with increasing time spent in that location. The reduced-form object of interest is a convergence rate. To insure that the estimates are not confounded by sorting into different areas, the model relies on comparisons between children who started in the same neighborhood and moved to the same neighborhood.[6] The identifying assumption is that the degree of selection into locations does not vary systematically with children's age at the time of the move. In support of this assumption, I show that my results hold up to a series of robustness checks, notably family fixed effects specifications and controlling for time-varying observables around the time of the move.

I find that movers' outcomes improve linearly with each year spent in a better location during childhood, where neighborhood quality is measured using the mean long-term outcomes of permanent residents. My estimates suggest that movers' educational attainment converges at an annual rate of about 4.5% towards the outcomes of the permanent residents of their destination neighborhood. Put differently, moving one year earlier to a place where permanent residents have 1 more year of education than those of one's origin location increases one's own educational attainment by 0.045 years. Extrapolating over 15 years of childhood, these effects account for about $\frac{2}{3}$ of the differences in outcomes of permanent residents between origin and destination. The magnitude of these effects is remarkably similar to that reported in Chetty and Hendren (2018*a*) despite important differences between the two settings, notably my emphasis on smaller geographic units and within-city variation.

[3] Private schools are widespread and relatively affordable in Montreal, generating more school and neighborhood independent variation, and further loosening the mechanical relationship between the two dimensions. Private school students are included in my database.

[4] Boundaries that coincide with major geographical features such as highways or canals are excluded.

[5] Aaronson (1998) and Weinhardt (2014) also use variation from movers to identify neighborhood effects. Chetty and Hendren (2018*a*,*b*) use rich tax data to track families with children who move across commuting zones and counties in the U.S. and estimate the causal effect of places on earnings. Similar identification strategies have also been used to analyze health care utilization (Finkelstein, Gentzkow and Williams, 2016), physician practice style (Molitor, 2018), the impact of EITC on labor supply (Chetty, Friedman and Saez, 2013), and brand preferences (Bronnenberg, Dubé and Gentzkow, 2012).

[6] The main empirical specification includes both origin-by-destination and age-at-move fixed effects.

Having established the presence of substantial contextual effects in Montreal, I then explore how much of the benefits of moving to better areas are driven by school quality. Key to this analysis is the fact that since school effects can be identified after conditioning on neighborhoods, the mean outcomes of permanent residents of a given neighborhood can be partitioned into a part reflecting the quality of local schools and a neighborhood "residual". Building on this insight, and with forecast-unbiased measures of school quality in hand (from the RD analysis), I separately estimate the effect of moving to a place with schools of greater quality and the effect of moving to a place with better (non-school) neighborhood amenities. I show that the total convergence rate is a weighted average of these two partial exposure effects and derive the mapping between the total rate and these other reduced-form parameters. Then, I calculate a restricted convergence rate for which the effect of moving to a place with schools of greater quality is set to zero. Comparing this restricted rate to the total rate that incorporates both school and neighborhood effects, I find that between 50% and 70% of the benefits of moving to a better area are due to access to better schools. Even in a context where students have the option to opt out of their local educational institutions, causal place effects are driven for the most part by schools rather than by neighborhoods themselves. Nonetheless, a small residual neighborhood exposure effect persists above and beyond the contribution of schools.

This paper brings together several literatures. First, it speaks directly to the classic question "Do neighborhoods matter?". Correlational analyses generally find strong associations between neighborhood poverty and success at school (Sharkey and Faber, 2014; Burdick-Will et al., 2011). In contrast, most experimental and quasi-experimental studies that tackle the challenging task of isolating "place" effects from non-random sorting of families into neighborhoods have found limited evidence of static neighborhood effects on educational and economic outcomes (Ludwig et al., 2013; Kling, Liebman and Katz, 2007; Oreopoulos, 2008, 2003; Jacob, 2004).[7] In a recent re-analysis of the Moving to Opportunity (MTO) experiment, Chetty, Hendren and Katz (2016) show that children do benefit from moving to better locations both in terms of earnings and college enrollment, but that these gains only materialize for youth who moved before the age of 13, consistent with cumulative exposure effects. Similarly, Chetty and Hendren (2018*a*,*b*) estimate large exposure effects for children moving across U.S. commuting zones. Given that school attendance is generally residence-based, these estimates of neighborhood exposure effects also reflect differences in local school quality (Altonji and Mansfield, 2014). My estimates of total exposure effects are consistent with this prior literature, but my main focus is on unpacking the role of schools as a mechanism. Accounting for the cumulative nature of long-term contextual effects, I demonstrate that neighborhood exposure effects operate mostly via schools rather than through neighborhoods themselves.

Second, my paper also relates to a parallel stream of research evaluating the causal impact of schools on educational and labor market outcomes. Large effects of attending a better school are found using quasi-experiments (Gould, Lavy and Paserman, 2004), lottery-based designs (Angrist et al., 2017; Deming et al., 2014; Dobbie and Fryer, 2015, 2011), and admission threshold rules (Pop-Eleches and Urquiola, 2013; Jackson, 2010). Using similar research designs, however, Abdulkadiroğlu, Angrist and Pathak (2014) and Cullen, Jacob and Levitt (2006) respectively find no positive effects of attending an elite school or of attending one's preferred school in a school choice program. My paper takes a different approach and instead exploits spatial discontinuities in the spirit of Black (1999). I show that the early schooling environment has a long-term impact: residing on the better side of a French primary school boundary at age 6 affects

[7] Notable exceptions include Goux and Maurin (2007) who find positive effects of neighborhood peers on the probability of repeating a grade using variation from public housing projects in France, and Gould, Lavy and Paserman (2011) who find significant effects of childhood conditions on adult outcomes in Israel.

educational outcomes measured more than 10 years later.

I also contribute to a growing body of research that contrasts the magnitude of school and neighborhood effects.[8] Historically, researchers have generally focused on either schools or communities, but a few recent review papers have speculated on the relative effectiveness of school and neighborhood interventions by comparing separate studies.[9] Fryer and Katz (2013) and Katz (2015) contrast results from the MTO experiment (Ludwig et al., 2013; Kling, Liebman and Katz, 2007), which induced low-income families to move to low-poverty neighborhoods, with the effects of the Harlem Children's Zone experiment, which combines both school-level and community-level interventions (Dobbie and Fryer, 2015, 2011). They conclude that school interventions are likely more effective than community programs for educational outcomes, a conclusion also reached by Oreopoulos (2012).[10] My paper adds to this evidence by directly separating school from neighborhood effects using two instruments simultaneously and shows that school quality goes a long way explaining *why* neighborhoods matter for educational attainment.

The methods used in this paper have several empirical benefits. Movers likely constitute a more diverse cross-section of the population than samples of experimental studies that focus on very disadvantaged households (Oreopoulos, 2003) or negatively-selected populations of lottery applicants (Chyn, 2016), contributing to the external validity of the results. Also, by using outcome-based measures of neighborhood and school quality, I circumvent the issue of choosing which observable characteristics to use to proxy for quality. For instance, school input measures and teacher observable characteristics often fail to predict effectiveness, despite the evidence that both schools and teachers have large causal effects on student outcomes (Dobbie and Fryer, 2013; Chetty, Friedman and Rockoff, 2014*b*; Rivkin, Hanushek and Kain, 2005; Hanushek, 1986).

The rest of the paper proceeds as follows. First, I describe the institutional context and the data in Section II. Then, to fix ideas and motivate the empirical analyses, I set up a conceptual framework in Section III, and present the associated econometric models in Section IV. The results are reported in Section V and a host of robustness checks are conducted in Section VI. Section VII concludes.

II Data and Background

II.A Quebec's Education System

Levels of education In Quebec, education is compulsory from age 6 to 16, and most children enroll in kindergarten at age 5. Children complete six years of primary education (grades 1-6), and then attend a secondary school for five more years (grades 7-11), until they obtain a secondary school diploma (*diplôme d'études secondaires* – DES), or equivalent qualifications. Grade repetition is common and over 20% of students drop out of secondary school before obtaining any degree.

[8] In sociology, Carlson and Cowen (2015) examine short-run variation in test score gains across schools and neighborhoods in Milwaukee, and Wodtke and Parbst (2017) explore how school poverty mediates neighborhood effects on math and reading tests in the PSID. Sykes and Musterd (2011) study how school characteristics mediate the relationship between neighborhood characteristics and test scores in the Netherlands.

[9] Papers that explicitly examine schools and neighborhoods separately include Card and Rothstein (2007) on the effect of segregation on test scores, and Billings, Deming and Ross (2016) on the formation of criminal networks.

[10] Rothstein (2017) finds that differences in quality of K-12 education (measured by test scores) account for little of the between-city differences in intergenerational mobility, while Card, Domnisoru and Taylor (2018) find that state- and county-level school quality was a key factor driving regional differences in upward mobility in the early 20th century. In contrast to these studies, my paper studies school quality as a within-city mechanism.

The higher education system differs considerably from standard North American systems. In Quebec, there is a sharp hierarchical distinction between *college* and *university*, the former being a pre-requisite for the latter.[11] After secondary school, most students enroll in college in either a pre-university (2 years) or a technical program (3 years).[12] Pre-university college degrees are categorized in three broad fields – social sciences, natural sciences, and arts – which are chosen at the time of applying to college. The typical student who obtains a pre-university college degree then enrolls in a 3-year bachelor degree program in university. As in college, students apply and are admitted directly to a specific university program. A college degree is a necessary condition for university admission, with few exceptions.

Figure A.1 shows the typical education course towards a bachelor degree in Quebec and in a standard North American system. No transition between levels of education in Quebec coincide with the age at which students transition in other educational systems. The number of years of education associated with a bachelor degree, however, remains the same.

In the empirical application, I measure neighborhood and school exposure up until the academic year a student is aged 15 on September 30, inclusive. All educational investments made after that point are considered outcomes.

School choice between sectors Quebec's education system possesses multiple elements of school choice that contribute to breaking the mechanical link between area of residence and school attendance. At the primary and secondary levels, two public school systems operate in parallel – one French and one English. Public schools are governed by schools boards, which are responsible for personnel, transportation, infrastructure, and the allocation of resources across schools. School boards are language-specific, and any place of residence falls within the territory of exactly one English and one French school board.[13] Importantly, the attendance zones of English and French schools are not the same. Hence, two neighbors with different mother tongues who both attend their nearest language-specific public school likely have school peers who originate from different neighborhoods. Access to instruction in English is restricted to anglophones born in Canada. This rule is strictly enforced and parents must obtain a certificate of eligibility before enrolling their child in an English school.[14] Note that language restrictions do not apply to post-secondary institutions.

In comparison with other Canadian provinces and the U.S., the private sector is widespread in Quebec, notably at the secondary school level. In Montreal, almost a third of all students attend a private secondary school. Private schools do not have attendance zones and are relatively accessible given that they are highly subsidized and that very few schools charge the maximum fee allowed by law (Lefebvre, Merrigan and Verstraete, 2011). Subsidized private schools are also subject to the language of instruction restriction.[15]

[11] Collegial institutions in general are informally known in Quebec as *Cégeps,* although only public colleges officially bear that name. Cégep is an acronym for *Collège d'enseignement général et professionnel.*

[12] Completing a college technical program in Quebec is roughly equivalent to a 2-year college degree in the U.S.

[13] Before 1998, school boards were religion-specific (Catholic or Protestant), but individual schools were still either French or English.

[14] In the language of the law, anglophones are students whose mother or father attended an English primary or secondary school in Canada. Under this rule, almost all immigrants are *de facto* forbidden to attend school in English. Exceptions to the rule are rare.

[15] Non-subsidized English schools are allowed to enroll non-English speaking students. However, these schools are uncommon and represent less than 1% of total enrollment (Duhaime-Ross, 2015). A minority of subsidized private schools have entrance exams, yet the vast majority of students taking such exams are admitted to their preferred school (Lapierre, Lefebvre and Merrigan, 2016).

School choice within sectors Quebec's open enrollment policy stipulates that parents have the right to enroll their child in the school of their choice (*libre choix*), subject to capacity constraints and the language restrictions described above. In practice, school boards assign children to default neighborhood schools, and parents who desire to enroll their child in a public school other than the one they are assigned to must fill in the relevant paperwork *at* the neighborhood school. Default options may induce two sets of parents living in the same area to enroll their children in different schools since catchment area boundaries often cut through neighborhoods.[16] These boundaries serve as the basis for a regression-discontinuity analysis described in Section IV.B.

Importantly, over the time period considered in this study, there existed no public information about relative primary school quality and performance such as rankings on outcome-based measures.[17] If enrollment exceeds capacity, priority is given to children residing in the school's catchment area and to siblings of children attending the school, and students opting-out of their assigned school are not eligible for school bus transportation. Other non-residence based admission criteria are used for elite magnet schools. The neighborhood school therefore acts as a default option, and catchment area boundaries as cost shifters. In my data, every neighborhood school enrolls at least some students residing outside its catchment area.[18]

II.B Data

The main source of data used in this paper consists of student-level administrative records provided by Quebec's Ministry of Education that cover all levels of education from primary school to university. Separate files from four different branches of the Ministry were matched using unique student identifiers. For each year students are enrolled in primary and secondary education, school attended, grade level, and the six-digit postal code of residence are recorded. Postal codes are very small geographic areas, generally equivalent to a block-face or a unique apartment building. One's postal code determines the default neighborhood schools (one English and one French). Catchment areas were manually geocoded on that basis.

In addition to the assigned neighborhood schools, I also calculated for each postal code the distance to the nearest catchment area boundary, distance to the nearest public school, associated Census Tract and Forward Sorting Area (FSA). FSAs are postal-code-based neighborhoods and constitute the main geographic unit of analysis.[19] All distances were calculated separately for the English and French public school systems. In Montreal, students reside in over 500 different Census Tracts and about 100 FSAs. For confidentiality reasons, school identifiers and six-digit postal codes are de-identified in the analytical dataset.

[16] For example, Appendix Figure A.2 shows that many census tracts overlap with more than one French primary school catchment area. In this paper, I focus exclusively on French primary school boundaries since English primary school boundaries are not as well defined. Some English schools offer different programs (e.g. English Core vs. Bilingual) and their catchment areas may vary by program. At the secondary school level, English public schools in Montreal do not have catchment areas, but French public schools do.

[17] All Montreal school boards strongly oppose public disclosure of rankings or quality indicators. Parents who decide to opt-out of their neighborhood primary school must either acquire their information from social networks or by visiting schools during open-house events. On the other hand, secondary school rankings are published yearly in mainstream media. In 2015, a well-known newspaper published partial rankings of Montreal public primary schools for the first time. The cohorts of students analyzed in this paper had left primary school many years before that event.

[18] One important reason why capacity constraints do not appear to be binding is that Quebec's school system was experiencing a decline in school-age population over the time period covered here, which notably led to several public school closures in the early 2000s.

[19] A FSA is defined by the first three digits of a postal code. Their boundaries do not necessarily overlap with census tract boundaries. FSAs are regularly used to operationalize neighborhoods in Canadian research (Card, Dooley and Payne, 2010).

Student demographics – age on September 30, gender, mother tongue, country of origin, language spoken at home – are included, in addition to time-varying variables such as school day care use (primary school only) and an indicator of whether a student is currently considered to have learning difficulties (primary and secondary school).[20] In addition, I append neighborhood characteristics from the 2001 Canadian Census using students' census tract of residence.

In terms of long-term educational outcomes, the data include enrollment and graduation information in secondary school as well as for all vocational, college, and university programs. I use these to calculate – among other outcomes – university enrollment, timely secondary school graduation, and number of years of education. More detailed information regarding the construction of the outcome variables is provided in the Data Appendix.

The sample is focused on residents of the Island of Montreal, Quebec's most populous region and main urban center. This territory fully includes the city of Montreal and a few smaller municipalities that are either located in the suburban westernmost part of the Island or enclaved within the city of Montreal.[21] The Island of Montreal encompasses three francophone and two anglophone school boards.

Administrative records were obtained for five cohorts of students who started primary school between 1995 and 2001, following students until the 2014-2015 academic year.[22] The sample consists of all students who resided on the Island of Montreal at the time of entering grade 1 (100,929 students). This selection rule conditions on a common starting point, and therefore excludes students who moved to Montreal after completing grade 1 elsewhere.[23] The main sample (92,764 students) excludes all students who left Quebec's education system before turning 16.[24]

II.C Descriptive and Summary Statistics

Descriptive statistics for the main sample are shown separately by mobility status in Table 1. Permanent residents are defined as those who, by the age of 15, had always resided in the same FSA (44,912 students). I distinguish between movers who were still living in Montreal by age 15 (31,525 students), and those who had moved off the Island but remained in the province. Because of the within-city focus of this paper, students who left Montreal are excluded from the empirical analyses. Note that residential mobility is greater within than across cities – for example, while less than 50% of students in my sample qualify as permanent residents, over 80% of families in Chetty and Hendren (2018*a*) do.

In Montreal, students are on average 6 years old when they enter primary school. Only half the sample consists of native French speakers, but 75% of students attend school in French. *Allophones* – defined as

[20]On any given year in primary and secondary school, students with social maladjustment or learning disabilities can be identified as being "in difficulty". School boards receive extra funding to support these students, and many observers worry that schools may 'over-diagnose' students as a result. Yet, the predictive power of this variable with respect to educational attainment is stunning. The probability that one obtains a secondary school diploma on time (five years after starting secondary school) decreases monotonically with each year flagged in difficulty (Figure A.3). For the earliest cohorts, the probability of obtaining a bachelor degree is 36% for students never identified in difficulty, while it is only 5% among those who were flagged once or more.

[21]These administrative divisions are irrelevant for school resources administration purposes.

[22]Data for the 1997 and 1999 cohorts are not available.

[23]While many families with school-age children move from Montreal to suburban areas outside of the Island, fewer move in the opposite direction. Also, by focusing on students who started primary school in Quebec, most international immigrants who arrived at age 7 or older are excluded.

[24]In primary and secondary school, attrition is generally due to students leaving the province. In exceptional cases, some students may disappear from administrative records if they attend illegal schools, or are home schooled. Students leaving the system are disproportionately non-French speakers and immigrants. See the Data Appendix for more details on attrition.

individuals whose mother tongue is neither French nor English – make up almost a third of the sample. Nevertheless, the vast majority of students was born in Canada (90%). Anglophones are overrepresented among permanent residents, while francophones are disproportionately more likely to move outside of Montreal, and allophones to move within Montreal.

At baseline (in grade 1), 4% of students are considered to have learning difficulties (flagged "in difficulty"), and the fraction increases sharply over time. By the time they reach the age of 15, almost a third of the sample will have been flagged at least once. In general, movers appear to be negatively selected: In first grade, 3% of permanent residents are in difficulty, while 5% of movers are. At age 15, one permanent resident out of four has been flagged at least once, while more than a third of movers have.

The number of years for which I track students varies across cohorts, hence observed educational attainment will be higher for earlier cohorts, by construction. Appendix Table A.1 reports summary statistics for some educational outcomes separately by cohort.[25] Roughly 76% of students obtain a secondary school diploma (*DES*), but only 61% do so on time (in five years), with little variation across cohorts. The college enrollment rate is consistent across cohorts, at 70%. In terms of university-level outcomes, as of 2015, 46% of students who started primary school in 1995 had enrolled in university and 28% had completed a bachelor degree. Virtually no student of the 2001 cohort has a bachelor degree yet, but 22% of them are enrolled in university. Every econometric model in this paper includes cohort fixed effects to account for these differences.

Educational attainment varies dramatically across neighborhoods of Montreal. Figure I maps differences in mean educational attainment of permanent residents across FSAs.[26] The gap between neighborhoods with best and worst outcomes is abysmal, with local fractions of students completing high school on time ranging from 32% to 92%. The gap grows even larger in terms of university enrollment, with a minimum rate of 15% and a maximum of 80%. Even starker disparities emerge across census tracts (Figure A.7).

School attendance Given the variety of school choice options available in Montreal, students living in the same neighborhood need not attend the same school. For instance, at baseline, students living in the average FSA attend as many as 57 different primary schools.[27] When entering grade 1, 63% of students in French schools attend their neighborhood school and 50% of students in English schools do so. In total, 41% of students opt-out of their default option at baseline. By the end of secondary school, this proportion exceeds 70%.[28] Opt-out rates vary between the primary and secondary school levels primarily because of differences in availability of private school options. Around 12% of Montreal students are in the private sector in primary school, and that proportion rises to almost 30% in secondary school (Figure A.6). Yet, geography remains an important factor for many parents when it comes to deciding which school their child will attend. For example, among students in French schools at baseline, 68% attend their default school if that school is the nearest French public school from their house, while only 50% do so if it is not.[29]

To examine whether default options affect enrollment, I randomly pick one neighborhood school for each

[25] These statistics exclude almost 1,000 individuals who enroll in a Quebec post-secondary institution at some point, but who had left the primary and secondary school system before turning 16 and therefore are excluded from the main sample.

[26] The values of $\bar{y}_n^{PR}$ are adjusted for cohort differences. More precisely, these mean-outcomes are neighborhood fixed effects from a regression of outcome y on neighborhood and cohort fixed effects. The fixed effects are then re-centered so that their average is equal to the unconditional mean $\bar{y}$.

[27] See Appendix Figure A.5 for distribution of FSAs by number of schools.

[28] By definition, students in English secondary schools all opt-out since English public schools do not have attendance zones at the secondary level. At the age of 15, 58% of students in French schools attend a school other than their default option.

[29] For 30% of students in French schools the default option is not the nearest French public primary school.

boundary and plot the probability of attending that school as a function of distance to the nearest boundary. Figure A.4 shows the discontinuity in attendance for students enrolled in French primary schools at baseline.[30] Students at positive distances are residing in the catchment area of the randomly chosen school. On the left side of the border (negative distances), 20% of students attend the school located on the other side, rather than their own default option or any other French school. Despite the open-enrollment policy, there is a large discontinuity in attendance rates at the border, suggesting that many parents passively select the default option. This is consistent with a body of evidence in behavioral economics and psychology on the importance of default options (Chetty, 2015; Lavecchia, Liu and Oreopoulos, 2014).

III Conceptual Framework

This section lays out a model of human capital accumulation that incorporates both cumulative neighborhood and school effects, expanding the framework of Chetty and Hendren (2018*b*) to allow for multiple contextual inputs. It first describes the outcomes of permanent residents and movers parsimoniously in terms of the parameters of an education production function. The model is then used to clarify the interpretation of reduced-form estimates of exposure effects. I then discuss how a decomposition of total exposure effects can be achieved. The econometric specifications used to implement this decomposition are presented in Section IV.

Education production function Consider a general framework in which educational investment in children takes place over compulsory schooling years (up to year A) and a long-term outcome is realized and measured after the investment years. The education production function is cumulative and separately additive in family, school, and neighborhood (non-school) inputs:

$$y_i = \sum_{a=0}^{A} \left[\lambda \mu_{n(i,a)} + \omega \psi_{s(i,a)}\right] + A\tilde{\theta}_i$$

where y_i is a measure of educational attainment for student i, $n(i,a)$ denotes the neighborhood in which the student resided at age a, $s(i,a)$ the school she attended that year, and $\tilde{\theta}_i$ are annual average family inputs. Neighborhood and school quality are denoted by variables $\mu_{n(i,a)}$ and $\psi_{s(ia,)}$, and parameters λ and ω respectively represent the causal effect of one year of exposure to better non-school neighborhood amenities and the causal effect of attending a better school for one year.[31] For ease of exposition, I collapse the sum of school inputs into annual averages, with $\tilde{\psi}_{s(n(i))}$ denoting average school quality for years during which student i resided in neighborhood n. The production function can then be written as the sum of inputs received while living in each location:

[30] Since attendance zones for English schools are not as well defined as for French schools, I focus on French boundaries.

[31] To keep the model tractable, I do not explicitly include disruption costs associated with moving or switching school in the production function. The empirical model developed in Section IV accounts for any age-variant disruption costs with the inclusion of age-at-move fixed effects.

$$y_i = \sum_n a_{in} \left[\lambda\mu_n + \omega\tilde{\psi}_{s(n(i))}\right] + A\tilde{\theta}_i$$

where a_{in} is the number of years student i resided in location n and $\sum_n a_{in} = A$. Note that average school quality $\tilde{\psi}_{s(n(i))}$ remains indexed by i because students living in the same area can attend different schools.

School effects To isolate school effects from any neighborhood-related variation, I focus on the subsample of **permanent residents (PR)** – children who always resided in the same place. For these students, $a_{ik} = A$ for neighborhood n, and $a_{ik} = 0$ for all other locations $k \neq n$. Their educational outcomes simplify to $y^{PR}_{n(i)} = A\lambda\mu_n + A\omega\tilde{\psi}_{s(n(i))} + A\tilde{\theta}_i$, and neighborhood-level mean outcomes of PRs are

$$\bar{y}^{PR}_n = A\left[\lambda\mu_n + \omega\bar{\psi}^{PR}_n + \bar{\theta}^{PR}_n\right] \tag{1}$$

where $\bar{\psi}^{PR}_n = E\left[\tilde{\psi}_{s(n(i))}|n(i,a) = n\forall a\right]$ is the average annual school input of permanent residents of location n, and $\bar{\theta}^{PR}_n = E\left[\tilde{\theta}_i|n(i,a) = n\forall a\right]$ their average family input. In practice, school quality $\tilde{\psi}_{s(n(i))}$ and neighborhood quality μ_n are unobserved. Hence, I partition educational attainment into measurable school-related and neighborhood-related terms, as well as an idiosyncratic residual ν_i that is unrelated to either schools or neighborhoods:

$$\bar{y}^{PR}_{n(i)} = \Omega_{s(n(i))} + \Lambda_n + \nu_i$$

where $\Omega_{s(n(i))}$ reflects both cumulative causal school effects over student i's childhood $A\omega\tilde{\psi}_{s(n(i))}$, as well as average sorting into schools, and Λ_n is defined accordingly for neighborhood non-school amenities. Put differently, $\Omega_{s(n(i))}$ is a biased measure of true school effects $A\omega\tilde{\psi}_{s(n(i))}$ because it incorporates the partial correlation between school quality and parental inputs.

Let π denote the fraction of the effect of $\Omega_{s(n(i))}$ on $y^{PR}_{n(i)}$ reflecting causal variation. My first empirical objective is to obtain a consistent estimate of π to fix measures of predicted gains $\Omega_{s(n(i))}$ by properly deflating them.[32] This can be achieved by using a valid instrumental variable that exogenously shifts school quality $\tilde{\psi}_{s(n(i))}$ independently of neighborhood quality ("first-stage") and that is uncorrelated with parental inputs ("exclusion restriction"). Note that an OLS regression of $y^{PR}_{n(i)}$ on $\Omega_{s(n(i))}$ and a set of neighborhood fixed effects yields a coefficient on $\Omega_{s(n(i))}$ of one, by construction. In contrast, when instrumenting for $\Omega_{s(n(i))}$, the regression coefficient obtained reflects only the proportion of the effect of a one-unit change in $\Omega_{s(n(i))}$ that is due to true school effects and thereby corresponds to π. Forecast-unbiased measures of predicted gains can then be recovered using $\pi\Omega_{s(n(i))}$ (Chetty, Friedman and Rockoff, 2014*a*). Similarly, $\pi\bar{\Omega}^{PR}_n$ is a forecast-unbiased measure of the average cumulative causal school effects for PRs of neighborhood n, $A\omega\bar{\psi}^{PR}_n$.

[32] More details about the interpretation of π are provided in the Mathematical Appendix.

Identifying total exposure effects The total effect of growing up in a given area incorporates both school and non-school neighborhood inputs. To obtain causal estimates of these *total exposure effects*, I rely on **movers.** For one-time movers, let $o(i)$ denote the origin neighborhood of mover i, $d(i)$ denote the destination, and m_i the age at which student i moved. For these students, $a_{io} = m_i - 1$, $a_{id} = A - (m_i - 1)$, and $a_{ik} = 0 \ \forall k \neq o, d$. Their educational attainment is given by

$$y_i = A\left[\lambda\mu_{d(i)} + \omega\tilde{\psi}_{s(d(i))} + \tilde{\theta}_i\right] - (m_i - 1)\underbrace{\left[\lambda\left(\mu_{d(i)} - \mu_{o(i)}\right) + \omega\left(\tilde{\psi}_{s(d(i))} - \tilde{\psi}_{s(o(i))}\right)\right]}_{\text{Total exposure effects } (e_i(o,d))}. \tag{2}$$

Equation (2) highlights that the long-term outcomes of movers depend on the quality of schools and neighborhoods in both places as well as on the length of *exposure* to each place, which varies with age-at-move. Total exposure effects $e_i(o, d)$ are the gains of living in and attending schools of area d for one year relative to area o. Unfortunately, terms incorporated in $e_i(o, d)$ are unobservable. To take equation (2) to the data, it is useful to re-write it as a function of variables that can be readily measured. For instance, since permanent residents' mean outcomes are a function of the same contextual inputs, the total effect of living one year in area d relative to area o is directly related to the outcomes of PRs in both locations. Substituting the difference in outcomes between permanent residents of neighborhoods d and o, $\Delta\bar{y}_{od} = \bar{y}_d^{PR} - \bar{y}_o^{PR}$, into equation (2):

$$e_i(o, d) = \frac{1}{A}\Delta\bar{y}_{od} + \omega\left[\left(\tilde{\psi}_{s(d(i))} - \tilde{\psi}_{s(o(i))}\right) - \left(\bar{\psi}_d^{PR} - \bar{\psi}_o^{PR}\right)\right] - \left(\bar{\theta}_d^{PR} - \bar{\theta}_o^{PR}\right). \tag{3}$$

Positive exposure effects imply that the cumulative gains of moving to a $\Delta\bar{y}_{od}$-unit better area should grow (shrink) with the amount of time spent the destination (with age-at-move). Empirically, the magnitude of annual exposure effects can be assessed by comparing students who moved from the same origin to the same destination at different ages. Intuitively, if d is "better" than o, then a student who moved at age 9 is expected to have better outcomes than her peer who made the same move at age 12 since she will have been exposed to the better area for three more years.[33]

A reduced-form object of interest is the rate at which movers' outcomes *converge* towards those of the permanent residents of their destination with the number of years of exposure to that location, which can be estimated by regressing movers' outcomes y_i on the interaction term $(m_i - 1) \times \Delta\bar{y}_{od}$. Equation (3) indicates that the magnitude of this convergence rate will depend on the degree of sorting of permanent residents (e.g. the extent to which variation in $\Delta\bar{y}_{od}$ reflects differences in $\bar{\theta}_d^{PR} - \bar{\theta}_o^{PR}$). Greater sorting of permanent residents translates into smaller convergence rates.[34] Also, the size of the estimates increases with the propensity of movers to attend schools of comparable quality to those attended by permanent residents in their origin and destination. In this sense, the estimated coefficients can be interpreted as

[33] If individual inputs adjust in response to changes in other inputs, then the effect of moving a student across neighborhoods should be interpreted as a policy effect which encompasses parental responses (Todd and Wolpin, 2003). For instance, prior research suggests that parental effort and school quality are treated as substitutes (Pop-Eleches and Urquiola, 2013; Houtenville and Conway, 2008).

[34] This is under the assumption that families with high unobservable characteristics select into better schools and neighborhoods: $Cov(\lambda\mu_n + \omega\bar{\psi}_n^{PR}, \bar{\theta}_n^{PR}) > 0$. If the parents of students with low family inputs are more likely to sort into better schools and neighborhoods, then the convergence rate increases with sorting of permanent residents.

intention-to-treat (ITT) effects, with $E\left[\frac{\tilde{\psi}_{s(d(i))}-\tilde{\psi}_{s(o(i))}}{\bar{\psi}_d^{PR}-\bar{\psi}_o^{PR}}\right]$ representing the relevant compliance rate. Under full compliance, i.e. $\left(\tilde{\psi}_{s(d(i))}-\tilde{\psi}_{s(o(i))}\right)=\left(\bar{\psi}_d^{PR}-\bar{\psi}_o^{PR}\right) \quad \forall i$, and no sorting of permanent residents, the convergence rate is equal to $\frac{1}{A}$. A non-zero convergence rate indicates that there are benefits to moving to a better area, but does not necessarily imply that neighborhoods matter independently of schools. If neither schools nor neighborhoods matter (i.e. $\lambda=\omega=0$), then the convergence rate is zero.

Decomposing total exposure effects Total exposure effects encompass both the changes in school and non-school neighborhood inputs experienced by movers. Isolating the part of $\Delta\bar{y}_{od}$ that reflects causal school effects and rearranging equation (3) accordingly yields

$$
\begin{aligned}
e_i(o,d) = & \frac{1}{A}\pi\Delta\Omega_{od}+\omega\left[\left(\tilde{\psi}_{s(d(i))}-\tilde{\psi}_{s(o(i))}\right)-\left(\bar{\psi}_d^{PR}-\bar{\psi}_o^{PR}\right)\right] \\
& +\frac{1}{A}\Delta\bar{y}_{od}^{-s}-\left(\bar{\theta}_d^{PR}-\bar{\theta}_o^{PR}\right)
\end{aligned} \tag{4}
$$

where $\pi\Delta\Omega_{od}=\pi\left(\bar{\Omega}_d^{PR}-\bar{\Omega}_o^{PR}\right)$ and $\Delta\bar{y}_{od}^{-s}\equiv\Delta\bar{y}_{od}-\pi\Delta\Omega_{od}$. With measures of $\Delta\Omega_{od}$ and π in hand, one can estimate separate convergence rates for (a) moving to an area with $\pi\Delta\Omega_{od}$-unit better schools, and for (b) moving to an area with $\Delta\bar{y}_{od}^{-s}$-unit better outcomes associated with non-school neighborhood amenities.

The extent to which schools are directly driving total exposure effects can be assessed by calculating a *restricted* convergence rate for which the school channel has been shut down. I achieve this by setting the school-specific convergence rate to zero and calculating the associated residual convergence rate using the appropriate correspondence. I can then examine the fraction of total exposure effects that operate through schools by comparing the resulting restricted convergence rate with the benchmark total rate that encompasses both school and neighborhood effects.

The education production function used in this paper has several restrictions. Firstly, neighborhood and school effects are both linear in years of exposure.[35] This assumption appears to be supported by the data (see Section V). The model also rules out complementary between schools and neighborhoods. I provide evidence that there is no systematic interaction between school and neighborhood quality in my data in the next section. Also, additive separability of schools and neighborhoods is relatively standard in the literature (Gibbons, Silva and Weinhardt, 2013; Card and Rothstein, 2007), and is consistent with results from the Harlem Children's Zone (Fryer and Katz, 2013). Finally, school and neighborhood effects are assumed to be constant across students. This assumption is common to most work on school (Deming, 2014), teacher (Chetty, Friedman and Rockoff, 2014*b*) and college (Hoxby, 2015) value-added.

IV Empirical Roadmap

This section presents the econometric specifications used to obtain the empirical objects necessary to implement a decomposition of exposure effects.

[35] Angrist et al. (2017), Dobbie and Fryer (2013), Abdulkadiroğlu et al. (2011) and Autor et al. (2016) also assume that school effects are proportional with number of years. Chetty and Hendren (2018*a*) make a similar assumption for place effects.

IV.A Schools and neighborhoods: Measurement

To obtain measures of $\Omega_{s(n(i))}$ and Λ_n, I estimate a simple two-way fixed effects model on the subsample of permanent residents. The estimating equation is

$$y_{insc} = \delta_n + \boldsymbol{\delta}_{s(i)} + \delta_c + \epsilon_{insc} \tag{5}$$

where y_{incs} is a long-term educational outcome for student i from cohort c, living in neighborhood n and attending the set of schools $s(i)$. The model includes cohort (δ_c), FSA (δ_n) and school ($\boldsymbol{\delta}_{s(i)}$) fixed effects. Intuitively, this model is identified because the set of students living in the same area attend a variety of different schools, and students in the same school reside in different neighborhoods.[36] Since students generally attend two different schools during childhood – one primary and one secondary school – I parameterize the vector of school effects to include a fixed effect for primary school attended at baseline (δ_s^P) and a fixed effect for secondary school attended at age 15 (δ_s^S). I therefore obtain a proxy for school quality for each school in the data set. Note that these measures of school quality are net of neighborhood fixed effects and therefore reflect the contribution of schools (and sorting into schools) that cannot be accounted for by where schools gather their students from.[37] These outcome-based measures of school quality can be interpreted as predicted gains and reflect any observed and unobserved differences in productive school inputs – e.g. teacher and principal quality. In contrast, traditional measures of school quality based on test scores may not fully capture other important dimensions of school effectiveness for long-term educational attainment, such as effects on non-cognitive skills (Jackson, 2016; Heckman, Stixrud and Urzua, 2006).

To describe the amount of variation in the data, Table II reports the student-level standard deviation in school ($\boldsymbol{\delta}_{s(i)}$) and FSA ($\delta_n$) fixed effects for the three main outcomes of interest: university enrollment, finishing secondary school on time (DES in 5 years), and years of education. As a benchmark, I first report in columns (1), (3) and (5) the raw variation across school and neighborhood fixed effects, not accounting for variation in the other dimension. These reflect the dispersion of neighborhood and school fixed effects estimated in separate regressions. For all three outcomes, the variance across schools is about twice as large as the variance between FSAs.[38] In columns (2), (4) and (6), fixed effects for schools and neighborhoods are estimated simultaneously. While the magnitude of the variation across schools barely shrinks when FSA fixed effects are included, a large fraction – between 55 and 65 percent – of the raw student-level variation across FSAs is accounted for by school attendance. Nevertheless, this preliminary piece of descriptive evidence suggests that there is independent variation across both schools and neighborhoods that cannot be accounted for by the other dimension. It is worth noting that these estimates may be subject to sampling error, an issue return to in Section VI.E.[39]

[36] Just like models of worker and firm fixed effects are identified from switchers (Abowd, Kramarz and Margolis, 1999), this model requires that students of a given neighborhood be observed in multiple schools and that students from a given school be observed in multiple neighborhoods.

[37] Primary school quality is net of the secondary schools its students will eventually attend, and secondary school quality is net of the primary schools it gathers its students from.

[38] This result is not due to the fact that there are fewer FSAs than primary schools, as the patterns replicate at the census tract level (Table A.2). Also, these patterns closely reflect the conclusions of Carlson and Cowen (2015), who focus on the variance in test scores growth across neighborhoods and schools in Milwaukee's open enrollment system.

[39] To maintain the mapping between $\bar{\Omega}_n^{PR}$, $\bar{\Lambda}_n^{PR}$ and $\bar{y}_n^{PR}$ intact, I work with unadjusted estimates in the main analyses. Appendix Table A.3 reports standard deviations of $\boldsymbol{\delta}_{s(i)}$ and δ_n for "shrunk" estimates obtained using empirical Bayes techniques

I then use the fixed effect estimates reported in column (6) to document two additional stylized facts. Firstly, the student-level correlation between school and FSA fixed effects for years of education is small but positive (0.17), indicating that students residing in better neighborhoods attend better schools on average.[40] Secondly, I follow the approach developed in Card, Heining and Kline (2013) to examine whether there are systematic interactions between the two contexts. Figure A.8 is constructed by slicing the distributions of school and FSA fixed effects into deciles, and then plotting the average residuals in each school-by-neighborhood decile cell. Most average residuals are smaller than 0.1 year of schooling, or less than 5% of a standard deviation in the sample of permanent residents. If there were positive interactions between school and neighborhood quality, one would expect abnormally large and positive mean residuals for cells corresponding with high or low deciles in both dimensions. The figure shows no such discernible pattern, which lends support to the additive separability assumption made in Section III. In addition, allowing for unrestricted match effects between schools and neighborhoods (i.e. a full set of indicator variables for each possible combination of neighborhood and primary/secondary school) only slightly improves the model's fit – e.g. for years of education, the adjusted R^2 increases from 0.3710 to 0.3735.

Finally, I collapse the estimated fixed effects, $\hat{\boldsymbol{\delta}}_{s(i)}$ and $\hat{\delta}_n$, at the FSA-level to obtain measures of $\bar{\Omega}_n^{PR}$ and $\bar{\Lambda}_n^{PR}$. Appendix Figure A.9 shows the spatial variation in these measures. Importantly, the two maps exhibit little overlap. Places with low values of the school component $\bar{\Omega}_n^{PR}$ do no necessarily also have a low neighborhood component $\bar{\Lambda}_n^{PR}$. This non-collinearity between the two dimensions is critical to the feasibility of decomposing total exposure effects.

IV.B Effect of attending better schools

In this section, I present the RD-IV design used to estimate the effect of attending better schools on long-term educational outcomes. The approach is based on the fact that schools' catchment areas cut through neighborhoods in such ways that students on opposite sides of a boundary reside in the same communities and enjoy the same neighborhood amenities (Black, 1999).[41] Yet, these boundaries shift the quality of schools two neighbors may be exposed to by varying their default option. I focus on French primary school boundaries throughout.

For each boundary, I first identify which of the two default schools is of better quality, that is which yields greater predicted gains (i.e. has a relatively higher fixed effect $\hat{\delta}_s^P$). Note that because these fixed effects are net of FSA-level variation and secondary school attendance, the "better" school for a given boundary is not necessarily the one where students have the best outcomes in absolute terms. For each student, I then define an indicator variable $HighSide_{ib}$ for whether student i resides on the better side of the nearest French primary school boundary b.[42] These indicator variables are then used as instruments in the following two-stage regression-discontinuity framework:

(Kane and Staiger, 2008; Chandra et al., 2016; Best, Hjort and Szakonyi, 2017). Such an adjustment leaves unchanged the observation that there is more variance across schools than neighborhoods. If anything, the between-neighborhood variation is noisier. Using these empirical Bayes estimates in later analyses yields a larger total convergence rate and reinforces the conclusion that schools account for most of these effects.

[40] The correlations are 0.05 and 0.13 for graduating secondary school on time and university enrollment, respectively. The correlations are slightly higher if one uses empirical Bayes shrunk estimates: 0.20, 0.08 and 0.015 for years of education, timely secondary school graduation and university enrollment, respectively.

[41] Boundaries that coincide with natural divisions such as highways or canals are excluded.

[42] The boundary-specific higher quality default school is not the one with relatively higher raw outcomes for over a quarter of all permanent residents. In other words, if I were to assign values of $HighSide_{ib}$ on the basis of raw outcomes rather than of adjusted school quality $\hat{\delta}_s^P$, the values of the dummy would flip for a fourth of my sample. The fact that I detect no evidence

$$y_{icnb}^{PR} = \pi\Omega_{s(n(i))}^{-i} + f(distance_{ib}) + \gamma X_{icnb} + \alpha_b + \alpha_n + \alpha_c + \epsilon_{icnb} \tag{6}$$

$$\Omega_{s(n(i))}^{-i} = \zeta HighSide_b + f(distance_{ib}) + \gamma X_{icnb} + \alpha_b + \alpha_n + \alpha_c + \epsilon_{icnb} \tag{7}$$

where (7) and (6) are first and second stage equations, respectively. The dependent variable y_{icnb}^{PR} is an educational outcome for permanent resident i of neighborhood n. Student-level individual characteristics X_{icnb} are included to improve precision.[43] Each student is matched to the boundary b that is the nearest from her home. The main regressor of interest, $\Omega_{s(n(i))}^{-i}$, is a leave-self-out measure of average school quality over i's entire childhood.[44] In both stages, a control function for distance to the nearest boundary $f(distance_{ib})$ is included, as well as FSA (α_n), boundary (α_b), and cohort (α_c) fixed effects.[45] Standard errors are clustered at the French primary school boundary level.

The validity of the RD approach rests on the assumption that right around boundaries, the quality of default school options is as good as random. In education systems where school attendance is fully determined by residence, households may sort right around the boundaries, generating discontinuities in sociodemographic characteristics (Bayer, Ferreira and McMillan, 2007). However, in Montreal, any incentive to sort at the boundary is substantially weakened by opportunities to opt-out of one's default public school. Similarly, the large set of available private school options strongly reduces sorting incentives. For instance, Fack and Grenet (2010) find that the capitalization of school quality in house prices in Paris falls sharply with private school availability, and is effectively null in areas with many private schools. More importantly, given that rankings of Montreal primary schools are not publicly available, distinguishing good from bad nearby schools is difficult and parents may have little ability to sort at boundaries.[46]

To validate that any jump in school quality at boundaries does not reflect discrete changes in student characteristics, I verify that observable characteristics are balanced around these boundaries (Figures A.11, A.12 and A.13). The distribution of covariates does appear to be smooth at the threshold (panels (a) to (j)). Similarly, there is no selective attrition around boundaries (panels (k) and (l)). Combining all covariates to generate measures of predicted educational attainment, I find no discontinuity in predicted outcomes (Figure A.14).[47]

I also indirectly test the identifying assumption in Section V by conducting a placebo test. I demonstrate that there is no discontinuity in educational outcomes for students in English schools around French boundaries. This placebo test suggests the boundaries do not coincide with discontinuous changes in non-school local amenities that would equally benefit English and French students. Note that any sorting of families around boundaries on the basis of their willingness to pay for school quality (via house prices) should affect both

of sorting at the boundaries is consistent with findings that parental preferences are unrelated to school effectiveness once peer quality is accounted for (Abdulkadiroğlu et al., 2017; Rothstein, 2006), and that school value-added is not capitalized in house prices (Imberman and Lovenheim, 2016; Kane, Riegg and Staiger, 2006).

[43] The RD point estimates are virtually identical if baseline characteristics X_{icnb} are omitted.

[44] The childhood school quality measure $\Omega_{s(n(i))}^{-i}$ is obtained by taking the sum of leave-self-out transformations of the primary ($\delta_{s(i)}^{P}$) and secondary school ($\delta_{s(i)}^{S}$) fixed effects estimated in Section IV.A. The exact procedure is described in the Data Appendix. Jackknife and split-sample approaches yield almost identical results.

[45] In the main specification, I follow Lee and Lemieux (2010) and parameterize $f(distance_{ib})$ with a rectangular kernel. In Section VI, I show that my results are robust to functional form assumptions and bandwidth restrictions.

[46] Appendix Figure A.10 shows a density plot by distance to boundaries. No excess density is observed on the right side of the threshold (side with relatively better schools in term of university enrollment). A formal McCrary (2008) test finds no statistically significant gap: the log difference in height is 0.006 with a standard error of 0.018.

[47] The associated regression estimates are shown in Table A.4.

French and English households, as they all participate in the same housing market. Importantlty, there is no evidence that non-English families disproportionately bunch on the better side of boundaries relative to English families, a sorting pattern that would generate a discontinuity in the fraction of English families. Panel (f) of Figures A.11, A.12 and A.13 indicates that this type of sorting does not occur.

IV.C Total exposure effects

The empirical approach used to estimate the combination of school and neighborhood effects – total exposure effects – relies on variation in the timing of moves. More specifically, I first investigate whether the outcomes of movers converge towards those of the permanent residents of the FSA to which they move in proportion with time spent in that destination neighborhood. As in equation (3), the econometric framework models movers' outcomes as a function of the outcomes of permanent residents of the neighborhoods in which they have resided, weighted by time spent in these locations. The main estimating equation is

$$y_{icmod} = \beta\left(m_i \times \Delta\bar{y}_{od}\right) + \gamma X_{icmod} + \alpha_{od} + \alpha_m + \alpha_c + \varepsilon_{icmod} \qquad (8)$$

where y_{icod} is some educational outcome of student i, from cohort c, who resided in neighborhood o (origin) at baseline, and moved to neighborhood d (destination) at age m_i. The coefficient of interest is on the interaction between age-at-move m_i and $\Delta\bar{y}_{od}$, the difference between the mean outcomes of permanent residents of neighborhoods d and o. If exposure matters, we would expect that $\beta < 0$, which implies that the outcomes of movers converge to that of the permanent residents in the destination neighborhood with the number of years they lived in that area.

The origin is the FSA in which students resided at baseline, while the destination is the one in which they lived during the academic year they were aged 15 on September 30. Sorting to better areas is accounted for by origin-by-destination fixed effects (α_{od}) and unobserved differences between students who move at different ages, notably differential disruption costs, are absorbed by age-at-move fixed effects (α_m). Cohorts fixed effects (α_c) are also included to account for the different number of years for which students are tracked in the data. Standard errors are clustered at the destination neighborhood level to allow for arbitrary correlation among families moving to the same place.

Benchmark results are reported in Section V, and a series of robustness checks, including family fixed-effects models and verification of balance on covariates, are conducted in Section VI. Note that there is no systematic correlation between m_i and $\Delta\bar{y}_{od}$ in the data. Children who move at early ages are no more likely to move to better or worse areas (relative to their origin) than children who move at later ages (Figure A.15). Using a Kolmogorov-Smirnov test, I cannot reject the null of equality of distributions of $\Delta\bar{y}_{od}$ between early (age 7-11) and late (age 12-15) movers (p-val=0.22).

To maximize power, in most specifications the sample includes all movers irrespective of the number of times they moved across FSAs, as long as both origin and destination are within Montreal and are not the same. For multiple-times movers, the average quality of neighborhoods exposed to prior to moving to the final destination is therefore measured with error.[48] The model is therefore also estimated on the

[48] To keep the decomposition tractable, I focus on specifications that exploit variation from only two locations (the origin

sub sample of one-time movers, in which case the econometric model maps directly onto the conceptual framework discussed in Section III. In all cases, the sample is always restricted to movers whose origin and destination both have at least 10 permanent residents.

IV.D Decomposing exposure effects

As discussed in Section III, the total convergence rate β reflects the combined effect of changes in both school and neighborhood (non-school) quality. To investigate the quantitative importance of schools as a driver of this total effect, I estimate a "horse-race"-type model that simultaneously includes changes in both components of permanent residents' outcomes. The reduced-form counterpart to equation (4) is

$$y_{icmod} = \beta_s \left(m_i \times \pi\Delta\Omega_{od}\right) + \beta_n \left(m_i \times \Delta\bar{y}^{-s}_{od}\right) + \gamma X_{icmod} + \alpha_{od} + \alpha_m + \alpha_c + \varepsilon_{icmod} \tag{9}$$

where $\Delta\Omega_{od}$ and $\Delta\bar{y}^{-s}_{od}$ are measured using the fixed effects estimated in Section IV.A.[49] As for total exposure effects, the school- and neighborhood-specific convergence rates β_s and β_n are identified from variation in the timing of moves. These are partial regression coefficients that reveal the annual effect of a change in one contextual dimension, holding the other constant.

Given that $\Delta\bar{y}_{od} = \pi\Delta\Omega_{od} + \Delta\bar{y}^{-s}_{od}$, there exists a direct mapping between estimates of total exposure effects obtained from equation (8) and the coefficients of equation (9). In fact, one can recover the full convergence rate β by using estimates of β_s and β_n, as well as sample estimates of $Var(\pi\Delta\Omega_{od})$, $Var(\Delta\bar{y}^{-s}_{od})$ and $Cov\left(\pi\Delta\Omega_{od}, \Delta\bar{y}^{-s}_{od}\right)$ in the following decomposition equation:

$$\beta \simeq \underbrace{\beta_s \left[\frac{Var(\pi\Delta\Omega_{od}) + Cov(\pi\Delta\Omega_{od}, \Delta\bar{y}^{-s}_{od})}{Var(\Delta\bar{y}_{od})}\right]}_{\text{Convergence due to school effects}} + \underbrace{\beta_n \left[\frac{Var(\Delta\bar{y}^{-s}_{od}) + Cov(\pi\Delta\Omega_{od}, \Delta\bar{y}^{-s}_{od})}{Var(\Delta\bar{y}_{od})}\right]}_{\text{Residual convergence due to (non-school) neighborhood factors}} . \tag{10}$$

The full convergence rate is the sum of school-specific and neighborhood-specific terms.[50] Intuitively, the total effect of moving to a better area captures independent variation in school and neighborhood quality, as well as joint variation in these two dimensions. As equation (10) makes clear, because of possible differences in variances, equality of β_s and β_n does not imply that schools and neighborhoods matter equally. In other words, even if students benefit greatly from having access to better schools (i.e. if β_s is large in magnitude), schools could nonetheless explain only a small share of the estimated gains of moving to a better neighborhood if there is little variation in school quality across FSAs in the data (i.e. if $Var(\pi\Delta\Omega_{od})$ is small).

To examine how schools account for the observed convergence of movers' outcomes towards those of permanent residents, I calculate a restricted convergence rate β^{-s} by shutting down the school channel, that is by setting the effect of moving to a place with schools of greater quality β_s equal to zero:

and the destination). Appendix Table A.11 reports results for models in which I substitute an exposure-weighted average of neighborhood quality of all locations prior to moving to the final destination for the quality of the origin area $\bar{y}_o$. About $\frac{2}{3}$ of movers move across FSAs only once, and only 6% move more than three times.

[49] As a special case, if $\pi = 1$, then $\Delta\bar{y}^{-s}_{od} = \Delta\Lambda_{od}$.

[50] See Mathematical Appendix for derivation.

$$\beta^{-s} \equiv \beta_n \left[\frac{Var(\Delta\bar{y}_{od}^{-s}) + Cov(\pi\Delta\Omega_{od}, \Delta\bar{y}_{od}^{-s})}{Var(\Delta\bar{y}_{od})} \right].$$

This restricted rate corresponds to any residual exposure effects that are not driven by changes in school quality. I then use this restricted rate to calculate the school share of total exposure effects by taking the following ratio $\left(\frac{\beta-\beta^{-s}}{\beta}\right) = \frac{\beta_s\left(Var(\pi\Delta\Omega_{od})+Cov(\pi\Delta\Omega_{od},\Delta\bar{y}_{od}^{-s})\right)}{\beta(Var(\Delta\bar{y}_{od}))}$.

Overall, the analysis of the extent to which exposure effects are driven by causal school effects relies on a system of estimating equations and a clear mapping between them. Equation (8) pins down the total impact of moving to a better area, while equations (6) and (7) identify the forecast-unbiased school effects that are fed into equations (9) and (10) to implement the decomposition of interest. The next section reports the results of these statistical analyses.

V Results

This section reports baseline results, presenting estimates of school effects and the analysis of total exposure effects that focuses on movers. It then combines these two sets of results to evaluate the fraction of the benefits of moving to a better area that is driven by schools.

V.A School effects

Because students are allowed to opt-out of their neighborhood schools, boundaries might not necessarily generate large breaks in the quality of schools *actually* attended by students. I therefore first examine whether default options produce such first-stage variation.

Figure II plots the student-level mean quality of primary schools $\delta^P_{s(i)}$ – here measured in terms of university enrollment – at baseline by distance to the nearest boundary, where students assigned to the better school of any boundary-specific pair are depicted on the right of the threshold (positive distances). For visual clarity, I restrict the sample to permanent residents living within 500 meters of their nearest boundary. Panel A shows the quality of default primary schools, whereas Panel B plots the quality of schools *actually* attended by students. In both cases, a large jump in quality is observed right at the boundary. In Panel A, a break occurs by construction. Yet, the jump might be very small in magnitude if differences in school quality between nearby schools were small. This is not the case. The RD estimate implies a 6.3 percentage points jump in school quality measured in terms of university enrollment rates. Panel B confirms that default options have a strong impact on the quality of schools parents send their children to (statistically significant RD estimate of 2.6 percentage points). Panels C and D are placebo tests which only include students in English schools. These students should not be directly affected by French school catchment areas, but do enjoy the same neighborhood amenities. The similarity between Panels A and C indicate that English-school students reside around *French* boundaries that are no different than the boundaries faced by the full sample. Nonetheless, at these boundaries, there is no jump in the quality of schools attended by English students (Panel D).

Next, corresponding graphs for the first-stage equation (7) as well as the reduced-form relationship between distance to boundaries and university enrollment are shown in Figure III. Again, Panels A and B include all permanent residents, and Panels C and D are restricted to students in English schools. The first graph confirms that the instrument has a strong first-stage. Being assigned a better school at baseline does significantly shift the average quality of schools a student attends during childhood ($\Omega_{s(n(i))}^{-i}$). Educational attainment also jumps right at the threshold: students on the better side of a boundary at age 6 are about 2 percentage points more likely to eventually enroll in university later in life. Importantly, there is no break in school quality or university enrollment for students in English schools. The sharp changes observed at the threshold for the full sample are therefore due to schools themselves rather than to some other productive neighborhood characteristic that varies discontinuously and coincides with these boundaries.

Regression results analog to the above figures are presented in Table III. The baseline specification includes control variables X_{icnb} – gender, place of birth indicators, language at home indicators, use of day care, 'in difficulty' status at baseline, handicapped status – as well as cohort, FSA, and nearest boundary fixed effects. To increase precision, the main sample imposes no bandwidth restriction and includes all permanent residents.[51] Columns (1) through (4) are first-stage and reduced-form regressions and are estimated by OLS. Consistent with the visual evidence, the average quality gap between default schools on opposite sides of a shared boundary is 0.0631 percentage points (s.e. 0.003) in terms of university enrollment (column (1)). For all three main outcomes, these differences in default options do translate into significant differences in the quality of schools attended at baseline (gap of 0.0245 (s.e. 0.0027) in column (2) for university enrollment). Importantly, this initial shift in school quality strongly affects average childhood school quality $\Omega_{s(n(i))}^{-i}$ (column (3)). The results in column (4) indicate statistically significant reduced-form relationships between each measure of educational attainment and the assignment variable. For example, students living on the better side of boundary are 3.5 percentage points (s.e. 0.0084) more likely to obtain a secondary school diploma in five years than students on the opposite side. Crucially, for columns (2) through (4), all coefficients for placebo tests reported in the bottom panel are close to zero and statistically indistinguishable from zero.

The last column reports two-stage least square estimates of cumulative school effects. Here, there is some variation across outcomes. The RD-IV coefficient of π is below one for university enrollment and years of study, which implies the presence of some degree of sorting into schools that is not accounted for by place of residence. For these two outcomes, one may therefore overstate the importance of schools if this bias is ignored. In contrast, the coefficient for finishing secondary school on time is very close to one. Speculatively, for a given degree of sorting, schools likely have a more direct influence on immediate outcomes such as graduating on time than on higher education investments made later in life. In terms of the conceptual model of Section III, the value of ω might be relatively higher for outcomes on which schools can act directly. Section VI documents the robustness of these results to functional form assumptions and bandwidth restrictions.

V.B Total exposure effects

In this section, I first provide visual evidence of the convergence of movers' outcomes towards those of the permanent residents of their destination by estimating a non-parametric version of equation (8). More

[51] I document the robustness of the results to the choice of bandwidth in Section VI.A.

specifically, I interact $\Delta \bar{y}_{od}$ with a set of indicators for each possible value of age-at-move m_i (age 7 to 15). Figure IV plots the results. As expected, the coefficients on $\Delta \bar{y}_{od}$ shrink (increase) with age-at-move (time spent in destination neighborhood). Importantly, they decrease approximately linearly with age-at-move, which validates that the assumption that exposure effects are linear with age is reasonable.

I then report baseline estimates of the convergence rate, which is the *slope* of the line that would best fit the points shown on Figure IV. Table IV reports the results for the main outcomes considered – university enrollment, finishing secondary school on time, and years of education. In the first two columns, I include all movers regardless of the number of times they moved. For this sample, the quality of neighborhoods exposed to prior to the move is necessarily measured with some error. I condition on one-time movers in columns (3) and (4). In columns (2) and (4), I include a set of dummies for the number of times one has been flagged in difficulty prior to moving to control for pre-move schooling ability. All models are estimated by ordinary least squares and standard errors are clustered at the destination FSA level.

For the two binary outcomes, moving one year earlier to a neighborhood where permanent residents exhibit 10-percentage-points higher outcomes, relative to the origin, increases movers' educational attainment by about 0.4 percentage points. Extrapolating over 15 years, the cumulative effect would therefore be 6 percentage points, or 60% of the difference between permanent residents of the destination and origin locations. These point estimates are all statistically significant at the 1% level. Slightly larger coefficients are obtained for years of education, implying a convergence rate of about 4.5%. Overall, the estimates are very stable across specifications and outcomes. Controlling for poor schooling outcomes prior to moving (dummies for the number of times in difficulty) or restricting the sample to one-time movers barely affect the magnitudes of the coefficients.[52][53]

To put these estimates in perspective, consider the raw variation across FSAs documented in Table II. The standard deviation of university enrollment rates across FSAs for permanent residents is 14 percentage points. Hence, accumulated over 15 years, the exposure effects estimated from movers can account for almost half of these spatial differences. Alternatively, the cumulative effect of a move to a place where university enrollment is 10 percentage points higher is about half the size of the unconditional gender gap in university enrollment (11 percentage points in favor of women).

My estimates of total exposure effect are also surprisingly close to those reported by Chetty and Hendren (2018*a*), who find a convergence rate of 4% in earnings for millions of movers across commuting zones in the US. While one may expect a larger influence of neighborhoods at finer levels of geography, my estimates are more likely to be attenuated due to sampling error in the calculation of the average outcomes of permanent residents. Nonetheless, it is remarkable that our findings so closely align given the differences in the locations we study as well as differences in the populations of interest. While movers *across* cities tend to have a slight income advantage relative to stayers, movers *within* cities appear to be poorer than permanent residents (at least in Montreal).[54]

[52] Allowing permanent residents' outcomes to be cohort-specific (i.e. $\bar{y}^{PR}_{nc}$) increases sampling error and therefore yields convergence rates of slightly smaller magnitudes (3.4 – 4.3%). Similar patterns emerge if I use mutually exclusive cohorts to calculate $\bar{y}^{PR}_{n}$ and to estimate total exposure effects. These results are available upon request.

[53] In Table A.12, I further include 6-digit postal code fixed effects, thereby restricting the comparison between children who at the age of 15 lived either on the same block or in the same appartment building, as an attempt to absorb as much of the variation in parental income as possible. The fact that many postal codes contain only one observation shrinks the sample size substantially, but the estimated convergence rates remain qualitatively similar to the benchmark estimates.

[54] For completeness, I also estimate the main exposure model at the census tract level (Table A.5, Panel A). Because census tracts are much smaller than FSAs, including origin-by-destination fixed effects generates a large number of singletons. For

Results for alternative measures of educational attainment are shown in Appendix Table A.6. For most of these, the observed patterns mirror the main results. The magnitude of the effect appears marginally smaller for bachelor degree completion, and slightly larger for dropping out of high school with no degree or qualification. In the last two rows, I compute measures of expected earnings on the basis of (a) the highest level of education alone and (b) the level of education combined with the field of study, using the Public Use Microdata File of the 2006 Canadian Census.[55] Convergence rates for expected earnings on the basis of the level of education are about $4 - 4.5\%$, while those that also take into account fields of study a slightly smaller $(3.3 - 4\%)$.

The estimates of the total effect of one year of exposure to a one-unit "better" area are valid under the assumption that the degree of selection to better FSAs does not vary systematically with age. In Section VI, a host of robustness checks are conducted to corroborate the validity of this assumption.

V.C Schools or neighborhoods?

While the previous section showed that exposure to better locations does matter, it is unclear whether it is schools or neighborhood themselves that drive these effects. For instance, the descriptive statistics presented in Section IV.A show that a large fraction of the between-FSA variance is accounted for by school attendance. Also, the fact that many parents appear to passively enroll their child in the default school suggests that patterns of school attendance are not completely unrelated to geography.

In this section, I use decomposition equation (10) to evaluate the extent to which exposure effects operate through schools. Again, I start by presenting visual evidence based on non-parametric estimates. Figure V reproduces in light grey the total exposure regression coefficients that were previously shown in Figure IV. In addition, it displays in red the corresponding restricted coefficients for which convergence on the school component of differences in permanent residents' outcomes $\pi\Delta\Omega_{od}$ has been set to zero.[56] For all three outcomes, the slope of the line that connects these points is considerably flatter, indicating a much lower rate of convergence once the school channel has been shut down. In contrast, a similar exercise that instead shuts down direct neighborhood effects yields restricted coefficients that barely deviate from the ones that depict full exposure effects (Appendix Figure A.16).

I conduct a thorough investigation of the role of schools in Table V. In the first column, school quality is measured by the simple neighborhood-level average of the sum of primary and secondary school effects $(\bar{\Omega}_n^{PR})$. Setting to zero the effect of schools $(\beta_s = 0)$, I obtain restricted convergence rates β^{-s} of 1% for university enrollment, 1.1% for finishing secondary school on time, and 1.5% for years of education.

that reason, I also consider a less restrictive model in Panel B, which includes origin and destination fixed effects separately. Overall, the estimated coefficients vary between 2 and 4 %, with most being smaller than the associated coefficients at the FSA level. This is consistent with the idea that measurement error is plausibly more important at the census tract level. Firstly, permanent residents' outcomes will be measured less accurately because of sampling error. Also, census tracts may less precisely capture all features of the community in which children live and socialize, which is arguably larger than a single census tract. The smaller convergence rates could also reflect greater sorting of permanent residents at this level of geography.

[55] Details on the measurement of all outcome variables are provided in the Data Appendix.

[56] To construct this figure, a non-parametric version of horse-race equation (9) is first estimated:

$$y_{icmod} = \sum_{m=7}^{15} \beta_{s,m} \left(\pi\Delta\Omega_{od} \times 1\{m_i = m\}\right) + \sum_{m=7}^{15} \beta_{n,m} \left(\Delta\bar{y}_{od}^{-s} \times 1\{m_i = m\}\right) + \gamma X_{icmod} + \alpha_{od} + \alpha_m + \alpha_c + \varepsilon_{icmod}.$$

Then, a restricted coefficient β_m^{-s} is calculated for each possible value of age-at-move using the mapping given by equation (10). The standard errors around these coefficients are obtained by the delta method.

Taking the ratio of these restricted rates over the full convergence rate, the results imply that schools are responsible for 75% of total exposure effects on university enrollment. For timely graduation from secondary school and years of education, the proportions are 74% and 70% respectively. Column (2) reports results of a similar decomposition in which school quality is measured by the FSA average of permanent residents' leave-self-out childhood school quality $\bar{\Omega}_{s(n)}^{-i} = E\left[\Omega_{s(n(i))}^{-i} | n(i) = n \forall a\right]$. This slight change in measurement has little effect on the results – the fraction of exposure effects explained by schools remains in the vicinity of $70 - 75\%$.

Decompositions that do not take into account that biased measures of school quality $\bar{\Omega}_{s(n)}^{-i}$ also partly reflect selection may overstate the importance of schools. In column (3), I therefore use the baseline RD-IV estimates to isolate causal school effects. The school-related share of total exposure effects drops to 65% for university enrollment, to 73% for finishing secondary school on time, and to 46% for years of education.[57]

Overall, this decomposition analysis indicates that schools matter more than neighborhoods for long-term educational attainment. Most of the long-term benefits of moving to a better area are driven by changes in school quality. Nonetheless, schools do not *fully* account for the these total exposure effects – neighborhoods do have a small independent effect on human capital accumulation. I further assess the robustness of this conclusion in the next section, in which I notably validate that movers do experience a substantial change in school quality as a result of the move.

VI Robustness

VI.A Regression-discontinuity estimates

The benchmark specification for estimating school effects imposes several restrictions. Firstly, it assumes that the relationship between distance to the boundary and student outcomes is linear. Appendix Table A.8 allows for a quadratic functional form. RD-IV estimates for finishing secondary school on time and years of education appear insensitive to this assumption. The estimate of π with quadratic functions for university enrollment (0.71), however, is smaller than the baseline (0.85). Both the reduced-form and first-stage coefficients are moderately smaller than in Table III. The reduction in the reduced-form point estimate slightly exceed that of the first-stage coefficients, which leads to a somewhat smaller RD-IV estimate. Similarly, using a triangular kernel for the control function yields results almost identical to the baseline results (Appendix Table A.9).

Appendix Figure A.17 examines the sensitivity of results to bandwidth restrictions. Moving along the horizontal axis, I gradually expand the sample by including students living farther away from boundaries. The point estimates do fluctuate across these sample restrictions, following no monotonic pattern. For instance, for university enrollment, keeping only students living within 750 meters of a boundary yields a considerably smaller RD-IV coefficient (0.62), while further restricting the bandwidth to 300 meters gives a

[57] In Appendix Table A.7, I re-arrange equation (10) and consider an alternative decomposition in which school effects are given by $\left(\frac{\beta_s Var(\pi\Delta\Omega_{od}) + \beta_n Cov(\pi\Delta\Omega_{od}, \Delta\bar{y}_{od}^{-s})}{Var(\Delta\bar{y}_{od})}\right)$ and neighborhood effects by $\left(\frac{\beta_n Var(\Delta\bar{y}_{od}^{-s}) + \beta_s Cov(\pi\Delta\Omega_{od}, \Delta\bar{y}_{od}^{-s})}{Var(\Delta\bar{y}_{od})}\right)$. This method differs in how it weighs the covariance term. The interpretation is now cast in terms of direct and indirect effects. For example, an increase in $\pi\Delta\Omega_{od}$ has a direct effect β_s on y_i, as well an indirect effect β_n via its correlation with $\Delta\bar{y}_{od}^{-s}$. It turns out that the covariance term is very small in practice, hence making the results under this approach almost identical to the main results.

coefficient very close to the baseline (0.83).[58] These movements in point estimates are plausibly driven by differences across the set of schools and neighborhoods that are dropped when the bandwidth is changed. For instance, denser parts of Montreal are unaffected by these restrictions since all students living in these areas live very close to a boundary. Large distances from boundaries are only observed in the suburbs.[59] Nevertheless, most estimates shown in Figure A.17 remain within short range of the baseline results. The conclusions of the decomposition exercise are therefore unaffected by the choice of bandwidth – the vast majority of estimates of the fraction of total exposure effects driven by schools fall between 50 and 70% (Appendix Figure A.18).

A separate issue arise in the decomposition exercise: the RD estimates may reflect local average treatment effects for a different subpopulation than the one that identifies total exposure effects. To address concerns related to heterogeneous treatment effects, I use a nearest-neighbor matching algorithm to re-weight the sample of permanent residents so that their distribution of observables matches the one of the movers' subsample (Jann, 2017; Abadie and Imbens, 2011). School effects estimates for this reweighted sample are close to the baseline results (Table A.10).

VI.B Movers estimates: Time-invariant confounds

My estimates of total exposure effects may be biased if students with higher (lower) unobserved family inputs ($\tilde{\theta}_i$) who move to better (worse) areas tend to do so earlier. This section examines the validity of the identifying assumption by running a set of robustness checks that address issues of time-invariant and time-varying unobserved heterogeneity.

Within-family exposure effects The first test I run involves estimating the exposure model with household fixed effects to account for any time-invariant family unobserved heterogeneity. In this specification, identification relies on age differences between siblings. In this context, positive exposure effects would generate a relationship between the change in neighborhood and school quality, on one hand, and the difference in educational outcomes among siblings, on the other hand, that varies proportionally to the age-difference of siblings.

Since siblings are not directly identified in the data, I match students using unique moves at a very fine level of geography. More precisely, I assume that two students who move from and to the exact same six-digit postal codes in the same year must belong to the same household.[60] Many household units are not consistent over time given the prevalence of step- and blended-families. For instance, two students from different biological parents may have been living under the same roof only for a fraction of their lives. I therefore exclude household units for which the children have lived at a common postal code for less than 75% of the years for which I can observe them.

In columns (1) and (2) of Table VI, I estimate the main exposure model with origin-by-destination fixed effects on the subsample of siblings. Standard errors are considerably larger than in the main specification

[58] Optimal bandwidths based on the methods developed in Calonico, Cattaneo and Titiunik (2014) are all fairly close to 300m, with small differences across outcomes. The associated values of π (bandwidth) are 0.71 (272m) for university enrollment, 1.05 (365m) for timely secondary school graduation, and 0.70 (281m) for years of education.

[59] In the sample with no bandwidth restriction, the median distance to the nearest boundary is 204 meters.

[60] Out of the original 100,929 students, this method identifies about 13,000 siblings attached to roughly 6,000 different households.

since the sample size is much smaller, but the point estimates are in line with the results based on all movers. In columns (3) and (4), I substitute family fixed effects for the origin-by-destination fixed effects to account for any time-invariant heterogeneity across families and still find convergence rates of about 4.5%. These results support the idea that the estimated exposure effects are not driven by differences in unobservable time-invariant family characteristics. However, the estimates reported in Table VI might still be subject to bias if time-changing unobservables affect siblings differentially in proportion of their age-gap. I further address robustness to time-varying unobservables in Section VI.C.

Balance of Covariates The second approach tests for balance of covariates to verify that variation in the interaction term is arguably random conditional on age-at-move and origin-by-destination fixed effects. Here, I run a set of balancing checks by estimating the exposure model using individual characteristics as dependent variables. The balancing test equation takes the form

$$X_{icmod} = \beta_x \left(m_i \times \Delta \bar{y}_{od} \right) + \alpha_{od} + \alpha_m + \alpha_c + \epsilon_{icmod}.$$

Under the identifying assumption that the degree of sorting to better areas is independent of age-at-move, I should find coefficients of zero on the $m_i \times \Delta \bar{y}_{od}$ interaction. Pei, Pischke and Schwandt (2017) show that putting the covariates on the left-hand side is a more powerful test than gradually adding or removing these variables from the right-hand side of the main estimating equation, particularly if the individual characteristics are poor measures of the underlying confounders they are meant to account for. For instance, being 'in difficulty' is certainly a noisy measure of academic abilities.

Results of the test are shown in Table A.13. In columns (1) and (2), I use years of education of permanent residents to measure $\Delta \bar{y}_{od}$. Finishing secondary school on time and university enrollment are used in columns (3) and (4) and columns (5) and (6), respectively. The coefficients on immigrant status are marginally significant at the 5% level for some, but not all outcomes. In Montreal, immigrants do obtain more post-secondary education than domestic students. It might also be the case that they tend to move later if acquiring information about neighborhoods takes more time for this subgroup, given that they may have less prior information than native-born parents. The coefficients for learning difficulties at baseline reach statistical significance in some cases. For the complete history of learning difficulties prior to moving, all coefficients are effectively zero. Overall, most coefficients in the table are very small and statistically indistinguishable from zero. As a result, the baseline convergence rate is materially unchanged whether covariates are included or not, despite the fact that these covariates have non-trivial explanatory power with respect to educational attainment – e.g. for years of education, their inclusion increases the adjusted R^2 from 0.188 to 0.307.

VI.C Movers estimates: Time-varying characteristics

Another possible source of concern is that length of exposure to a one-unit better area mirrors exposure to different family circumstances. Put differently, one may be worried that if a move is triggered by a change in marital status or income, the age-specific unobserved parental inputs θ_{ia} may have also changed sharply and in proportion with m_i.

Selection on time-varying observables Unfortunately, my data set includes very few time-varying individual characteristics. For instance, parental income and marital status of parents are not observed. To account for possible changes in family circumstances that coincides with a move, I instead control for differences in census tract characteristics between the origin and the destination, as well as the interaction of these differences with age-at-move:

$$y_{icmod} = \beta\left(m_i \times \Delta\bar{y}_{od}\right) + \eta_0 \Delta Z_{iod} + \eta_1\left(m_i \times \Delta Z_{iod}\right) + \gamma X_{icmod} + \alpha_{od} + \alpha_m + \alpha_c + \epsilon_{icmod}$$

where ΔZ_{iod} is the difference in census tract characteristics between the areas in which student i resides after and before the move. Because census tracts are considerably smaller than FSAs, these controls vary within origin-by-destination cells and so the main effects η_0 are identified. The characteristics I consider are median household income, average dwelling value, percentage low income, percentage of adults with some college education, and fraction of lone parent families. Those are all obtained from the 2001 Canadian Census. The inclusion of these variables accounts for changes in family circumstances that are correlated with changes in neighborhood attributes, as well as any sorting on the basis of these observable neighborhood characteristics.[61] For example, a positive income shock may be associated with both a move to an area where property value is higher than in the origin and an increase in parental inputs. For any unobserved variable to generate bias in the exposure estimates under this specification, the confounding variable would have to generate variation orthogonal to changes in these neighborhood attributes. The inclusion of these neighborhood attributes likely absorbs part of the causal exposure effect of interest, and therefore over-adjusts for changes in family circumstances.

Results are quite robust to the inclusion of these controls (Table A.14). In columns (1) through (5), I control for changes in one time-varying characteristic at a time. In all of these cases, the exposure effects remain stable around 4 to 4.5%. Among all considered variables, the local fraction of lone-parent families is the one that most affects the main exposure effects. Yet, even in this case, the exposure effects remain large (≈4%). In column (6), I include all controls simultaneously. For each outcome, the exposure effects falls just under 4% and remains strongly statistically significant.

Event-study Next, I investigate whether students who move to better areas exhibit different trends in learning difficulties prior to moving. The idea is that family circumstances plausibly directly affect the likelihood that a student struggles in school, hence changes in unobserved family inputs should be reflected in the probability of being identified 'in difficulty'. I leverage year-to-year variation in $Diff_{iod,t}$, the indicator of whether student i was in difficulty in year t, to create an index of relative learning difficulties that summarizes the way movers compare to permanent residents in their origin and destination (Finkelstein, Gentzkow and Williams, 2016; Bronnenberg, Dubé and Gentzkow, 2012):

$$\sigma_{od(i,t)} = \frac{Diff_{iod,t} - \overline{Diff}_{o,t}}{\overline{Diff}_{d,t} - \overline{Diff}_{o,t}}$$

[61] Similarly, Altonji and Mansfield (2014) control for group-level average individual characteristics – arguably the basis on which households sort into neighborhoods – to account for unobserved individual heterogeneity, and thereby obtain a lower bound on contextual effects.

where $\overline{Diff}_{n,t}$ is the fraction of permanent residents of FSA n that were in difficulty at time t. Note that this index takes a value of zero if mover i's difficulty status is the same as the average in her origin and a value of one if it is equal to the average in the destination. An increase in $\sigma_{od(i,t)}$ over time indicates that student i's success in school (or lack of thereof) converges towards that of permanent residents in the destination relative to the origin.

I investigate patterns in $\sigma_{od(i,t)}$ around the time of moves. For instance, a positive pre-trend would indicate that movers started converging towards permanent residents of the destination before they even moved. Such a pattern could arise if moves to certain areas occur as a result of gradual changes in family circumstances. For example, if divorces are preceded by an erosion of the quality of the parents' relationship *and* are disproportionately followed by moves to worse places, my estimates of total exposure effects could be biased.

Figure A.19 shows the results of the following event-study analysis

$$\sigma_{od(i,t)} = \sum_{k=-4}^{4} \alpha_k 1\left\{t = m_i + k\right\} + \delta_i + \epsilon_{imodt}$$

where observations are weighted by $\left(\overline{Diff}_{dt} - \overline{Diff}_{ot}\right)^2$ as in Bronnenberg, Dubé and Gentzkow (2012) and δ_i are student fixed effects.[62] For descriptive purposes, I first show in Panel A estimates of raw trends with no student fixed effects. A jump in $\sigma_{od(i,t)}$ occurs on impact, and students schooling difficulties then converge gradually towards the destination's average. Importantly, there is no discernible pre-trend – coefficients are stable prior to moving. These results are consistent with Aaronson (1998), who finds no systematic pattern between pre-move changes in family circumstances and the quality of the destination neighborhood quality. For instance, moves preceded by a divorce are just as likely to lead to a better than to a worse destination.

Because students move at different ages, the panel is not balanced. As a result, any pre- and post-move trend may be the result of changes in the sample's composition. In Panel B, I follow Finkelstein, Gentzkow and Williams (2016) and include student fixed effects to address this issue. While the post-move trend now disappears, the jump at the time of the move remains significant. One concern is that this sudden jump is the product of a sharp and sudden change in family inputs. In Panel C and D, I therefore distinguish between students who did and did not switch school the year they moved. The evidence suggests that the break is instead the result of a change in the schooling environment possibly driven by differences in schools' propensity to flag marginal students. For students who did not change school at the time of the move, there is no jump in $\sigma_{od(i,t)}$. Overall, the event-study plots highlight the absence of pre-trends in schooling difficulties. It also emphasizes the potentially important role schools play in the decision to label a student as being in difficulty or not.

VI.D Movers estimates: Heterogeneity

In Table A.15, I explore whether exposure effects vary in magnitude by gender, language of instruction, or whether students are moving to a better or worse place. Columns (1) and (2) estimate the model in

[62] Observations outside this window remain in the analytic dataset, hence all coefficients α_k are relative to omitted years. Standard errors are clustered at the student-level.

equation (8) separately for boys and girls, respectively. While the coefficients are almost identical across genders for secondary school completion, girls appear to benefit slightly more from exposure to better areas than boys do in terms of university enrollment and years of education. This result is at odds with many studies that find that boys are more sensitive to their childhood environment (Autor et al., 2016; Chetty et al., 2016), but agrees with the findings in Deming et al. (2014) that girls who win a school choice lottery experience increases in college enrollment but that boys do not. In columns (3) and (4), students in English and French schools appear to benefit equally from moves to better areas, despite the fact that anglophones start from a much higher baseline – in the full sample, students in English schools are 10 percentage points more likely to enroll in university than students in French schools.

The main specification not only assumes that exposure effects are linear with age-at-move, but also that they are linear and symmetric in $\Delta\bar{y}_{od}$. I explore the validity of this assumption in columns (5) and (6). Significant exposure effects are found both for students moving to a better area and for those moving to a worse place, but the patterns here are not consistent across outcomes. Moving to a better FSA affects the probability of graduating from secondary school on time, but the associated convergence rate for moves to worse FSAs is not statistically different from zero. In contrast, negative moves appear to influence the propensity to enroll in university more strongly than do positive moves. I cannot, however, reject that the two coefficients are statistically equal.

VI.E Decomposition

Movers' school attendance In this subsection, instead of using measures of school quality from permanent residents, I directly account for movers' school attendance in the baseline total exposure effect model. I then examine the behavior of the estimated convergence rate as I include fixed effects for schools attended by movers themselves. The estimating equation becomes

$$y_{icmod} = \beta\left(m_i \times \Delta\bar{y}_{od}\right) + \alpha_{s(0)} + \alpha_{s(A)} + \gamma X_{icod} + \alpha_{od} + \alpha_m + \alpha_c + \epsilon_{icmod}$$

where $\alpha_{s(0)}$ and $\alpha_{s(A)}$ are sets of fixed effects for schools attended at baseline and at age 15, respectively. To account for variation in length of *exposure* to better schools that may be correlated with neighborhood exposure, the school fixed effects are allowed to vary linearly with age-at-move, i.e. $\alpha_{s(a)} = \alpha^0_{s(a)} + \alpha^1_{s(a)} \times m_i$, which is equivalent to allowing age-at-move effects to have a different slope in each school. Note that these fixed effects account for differences in school quality as well as any sorting into schools. The estimates therefore put an upper bound on the role of schools.

The results are presented in Table VII. Benchmark estimates of total exposure effects are reproduced in column (1). In column (2), fixed effects for schools attended at the beginning (the "origin" school) and at the end (the "destination" school) of the exposure period, as well as interactions with age-at-move, are added. In this case, the annual exposure effects shrink substantially to 1.1% for university enrollment, 1.2% for completing secondary school on time, and 1.8% for years of education. These point estimates are strikingly close to the restricted convergence rates reported in column (2) of Table V (1.1%, 1.3% and 1.6% respectively), validating that differences in permanent residents' school inputs ($\Delta\Omega_{od}$) accurately capture the change in school quality experienced by movers when they move across neighborhoods. As a further robustness check, I also interact the school fixed effects with the actual number of years spent in the associated schools instead age-at-move (column (3)). The results are in line with the main conclusion that schools account for a large fraction of exposure effects, but some residual neighborhood exposure effect still persists above and beyond the contribution of schools.

The model in Section III shows that the magnitude of the full convergence rate depends on movers' propensity to attend schools similar in quality to those attended by permanent residents. For schools to possibly drive the benefits of moving to a better area, it must be that the compliance rate $E\left[\frac{\bar{\psi}_{s(d(i))}-\bar{\psi}_{s(o(i))}}{\psi_d^{PR}-\psi_o^{PR}}\right]$ is not zero. As a sanity check, I conduct an event-study similar to the one described in Section VI.C to validate that the quality of schools attended by movers shifts towards that of the permanent residents of the destination neighborhood when they move. The index of relative school quality is given by

$$\sigma^{\psi}_{od(i,t)} = \frac{\delta_{s(i,t)} - \overline{\delta}_{s(o,t)}}{\overline{\delta}_{s(d,t)} - \overline{\delta}_{s(o,t)}}$$

where $\delta_{s(i,t)}$ is the quality of the school attended by student i at time t (measured by the fixed effects estimates obtained in Section IV.A), and $\overline{\delta}_{s(n,t)}$ is the average quality of schools attended by permanent residents of FSA n at time t. The corresponding event-study results are shown in Figure A.20. The index increases sharply in value right at the time of the move. While there seems to be a modest spike in the year preceding the move, this bump is very small compared to the break that occurs on impact. The magnitude of the jump implies a compliance rate close to 60%. Given the many school choice options in Montreal, it is not surprising that this rate is not 100%. Yet, this exercise demonstrates that movers do experience a substantial change in school quality as a result of a move.

Sampling error One possibility is that sampling error affects the estimation of school and neighborhood fixed effects differentially. For instance, it might be that estimation error accounts for a larger fraction of the variance in $\Delta\Omega_{od}$ than of the variance in $\Delta\Lambda_{od}$. To verify that this is not the case, I re-estimate equation (9) using estimates of the fixed effects $\Omega_{s(n(i))}$ and Λ_n that are shrunk towards zero using empirical Bayes techniques (Chandra et al., 2016; Best, Hjort and Szakonyi, 2017; Kane and Staiger, 2008). The associated decomposition results are shown in Table A.16.

The first row shows the associated total convergence rates, which are calculated using equation (10) rather than estimated directly with equation (8).[63] These rates are slightly larger than the ones presented in Table IV, which imply that my main estimates suffer from a small attenuation bias. The shares of total exposure effects due to schools reported in this table do not account for the endogeneity of school attendance and should therefore be compared to the corresponding results presented in column (1) of Table V. For all three outcomes, adjusting for measurement error only reinforces the conclusion that school effects account for most of the benefits of moving to a better area, with school shares exceeding 80% for university enrollment and finishing secondary school on time.

[63]Note that when using shrunk fixed effects, the total convergence rate estimated directly with equation (8) is not exactly equal to that obtained by plugging the coefficients of equation (9) into the decomposition equation. This is because contrary to unadjusted estimates, shrunk estimates of $\bar{y}_n^{PR}$ are not equal to the sum of shrunk estimates of $\bar{\Omega}_n^{PR}$ and $\bar{\Lambda}_n^{PR}$(the identity $\Delta\bar{y}_{od}^{-s} \equiv \Delta\bar{y}_{od} - \pi\Delta\Omega_{od}$ therefore also breaks down when using shrunk estimates). Yet, with empirical Bayes fixed effects, both ways of calculating the total convergence rate (eq. (8) or eq. (10)) produce rates larger in magnitude than those obtained with unadjusted fixed effects.

VII Conclusion

Establishing whether schools drive neighborhood exposure effects is crucial on a policy level to inform the development of community-wide versus in-school intervention programs. Yet, isolating the effects of neighborhoods from those of schools is a difficult task since in most places students are allocated to schools on the basis of residence. This paper overcomes these difficulties by bringing together two research designs in order to isolate the fraction of total long-term exposure effects that is driven by school effects.

The first contribution of this paper is to break the mechanical link between the two dimensions by exploiting institutional features of Quebec's education system. In Montreal, default options influence parents' decision over which schools their child will attend. Building upon this observation, I find that the quality of the primary and secondary schools children attend have large effects on their educational attainment. More precisely, immediate neighbors living on opposite sides of a French primary school boundary at age 6 exhibit significantly different propensities to enroll into university more than 10 years later.

My second set of results demonstrates that children who move to a better neighborhood at a young age benefit substantially from this change. In particular, I successfully replicate the findings of linear exposure effects of Chetty and Hendren (2018*a*) using within-city variation and implementing their methods in a different setting, looking at a much smaller scale of geography and examining different outcomes. My estimates suggest that movers' educational attainment improve linearly with each year spent in a better location at an annual rate of approximately 4.5%.

The main result of the paper is that schools are the main driver of total childhood exposure effects. Decompositions that take into account the endogeneity of school quality indicate that between 50 and 70% of the educational benefits of moving to a better location are due to schools rather than neighborhoods themselves. These findings strongly corroborate earlier conclusions made on the relative importance of schools and neighborhoods (Dobbie and Fryer, 2015; Fryer and Katz, 2013; Oreopoulos, 2012; Gould, Lavy and Paserman, 2004). By showing that spatial inequalities in long-term educational attainment are partly rooted in the quality of schools children attend, the results bear important policy implications. They notably suggest that school reforms or interventions might be more effective than community programs or relocation policies in raising educational attainment.

The decomposition approach developed in this paper also opens up new possibilities for examining mechanisms in other settings. For instance, the idea of partitioning the outcomes of non-movers and using variation from movers to pin down and decompose place effects could be valuable in investigations of the quantitative importance of physicians in driving hospital effects, or of teachers for school effects.

While the magnitude of the estimated exposure effects, and the main conclusion of this paper more broadly, may reflect a social reality unique to Montreal, I believe the results are very informative for other contexts as well. For instance, because of Quebec's open enrollment policies and the unusual availability of private school options in Montreal, the link between school attendance and residence is relatively loose. Hence, in jurisdictions where schools and neighborhoods are tightly linked, one may expect schools to contribute even more to spatial inequalities in educational attainment than the results in this paper suggest. I leave for future research the question of whether this conclusion extends to other socio-economic outcomes such as earnings and criminal behavior.

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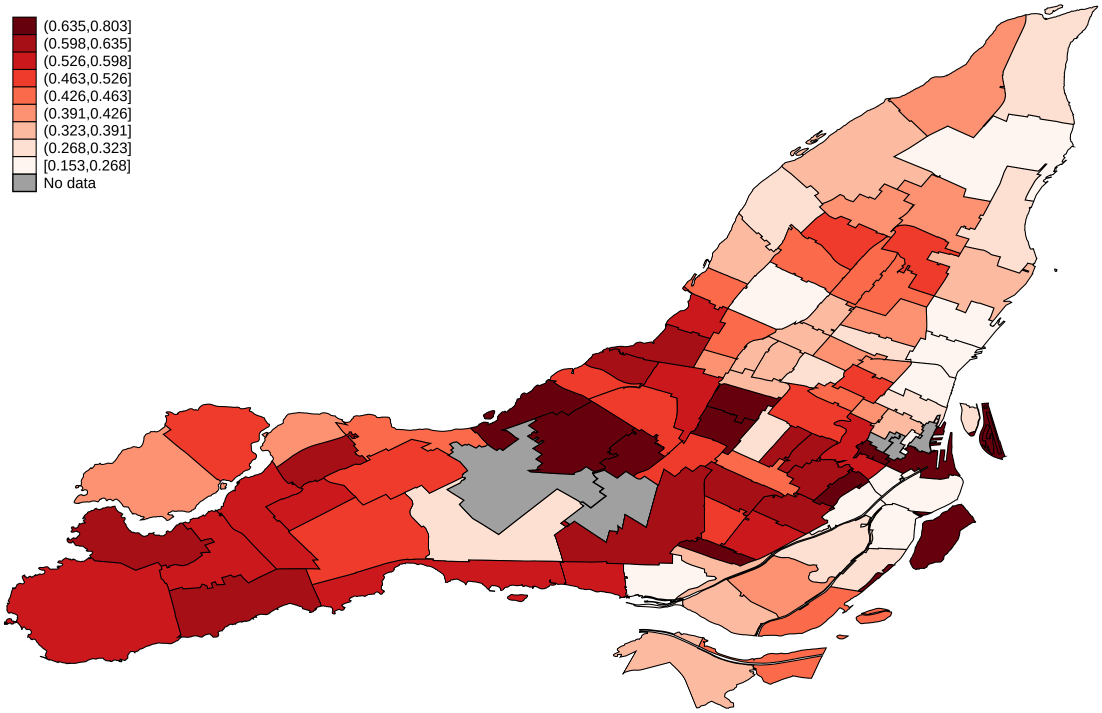
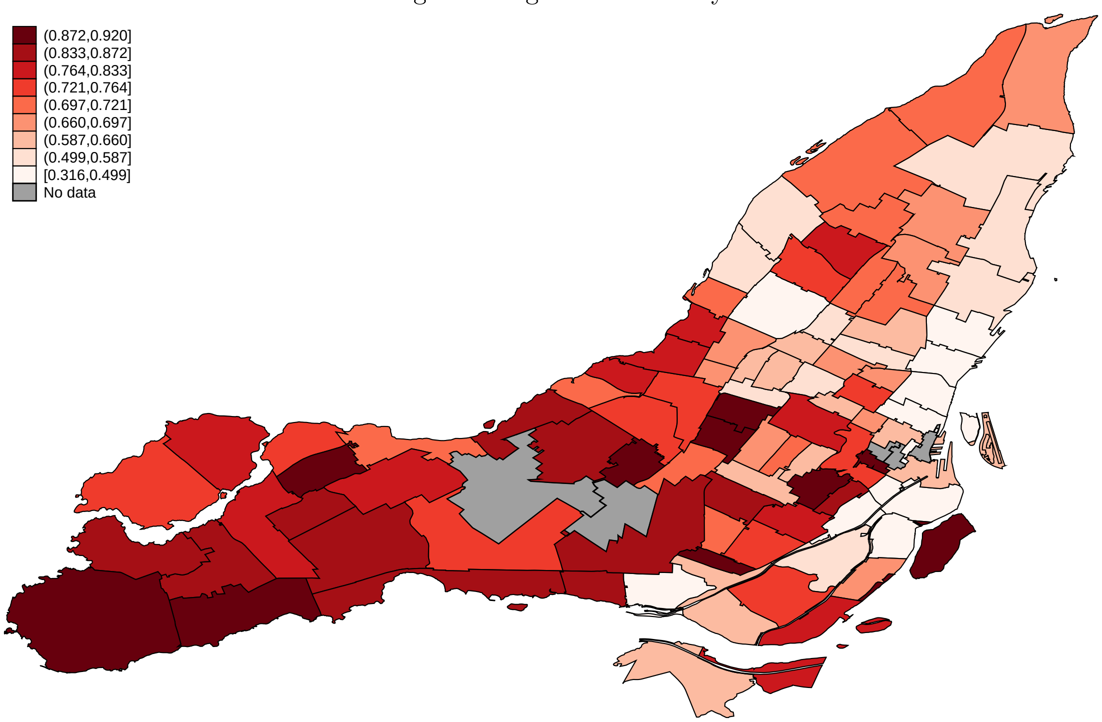
Tables and Figures

Figure I: Spatial Variation in Educational Outcomes

Panel A: Fraction ever enrolled in university



Panel B: Fraction graduating from secondary school on time

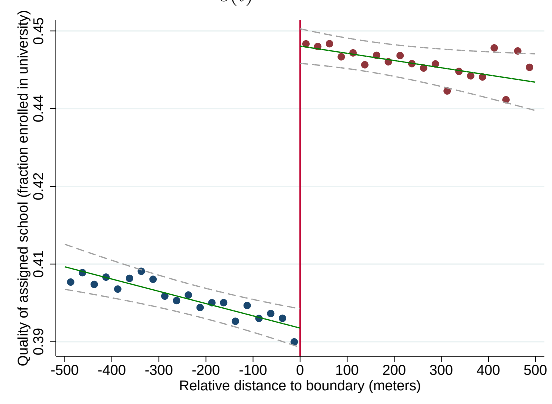


Notes: Statistics based on permanent residents. Outcomes are adjusted for cohort effects. Data for FSAs with fewer than 10 permanent residents are not shown (no data).

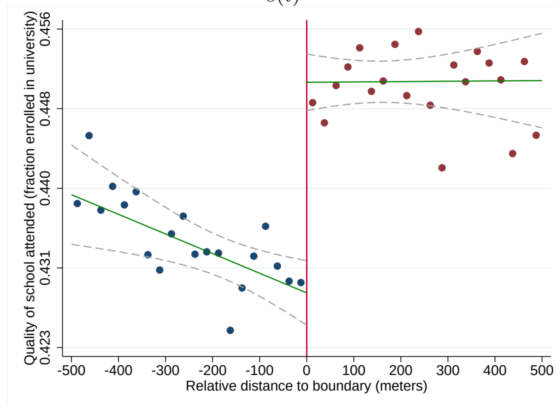
Figure II: Discontinuities in School Quality at French Primary School Boundaries

All permanent residents

Panel A: Quality $\delta^P_{s(i)}$ of assigned French school

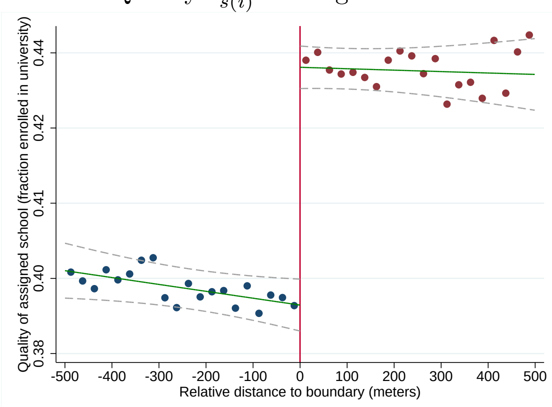


Panel B: Quality $\delta^P_{s(i)}$ of school attended

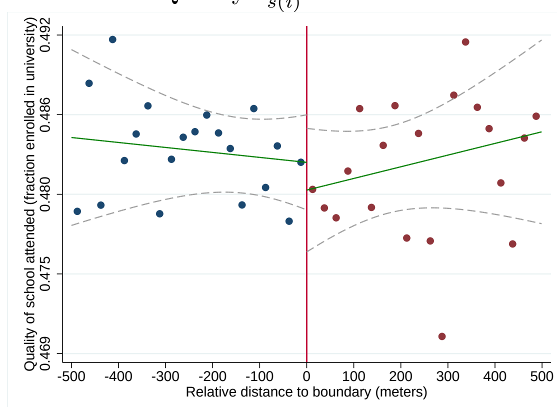


Students in English schools only (Placebo)

Panel C: Quality $\delta^P_{s(i)}$ of assigned French school



Panel D: Quality $\delta^P_{s(i)}$ of school attended

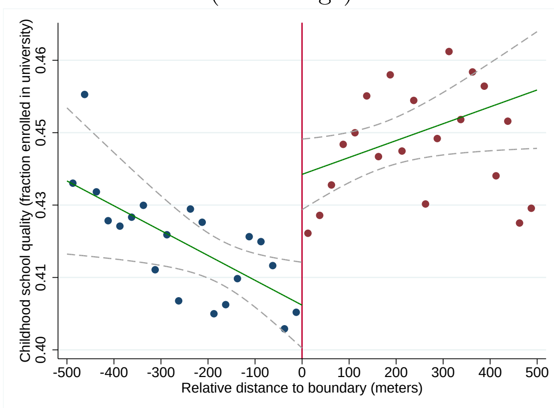


Notes: For each French primary school boundary, the neighborhood school with greater school quality – in terms of university enrollment – is assigned to the right. The variable on the vertical axis is first residualized on cohort, FSA, and boundary fixed effects. The figure shows the average school quality of schools attended by students at baseline, by distance to the boundary. Attendance recorded at baseline (grade 1). In Panels A and B, the sample includes all permanent residents, and in Panels C and D it is constituted of students enrolled in English schools only. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

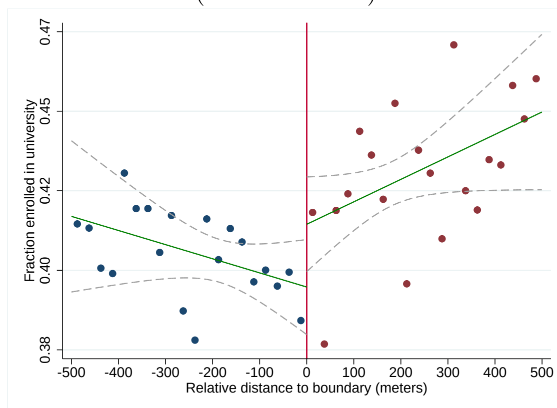
Figure III: Regression-discontinuity – First-stage and Reduced-form Relationships

All permanent residents

Panel A: Total childhood school quality $\Omega^{-i}_{s(n(i))}$ (First-stage)

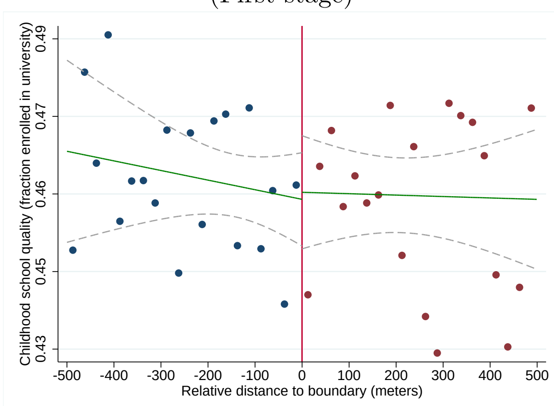


Panel B: University enrollment (Reduced-form)

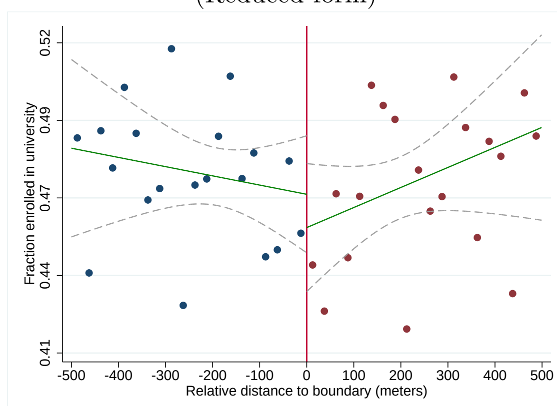


Students in English schools only (Placebo)

Panel C: Total childhood school quality $\Omega^{-i}_{s(n(i))}$ (First-stage)



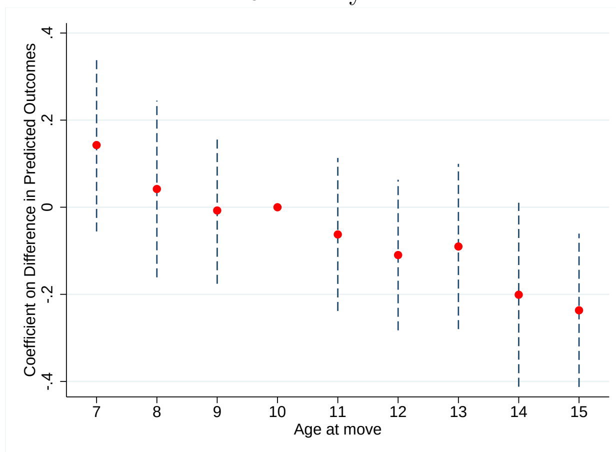
Panel D: University enrollment (Reduced-form)



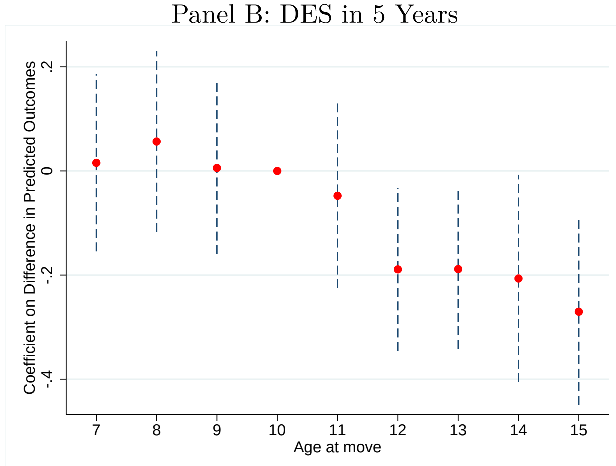
Notes: For each French primary schools boundary, the neighborhood school with greater school quality – in terms of university enrollment – is assigned to the right. The variable on the vertical axis is first residualized on cohort, FSA, and boundary fixed effects. The figure shows the average school quality of schools attended by students at baseline, by distance to the boundary. Attendance recorded at baseline (grade 1). In Panels A and B, the sample includes all permanent residents, and in Panels C and D it is constituted of students enrolled in English schools only. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

Figure IV: Non-parametric Total Exposure Effects

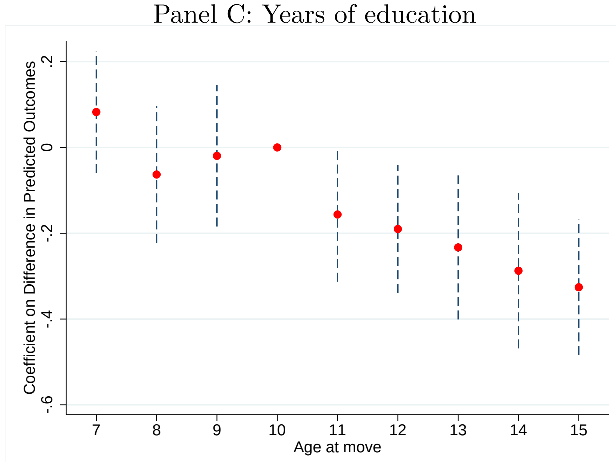
Panel A: University enrollment



Panel B: DES in 5 Years



Panel C: Years of education



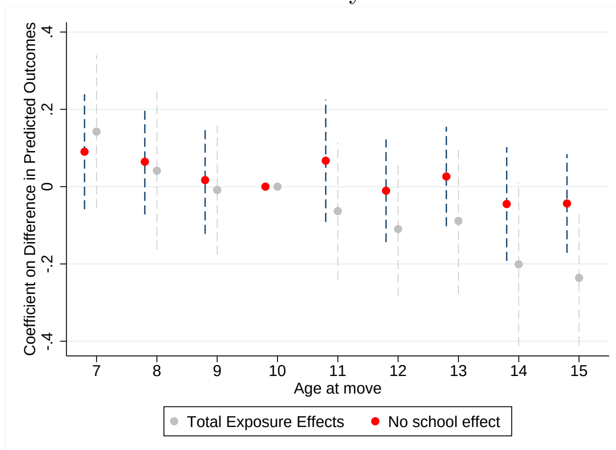
Notes: Sample includes all movers who remained within Montreal. Observation in FSAs with less than 10 permanent residents are omitted. Coefficients shown are obtained by regressing

$$y_{icmod} = \sum_{m=7}^{15} \beta_m \left(\Delta \bar{y}_{od} \times 1\{m_i = m\}\right) + \gamma X_{icmod} + \alpha_{od} + \alpha_m + \alpha_c + \epsilon_{icmod}$$

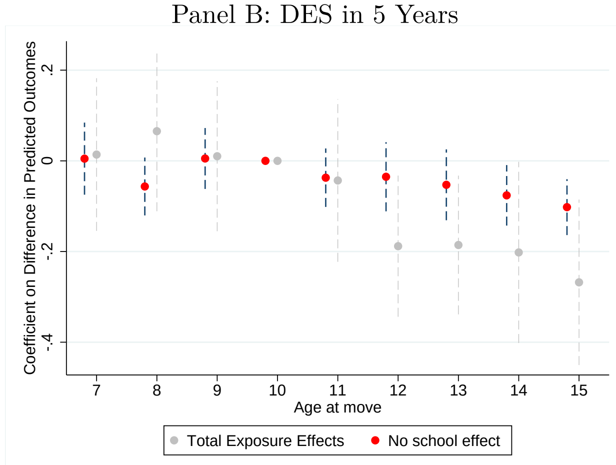
Standard errors are clustered at the destination level.

Figure V: Non-parametric Restricted Exposure Effects – No School Effect

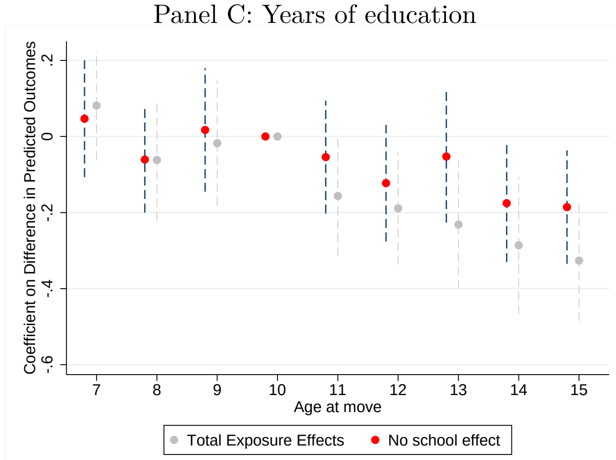
Panel A: University enrollment



Panel B: DES in 5 Years



Panel C: Years of education



Notes: Notes: Sample includes all movers who remained within Montreal. Observation in FSAs with less than 10 permanent residents are omitted. Coefficients in red correspond to age-specific restricted coefficients for which the school channel is shut down (β_m^{-s}). Standard errors are clustered at the destination level and calculated by the delta method.

Table I: Descriptive Statistics

Variables	All students	Permanent residents	Movers: Within Montreal	Movers: Left Montreal	Difference between (2) and (3)
	mean (s.d.)	mean (s.d.)	mean (s.d.)	mean (s.d.)	coef. (s.e.)
	(1)	(2)	(3)	(4)	(5)
Female	0.49	0.49	0.49	0.49	0.001
	[0.500]	[0.500]	[0.500]	[0.500]	[0.004]
Age on September 30	6.02	6.01	6.04	6.02	-0.033***
	[0.376]	[0.329]	[0.455]	[0.330]	[0.003]
Mother tongue: French	0.49	0.47	0.45	0.65	0.026***
	[0.500]	[0.499]	[0.497]	[0.478]	[0.004]
Mother tongue: English	0.21	0.26	0.20	0.09	0.063***
	[0.407]	[0.439]	[0.397]	[0.291]	[0.003]
Mother tongue: Other	0.30	0.27	0.36	0.26	-0.090***
	[0.457]	[0.444]	[0.479]	[0.439]	[0.003]
Language at home: French	0.54	0.50	0.50	0.69	0.006*
	[0.499]	[0.500]	[0.500]	[0.460]	[0.004]
Language at home: English	0.26	0.32	0.24	0.12	0.077***
	[0.437]	[0.466]	[0.427]	[0.326]	[0.003]
Language at home: Other	0.21	0.18	0.26	0.18	-0.083***
	[0.405]	[0.384]	[0.438]	[0.387]	[0.003]
Immigrant	0.10	0.07	0.14	0.11	-0.080***
	[0.296]	[0.247]	[0.347]	[0.308]	[0.002]
Language at school: French	0.75	0.69	0.77	0.88	-0.073***
	[0.433]	[0.461]	[0.423]	[0.324]	[0.003]
Uses School Day Care (baseline)	0.25	0.24	0.24	0.29	0.005
	[0.432]	[0.428]	[0.426]	[0.453]	[0.003]
In difficulty (baseline)	0.04	0.03	0.05	0.05	-0.016***
	[0.193]	[0.170]	[0.209]	[0.219]	[0.001]
Handicapped (baseline)	0.01	0.01	0.02	0.01	-0.003***
	[0.118]	[0.116]	[0.126]	[0.111]	[0.001]
Ever in difficulty by age 15	0.31	0.25	0.37	0.37	-0.116***
	[0.462]	[0.431]	[0.482]	[0.483]	[0.003]
Students	92,764	44,912	31,526	16,326	76,438

Notes: The main sample excludes students who left Quebec's system before turning 16. Permanent residents are defined as students who always resided in the same FSA until the age of 15. Movers within Montreal are those who moved across FSAs at least once and were still living on the Island of Montreal at age 15. Movers who left Montreal were residing in the province of Quebec but outside the Island of Montreal at age 15.

Table II: Variation Across Neighborhoods and Schools

	Outcome					
	DES in 5 years		University enrollment		Years of education	
	(1)	(2)	(3)	(4)	(5)	(6)
Student-level standard deviation of fixed effects:						
Schools	0.270	0.264	0.249	0.235	1.207	1.141
Neighborhoods (FSAs)	0.138	0.046	0.139	0.062	0.680	0.258
Dependent variable summary statistics:						
Mean	0.706		0.443		13.228	
Standard deviation	[0.456]		[0.497]		[2.113]	
Fixed effects estimated						
Separately	x		x		x	
Simultaneously		x		x		x
Number of students	44,912					
Number of primary schools	440					
Number of secondary schools	218					
Number of neighborhoods	95					

Notes: Sample restricted to permanent residents. School fixed effects are measured by the sum of a primary and a secondary school fixed effect. In columns (1), (3) and (5), school and neighborhood effects are respectively estimated in separate regressions. In columns (2), (4) and (6), all fixed effects are estimated simultaneously from equation (5).

Table III: School Effects – Regression-discontinuity Estimates

	First-stage(s)			Reduced-form	RD-IV
Dependent variable:	Quality of *assigned* school at baseline ($\delta^P_{s(i)}$)	Quality of school *attended* at baseline ($\delta^P_{s(i)}$)	Childhood average school quality ($\Omega^{-i}_{s(n(i))}$)	Outcome	Outcome
	(1)	(2)	(3)	(4)	(5)
Measure of educational attainment					
			All permanent residents		
University enrollment	0.0631***	0.0245***	0.0328***	0.0279***	0.8542***
	(0.0032)	(0.0027)	(0.0065)	(0.0087)	(0.1645)
Secondary school diploma in 5 years	0.0715***	0.0297***	0.0337***	0.0347***	1.0340***
	(0.0037)	(0.0025)	(0.0061)	(0.0084)	(0.1618)
Years of schooling	0.2933***	0.1157***	0.1511***	0.1165***	0.7739***
	(0.0148)	(0.0120)	(0.0298)	(0.0390)	(0.1575)
N	43296	43279	43291	43296	43291
			Placebo: Students in English schools		
University enrollment	0.0632***	-0.0017	-0.0012	-0.0098	-
	(0.0044)	(0.0041)	(0.0099)	(0.0157)	-
Secondary school diploma in 5 years	0.0722***	0.0031	0.0008	-0.0081	-
	(0.0059)	(0.0026)	(0.0071)	(0.0116)	-
Years of schooling	0.2836***	0.0052	0.0104	-0.0448	-
	(0.0204)	(0.0156)	(0.0433)	(0.0655)	-
N	13446	13444	13444	13446	
Cohort fixed effects	x	x	x	x	x
Individual characteristics	x	x	x	x	x
Neighborhood (FSA) fixed effects	x	x	x	x	x
Boundary fixed effects	x	x	x	x	x

Notes: This table reports RD estimates. In columns (1) and (2), primary school quality is measured using the fixed effects, $\delta^P_{s(i)}$, estimated in Section IV.A. In column (3), the dependent variable is childhood average school quality $\Omega^{-i}_{s(n(i))}$. Column (5) reports 2SLS estimates of equations (6) and (7). In all specifications, the control function for distance to boundary is linear and allows for different slopes on either side of the threshold. In the first three rows, the sample includes all permanent residents. In the last three rows, only permanent residents enrolled in English schools are included. All standard errors are clustered at the French primary school boundary level.
*** p<0.01, ** p<0.05, * p<0.1

Table IV: Total Exposure Effects

Sample:	All movers		One-time movers	
	(1)	(2)	(4)	(5)
Measure of educational attainment				
University enrollment	-0.0424***	-0.0412***	-0.0416***	-0.0408***
	(0.0090)	(0.0092)	(0.0116)	(0.0115)
Secondary school diploma in 5 years	-0.0421***	-0.0402***	-0.0506***	-0.0502***
	(0.0088)	(0.0088)	(0.0117)	(0.0117)
Years of schooling	-0.0488***	-0.0471***	-0.0444***	-0.0435***
	(0.0088)	(0.0094)	(0.0103)	(0.0102)
Cohort fixed effects	x	x	x	x
Individual characteristics	x	x	x	x
Age at move fixed effects	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x
Only moved once			x	x
Times in difficulty before moving		x		x
N	24316	24316	15533	15533

Notes: Coefficients shown in the table are convergence rates β. Individual characteristics include gender, immigrant status, allophone status, born in Canada but outside Quebec, English spoken at home, day care use at baseline, 'in difficulty' status at baseline, handicapped status. In columns (2) and (4), the model includes a set of dummies for each possible value of number of times in difficulty prior to moving. Standard errors are clustered at the destination neighborhood level. Note that the movers sample contains a total of 25,993 observations, of which 1,677 are singletons and therefore dropped in the estimation.
*** p<0.01, ** p<0.05, * p<0.1

Table V: Decomposition of Total Exposure Effects

	(1)	(2)	(3)
University enrollment			
	Total exposure effects		
β	-0.0424***	-0.0424***	-0.0424***
	(0.0090)	(0.0090)	(0.0090)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0105**	-0.0106**	-0.0148**
	(0.0042)	(0.0043)	(0.0059)
β^{-n} (No neighborhood effects)	-0.0318***	-0.0318***	-0.0276***
	(0.0072)	(0.0071)	(0.0069)
Share school effects	75%	75%	65%
	(0.0789)	(0.0785)	(0.1093)
Secondary school diploma in 5 years			
	Total exposure effects		
β	-0.0421***	-0.0421***	-0.0421***
	(0.0088)	(0.0088)	(0.0088)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0109***	-0.0129***	-0.0111***
	(0.0037)	(0.0042)	(0.0036)
β^{-n} (No neighborhood effects)	-0.0309***	-0.0289***	-0.0307***
	(0.0085)	(0.0082)	(0.0082)
Share school effects	74%	69%	73%
	(0.0879)	(0.0965)	(0.0831)
Years of education			
	Total exposure effects		
β	-0.0488***	-0.0488***	-0.0488***
	(0.0088)	(0.0088)	(0.0088)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0147***	-0.0160***	-0.0265***
	(0.0043)	(0.0045)	(0.0075)
β^{-n} (No neighborhood effects)	-0.0340***	-0.0328***	-0.0223***
	(0.0081)	(0.0078)	(0.0085)
Share school effects	70%	67%	46%
	(0.0839)	(0.0834)	(0.1386)
Measure of school quality	$\pi\Omega_{s(n)}$	$\pi\Omega^{-i}_{s(n)}$	$\pi\Omega^{-i}_{s(n)}$
π	1	1	RD estimate

Notes: Sample restricted to movers within Montreal. Standard errors are clustered at the destination FSA level, and obtained by the delta method for restricted convergence rates. Restricted convergence rates are calculated using equation (10). β^{-s} is a restricted rate for which $\beta_s = 0$, and β^{-n} is a restricted rate for which $\beta_n = 0$. Share school effects is given by the ratio $\frac{\beta-\beta^{-s}}{\beta}$.

Table VI: Total Exposure Effects – Siblings Subsample

Sample:	Siblings only			
	(1)	(2)	(3)	(4)
Measure of educational attainment				
University enrollment	-0.0453	-0.0571	-0.0478*	-0.0504*
	(0.0365)	(0.0359)	(0.0275)	(0.0274)
Secondary school diploma in 5 years	-0.0242	-0.0434	-0.0392	-0.0500*
	(0.0378)	(0.0365)	(0.0301)	(0.0300)
Years of schooling	-0.0453	-0.0629**	-0.0444*	-0.0486**
	(0.0311)	(0.0299)	(0.0236)	(0.0233)
Cohort fixed effects	x	x	x	x
Individual characteristics	x	x	x	x
Age at move fixed effects	x	x	x	x
Origin-by-destination fixed effects	x	x		
Household fixed effects			x	x
Times in difficulty before moving		x		x
N	3674	3674	3674	3674

Notes: Restricted to households in which siblings lived at the same address for at least 75% of the observed years. In columns (2) and (4), the model includes a set of dummies for each possible value of number of times in difficulty prior to moving. Standard errors are clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

Table VII: Exposure Effects Net of Movers' School Attendance

	(1)	(2)	(3)
University enrollment	-0.0424***	-0.0111	-0.0123
	(0.0090)	(0.0101)	(0.0089)
Share school effects		74%	71%
Secondary school diploma in 5 years	-0.0421***	-0.0117	-0.00749
	(0.0088)	(0.0093)	(0.0081)
Share school effects		72%	82%
Years of education	-0.0488***	-0.0178**	-0.0181**
	(0.0088)	(0.0079)	(0.0075)
Share school effects		64%	63%
Cohort fixed effects	x	x	x
Individual characteristics	x	x	x
Age at move fixed effects	x	x	x
Origin-by-destination fixed effects	x	x	x
School fixed effects			
(o) School at baseline		x	x
(o) School at baseline * age-at-move (linear)		x	
(o) School at baseline * years-exposure			x
(d) School at age 15		x	x
(d) School at age 15 * age-at-move (linear)		x	
(d) School at age 15 * years-exposure			x
N	24316	24244	24244

Notes: Primary school fixed effects are based on school attendance at baseline. Secondary school fixed effects are based on school attendance at age 15. In columns (2) and (3), school fixed effects are linearly interacted with age-at-move and years of exposure, respectively. Standard errors are clustered at the destination neighborhood level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Online Appendix - For Online Publication

Appendix Tables and Figures

Figure A.1: Quebec's Education System

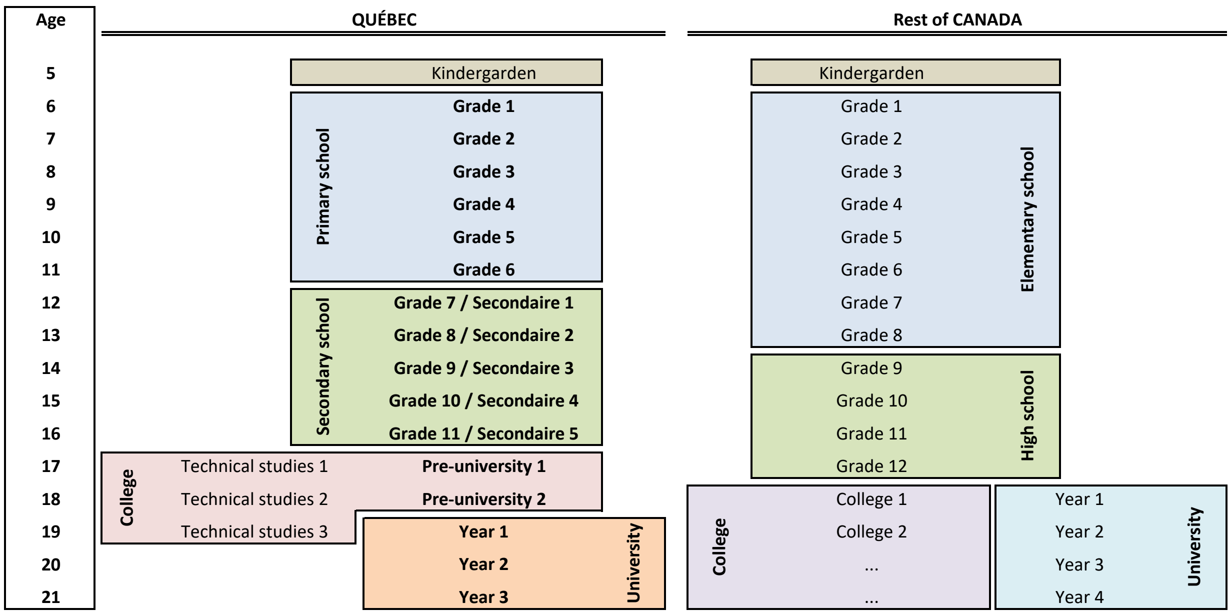
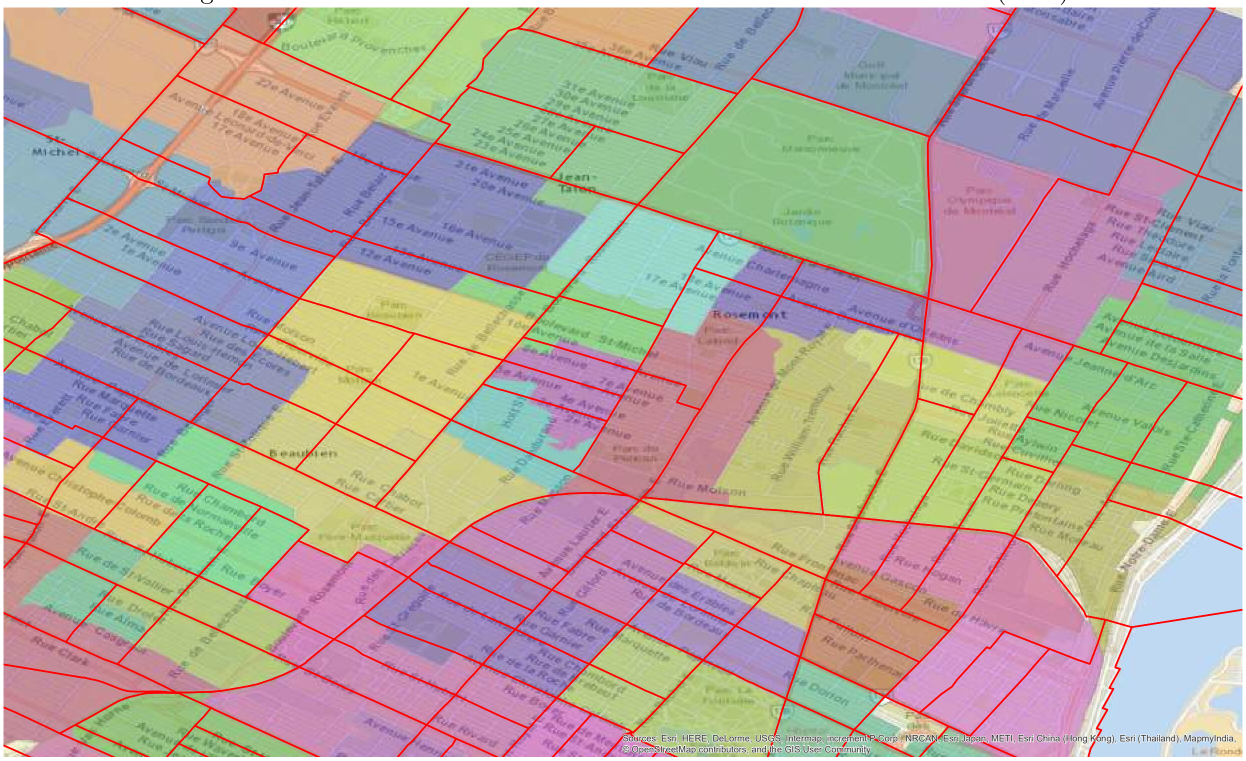
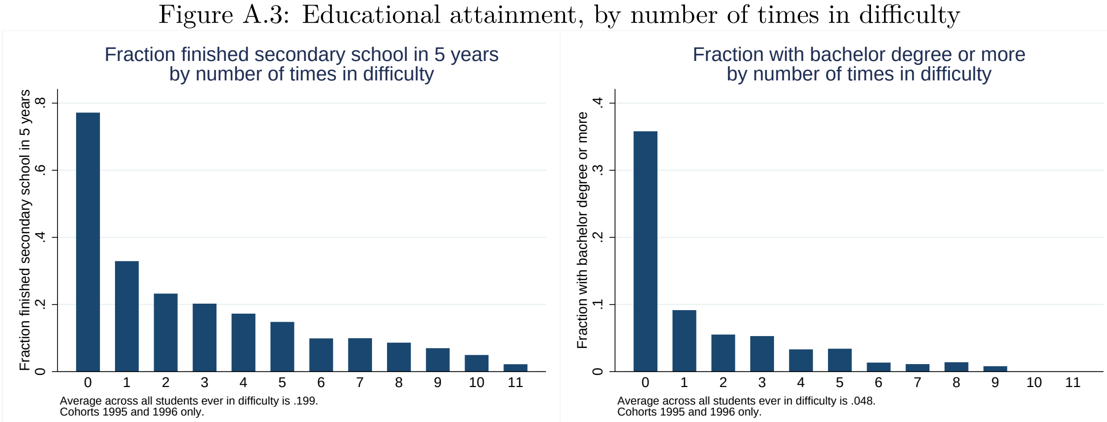


Figure A.2: Catchment Areas and Census Tracts in Eastern Montreal (2001)



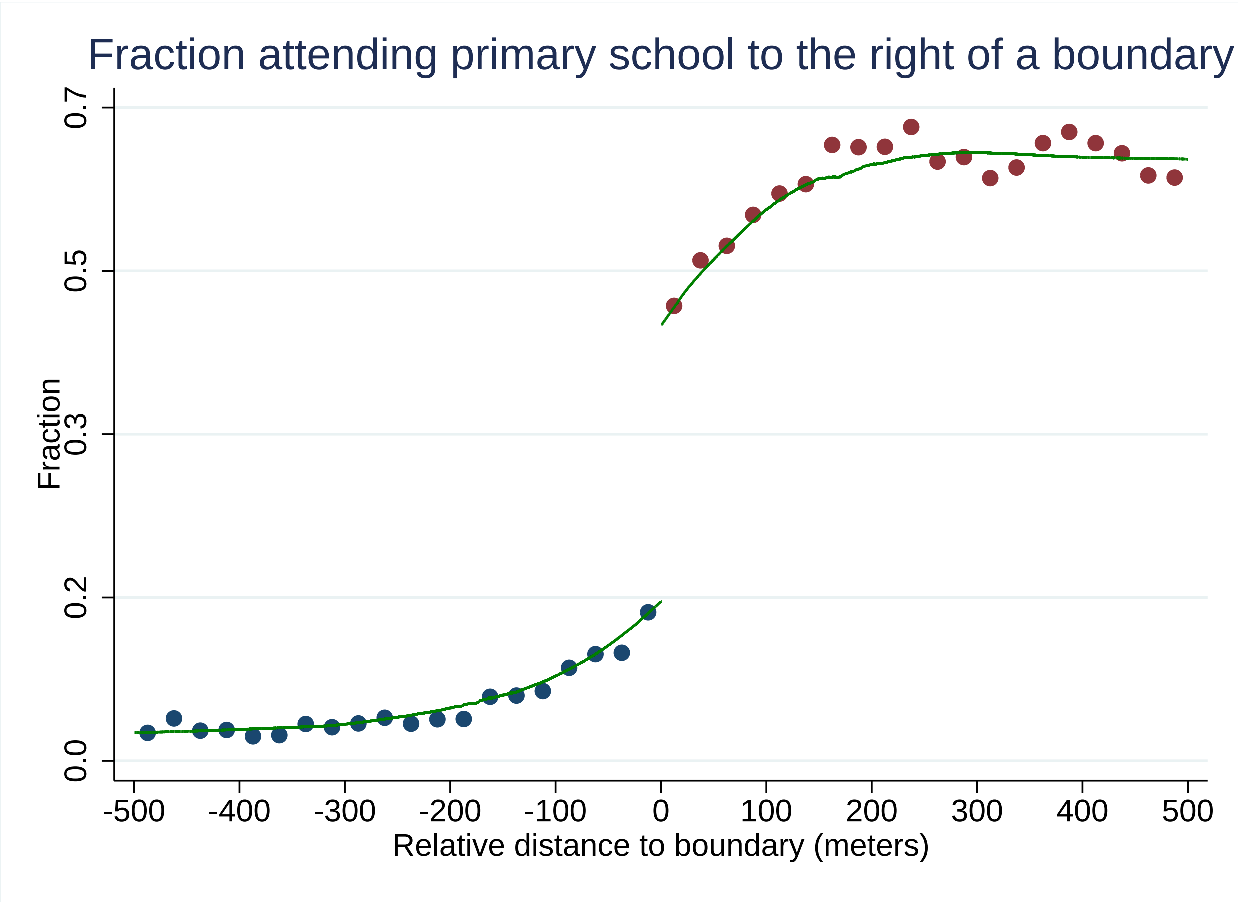
Notes: Colored areas indicate French primary school catchment areas as of 2001. Red lines denote census tracts.

Figure A.3: Educational attainment, by number of times in difficulty



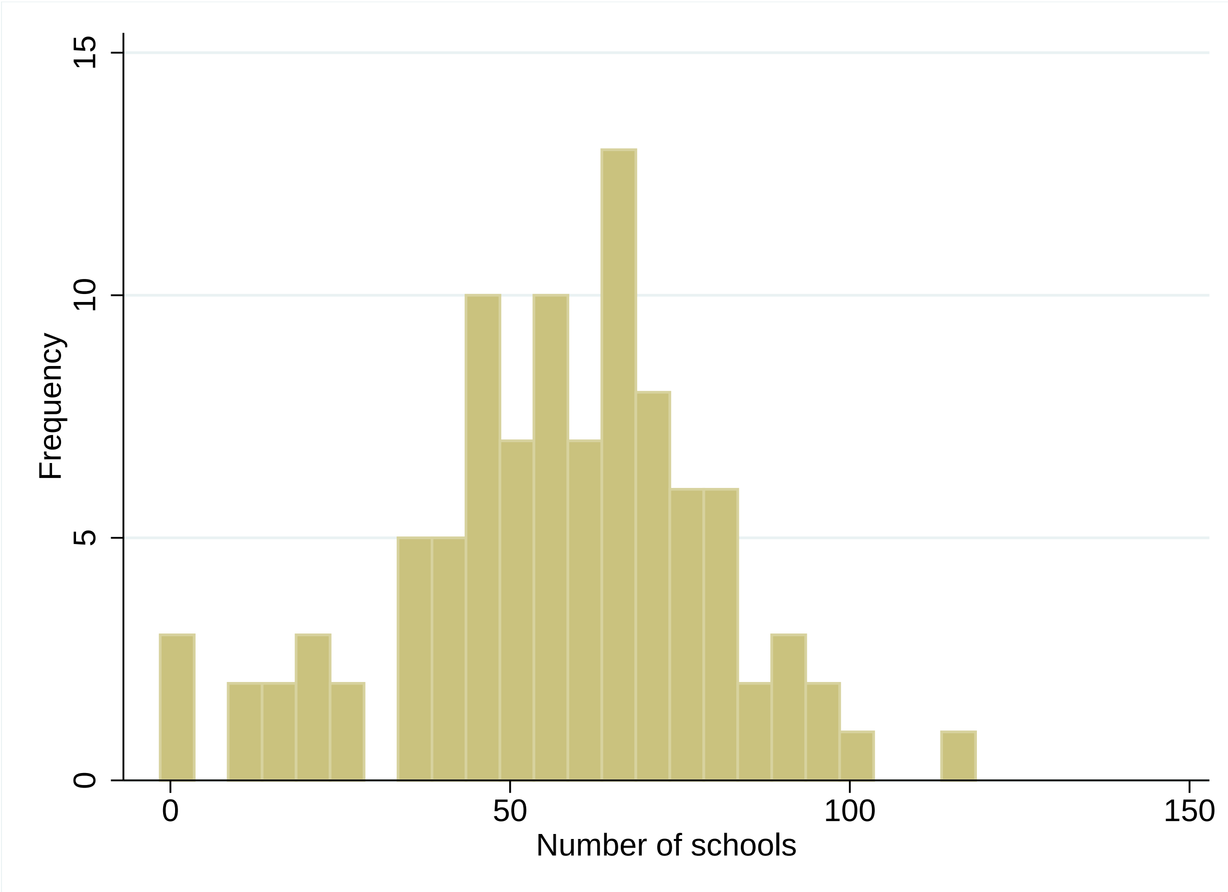
Notes: Sample restricted to students from the 1995 and 1996 cohorts. Too few students of the later cohorts have completed a bachelor degree by 2014-2015 to analyze this outcome for these students.

Figure A.4: Discontinuity in School Attendance At Boundaries



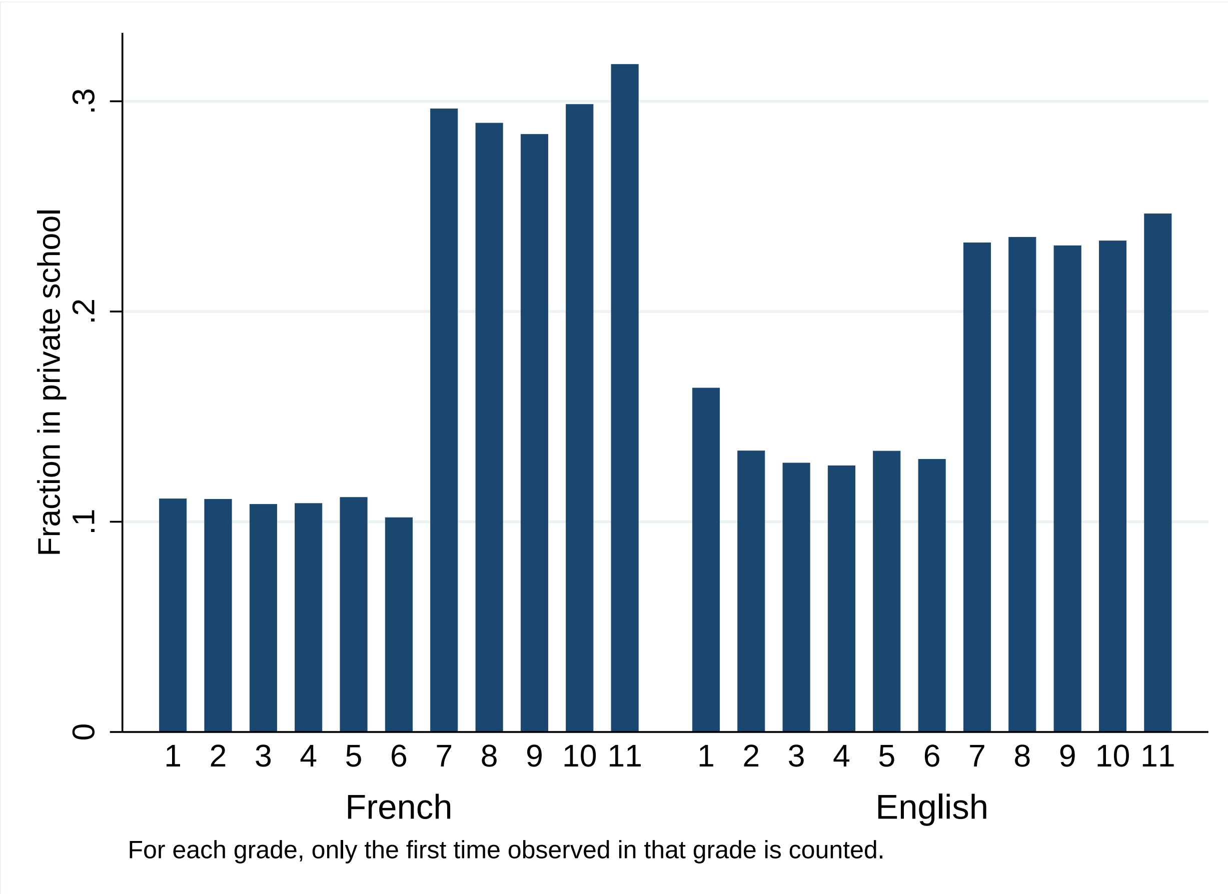
Notes: For each French primary school boundary, one neighborhood school is randomly assigned to the right. The figure shows the fraction of students enrolled in that school, by distance to the boundary. Students at positive distance are assigned the random chose default school. Students at negative distances are assigned to a school other than the one to the right. Attendance recorded at baseline (grade 1). Sample is restricted to students in French schools.

Figure A.5: Distribution of school choice across FSAs



Notes: The histogram shows the distribution of FSAs by number of different primary schools attend by its residents. School attendance measured at baseline (i.e. first enrollment in grade 1).

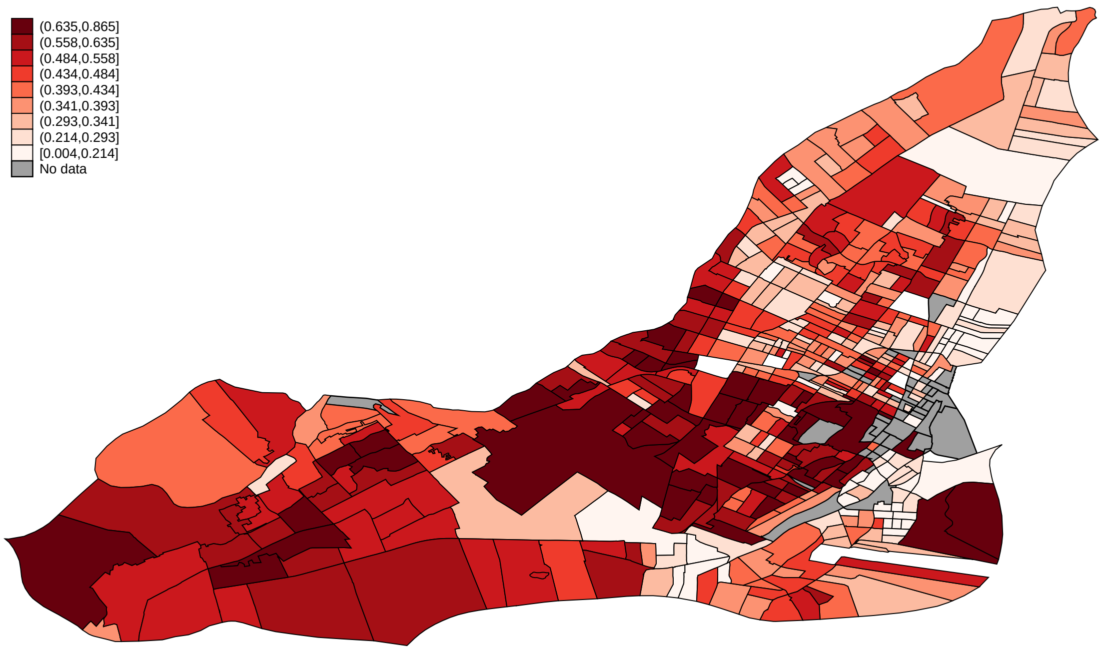
Figure A.6: Fraction in private schools, by grade and language of instruction



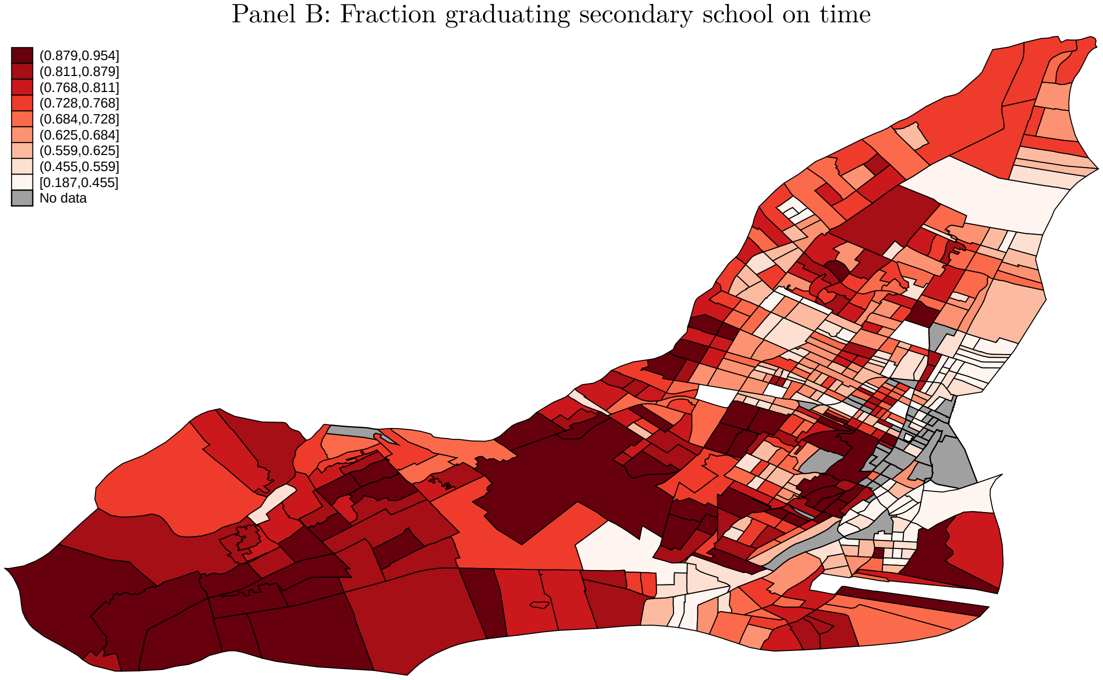
Notes: Statistics calculated over main analytical sample of 92,764 students. Data shown separately for students in French and English schools.

Figure A.7: Spatial variation in educational outcomes - Census tract level

Panel A: Fraction ever enrolled in university

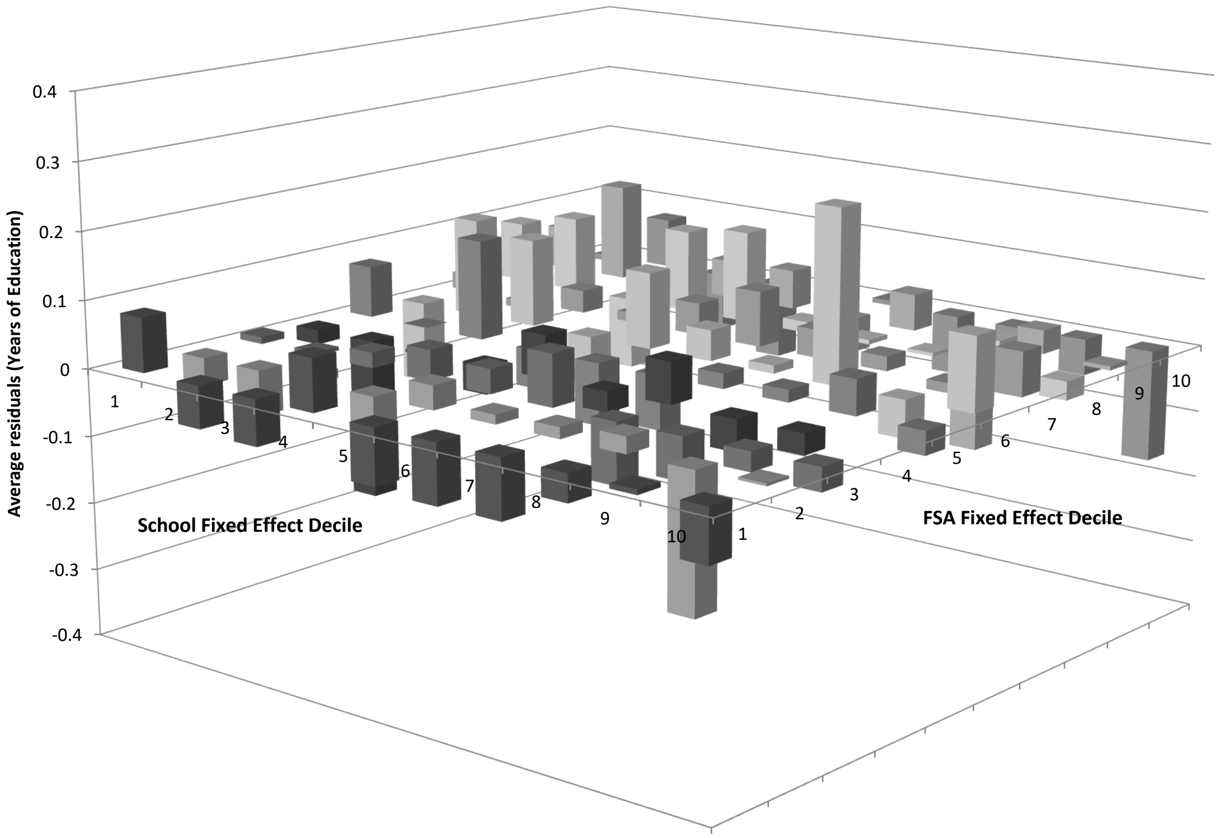


Panel B: Fraction graduating secondary school on time



Notes: Statistics based on permanent residents (students who always resided in the same census tract). Outcomes are adjusted for cohort effects. Data for census tracts with fewer than 10 permanent residents are not shown (no data).

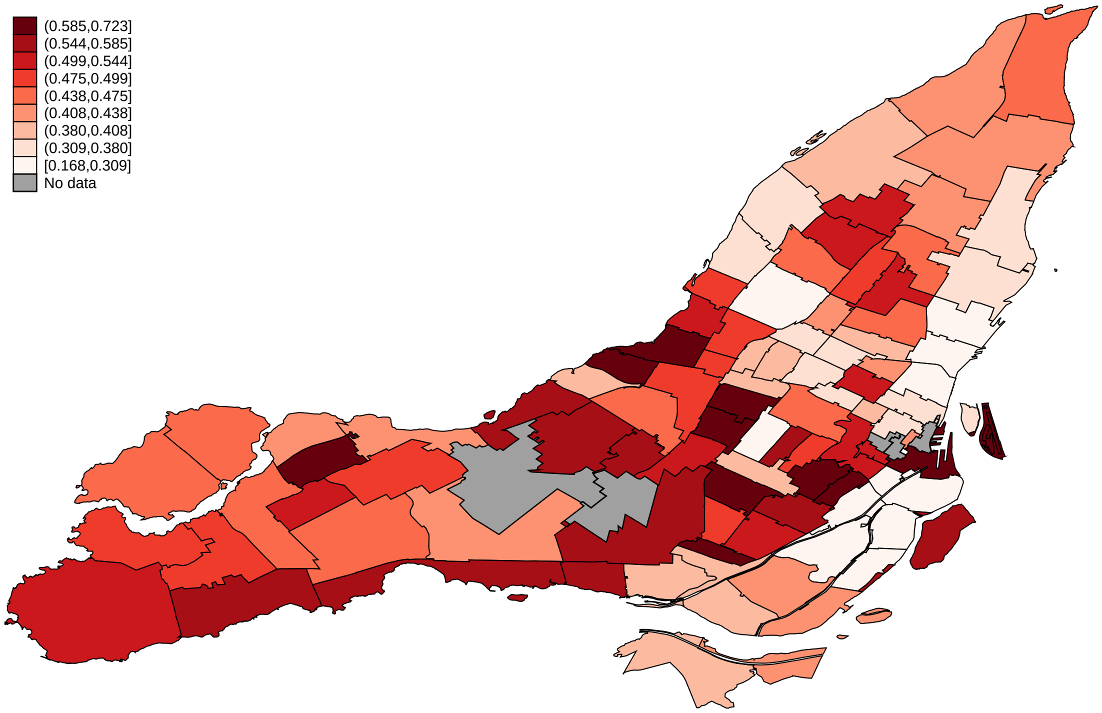
Figure A.8: Mean Years of Education (Residuals), by School and FSA Deciles



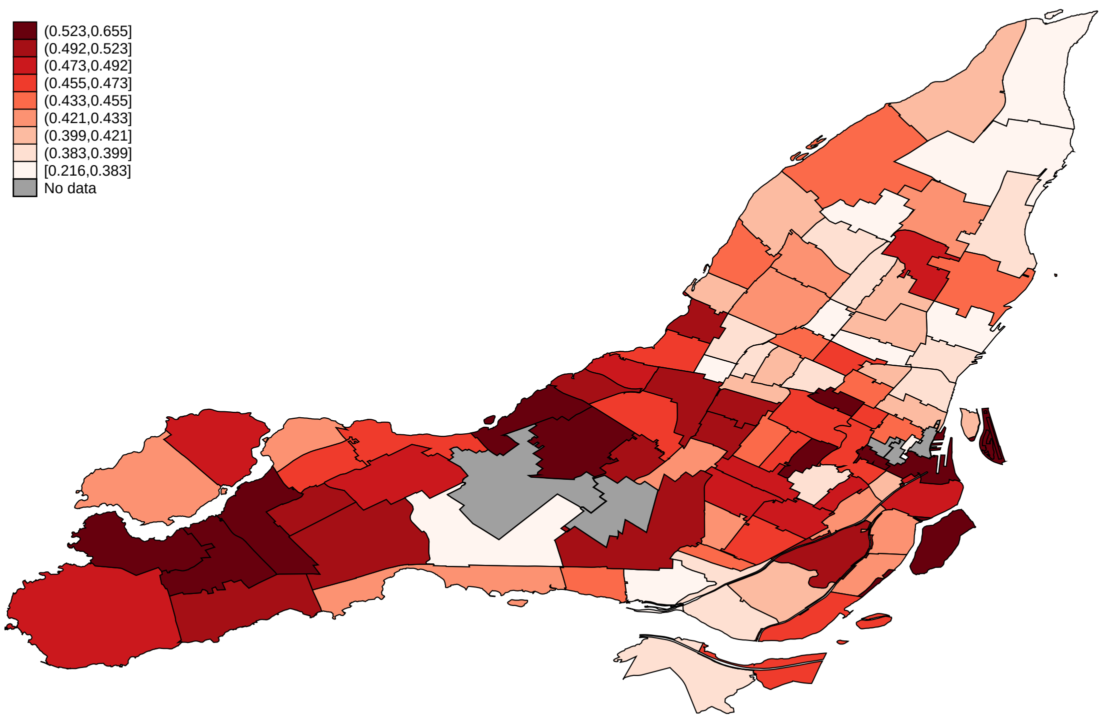
Notes: Residuals extracted from the estimation of a two-way fixed effect model, and correspond to the estimates reported in column (6) of Table II. The figure is constructed by slicing the distributions of school and FSA fixed effects into deciles, and then calculating the average residuals in each school-by-neighborhood decile cell.

Figure A.9: Spatial variation in $\bar{\Omega}_n^{PR}$ and $\bar{\Lambda}_n^{PR}$, for University Enrollment

Panel A: School variation ($\bar{\Omega}_n^{PR}$)

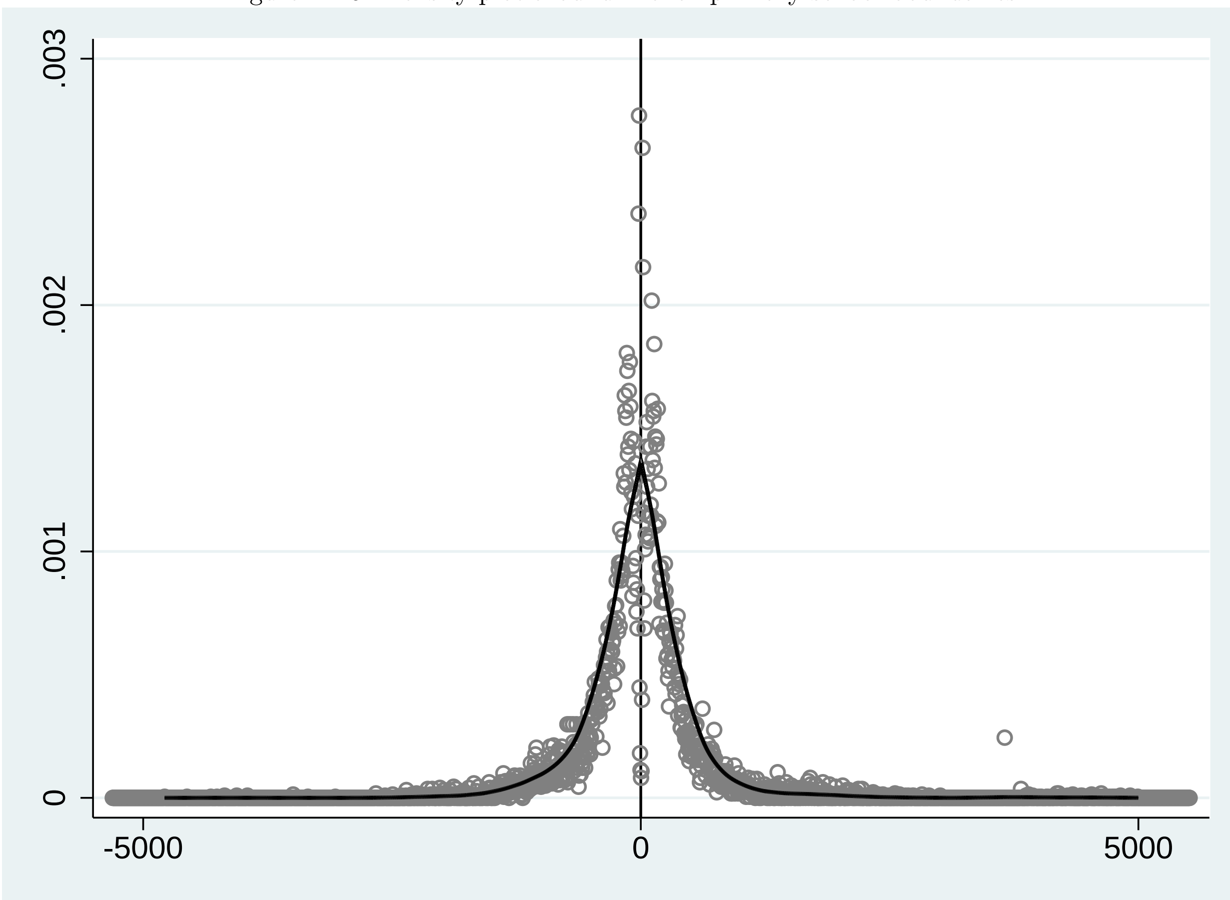


Panel B: Neighborhood variation ($\bar{\Lambda}_n^{PR}$)



Notes: Statistics based on permanent residents. Outcomes are adjusted for cohort effects. To ease the interpretation, the student-level fixed effects used to compute $\bar{\Omega}_n^{PR}$ and $\bar{\Lambda}_n^{PR}$ were first re-centered around the unconditional university enrollment rate for the the full sample. Data for FSAs with fewer than 10 permanent residents are not shown.

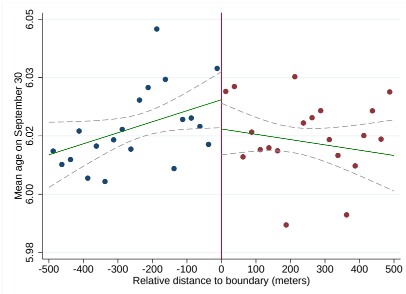
Figure A.10: Density plot around French primary school boundaries



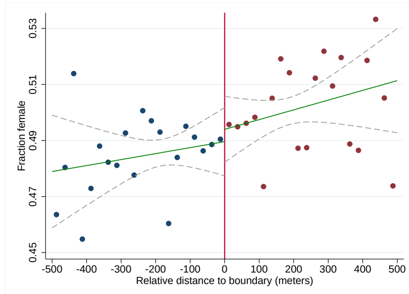
Notes: Figure produced with the stata package DCdensity.ado, which implements the test derived in McCrary (2008). The x-axis shows distance relative to the nearest boundary, in meters.

Figure A.11: Balance of Covariates at Boundaries - School quality in terms of university enrollment

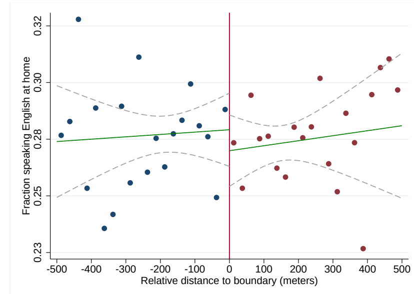
(a) Age



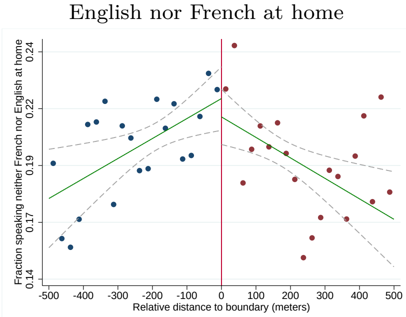
(b) Gender



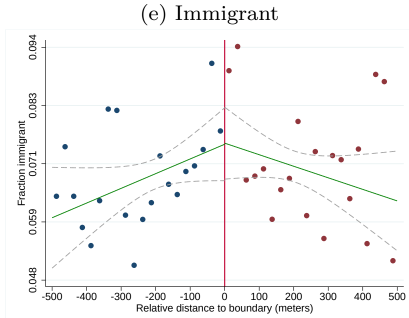
(c) Speaks English at home



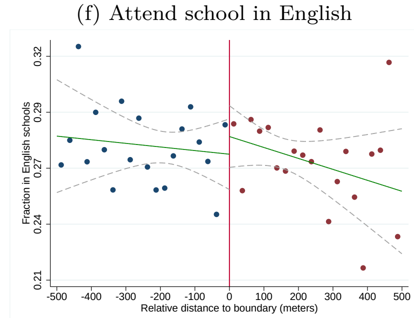
(d) Speaks neither English nor French at home



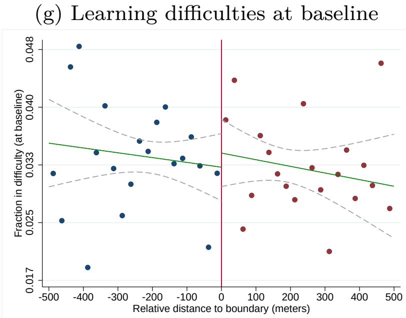
(e) Immigrant



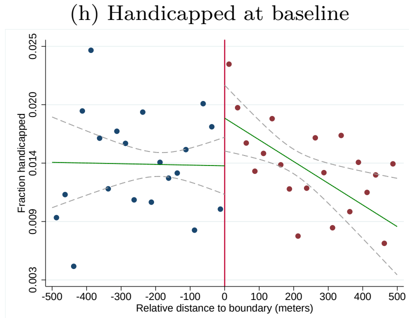
(f) Attend school in English



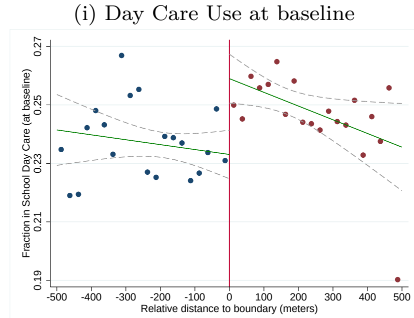
(g) Learning difficulties at baseline



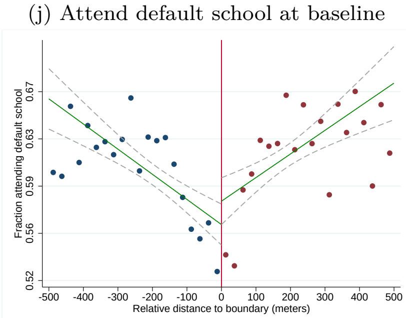
(h) Handicapped at baseline



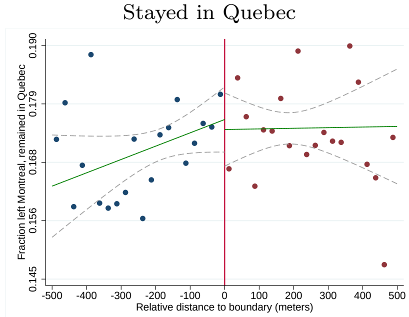
(i) Day Care Use at baseline



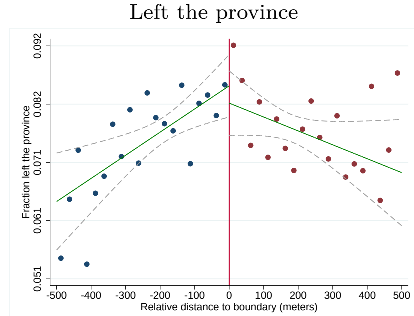
(j) Attend default school at baseline



(k) Attrition: Left Montreal, Stayed in Quebec

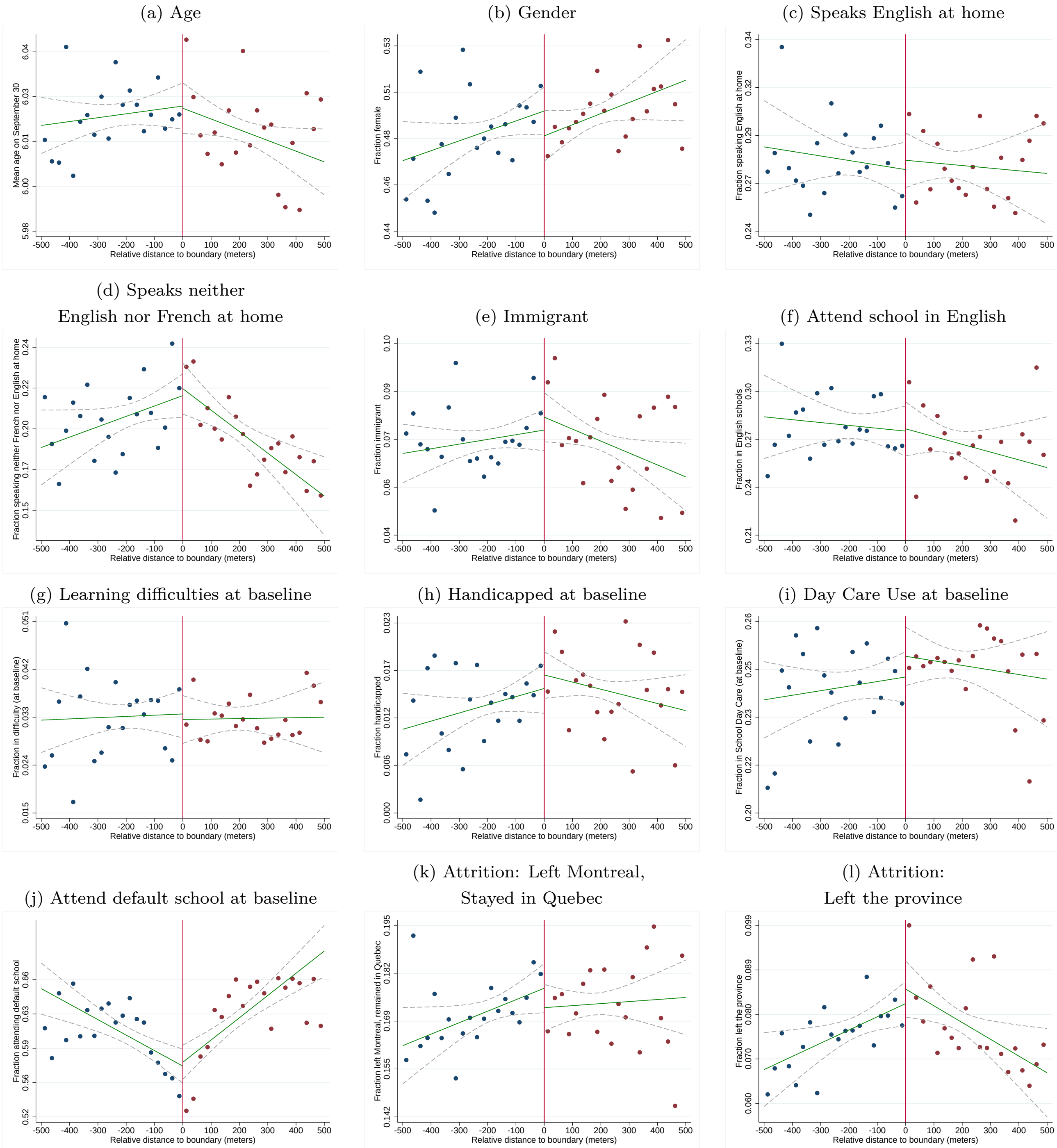


(l) Attrition: Left the province



Notes: In panels (a) to (j), the sample is restricted to permanent residents. In panels (k) and (l), there is no sample restriction, hence all students in the database are included. For each boundary, students assigned the default school with the highest fixed effect $\delta^P_{s(i)}$ (measured in units of university enrollment) are at positive distances. Variables are first residualized on cohort, boundary and FSA fixed effects. Standard errors are clustered at the boundary level. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

Figure A.12: Balance of Covariates at Boundaries - School quality in terms of *DES* in 5 years

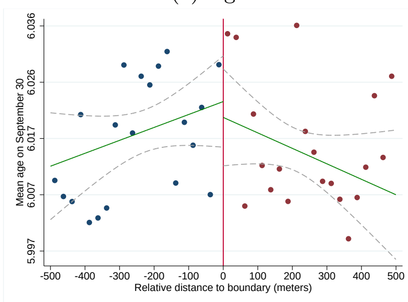


(a) Age (b) Gender (c) Speaks English at home (d) Speaks neither English nor French at home (e) Immigrant (f) Attend school in English (g) Learning difficulties at baseline (h) Handicapped at baseline (i) Day Care Use at baseline (j) Attend default school at baseline (k) Attrition: Left Montreal, Stayed in Quebec (l) Attrition: Left the province

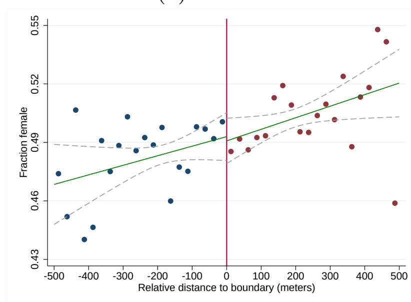
Notes: In panels (a) to (j), the sample is restricted to permanent residents. In panels (k) and (l), there is no sample restriction, hence all students in the database are included. For each boundary, students assigned the default school with the highest fixed effect $\delta^P_{s(i)}$ (measured in units of timely secondary school graduation) are at positive distances. Variables are first residualized on cohort, boundary and FSA fixed effects. Standard errors are clustered at the boundary level. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

Figure A.13: Balance of Covariates at Boundaries - School quality in terms of years of education

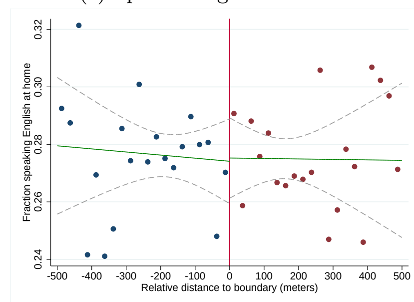
(a) Age



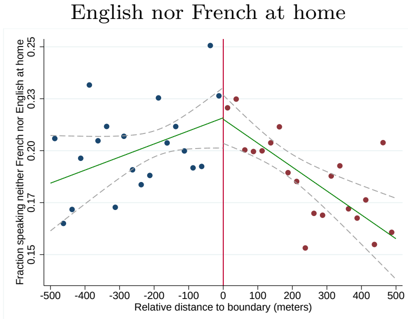
(b) Gender



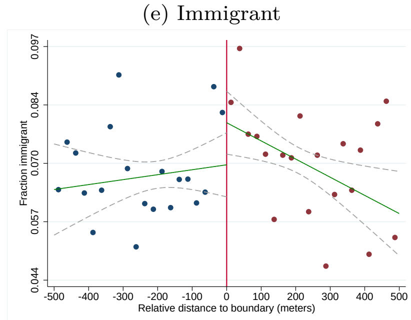
(c) Speaks English at home



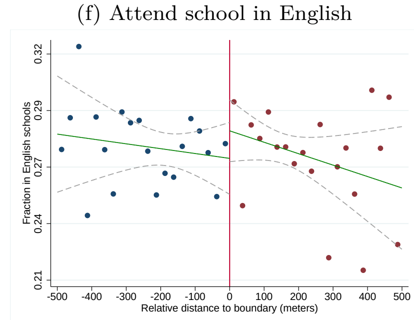
(d) Speaks neither English nor French at home



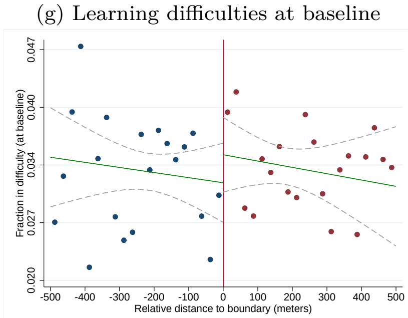
(e) Immigrant



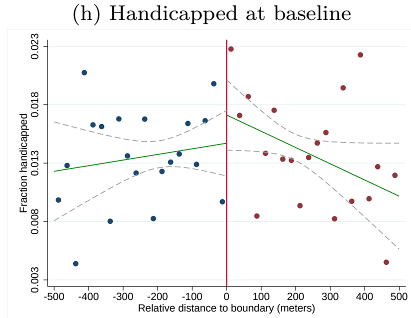
(f) Attend school in English



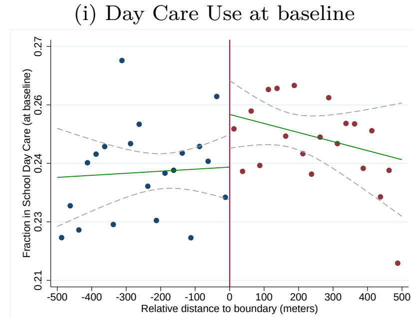
(g) Learning difficulties at baseline



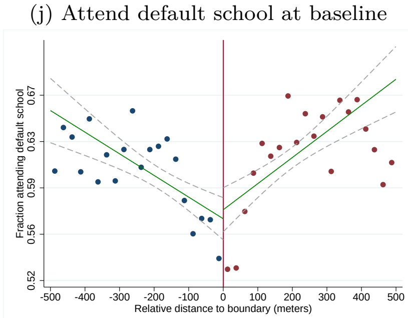
(h) Handicapped at baseline



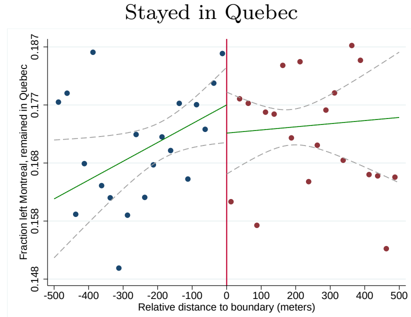
(i) Day Care Use at baseline



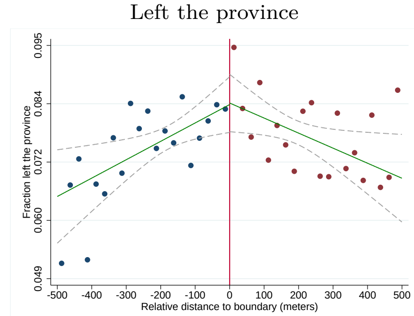
(j) Attend default school at baseline



(k) Attrition: Left Montreal, Stayed in Quebec



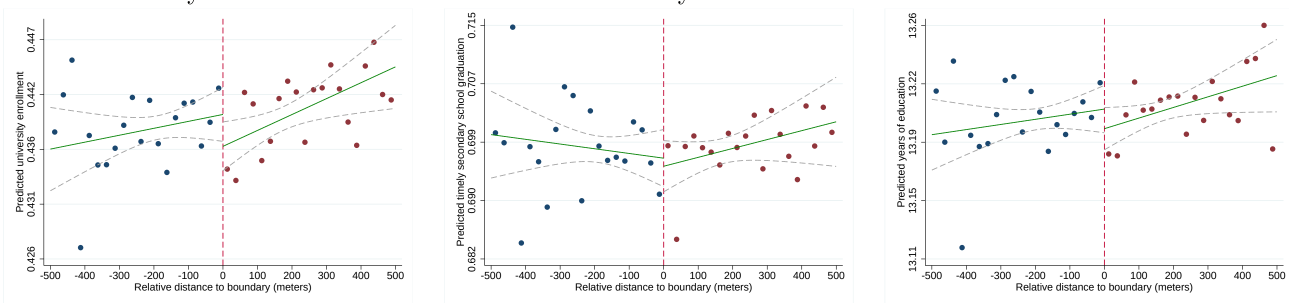
(l) Attrition: Left the province



Notes: In panels (a) to (j), the sample is restricted to permanent residents. In panels (k) and (l), there is no sample restriction, hence all students in the database are included. For each boundary, students assigned the default school with the highest fixed effect $\delta^P_{s(i)}$ (measured in units of university enrollment) are at positive distances. Variables are first residualized on cohort, boundary and FSA fixed effects. Standard errors are clustered at the boundary level. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

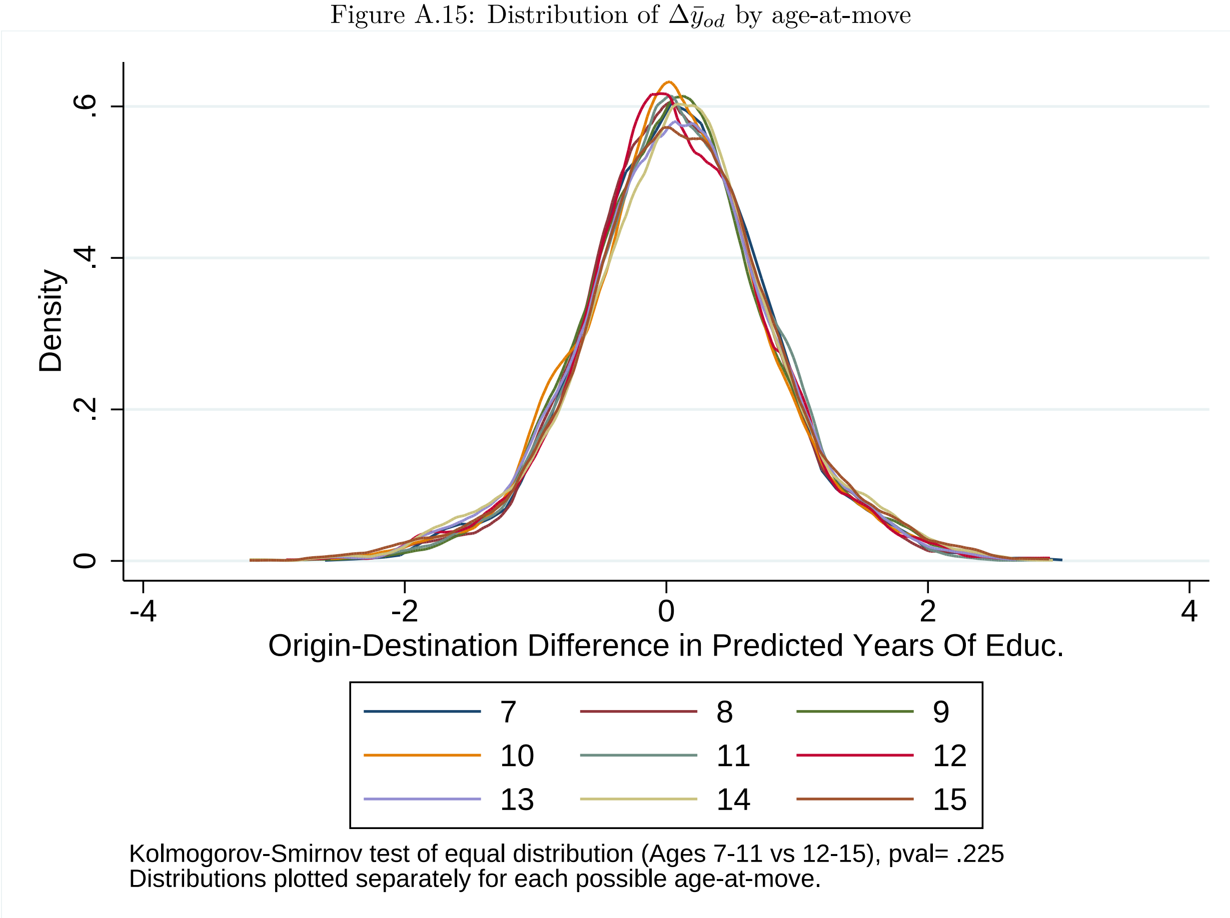
Figure A.14: Discontinuity in predicted educational attainment

University enrollment | *DES* in 5 years | Years of education



Notes: Predicted educational attainment is given by the fitted values of a regression of the outcome of interest on individual covariates (age, gender, ...) and cohort fixed effects. For each boundary, students assigned the default school with the highest fixed effect $\delta^{P}_{s(i)}$ are at positive distances. Variables on the vertical axis are first residualized on cohort, boundary and FSA fixed effects. Standard errors are clustered at the boundary level. For visual clarity, students living further than 500 meters away from their nearest boundary are excluded.

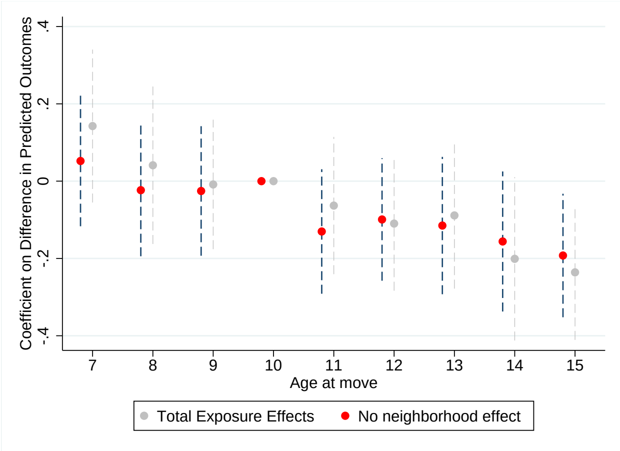
Figure A.15: Distribution of $\Delta\bar{y}_{od}$ by age-at-move



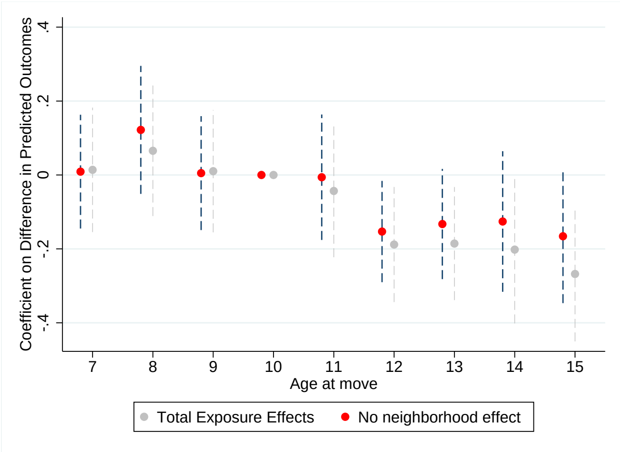
Notes: The kernel density of the distribution of $\Delta\bar{y}_{od}$ (in years of education) is plotted separated for each possible value of age-at-move.

Figure A.16: Non-parametric restricted exposure effects – No neighborhood effect

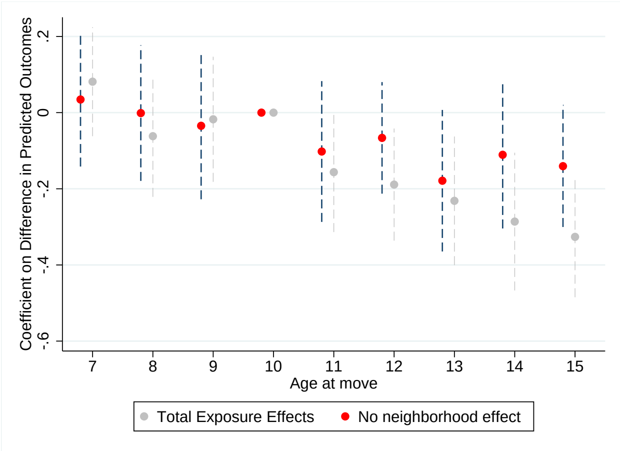
Panel A: University enrollment



Panel B: DES in 5 Years



Panel C: Years of education

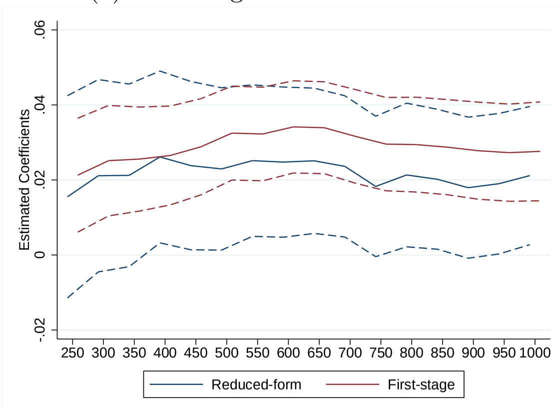


Notes: Notes: Sample includes all movers who remained within Montreal. Observation in FSAs with less than 10 permanent residents are omitted. Coefficients in red correspond to age-specific restricted coefficients for which the neighborhood channel is shut down (β_m^{-n}). Standard errors are clustered at the destination level and calculated by the delta method.

Figure A.17: Bandwidth sensitivity of RD estimates

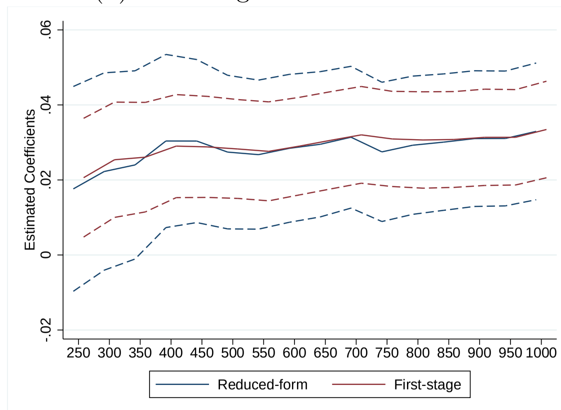
University enrollment

(a) First-stage and Reduced-form



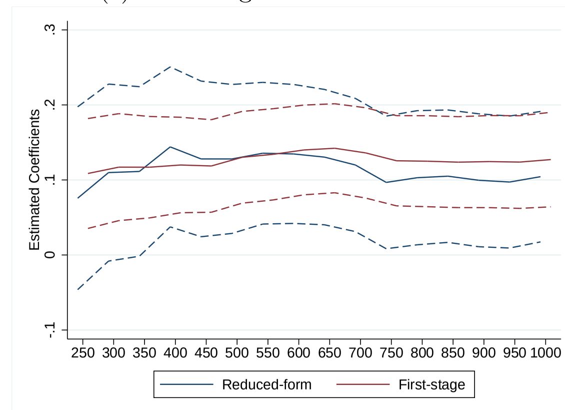
DES in 5 Years

(b) First-stage and Reduced-form

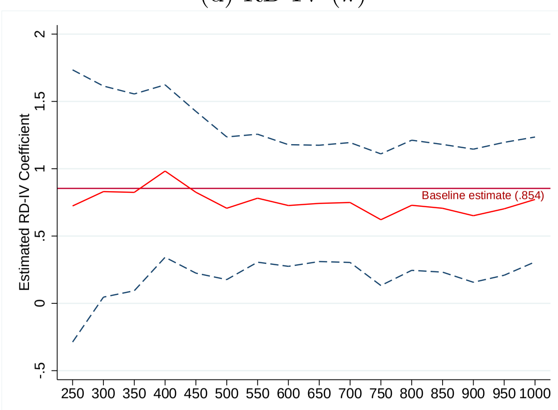


Years of education

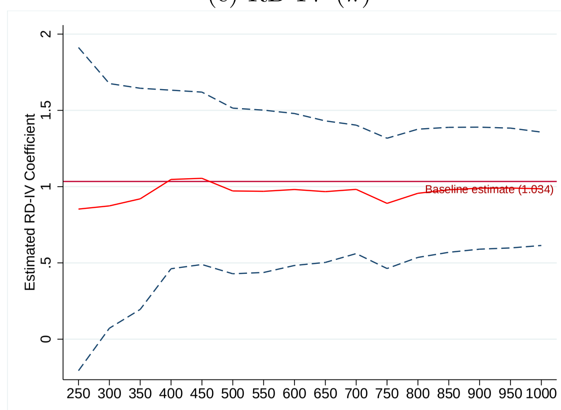
(c) First-stage and Reduced-form



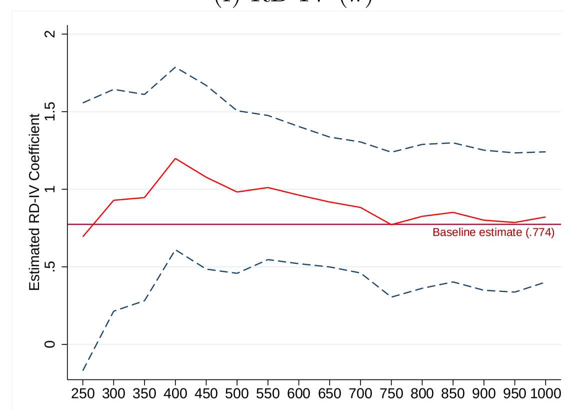
(d) RD-IV (π)



(e) RD-IV (π)



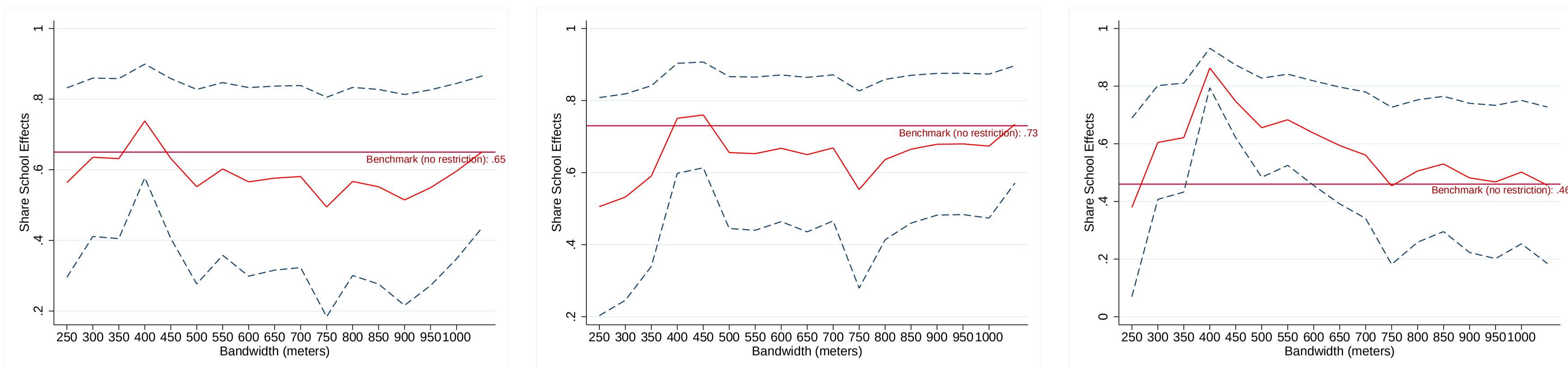
(f) RD-IV (π)



Notes: Panels (a) to (c) show first-stage and reduced-form RD coefficients for different bandwidth values. The dashed lines represent 95% confidence intervals with standard errors clustered at the boundary-level. Panels (d) to (f) show the associated RD-IV coefficients. The horizontal line shows the baseline RD-IV estimate under no bandwidth restriction.

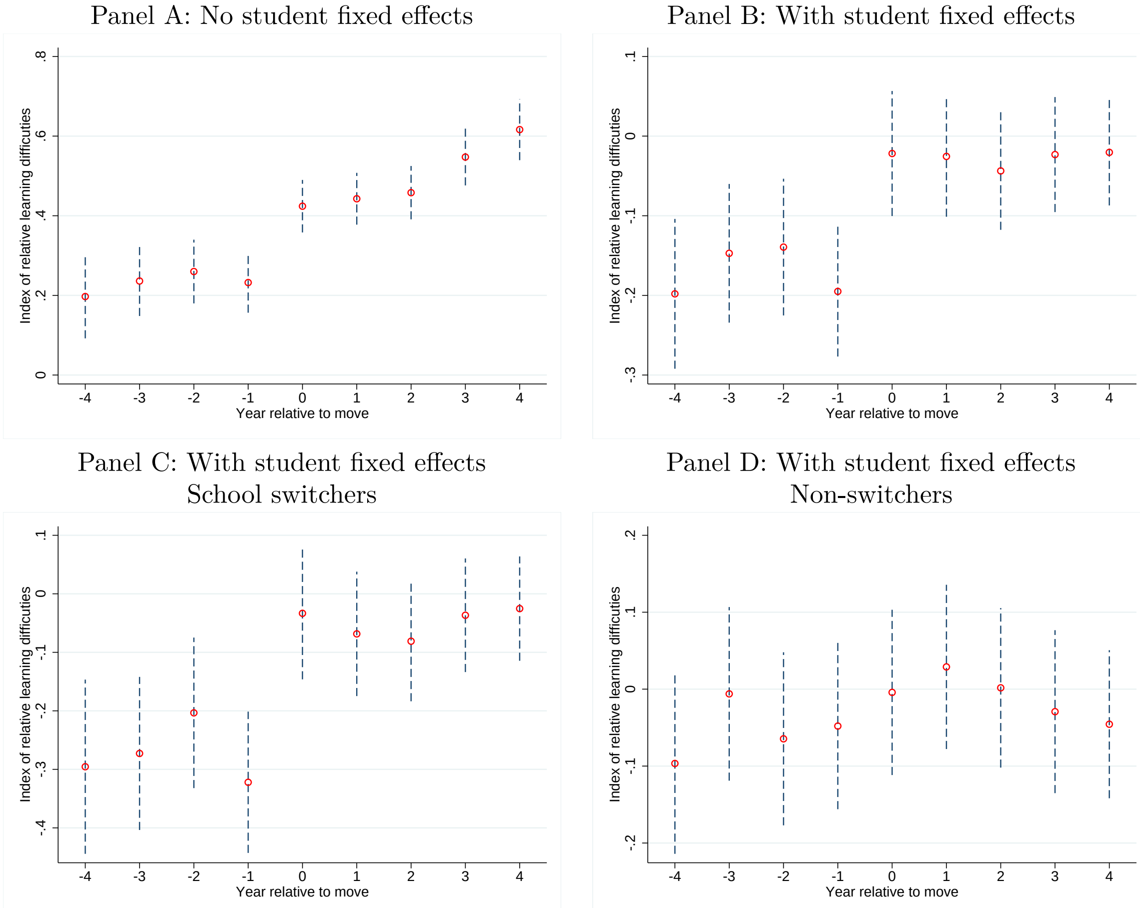
Figure A.18: Bandwidth sensitivity of share school effects (decomposition)

Panel A: University enrollment | Panel B: DES in 5 Years | Panel C: Years of education



Notes: For each panel, the red line shows the share of total exposure effects due to school for different RD bandwidth values. The dashed lines represent 95% confidence intervals with standard errors calculated by the delta method. The horizontal line shows the baseline estimate of the school share under no bandwidth restriction.

Figure A.19: Index of relative learning difficulties, by years relative to move

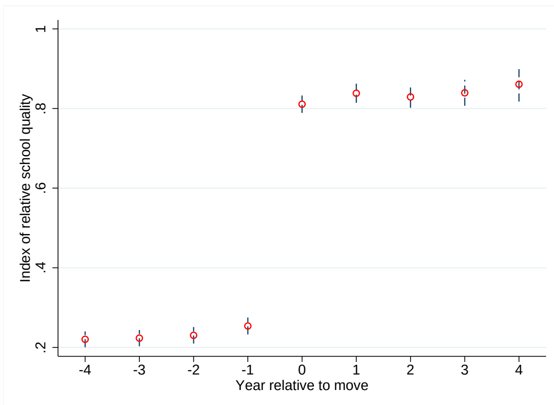


Notes: Standard errors are clustered at the individual level. The y-axis shows regression coefficients on $\sigma_{od(i,t)}$. Observations outside the event window are included in the regression, so all coefficients are relative to omitted relative-time periods. Panel C includes only students who switched school the year they moved. Panel D includes movers who did not switch school the year they moved.

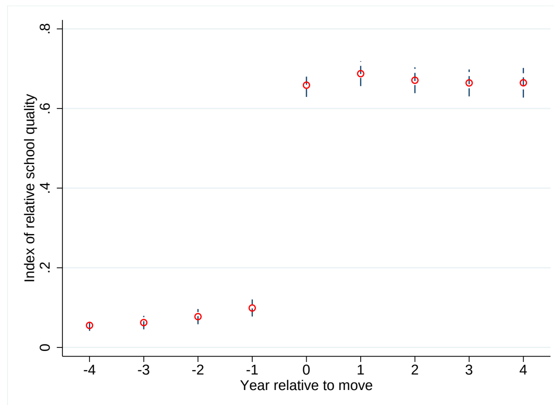
Figure A.20: Index of relative school quality, by years relative to move

University enrollment

Panel A: No student fixed effects

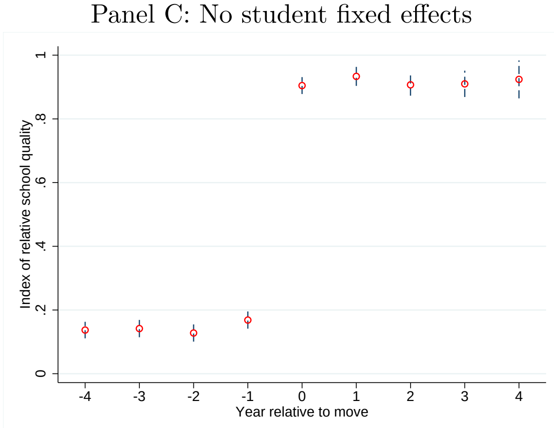


Panel B: With student fixed effects

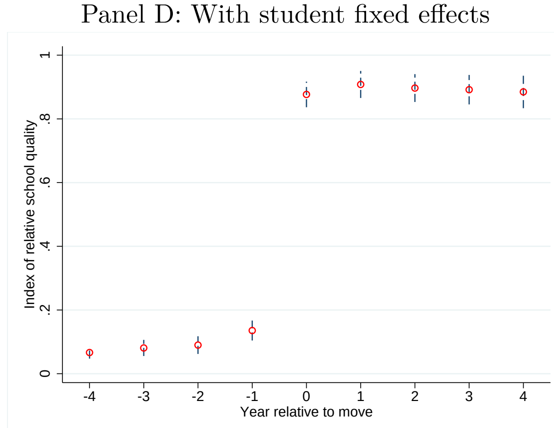


DES in 5 Years

Panel C: No student fixed effects

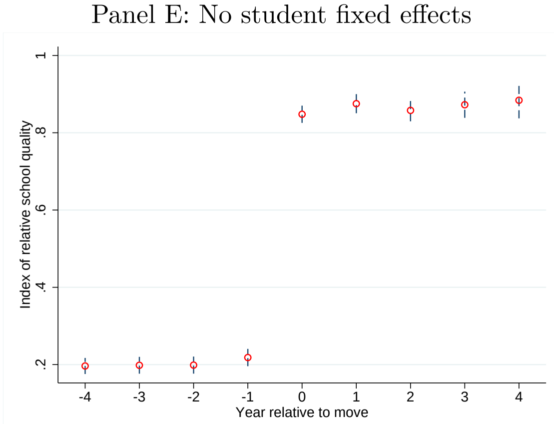


Panel D: With student fixed effects

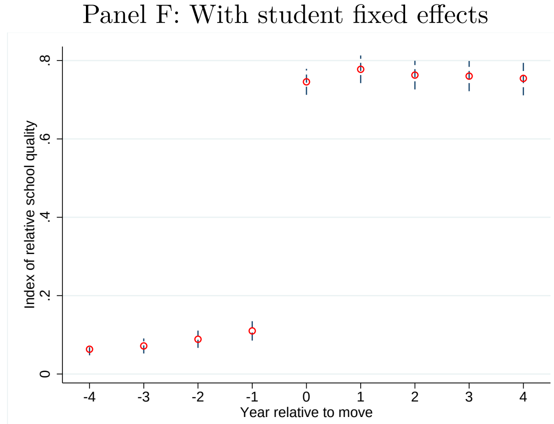


Years of education

Panel E: No student fixed effects



Panel F: With student fixed effects



Notes: Standard errors are clustered at the individual level. The y-axis shows regression coefficients on $\sigma^{\psi}_{od(i,t)} = \frac{\delta_{s(i,t)} - \overline{\delta}_{s(o,t)}}{\overline{\delta}_{s(d,t)} - \overline{\delta}_{s(o,t)}}$. For each period t, $\overline{\delta}_{s(n,t)}$ is measured by the relevant average primary school fixed effects if student i was in primary school in that year. Secondary school fixed effects are used for remaining years. Observations outside the event window are included in the regression, so all coefficients are relative to omitted relative-time periods.

Table A.1: Summary statistics: Educational outcomes across cohorts

	All	Cohort 1995	1996	1998	2000	2001
Primary and secondary school outcomes						
Did not start secondary school on time	0.113	0.156	0.153	0.124	0.073	0.068
Secondary school diploma	0.760	0.755	0.752	0.759	0.767	0.765
Secondary school diploma in 5 years	0.610	0.600	0.587	0.609	0.630	0.621
No secondary school qualification	0.200	0.208	0.209	0.195	0.189	0.198
Post-secondary outcomes						
Ever enrolled in college	0.695	0.678	0.682	0.699	0.710	0.705
Enrolled in college by age 17	0.530	0.497	0.503	0.532	0.560	0.555
Ever enrolled in university	0.373	0.460	0.451	0.424	0.332	0.220
Enrolled in university by age 19	0.170	0.166	0.166	0.169	0.175	0.175
Bachelor degree or more	0.128	0.275	0.249	0.140	0.003	0.004
Educational attainment						
Number of years of education	12.810	13.247	13.200	13.066	12.517	12.119
Observations	92,764	16,969	18,067	18,777	19,125	19,826

Notes: The table shows cohort-specific average outcomes.

Table A.2: Variation Across Census Tracts and Schools

	Outcome: DES in 5 years (1)	(2)	University enrollment (3)	(4)	Years of education (5)	(6)
Student-level standard deviation of fixed effects:						
Schools	0.261	0.255	0.248	0.235	1.172	1.123
Neighborhoods (Census Tracts)	0.152	0.068	0.159	0.081	0.734	0.328
Dependent variable summary statistics:						
Mean	0.729		0.460		13.323	
Standard deviation	[0.444]		[0.498]		[2.083]	
Fixed effects estimated						
Separately	x		x		x	
Simultaneously		x		x		x
Number of students	37,491					
Number of primary schools	435					
Number of secondary schools	211					
Number of neighborhoods	502					

Notes: Sample restricted to students who always resided in the same census tract. School fixed effects are the sum of a primary and a secondary school fixed effect. In columns (1), (3) and (5), school and neighborhood effects are respectively estimated in separate regressions. In columns (2), (4) and (6), all fixed effects are estimated simultaneously from equation (5).

Table A.3: Variation Across FSAs and Schools - Empirical Bayes Estimates

	Outcome					
	DES in 5 years		University enrollment		Years of education	
	(1)	(2)	(3)	(4)	(5)	(6)
Student-level standard deviation of shrunk fixed effects:						
Schools	0.263	0.251	0.243	0.218	1.180	1.073
Neighborhoods (FSAs)	0.127	0.016	0.129	0.035	0.636	0.148
Dependent variable summary statistics:						
Mean	0.706		0.443		13.228	
Standard deviation	[0.456]		[0.497]		[2.113]	
Fixed effects estimated						
Separately	x		x		x	
Simultaneously		x		x		x
Number of students	44,912					
Number of primary schools	440					
Number of secondary schools	218					
Number of neighborhoods	95					

Notes: Sample restricted to students who always resided in the same FSA. School fixed effects are the sum of a primary and a secondary school fixed effect. To shrink estimates, I first calculate standard errors for each school and neighborhood fixed effect using bootstrap resampling (100 samples with replacement, clustering within primary school-secondary school-FSA cells). I then shrink estimates toward their means using the empirical Bayes procedure described in Chandra et al. (2016). Note that the reported empirical Bayes measures of school effects are shrunk estimates of the sum of primary and secondary school effects, not the sum of shrunk estimates of primary school and shrunk estimates of secondary school effects.

Table A.4: Balance of Covariates at Boundaries

Outcome used to assign *HighSide*	University Enrollment	DES in 5 years	Years of education
	(1)	(2)	(3)
Covariates			
Age	-0.00914	-0.00748	-0.00439
	(0.00566)	(0.00586)	(0.00582)
Gender	0.0204***	0.00928	0.0176**
	(0.00705)	(0.00722)	(0.00723)
Speaks English at Home	-0.00406	0.00416	-0.00187
	(0.0114)	(0.0106)	(0.0114)
Speaks neither French nor English at Home	-0.00952	-0.00784	-0.0153*
	(0.00855)	(0.00851)	(0.00858)
Immigrant	0.000355	0.00123	0.00289
	(0.00457)	(0.00457)	(0.00442)
Attend school in English	-0.00196	-0.00991	-0.000384
	(0.0120)	(0.0115)	(0.0121)
Learning difficulties at baseline	0.000519	-0.00101	0.00217
	(0.00301)	(0.00309)	(0.00305)
Handicapped at baseline	0.00260	0.00260	0.00178
	(0.00168)	(0.00169)	(0.00171)
Day Care Useat baseline	0.0205***	0.00650	0.0123*
	(0.00674)	(0.00679)	(0.00675)
Attend default school at baseline	0.0206	0.0148	0.0165
	(0.0131)	(0.0134)	(0.0134)
Left Montreal	0.000889	-0.00181	-0.00181
	(0.00486)	(0.00483)	(0.00480)
Left the province	0.000407	0.00168	0.00214
	(0.00356)	(0.00346)	(0.00355)
Predicted educational attainment	0.0002	0.0009	0.0006
	(0.0016)	(0.0025)	(0.0097)
Cohort fixed effects	x	x	x
Individual characteristics	x	x	x
Neighborhood (FSA) fixed effects	x	x	x
Boundary fixed effects	x	x	x

Notes: In all specifications, the control function for distance to boundary is linear and allows for different slopes on either side of the threshold. The sample includes all permanent residents, expect for the attrition variables (Left Montreal and Left the province), where all students in the database are included. All standard errors are clustered at the French primary school boundary level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Exposure Effects: Moves across Census Tracts

Sample:	All movers		One-time movers	
	(1)	(2)	(3)	(4)
Measure of educational attainment				
Origin-by-destination fixed effects				
Secondary school diploma in 5 years	-0.0175	-0.0200*	-0.0300	-0.0355*
	(0.0123)	(0.0115)	(0.0202)	(0.0196)
University enrollment	-0.0182	-0.0218*	-0.0153	-0.0206
	(0.0116)	(0.0114)	(0.0190)	(0.0186)
Years of schooling	-0.0173	-0.0219*	-0.0194	-0.0268
	(0.0115)	(0.0112)	(0.0186)	(0.0180)
N	18981	18981	7460	7460
Origin + destination fixed effects				
Secondary school diploma in 5 years	-0.0222***	-0.0224***	-0.0366***	-0.0388***
	(0.00659)	(0.00608)	(0.00842)	(0.00798)
University enrollment	-0.0225***	-0.0236***	-0.0281***	-0.0313***
	(0.00643)	(0.00628)	(0.00820)	(0.00799)
Years of schooling	-0.0272***	-0.0277***	-0.0306***	-0.0341***
	(0.00615)	(0.00587)	(0.00794)	(0.00762)
N	31333	31333	15469	15469
Cohort fixed effects	x	x	x	x
Individual characteristics	x	x	x	x
Age at move fixed effects	x	x	x	x
Only moved once			x	x
Times in difficulty before moving		x		x

Notes: Coefficients shown in the table are convergence rates β. In columns (2) and (4), the model includes a set of dummies for each possible value of number of times in difficulty prior to moving. Standard errors are clustered at the destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.6: Exposure Effects: Alternative Outcomes

Sample:	All movers		One-time movers	
	(1)	(2)	(3)	(4)
Measure of educational attainment				
No Secondary school qualification	-0.0676***	-0.0648***	-0.0496***	-0.0496***
	(0.0137)	(0.0143)	(0.0159)	(0.0165)
College enrollment (ever)	-0.0373***	-0.0356***	-0.0267**	-0.0258*
	(0.0109)	(0.0118)	(0.0134)	(0.0138)
College enrollment by 17	-0.0412***	-0.0382***	-0.0408***	-0.0389***
	(0.00814)	(0.00806)	(0.0112)	(0.0114)
College degree	-0.0407***	-0.0389***	-0.0336***	-0.0321***
	(0.00873)	(0.00895)	(0.0110)	(0.0111)
University enrollment by 19	-0.0395***	-0.0381***	-0.0454***	-0.0442***
	(0.0110)	(0.0112)	(0.0160)	(0.0163)
Bachelor degree or more	-0.0374***	-0.0363***	-0.0261	-0.0258
	(0.0129)	(0.0130)	(0.0181)	(0.0182)
Expected earnings on basis of	-0.0454***	-0.0433***	-0.0411***	-0.0397***
level of education	(0.00869)	(0.00930)	(0.00972)	(0.00970)
Expected earnings on basis of	-0.0406***	-0.0391***	-0.0334***	-0.0324***
level and field of education	(0.00847)	(0.00897)	(0.0106)	(0.0105)
Cohort fixed effects	x	x	x	x
Individual characteristics	x	x	x	x
Age at move fixed effects	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x
Only moved once			x	x
Times in difficulty before moving		x		x
N	24316	24316	15533	15533

Notes: Note: Coefficients shown in the table are convergence rates β. In columns (2) and (4), the model includes a set of dummies for each possible value of number of times in difficulty prior to moving. Standard errors are clustered at the destination neighborhood level. Details on the measurement of outcomes are provided in the Data Appendix.
*** p<0.01, ** p<0.05, * p<0.1

Table A.7: Alternative Decomposition of Exposure Effects

	(1)	(2)	(3)
University enrollment			
	Total exposure effects		
β	-0.0424***	-0.0424***	-0.0424***
	(0.0090)	(0.0090)	(0.0090)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0107***	-0.0108***	-0.0154***
	(0.0036)	(0.0037)	(0.0042)
β^{-n} (No neighborhood effects)	-0.0317***	-0.0316***	-0.0270***
	(0.0069)	(0.0068)	(0.0058)
Share school effects	75%	75%	64%
	(0.0612)	(0.0615)	(0.0526)
Secondary school diploma in 5 years			
	Total exposure effects		
β	-0.0421***	-0.0421***	-0.0421***
	(0.0088)	(0.0088)	(0.0088)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0109***	-0.0124***	-0.0114***
	(0.0036)	(0.0038)	(0.0038)
β^{-n} (No neighborhood effects)	-0.0309***	-0.0294***	-0.0304***
	(0.0084)	(0.0080)	(0.0083)
Share school effects	74%	70%	73%
	(0.0876)	(0.0866)	(0.0896)
Years of education			
	Total exposure effects		
β	-0.0488***	-0.0488***	-0.0488***
	(0.0088)	(0.0088)	(0.0088)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0136***	-0.0146***	-0.0224***
	(0.0032)	(0.0033)	(0.0041)
β^{-n} (No neighborhood effects)	-0.0351***	-0.0342***	-0.0264***
	(0.0074)	(0.0071)	(0.0055)
Share school effects	72%	70%	54%
	(0.0542)	(0.0518)	(0.0401)
Measure of school quality	$\pi\Omega_{s(n)}$	$\pi\Omega^{-i}_{s(n)}$	$\pi\Omega^{-i}_{s(n)}$
π	1	1	RD estimate

Notes: Sample restricted to movers within Montreal. Standard errors are clustered at the destination FSA level, and obtained by the delta method for restricted convergence rates. β^{-s} is a restricted rate for which $\frac{\beta_s Var(\pi\Delta\Omega_{od})+\beta_n Cov(\pi\Delta\Omega_{od},\Delta\bar{y}^{-s}_{od})}{Var(\Delta\bar{y}_{od})} = 0$, and β^{-n} is a restricted rate for which $\frac{\beta_n Var(\Delta\bar{y}^{-s}_{od})+\beta_s Cov(\pi\Delta\Omega_{od},\Delta\bar{y}^{-s}_{od})}{Var(\Delta\bar{y}_{od})} = 0$. Share school effects is given by the ratio $\frac{\beta-\beta^{-s}}{\beta}$.

Table A.8: School effects: Quadratic control function

	First-stage(s)			Reduced-form RD	RD-IV
Dependent variable:	Quality of *assigned* school at baseline ($\delta^P_{s(i)}$)	Quality of school *attended* at baseline ($\delta^P_{s(i)}$)	Childhood average school quality ($\Omega^{-i}_{s(n(i))}$)	Outcome	Outcome
	(1)	(2)	(3)	(4)	(5)
Measure of educational attainment					
			All permanent residents		
University enrollment	0.0634***	0.0248***	0.0293***	0.0206**	0.7086***
	(0.0032)	(0.0028)	(0.0065)	(0.0093)	(0.2235)
Secondary school diploma in 5 years	0.0714***	0.0304***	0.0325***	0.0351***	1.0812***
	(0.0036)	(0.0026)	(0.0063)	(0.0091)	(0.1891)
Years of schooling	0.2961***	0.1192***	0.1372***	0.1098**	0.8023***
	(0.0146)	(0.0126)	(0.0309)	(0.0428)	(0.1914)
N	43296	43279	43291	43296	43291
			Placebo: Students in English schools		
University enrollment	0.0624***	-0.0051	-0.0089	-0.0169	-
	(0.0043)	(0.0041)	(0.0106)	(0.0178)	-
Secondary school diploma in 5 years	0.0712***	0.0056**	0.0012	-0.0052	-
	(0.0054)	(0.0028)	(0.0077)	(0.0135)	-
Years of schooling	0.2810***	0.0042	-0.0075	-0.0438	-
	(0.0189)	(0.0167)	(0.0478)	(0.0770)	-
N	13446	13444	13444	13446	
Cohort fixed effects	x	x	x	x	x
Individual characteristics	x	x	x	x	x
Neighborhood (FSA) fixed effects	x	x	x	x	x
Boundary fixed effects	x	x	x	x	x

Notes: This table reports RD estimates. In columns (1) and (2), primary school quality is measured using the fixed effects, $\delta^P_{s(i)}$, estimated in Section (IV.A). In column (3), the dependent variable is childhood average school quality $\Omega^{-i}_{s(n(i))}$. Column (5) reports 2SLS estimates of equations (6) and (7). In all specifications, the control function for distance to boundary is quadratic and allows for different functions on either side of the threshold. In the first three rows, the sample includes all permanent residents. In the last three rows, only permanent residents enrolled in English schools are included. All standard errors are clustered at the French primary school boundary level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.9: School effects: Triangular kernel control function

	First-stage(s)			Reduced-form RD	RD-IV
Dependent variable:	Quality of *assigned* school at baseline ($\delta^P_{s(i)}$)	Quality of school *attended* at baseline ($\delta^P_{s(i)}$)	Childhood average school quality ($\Omega^{-i}_{s(n(i))}$)	Outcome	Outcome
	(1)	(2)	(3)	(4)	(5)
Measure of educational attainment					
			All permanent residents		
University enrollment	0.0632***	0.0245***	0.0315***	0.0253***	0.8081***
	(0.0032)	(0.0027)	(0.0064)	(0.0086)	(0.1774)
Secondary school diploma in 5 years	0.0715***	0.0298***	0.0330***	0.0344***	1.0459***
	(0.0036)	(0.0025)	(0.0061)	(0.0086)	(0.1700)
Years of schooling	0.2946***	0.1162***	0.1456***	0.1129***	0.7779***
	(0.0146)	(0.0121)	(0.0297)	(0.0398)	(0.1661)
N	43296	43279	43291	43296	43291
			Placebo: Students in English schools		
University enrollment	0.0630***	-0.0025	-0.0036	-0.0110	-
	(0.0043)	(0.0041)	(0.0098)	(0.0160)	-
Secondary school diploma in 5 years	0.0721***	0.0037	0.0005	-0.0084	-
	(0.0057)	(0.0026)	(0.0071)	(0.0118)	-
Years of schooling	0.2836***	0.0053	0.0044	-0.0471	-
	(0.0198)	(0.0156)	(0.0433)	(0.0658)	-
N	13446	13444	13444	13446	
Cohort fixed effects	x	x	x	x	x
Individual characteristics	x	x	x	x	x
Neighborhood (FSA) fixed effects	x	x	x	x	x
Boundary fixed effects	x	x	x	x	x

Notes: This table reports RD estimates. In columns (1) and (2), primary school quality is measured using the fixed effects, $\delta^P_{s(i)}$, estimated in Section (IV.A). In column (3), the dependent variable is childhood average school quality $\Omega^{-i}_{s(n(i))}$. Column (5) reports 2SLS estimates of equations (6) and (7). In all specifications, the control function for distance to boundary is a triangular kernel and allows for different functions on either side of the threshold. In the first three rows, the sample includes all permanent residents. In the last three rows, only permanent residents enrolled in English schools are included. All standard errors are clustered at the French primary school boundary level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.10: School effects: Re-weighted sample

	First-stage	Reduced-form	RD-IV
Dependent variable:	Childhood average school quality ($\Omega^{-i}_{s(n(i))}$)	Outcome	Outcome
	(1)	(2)	(3)
Measure of educational attainment			
University enrollment	0.0339***	0.0260***	0.7706***
	(0.0061)	(0.0083)	(0.1670)
Secondary school diploma in 5 years	0.0345***	0.0351***	1.0189***
	(0.0059)	(0.0089)	(0.1837)
Years of schooling	0.1557***	0.1095***	0.7050***
	(0.0286)	(0.0376)	(0.1580)
N	43287	43292	43287
Cohort fixed effects	x	x	x
Individual characteristics	x	x	x
Neighborhood (FSA) fixed effects	x	x	x
Boundary fixed effects	x	x	x

Notes: This table reports RD estimates, where the sample of permanent residents is re-weighted so that its distribution of covariates matches that of the movers' sample. The matching weights are obtained using nearest-neighbor matching (5 nearest neighbors) with the Stata command `kmatch` (Jann, 2017). In all specifications, the control function for distance to boundary is linear and allows for different slopes on either side of the threshold. All standard errors are clustered at the French primary school boundary level. *** p<0.01, ** p<0.05, * p<0.1

Table A.11: Robustness: Exposure-weighted neighborhood quality for multiple-times movers

Sample:	All movers			
	(1)	(2)	(3)	(4)
Measure of educational attainment				
University enrollment	-0.0534***	-0.0512***	-0.0498***	-0.0484***
	(0.00955)	(0.00960)	(0.00962)	(0.00969)
Secondary school diploma in 5 years	-0.0515***	-0.0483***	-0.0467***	-0.0451***
	(0.00885)	(0.00887)	(0.00903)	(0.00916)
Years of schooling	-0.0607***	-0.0582***	-0.0563***	-0.0550***
	(0.00953)	(0.00993)	(0.00981)	(0.0102)
N	24316	24316	24191	24191
Cohort fixed effects	x	x	x	x
Individual characteristics	x	x	x	x
Age at move fixed effects	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x
Number of moves fixed effects	x	x	x	x
Other locations controls			x	x
Times in difficulty before moving to *d*		x		x

Notes: Coefficients shown in the table are convergence rates β. The change in neighborhood quality is measured by $\bar{y}_d - E(\bar{y}_n|premove)$, where $E(\bar{y}_n|premove)$ is the exposure-weighted average neighborhood quality for all locations in which student i resided before moving to the final destination d. Note that $\bar{y}_d - E(\bar{y}_n|premove) = \Delta\bar{y}_{od}$ for one-time movers. All specifications include dummies for number of moves before the age of 15. In columns (3) and (4), fixed effects for the second and third location (prior to moving to area d), as well as for the age at which these moves occured, are included (the omitted categories are no second/third location). Standard errors are clustered at the final destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.12: Robustness: 6-digit postal code fixed effects

Sample:	All movers			One-time movers		
	(1)	(2)	(3)	(4)	(5)	(6)
Measure of educational attainment						
University enrollment	-0.0424***	-0.0412***	-0.0403***	-0.0416***	-0.0408***	-0.0538**
	(0.0090)	(0.0092)	(0.0117)	(0.0116)	(0.0115)	(0.0214)
Secondary school diploma in 5 years	-0.0421***	-0.0402***	-0.0443***	-0.0506***	-0.0502***	-0.0301
	(0.0088)	(0.0088)	(0.0151)	(0.0117)	(0.0117)	(0.0227)
Years of schooling	-0.0488***	-0.0471***	-0.0462***	-0.0444***	-0.0435***	-0.0409**
	(0.0088)	(0.0094)	(0.0116)	(0.0103)	(0.0102)	(0.0189)
Cohort fixed effects	x	x	x	x	x	x
Individual characteristics	x	x	x	x	x	x
Age at move fixed effects	x	x	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x	x	x
Only moved once				x	x	x
Times in difficulty before moving		x	x		x	x
Destination 6-digit postal code fixed effects			x			x
N	24316	24316	16525	15533	15533	8856

Notes: Coefficients shown in the table are convergence rates β. Individual characteristics include gender, immigrant status, allophone status, born in Canada but outside Quebec, English spoken at home, day care use at baseline, 'in difficulty' status at baseline, handicapped status. In columns (2) and (4), the model includes a set of dummies for each possible value of number of times in difficulty prior to moving. Columns (3) and (6) control for 6-digit postal code fixed effects at age 15. Standard errors are clustered at the destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.13: Balancing check for movers

Outcome of permanent residents:	Years of Education		DES in 5 years		University Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
Covariates						
Gender	0.0038*	0.0034	0.0175*	0.0174	0.0187*	0.0162
	(0.0019)	(0.0029)	(0.0093)	(0.0136)	(0.0095)	(0.0138)
Speaks English at Home	-0.0024	-0.0001	-0.0149*	-0.0035	-0.0121	-0.0034
	(0.0016)	(0.0021)	(0.0076)	(0.0107)	(0.0084)	(0.0113)
Speaks neither French nor English at Home	-0.0006	-0.0010	0.0033	0.0036	-0.0030	-0.0044
	(0.0015)	(0.0020)	(0.0077)	(0.0093)	(0.0081)	(0.0107)
Immigrant	-0.0032**	-0.0045**	-0.0054	-0.0104	-0.0175***	-0.0246**
	(0.0013)	(0.0019)	(0.0069)	(0.0095)	(0.0065)	(0.0098)
Handicapped	-0.0006	-0.0008	-0.0039	-0.0037	-0.0036	-0.0049
	(0.0006)	(0.0008)	(0.0027)	(0.0039)	(0.0027)	(0.0036)
Use Day Care at baseline	0.0003	-0.0005	-0.0004	-0.0021	0.0026	-0.0016
	(0.0014)	(0.0016)	(0.0069)	(0.0081)	(0.0066)	(0.0078)
In difficulty at baseline	0.0017**	0.0016*	0.0066*	0.0052	0.0078**	0.0085**
	(0.0007)	(0.0009)	(0.0036)	(0.0045)	(0.0035)	(0.0043)
Times in difficulty pre-move	0.0098	0.0078	0.0313	0.0125	0.0365	0.0391
	(0.0081)	(0.0083)	(0.0394)	(0.0399)	(0.0394)	(0.0407)
Cohort fixed effects	x	x	x	x	x	x
Age at move fixed effects	x	x	x	x	x	x
Origin-by-Destination fixed effects	x	x	x	x	x	x
One-time movers only		x		x		x
N	24316	15533	24316	15533	24316	15533

Notes: In Columns (1) and (2), $\Delta\bar{y}_{od}$ is measured using years of education. In columns (3) and (4), fractions of students finishing secondary school in 5 years are used, and in columns (5) and (6), university enrollment rates are. Standard errors are clustered at the destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.14: Robustness to time-varying observables

	(1)	(2)	(3)	(4)	(5)	(6)
Measure of educational attainment						
Secondary school diploma in 5 years	-0.0392***	-0.0437***	-0.0423***	-0.0370***	-0.0380***	-0.0318***
	(0.0101)	(0.00872)	(0.00933)	(0.00996)	(0.0105)	(0.0111)
University enrollment	-0.0373***	-0.0436***	-0.0392***	-0.0350***	-0.0399***	-0.0332***
	(0.0107)	(0.00948)	(0.00969)	(0.0110)	(0.0107)	(0.0117)
Years of schooling	-0.0435***	-0.0484***	-0.0437***	-0.0422***	-0.0457***	-0.0376***
	(0.00965)	(0.00903)	(0.00902)	(0.0103)	(0.00946)	(0.0105)
Time-varying controls						
Income	x					x
Percent low-income		x				x
Dwelling value			x			x
Percent lone family				x		x
Percent with college					x	x
Cohort fixed effects	x	x	x	x	x	x
Individual characteristics	x	x	x	x	x	x
Age at move fixed effects	x	x	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x	x	x
N	22735	22735	22735	22735	22735	22735

Notes: Time-varying controls are differences in census tract characteristics around the time of the move. The model includes both the main effect of these controls as well as their interaction with age-at-move. Each column includes a different set of observable time-varying variables. Standard errors are clustered at the destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.15: Heterogeneous Exposure Effects

Heterogeneity by:	Gender		Language at school		Moves to	
	Boys	Girls	French	English	Better FSA	Worse FSA
	(1)	(2)	(3)	(4)	(5)	(6)
Measure of educational attainment						
Secondary school diploma in 5 years	-0.0440***	-0.0476***	-0.0473***	-0.0449**	-0.0385***	-0.0115
	(0.0110)	(0.0140)	(0.0114)	(0.0205)	(0.0137)	(0.0235)
University enrollment	-0.0321**	-0.0571***	-0.0385***	-0.0390*	-0.0398**	-0.0536**
	(0.0123)	(0.0128)	(0.0107)	(0.0207)	(0.0192)	(0.0207)
Years of schooling	-0.0425***	-0.0587***	-0.0485***	-0.0525***	-0.0257*	-0.0520***
	(0.0119)	(0.0127)	(0.0124)	(0.0173)	(0.0151)	(0.0192)
Cohort fixed effects	x	x	x	x	x	x
Individual characteristics	x	x	x	x	x	x
Age at move fixed effects	x	x	x	x	x	x
Origin-by-destination fixed effects	x	x	x	x	x	x
N	11600	11283	17479	5832	10981	13335

Notes: Column (1) includes only boys and column (2) restricts the sample to girls. In columns (3) and (4), regressions are run separately by language of instruction at age 15. In column (5), the sample is restricted to movers for which $\Delta\bar{y}_{od} > 0$ and column (6) is restricted to cases where $\Delta\bar{y}_{od} < 0$. Standard errors are clustered at the destination neighborhood level.
*** p<0.01, ** p<0.05, * p<0.1

Table A.16: Decomposition of Exposure Effects - Empirical Bayes Estimates

Outcome:	University enrollment	DES in 5 years	Years of education
	(1)	(2)	(3)
	Total exposure effects		
β	-0.0494***	-0.0434***	-0.0548***
	(0.0105)	(0.0102)	(0.0102)
	Restricted convergence rates		
β^{-s} (No school effects)	-0.0093**	-0.0054**	-0.0137***
	(0.0042)	(0.0021)	(0.0044)
β^{-n} (No neighborhood effects)	-0.0401***	-0.0381***	-0.0411***
	(0.0091)	(0.0103)	(0.0100)
Share school effects	81%	88%	75%
	(0.0725)	(0.0543)	(0.0809)

Notes: Sample restricted to movers within Montreal. To shrink estimates of Ω_n and Λ_n, I first calculate standard errors for each school and neighborhood fixed effect using bootstrap resampling (100 samples with replacement, clustering within primary school-secondary school-FSA cells). I then shrink estimates toward their means using the empirical Bayes procedure described in Chandra et al. (2016). Standard errors on the convergence rates are clustered at the destination FSA level, and obtained by the delta method for restricted convergence rates. β^{-s} is a restricted rate for which $\beta_s = 0$, and β^{-n} is a restricted rate for which $\beta_n = 0$. The total rate is constructed using equation (10) (i.e.$\beta = \beta^{-s} + \beta^{-n}$). Share school effects is given by the ratio $\frac{\beta - \beta^{-s}}{\beta}$.

A Data Appendix

Measurement of outcomes Different levels of education are governed by different departments of the Ministry of Education. Each department keeps separate student records in different formats, but these files can be matched using unique student IDs. Researchers interested in using these data must first submit a research protocol to the Ministry and file a data access request through the *Commission d'accès à l'information.*

Primary and secondary school levels, as well as vocational studies, are governed by the same department. These records notably include any secondary school degree or qualification received, vocational degrees awarded, and the year these degrees were earned. For vocational degrees, the subject is also recorded. From these files, I create an indicator variable for obtaining a secondary school diploma (*DES*) within 5 years of starting secondary school (i.e. the year a student is first observed in grade 7). Note that a student may have been held back in primary school and still obtain a secondary school diploma on time.

The College department records the year a student was first enrolled in any collegial program in Quebec, as well as the program and the institution of that first registration. If a college degree is awarded, the program in which the degree was awarded is recorded (e.g. pre-university degree in Natural Sciences). The exact date the degree was earned is not recorded, however. The files instead indicate whether the degree was completed either (a) on time, (b) less than 2 years after expected duration, or (c) more than 2 years after the expected duration. There is a further caveat: degree completion is only recorded for students who first enrolled in a "normal" college program (*DEC*). For example, degree completion is not recorded for students who first enrolled in a transition program. I use these files to create indicators of college enrollment and college completion. I also approximate the year of completion using the coarse information on time to completion.

The University department records enrollment separately by semester (Fall, Winter and Summer). For each semester, if a student is enrolled in a Quebec university, the number of credits taken, the institution and the field of study are recorded. A separate file is kept for degrees awarded. This file includes the year a degree is awarded, the granting institution, and the type of degree (bachelor, masters, doctoral, 1-year diploma, etc.). With these files, I notably create an indicator of university enrollment and one for bachelor degree completion.

Combining information from all three departments, I then calculate each student's highest level of education. The categories I consider are:

- No secondary school diploma or qualification
- Secondary school diploma (DES)
- Secondary school qualifications
- Vocational degree (DEP)
- Some collegial, started in "normal" program, no degree yet
- Some collegial, did not start in "normal" program
- Pre-university college degree

- Technical college degree
- Other college degree (includes 1-year degrees)
- Some university, no degree yet
- 1-year university diploma
- Bachelor degree or higher

I also calculate each student's number of years of education. Note that this variable might vary within the categories listed above. For instance, someone who dropped out in grade 9 has 9 years of education, while someone who dropped out in grade 10 has 10. Someone who took 13 years of primary/secondary schooling to obtain a DES and has no further schooling is coded as having 11 years of education (i.e. the normal time it takes to get a DES). Students who were in university for one year and then dropped out have 14 years of education (11 for primary+secondary school, 2 for college, and 1 in university), while those who stayed in university for two years before dropping out have 15 years of education. I top code the number of years of education at 16 (the time it takes to obtain a bachelor degree), however, to avoid my results being driven by outliers. For instance, I do observe a few hundreds students with 19 years of education or more (i.e. people from earlier cohorts in master and PhD programs). The number of years of education therefore incorporates information on multiple margins, e.g. retention in university, college enrollment, vocational studies after secondary school, drop out behavior, etc.

Finally, I create measures of expected earnings. To do so, I calculate earnings percentile ranks (in the national earnings distribution) for all workers aged 30-44 in the Public Use Microdata File of the 2006 Canadian Census, separately by age-group. I then calculate the mean earnings rank for each category of highest level of education, as well as for all possible combinations of level-of-education and field-of-study. Finally, I assign to each student in my data the mean earnings rank associated with her level of education in the 2006 Canadian Census (or combination of highest level of education and field of study). Note that students in the 1995 cohort normally finished secondary school in 2005-2006, meaning that 2006 is the year they were making their decision to pursue a post-secondary education.

Measurement of $\Omega^{-i}_{s(n(i))}$ Equation (5) simultaneously includes primary and secondary school fixed effects. This yields one fixed effect for each school in Montreal. Note that students attending a given secondary school need not have attended the same primary school – secondary schools do not nest primary schools.[64]

For each student, I then create a leave-self-out measure for both primary and secondary schools. For instance, for student i and primary school s (which student i attended), I calculate $\delta_s^{-i,P} = \frac{\delta^P_{s(i)} N_s - \tilde{y}_i}{N_s - 1}$, where N_s is the number of permanent residents who attend school s and $\tilde{y}_i = y_i - \bar{y}$ is the deviation of student i's outcome from the sample mean. Student i's outcome must be first re-centered around the sample mean because fixed effects are normalized to have a mean of zero.[65]

Then, I assign the relevant leave-self-out measure to each student-year observation. For years in which a student is in a primary school other than the one he was attending at baseline, no leave-self-out adjustment

[64] Default French primary schools do feed into default secondary schools. But with open enrollment, and the large number of private secondary schools, the connection between local primary and secondary schools is weak.

[65] Jackknife estimates of school fixed effects δ_s^{-i} , in which one regression is ran for each observation, are almost perfectly correlated (0.99) with my hand-calculated leave-self-out measures.

is necessary since that student was not in that school during the year on which the fixed effect estimation is based. I then take the student-level average of $\delta_s^{-i,P}$ over all primary school years, and similarly calculate a student-level average of $\delta_s^{-i,S}$ for secondary school years. The childhood school quality measure $\Omega^{-i}_{s(n(i))}$ is then the simple sum of these two averages. Note this averaging over primary/secondary school years only matters for permanent residents who have switched school at some point. For the majority of students who only attended one primary and one secondary school, the averaging is redundant, and it is simply the case that $\Omega^{-i}_{s(n(i))}=\delta_s^{-i,P}+\delta_s^{-i,S}$.

In unreported analyses, I use a split-sample approach in which a random half of the sample of permanent residents is used to measure school and neighborhood quality and the other half is used to estimate the regression-discontinuity design. Split-sample and leave-self-out measures of school quality are highly correlated (0.98), hence the results presented in this paper are very similar under the split-sample approach.

Catchment Areas To my knowledge, no electronic, geocoded version of the catchment areas that prevailed in the years 1995-2001 exists. I therefore re-constructed such maps using the following procedure.

To first generate a benchmark, the default school associated with each six-digit postal code of the Island of Montreal as of 2015 was recorded by "feeding" each of these ≈45,000 postal codes in the search engines of the websites of the three francophone schools boards. Using shapefiles for Canadian postal codes, I then created a map of all 2015 French catchment areas on the Island of Montreal, down to the six-digit postal code level.

To infer what the boundaries were in the years the cohorts of students I track started grade school, I used two additional sources of information. First, the Ministry of Education provided me temporarily with baseline enrollment data for all 100,929 students in my data set along with their six-digit postal codes (in the analytical data set, six-digit postal codes are de-identified).[66] I then mapped actual attendance patterns and compared with the 2015 boundaries. Second, I used the Internet Archives WaybackMachine (https://archive.org/web/) to document each school opening/closure that happened since 1995, and extracted old maps of catchment areas from archived versions of the school boards websites (when available). Combining all these sources of information, I deducted where the boundaries must have been drawn, and assigned the appropriate default schools to each postal code by hand. It must be noted that for many schools, the boundaries have not changed since 1995, hence no manual re-coding was necessary. Using ArcGIS, I also calculated, for each postal code, the distance to the nearest boundary and the unique ID of that boundary. Only boundaries that do not coincide with natural divisions such as highways and canals were considered. Using these same sources of information, I also inputted catchment areas for English public schools. As explained in the text, however, these boundaries are not well-defined and therefore not used in the analyses.

Attrition About 8% of the total number of students who started grade 1 in Montreal had vanished from primary/secondary school educational records before turning 16. These students are excluded from the main sample used this paper. Interestingly, about 1,000 of these students did enroll in a Quebec university at some point, even though they did not graduate from secondary school in the province. Students who had left Montreal (but remained in Quebec) by the time they turned 15 are also excluded from all analyses.

[66]This first data delivery contained only two variables: school attended (name and code) and postal code of residence. For confidentiality reasons, this file had to be destroyed before the analytical files could be transferred to me.

For higher-education, enrollment in colleges and universities outside the province is not comprised in my dataset. As a result, I may wrongly infer that some students in my main sample never attended college, when in fact they did out-of-province. However, this phenomenon likely only affects a very small proportion of my sample. A few factors provide strong incentives for college and university students to remain in the province, at least for their undergraduate studies. Firstly, tuition fees in Quebec are the lowest in Canada. Secondly, the discrepancies between Quebec's and other North American educational systems generate important timing issues in meeting college requirements. For instance, at the end of secondary school, students in Quebec only have 11 years of school, rather than 12. Finally, there is a language barrier for the large majority of students who went to primary and secondary school in French.

To assess the possible magnitude of this measurement issue, I use data from the loans and bursaries records of the Ministry of Education. For each year between 1995-1996 and 2014-2015, I was given a series of indicator variables that flag whether student i in my sample was receiving loans or bursaries in year t. Students who resided in Quebec in childhood but go abroad for college are still eligible for loans and bursaries from the Quebec government. Since at the time of enrolling in a foreign college the student's permanent address is often still a Quebec one, it is easier for them to take up loans from Quebec than from another province. I can therefore check the proportion of students who take up students loans while not being enrolled in any postsecondary institution in Quebec to assess the size of the phenomenon. Under this method, I find that about 1% of my sample attended a higher education institution outside the province at some point (many of which also attended a college or a university in Quebec before doing so out-of-province). Finally, it is worth noting that any mis-measurement of educational attainment due to students leaving the province would plausibly lead me to *underestimate* differences across schools and neighborhoods. Students studying abroad, where tuition is much more expensive, are arguably from higher-SES backgrounds, leading me to underestimate educational attainment in places where it is the highest.

B Mathematical Appendix

Interpretation of π: Example Suppose we did observe μ_n and $\tilde{\psi}_{s(n(i))}$, and ran a simple cross-sectional regression of $y^{PR}_{n(i)}$ on both these variables for the subsample of permanent residents. The regression equation would take the following form:

$$y^{PR}_{n(i)} = \alpha_n \mu_n + \alpha_s \tilde{\psi}_{s(n(i))} + \epsilon_i. \tag{11}$$

The OLS estimate of α_s is equal to $A\omega + A\rho_s$, where ρ_s corresponds to the omitted variable bias. Alternatively, ρ_s is a partial regression coefficient in a linear projection of family inputs $\tilde{\theta}_i$ onto μ_n and $\tilde{\psi}_{s(n(i))}$. Then, $\Omega_{s(n(i))} = \alpha_s \tilde{\psi}_{s(n(i))}$, $\pi = \frac{\omega}{\omega+\rho_s}$ and $\pi\Omega_{s(n(i))} = A\omega\tilde{\psi}_{s(n(i))}$.

Now, consider the feasible regression of $y^{PR}_{n(i)}$ on $\Omega_{s(n(i))}$ as well as on a set of neighborhood fixed effects, and let $\dot{\Omega}_{s(n(i))}$ denote the residuals from a regression of $\Omega_{s(n(i))}$ on neighborhood fixed effects. The OLS estimate of the coefficient on $\Omega_{s(n(i))}$ is

$$\frac{Cov\left(y^{PR}_{n(i)},\dot{\Omega}_{s(n(i))}\right)}{Var(\dot{\Omega}_{s(n(i))})}=\underbrace{\frac{Cov\left(A\lambda\mu_n,\dot{\Omega}_{s(n(i))}\right)}{Var(\dot{\Omega}_{s(n(i))})}}_{=0\text{ (fixed effects)}}+\frac{Cov\left(A\omega\tilde{\psi}_{s(n(i))},\dot{\Omega}_{s(n(i))}\right)}{Var(\dot{\Omega}_{s(n(i))})}+\frac{Cov\left(A\tilde{\theta}_i,\dot{\Omega}_{s(n(i))}\right)}{Var(\dot{\Omega}_{s(n(i))})}$$
$$=\frac{A\omega}{\alpha_s}+\frac{A\rho_s}{\alpha_s}=1.$$

Note that $\Omega_{s(n(i))}$ is a measure of predicted gains (estimated school effects), while $A\omega\tilde{\psi}_{s(n(i))}$ corresponds to true gains (true school effects). Now consider an experimental sample of permanent residents that is randomly assigned to schools and neighborhoods. Their outcomes are given by $y^E_i=A\left[\lambda\mu_{n(i)}+\omega\tilde{\psi}_{s(n(i))}+\nu_i\right]$, where ν_i is uncorrelated with $\mu_{n(i)}$ and $\tilde{\psi}_{s(n(i))}$ by virtue of random assignment. Consider a regression of y^E_i on a measure of $\Omega_{s(n(i))}$ constructed using an external, non-experimental sample. The OLS coefficient obtained by regressing y^E_i on $\Omega_{s(n(i))}$ and a set of neighborhood fixed effects is

$$\frac{Cov(y^E_i,\dot{\Omega}_{s(n(i))})}{Var(\dot{\Omega}_{s(n(i))})}=\frac{Cov\left(A\left[\lambda\mu_{n(i)}+\omega\tilde{\psi}_{s(n(i))}+\nu_i\right],\dot{\Omega}\right)}{Var(\dot{\Omega})}$$
$$=\underbrace{\frac{Cov\left(A\lambda\mu_{n(i)},\dot{\Omega}\right)}{Var(\dot{\Omega})}}_{=0\text{ (fixed effects)}}+\frac{Cov\left(A\omega\tilde{\psi}_{s(n(i))},\dot{\Omega}\right)}{Var(\dot{\Omega})}+\underbrace{\frac{Cov\left(\nu_i,\dot{\Omega}\right)}{Var(\dot{\Omega})}}_{=0\text{ (randomization)}}$$
$$=\frac{Cov\left(A\omega\tilde{\psi}_{s(n(i))},\dot{\Omega}\right)}{Var(\dot{\Omega})}=\frac{A\omega}{\alpha_s}=\frac{\omega}{\omega+\rho_s}=\pi.$$

The coefficient π is therefore the ratio of the causal effect of attending a better school over total school variation (the causal effect plus the sorting component). In the language of Chetty, Friedman and Rockoff (2014*a*), π is the relationship between true school effects and estimated school effects. It follows that $1-\pi=\frac{\rho_s}{\omega+\rho_s}$ is the amount of forecast bias in $\Omega_{s(n(i))}$.

Without such an experimental sample, one can still estimate the amount of forecast bias using a valid instrumental variable Z_i that shifts $\tilde{\psi}_{s(n(i))}$ but is otherwise orthogonal to parental inputs $\tilde{\theta}_i$. The IV estimate of the coefficient on $\Omega_{s(n(i))}$ in a regression of $y^{PR}_{n(i)}$ on $\Omega_{s(n(i))}$ as well as on a set of neighborhood fixed effects is

$$\frac{Cov(y^{PR}_{n(i)},\dot{Z}_i)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}=\underbrace{\frac{Cov\left(A\lambda\mu_n,\dot{Z}_i\right)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}}_{=0\text{ (fixed effects)}}+\frac{Cov\left(A\omega\tilde{\psi}_{s(n(i))},\dot{Z}_i\right)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}+\underbrace{\frac{Cov\left(A\tilde{\theta}_i,\dot{Z}_i\right)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}}_{=0\text{ (exclusion restriction)}}$$
$$=A\omega\frac{Cov\left(\tilde{\psi}_{s(n(i))},\dot{Z}_i\right)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}=\frac{A\omega}{\alpha_s}\frac{Cov\left(\Omega_{s(n(i))},\dot{Z}_i\right)}{Cov(\Omega_{s(n(i))},\dot{Z}_i)}=\frac{\omega}{\omega+\rho_s}=\pi$$

where $\dot{Z}_i$ denotes the residuals from a regression of Z_i the neighborhood fixed effects.

Decomposition equation For ease of exposition, ignore the conditioning variables and fixed effects in equations (8) and (9), and focus on the associated regression coefficients β, β_s and β_n. Also, for simplicity, set $\pi = 1$ so that $(\Delta\bar{y}_{od} - \pi\Delta\Omega_{od}) = \Delta\Lambda_{od}$, and let $\widehat{m\Delta\Omega}_{od}$ denote the residuals of a regression of $m\Delta\Omega_{od}$ on $m\Delta\Lambda_{od}$: $\widehat{m\Delta\Omega}_{od} = m\Delta\Omega_{od} - \frac{Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})}{Var(m\Delta\Lambda_{od})} m\Delta\Lambda_{od}$. Define $\widehat{m\Delta\Lambda}_{od}$ accordingly. Then, the coefficients of the simplified horse-race regression are

$$\beta_s = \frac{Cov\left(\widehat{m\Delta\Omega}_{od}, y_i\right)}{Var(\widehat{m\Delta\Omega}_{od})} \quad ; \quad \beta_n = \frac{Cov\left(\widehat{m\Delta\Lambda}_{od}, y_i\right)}{Var(\widehat{m\Delta\Lambda}_{od})}.$$

The full convergence rate is $\beta = \frac{Cov(m\Delta\bar{y}_{od}, y_i)}{Var(m\Delta\bar{y}_{od})}$. Re-organizing

$$\begin{aligned}
Var(m\Delta\bar{y}_{od}) \times \beta &= Cov\left(m\Delta\bar{y}_{od}, y_i\right) \\
&= Cov\left(m\Delta\Omega_{od}, y_i\right) + Cov\left(m\Delta\Lambda_{od}, y_i\right) \\
&= Cov\left(\widehat{m\Delta\Omega}_{od}, y_i\right) + \frac{Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})}{Var(m\Delta\Lambda_{od})} Cov\left(m\Delta\Lambda_{od}, y_i\right) \\
&+ Cov\left(\widehat{m\Delta\Lambda}_{od}, y_i\right) + \frac{Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})}{Var(m\Delta\Omega_{od})} Cov\left(m\Delta\Omega_{od}, y_i\right) \\
&= \beta_s Var(\widehat{m\Delta\Omega}_{od}) + \beta_n Var\left(\widehat{m\Delta\Lambda}_{od}\right) \\
&+ Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od}) \left[\frac{Cov\left(m\Delta\Lambda_{od}, y_i\right)}{Var(m\Delta\Lambda_{od})} + \frac{Cov\left(m\Delta\Omega_{od}, y_i\right)}{Var(m\Delta\Omega_{od})}\right] \\
&= \beta_s \left[Var(m\Delta\Omega_{od}) - \frac{Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})^2}{Var(m\Delta\Lambda_{od})}\right] \\
&+ \beta_n \left[Var(m\Delta\Lambda_{od}) - \frac{Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})^2}{Var(m\Delta\Omega_{od})}\right] \\
&+ Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od}) \left[\frac{Cov\left(m\Delta\Lambda_{od}, y_i\right)}{Var(m\Delta\Lambda_{od})} + \frac{Cov\left(m\Delta\Omega_{od}, y_i\right)}{Var(m\Delta\Omega_{od})}\right] \\
&= \beta_s Var(m\Delta\Omega_{od}) + \beta_n Var(m\Delta\Lambda_{od}) + \left(\beta_s + \beta_n\right) Cov(m\Delta\Omega_{od}, m\Delta\Lambda_{od})
\end{aligned}$$

$$\beta \simeq \frac{1}{Var(\Delta\bar{y}_{od})} \left[\beta_s Var(\Delta\Omega_{od}) + \beta_n Var(\Delta\Lambda_{od}) + \left(\beta_s + \beta_n\right) Cov(\Delta\Omega_{od}, \Delta\Lambda_{od})\right].$$