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The Impact of Better School Accessibility on Student Outcomes

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Abstract
This paper identifies and quantifies the effects of better transport accessibility on student performance measured by mathematics test scores. A 27 km new subway line and the extension of an existing line in Santiago (Chile) in the mid-2000s reduced the distance between some schools and their nearest subway station. Estimates are derived using fixed effects models that account for endogeneity in the relation between student performance and school-subway network distance. Increased proximity to the subway network is associated with substantially lower test scores.

Keywords: School accessibility, subway, test scores, student achievement, transport innovations
JEL classification: R42, H41, I29

1. Introduction

High cognitive achievement is closely associated with outcomes such as higher future wages (Neal and Johnson 1996), higher schooling in childhood, marriage rate and not going on welfare (Herrnstein and Murray 2010). However, empirical evidence is not conclusive about the main factors affecting student achievement. Researchers in education have typically focused in traditional schooling inputs such as teaching quality (see, for example, Rockoff (2004)) or class size (Krueger and Whitmore 2001). Despite the intuitive link between school accessibility and student and teacher supply and the vast literature investigating the role of class size and teacher quality on student outcomes, little attention has been given to the effect of school accessibility on student outcomes.

Chile is an interesting place to study the effect of school accessibility on student outcomes for several reasons. First, more than 50% of schools in the Santiago Metropolitan area (henceforth, Santiago), experienced an increase in accessibility when a new 27 km subway line plus six stations in an existing line were inaugurated in 2005. Nowadays, such change in school accessibility is unusual in other OECD countries. Second, Chile's institutional context in primary and secondary education enables school accessibility to have an effect on student outcomes through changes in school enrolment. Chile's educational system provides students the freedom to choose any school within their budget constraint (i.e. no catchment areas); in turn, changes in enrolment imply changes in schools' income given a government subsidy for public and private (voucher) schools which is proportional to students' class attendance. Third, I have a detailed administrative individual panel dataset with students' test scores in Chile's national standardised test (SIMCE) one year before and one year after the inauguration of the new subway stations in Santiago. The panel nature of the dataset enables me not only to control for students' fixed characteristics but also to avoid changes in school composition due to better accessibility by considering as my treated population all students who attended treated schools during the pre-intervention period (regardless of whether they remained in treated schools after the transport innovation). This type of estimator has been called an intent-to-treat estimator (Little and Yau 1996).

To my best knowledge, there are no previous studies exploring the impact of school accessibility on student performance. On related topics, two studies have explored the impact of school accessibility (proxied by distance to school and commuting time) on post-compulsory education enrolment and graduation from upper-secondary schools. Using British data, Dickerson and

McIntosh (2013), found that less distance (measuring distance "as the crow flies") between the students' home and their closest school is positively related with the probability that mediocre students continue into post-compulsory education. This is consistent with Falch et al.'s (2013) finding, who concluded that reduced commuting time has a positive effect on graduation from upper secondary schools in Norway and that this effect is larger for students with meagre academic achievement.

These two papers have limitations for illuminating my research question. Dickerson and McIntosh's (2013) estimate of the impact of school accessibility on post-compulsory education enrolment may be biased upwards because of omitted variables such as household income. Falch et al.'s (2013) paper explores the impact of school accessibility on upper secondary school graduation, not on test scores as my paper does. Test scores are of interest because could signal the impact of school accessibility not only on mediocre student achievement but on the whole distribution of students.

Some researchers have studied the mechanisms by which school accessibility could affect test scores. There are several potential mechanisms. First, increased transport accessibility could have led to an upturn in school enrolment. The latter could imply greater class sizes, which in turn could decrease student performance. While in the USA Krueger and Whitmore (2001) found that a decrease in class sizes from 22—25 students to 13—17 students in the Tennessee STAR project improved test scores taken twelve years after the beginning of the intervention by 13% of a standard deviation, in India, Banerjee et al. (2007) found that the inclusion of an additional teacher in each class improved test scores by 10% of a standard deviation one year after the program.

Second, better transport accessibility could affect test scores through school competition. In an educational market with free school choice as the Chilean one, better connected schools face more competition from other schools. Regarding the competition mechanism, while Card, Dooley, and Payne (2010) found a positive effect of competition on test scores (6%-8% of a standard deviation), Gibbons et al. (2008) found modest effects for faith-based voucher schools.

Third, better transport accessibility may affect student performance through increased pupil turnover or changes in neighbours' characteristics. Gibbons and Telhaj (2011) found that increased pupil mobility affects student test scores in a modest, negative way due to school disruption. In addition, Gibbons et al. (2013) found no effect of changes in socioeconomic characteristics of neighbours on students' test scores.

When estimating the impact of school accessibility on student achievement this paper has several features. The first is the use of a convincing empirical strategy to show that student test scores respond to sizable improvements in school accessibility (proxied by school-subway network distance). To obtain a causal estimate I exploit the inauguration of new subway stations in Santiago in 2005 and argue that, conditional on a variety of controls for potential differential test score trends, the transport innovation is an exogenous shock to school accessibility.

This paper's second feature is the analysis to demonstrate the robustness of the conclusions. The analysis is as follows. a) I incorporate in my models a variety of school and spatial fixed effects that account for test score differential trends. b) I explore not only the effect of linear school-subway network distance reduction (henceforth, distance reduction) but also the non-linear effects of the same variable by introducing distance reduction categories. c) I am also able to distinguish the heterogeneous effects of school-subway network distance reduction depending on the distance to the new subway stations. d) To avoid the assumption of no spatial correlation between the regression errors in my OLS time-differenced estimates I implement a permutation test on the school-subway network distance reduction category that is exact regardless of the presence of spatial correlation. e) In contrast to an important part of the literature that uses "as the crow flies" distance (e.g. Dickerson and McIntosh (2013)) I measure school-subway station distances using walking distance. The latter is arguably a more accurate measurement of distance than ignoring the shape and connectivity of transport networks in Santiago.

A final feature of this paper is that I establish my findings using administrative, individual panel data for all students in a same cohort rather than a cross-section of survey data. As stated before, the individual nature of the panel data enables me to calculate an intent-to-treat effect that avoids selection of students into treated or non-treated areas induced by the transport innovation. In addition, because I use data for the whole population of students in Santiago, I am able to introduce detailed spatial controls (1 kilometre rings around the pre-treatment subway network, 42 municipalities in urban Santiago) that account for unobserved test score trends for small spatial units.

As a preview of my results, school-subway network distance reductions of 4.7 km or more for schools that end up nearer than 2 km from the new subway stations worsen those schools' scores by 15% of a standard deviation. Conversely, on average, distance reductions of the same magnitude for schools farther or at a 2 km distance from the new subway stations have no effect on test scores. Moreover, on average, schools that experienced large distance reductions to the subway network also experienced an increase in their enrolled students.

The rest of the paper is structured as follows. Section 2 explains my method. Section 3 describes the institutional context and data. Section 4 presents and discusses my results. Finally, section 5 summarises this paper and presents concluding remarks.

2. Methods

2.1. Methodological Framework

This section discusses methods for quantifying the impact of better school accessibility on student achievement. To provide a basic reference point I start describing a simple cross-section regression for studying such relation. Then I describe a school fixed-effects regression that accounts for unobserved fixed characteristics in each school. Finally, I address the issues that could bias my fixed-effects estimates of the impact of school accessibility on test scores.

I start describing a simple regression model relating test scores to school accessibility measured in terms of distance between each school and its nearest subway station. This is the model that has been typically used in the past to study the relation between accessibility and student achievement (see, for example, Dickerson and McIntosh (2013)):

$$y_{it} = d_{it}\beta + x_{it}'\gamma + f_i + g_t + \varepsilon_{it} \tag{1}$$

As explained in the introduction, given that I am studying school accessibility, I work with school-level data. y_{it} is school i 's average mathematics test score in period t, d_{it} is the distance between school i and its nearest subway station at time t, x_{it} is a vector of other school and location characteristics, f_i are school and place-specific unobserved characteristics that are fixed over time, g_t are general time effects and ε_{it} is equation (1)'s error term. The key parameter in equation (1) is β, the effect of distance reduction on test scores. I work with mathematics test scores—rather than language ones—because the former are more susceptible to modification by school inputs (Chetty, Friedman, and Rockoff 2011).

The problem with equation (1) is that there could be unobserved school characteristics such as students' ability, family background or the education quality provided by its teachers that could be correlated both with the schools' average test score and the school-subway distance. This could happen if, for example, schools with a high proportion of students from higher socioeconomic status households were located nearer to the subway stations compared to schools with a high proportion of students from lower socioeconomic households. If this is the case, an analysis based on (1) would suffer from omitted variable bias.

To account for schools unobserved fixed characteristics whose effect does not change in time (variable f_i in equation (1)) I work with time differences instead of a cross-section. To study the effects of variation in the key variable (accessibility or distance between schools and their nearest subway station), models based in time differences need variation in the key variable that—conditional on the regressors—is uncorrelated with the dependent variable's (test scores) trend. As I explain in the introduction, one of the largest changes in Santiago's subway network occurred at the end of 2005 with the inauguration of a 24.7 km subway line (Line 4) that goes from the central business district to the South of Santiago plus six subway stations in the northern and southern periphery of Santiago (Line 5). This massive change in transport accessibility decreased the distance to the nearest subway station to many students in Santiago. I exploit these transport innovations and Chile's administrative SIMCE test panel data (described in detail in Section 3.3) to identify the impact of school accessibility on student achievement.

A convenient way to estimate equation (1) is to rewrite it in time differences:

$$(y_{i1} - y_{i0}) = (d_{i1} - d_{i0})\beta + (x'_{i1} - x'_{i0})\gamma + (g_1 - g_0) + (\varepsilon_{i1} - \varepsilon_{i0}) \tag{2}$$

By contrast with equation (1), equation (2) does not contain the school unobserved characteristics that do not change in time (f_i) yet still contains the parameter of interest β. The two periods are before the construction of the new subway stations (t=0, at the end of 2004) and after the construction of the new subway stations (t=1, at the end of 2006).

A more general specification allows for the possibility that a distance reduction for a school within walking distance to a subway station could have a larger impact than the same distance reduction for a school that is several kilometres away from the subway network. To allow for such flexibility, in the spirit of Gibbons and Machin (2005), I interact the distance to the subway network with an indicator function that takes value one when the school is at a maximum distance of 2km from the new subway stations and zero otherwise[1]. Defining the indicator function as $h_{it} = I(d_{it} \leq 2km)$, where $I(\dots)$ equals one when the condition in the parenthesis is true, I have

[1] I choose two kilometres as the threshold distance by considering feasible walking distances to the nearest subway station (0-3 km) and maximising the equation's R-squared in 0.5 km grids. This ended up being the same threshold distance used by Gibbons and Machin (2005).

$$(y_{i1} - y_{i0}) = (d_{i1} - d_{i0})h_{i1}\beta_1 + (d_{i1} - d_{i0})(1 - h_{i1})\beta_2 + (x'_{i1} - x'_{i0})\gamma + (g_1 - g_0) + (\varepsilon_{i1} - \varepsilon_{i0}) \quad (3)$$

In equation (3), β_1 is the impact of better school accessibility on student test scores.

Equations (1), (2) and (3) assume that the effect of school-subway network distance reduction ($d_{i1} - d_{i0}$, henceforth distance reduction) on test scores is linear (i.e. the marginal effect is the same for schools who experience a one or a ten kilometre distance reduction). However, there are no reasons to assume that such effect is linear. One way for allowing non-linear effects is to categorise schools according to their distance reduction. In this case, the time-differenced model that allows for non-linear effects of distance reduction on test scores is:

$$(y_{i1} - y_{i0}) = \sum_j c_j\, h_{i1}\beta_{1j} + \sum_j c_j\,(1 - h_{i1})\beta_{2j} + (x'_{i1} - x'_{i0})\gamma + (g_1 - g_0) + (\varepsilon_{i1} - \varepsilon_{i0}) \quad (4)$$

In (4), c_j are dummy variables, one for each of the j categories of distance reduction.

2.2. Identification issues in the school fixed-effects model

As explained earlier, identification of the effect of better school accessibility on student performance rests on the assumption that there are no variables that are correlated both with schools' average test score and with the 2005 school-subway network distance reduction. This assumption could be violated for five reasons. First, the identifying assumption would be violated if the shock (improvement) in school accessibility provided by the new subway stations induces selection into schools in the post-treatment period. This would happen if, for example, brighter students migrate more to or from treated schools because of increased accessibility to their schools and/or their place of residence. Second, there may be a pre-existing test score trend where initially worse (better) performing schools would improve differently from better (worse) performing schools even in absence of the new subway stations. If the previous test score trend is correlated with the magnitude of the future school-subway network distance reduction this would bias my estimates.

Third, the assumption would also be violated if schools administered by different entities (municipality, municipal corporation, voucher or private entity) have differential average test score trends and the type of administration is correlated with the distance reduction magnitude. Fourth, the assumption would be violated if municipalities close (or far away) from the new subway stations were pursuing educational policies that improved the quality of education and student achievement in schools in their jurisdiction. In addition, estimates would be biased if some mayors of municipalities located close (or far away) from the new subway stations were better at lobbying to get the new subway lines to pass through their jurisdiction. Fifth and finally, the identifying assumption would be violated if there were pre-existing spatial test score trends related to the school-subway network distance before the construction of the new subway stations. These five issues are likely to occur. However, in the next paragraph I deal with each of the five identification issues.

With suitable data, I can address each of the five concerns about the internal validity of the fixed effects estimates. The key idea is to control for test score differential trends. To deal with the first issue, I estimate equation (4) calculating an intent-to-treat effect (Lachin 2000). I do this by calculating schools' post-treatment average test score, averaging the post-treatment score of all pre-treatment students in the school regardless of their post-treatment actual school. This avoids selection into treated and non-treated schools due to the new subway lines and a potential resulting bias in the estimated effect of increased school accessibility. To deal with the second issue I control for schools' pre-intervention average score in equation (4) to address pre-existing test score trends depending on schools' initial score. To deal with the third issue I address pre-existing test score trends depending on schools' type of administration by including in equation (4) schools' type of administrative entity.

To deal with the fourth issue I address potential differential test score trends for schools in different municipalities by including municipality dummy variables in equation (4). There are 42 municipalities in urban Santiago, so I control for such potential differential trends by including 41 dummy variables, one for each municipality, in equation (4). In my preferred specifications (column (4) in Table 2 and Table 3), I control for the interacted school-type-of-administration and municipality to control for test score differential trends for each type of administration in each municipality. To deal with the fifth identification issue, to address potential pre-existing differential test score trends for schools located at different distances from the old subway network, I include distance to the pre-intervention subway network in equation (4). A robust way to control for such trends is to do it non-parametrically in distance reduction by including one dummy variable for each kilometre of school–pre-treatment network distance.

In practice, the model that addresses the five identification issues exploits the relation between distance reduction and variation in schools' average test score progression only for schools of similar initial average test score, same administrative entity, same municipality and in the same school-pre-intervention-subway-network distance band (one for each kilometre). Hence, the identifying assumption for the resulting model is that, controlling for test score trends along the five described variables, there are no omitted variables which are correlated with schools' average test score and the 2005 distance reduction to the subway network. In my opinion, this is a reasonable assumption.

2.3. Measurement Issues

Measuring accessibility is not straightforward. The British Department for Transport (2011) takes that accessibility is the "extent to which individuals and households can access day to day services, such as employment, education, healthcare, food stores and town centres." (p. 2). According to this definition, accessibility is intimately related with the cost (in time, money and effort) incurred by individuals when accessing their routine activities. In the specific case of the present study, the relevant accessibility for analysing students' performance is the students' access to their nearby schools.

The British Department for Transport definition of accessibility implies that such concept is related with time from origin to destination. I call this definition destination accessibility. Ahlfeldt (2011) used destination accessibility when considering the change in travelling distance of workers to all potential employers. However, to apply the destination accessibility concept to the present study, the researcher must know all students' addresses and commuting times to every potential school. Because of privacy issues, this is not possible. An alternative concept of accessibility is to define access as the distance to the nearest subway station. I call this definition station accessibility. The advantage of using station accessibility is that it is easier to measure it because only requires the location of every school and subway station in the city. Moreover, in context of school competition, school accessibility enables the researcher to use schools as units of observation. This facilitates the interpretation that the treatment is changes in school-subway network proximity and that this proximity, in turn, increases the interactions between schools. Because of data availability and the schools' institutional context, in this paper I use the station accessibility definition. Ahlfeldt (2011) found similar results using either destination and station accessibility definitions.

3. Institutional context, the new subway stations and data

3.1. Chile's educational context

Given that one of the main hypothesised channels for the impact of school accessibility on student achievement is through interactions between school via changes in school enrolment or competition for teachers, it is relevant to describe Chile's educational market. The Chilean education system is structured as an educational market where schools compete for greater student enrolment. In my sample in Santiago urban area (the area within 20 km of Santiago's 2006 subway network) there were 1,435 schools in the pre-intervention period (2004). While 52% (742 schools) were administered by a private institution and received a per-student subsidy from the government ("voucher schools"), 20% (281) of schools were directly administered by a local government ("municipality"), 15% (221) of all schools were administered by a Municipal Corporation (which is a private corporation headed by the municipal mayor) and 13% (191 schools) were administered by a private institution receiving no subsidy from the government[2] ("private schools").

Since 1981, and during our period of study, the Chilean school system was structured on four key characteristics. First, the government subsidy for municipal and voucher schools was a per capita sum proportional to the students' attendance. Second, voucher schools were enabled to select students from the applying pool of students and voucher schools may charge top-ups. Third, school entry had no major barriers (Gallego and Hernando 2008). Fourth, students were free to choose any school within their budget constraint (i.e. there were no catchment areas). As a cap on oversubscription, the Chilean law mandates that the maximum class size is 45 students. Oversubscribed municipal schools select using academic criteria and voucher and private schools use academic and other criteria such as the family's religious participation in faith-based schools and cultural background in international (e.g. British) schools.

To better understand how interactions between schools could be affected by better school accessibility, it is important to understand schools' funding mechanisms. Municipal and voucher schools' budget constraint in Chile in the 2004-2006 period were mainly determined by the income from the student-per-capita per-day-subsidy. However, municipalities transferred resources from schools that were more profitable (generally larger schools with good pupil

[2] An additional 1% of schools (28) were run by Company Associations or private entities that administered vocational schools.

attendance) to less profitable ones. Moreover, municipalities were allowed to transfer resources from their general budget to their schools. Hence, the budget constraint was softer in municipal schools than in voucher schools.

3.2. Santiago's transport network and the transport innovation

A master plan dating from 1968 established the construction of 5 subway lines for Santiago (Pávez Reyes 2007). The first three lines (Lines 1, 2 and 5) were inaugurated between 1975 and 1997 and encompassed a 40.2 km railway network (Agostini and Palmucci 2008). Historically, the southern part of the city has been poorly connected. Figure 1 shows Santiago's subway map as of 2006. In May 2001 the Chilean government announced the construction of Line 4, a 27 km new subway line running from Providencia, located 5 km east from Santiago's central business district to Puente Alto, located in Santiago's far south-east (see Figure 1). In December 2001, the exact location of the stations was announced. Finally, the new subway line was inaugurated in two phases; the first one in November, 2005 and the second one in March 2006. Before this date, many students living in Santiago's most excluded areas in the south-east of the city (Puente Alto) had to commute more than 4 hours per day in their return trips to get to schools in the central business district and the wealthier part of the city (Providencia and Las Condes) located in the north-eastern part of the city. In addition, Line 2, which runs in the north-south direction, also experienced a (small) expansion of six new subway stations to the north of the city between September 2004 and November 2005.

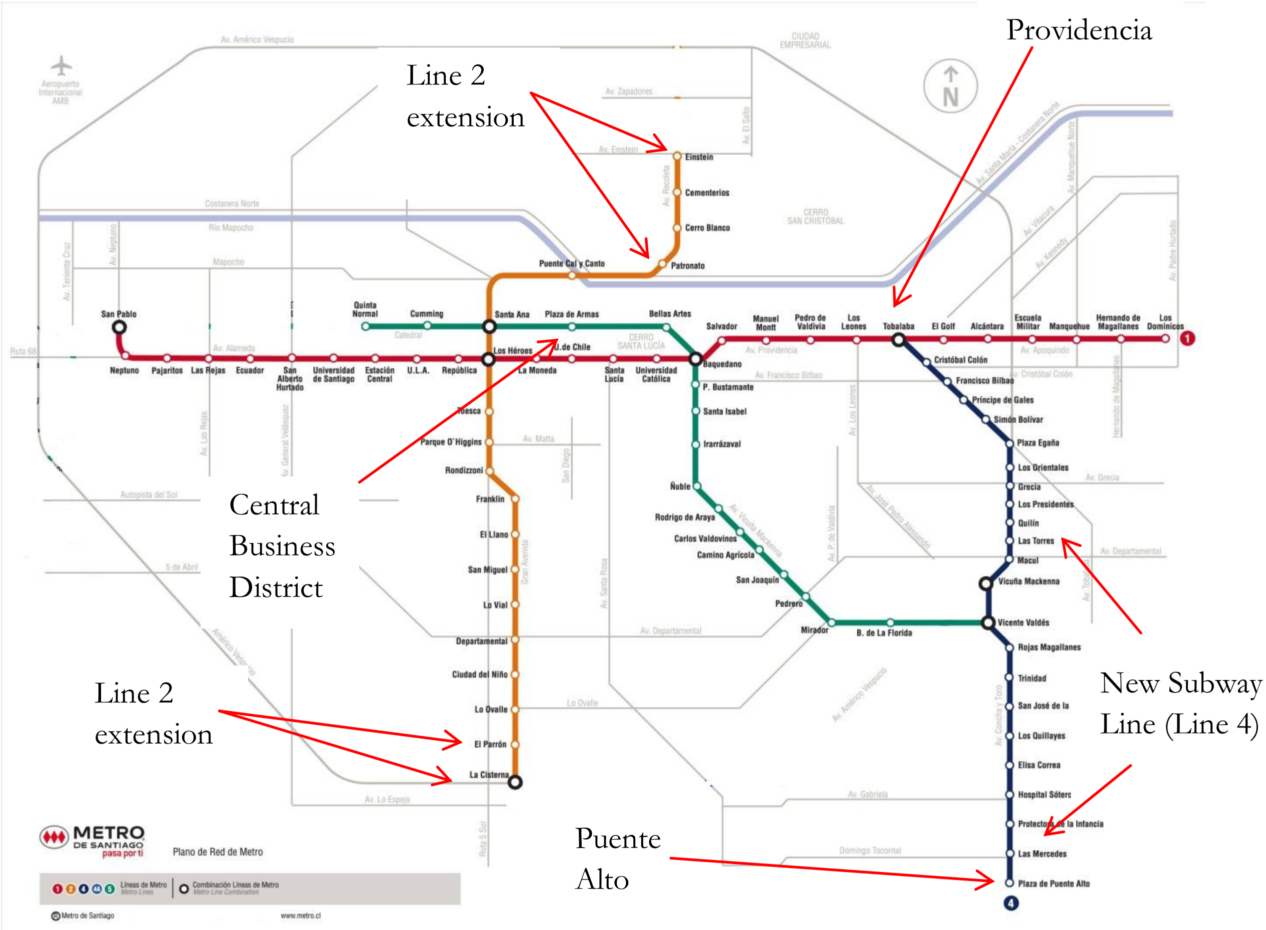

Fig. 1. Santiago post-intervention (July 2006) Subway Map**.** Source: Metro de Santiago.

3.3. Data

Two main sources of data are used. Chile's SIMCE dataset contains an individual panel with test scores in 8th and 10th grade for students who in 2004 were in 8th grade. This dataset contains language and mathematics test scores in both grades and 8th grade social science and natural science test scores and households' income. SIMCE is Chile's standardized test which at the time of the studied period was taken every year in 4th grade and some years in 8th or 10th grade. I then merged the previously mentioned SIMCE tests information with the schools' georreferenced addresses and other administrative information such as the schools' type of administrator (municipality, municipal corporation, voucher and private school). To obtain the schools' locations I normalised and geocoded the schools' addresses from Chile's Ministry of Education (publicly available) 2004 and 2006 archive. Using Ozimek and Miles' (2011) *traveltime* command in Stata which connects to Google Maps, I found the walking distance between every school in Santiago and its nearest subway station.

4. Results

4.1. Descriptive statistics

Summary statistics for schools in urban Santiago are shown in Table 1. The first two columns summarize the information about the zero school-subway-distance reduction subsample (the "zero distance reduction" or "untreated" sample), and the next two columns describe the positive school-subway-distance-reduction sub-sample ("positive distance reduction" or "treated" sub-sample). The eighth grade pre-intervention average SIMCE score of students in schools in urban Santiago whose school did not (did) experience a distance reduction was between 29%–34% (2%–7%) of a standard deviation above the national mean. In contrast, the average number of students in eighth grade in non-treated and treated schools is quite similar: 66.7 and 64.1 respectively.

Monthly household median income is higher in the untreated subsample (USD$421 per month) than in the treated subsample (USD$252.1). Voucher schools represent a 9.6 percentage points higher proportion in the treated compared to the untreated subsample. Conversely, private schools represent an 8.9 percentage point lower proportion in the untreated compared to the treated sub-ample. Hence, in terms of income and school type, students in the treated subsample are more vulnerable than in the untreated sample. This highlights the importance of controlling for differential test score trends for different socioeconomic groups and type of school in my preferred specifications in subsection 4.2. As expected, the average minimum school-subway network distance in 2004 was substantially lower for untreated schools compared to treated schools (4.2 km and 6.7 km respectively). The average distance reduction experienced by treated schools was 3.5 km.

4.2. Fixed effects estimates

In this sub-section I analyse the impact of school accessibility on student outcomes using empirical specifications (2), (3) and (4) and accounting for identification issues in the ways discussed earlier.

Controlling for unobserved fixed school characteristics such as students' ability and families' socioeconomic status, better accessibility to schools is associated with worse student outcomes. Recall that in the empirical specification depicted in equation (2) I assume a linear and homogeneous effect of distance reduction on mathematics test scores regardless of the final school-subway distance. The coefficient on distance reduction in column (1) in Table 2 (–1.013) suggests that, for each kilometre of distance reduction to the subway network, the average school test score worsens by 1% of a standard deviation. After accounting for differential school trends depending on school pre-treatment characteristics (size of each school's 8th grade cohort, mathematics, language, natural and social science SIMCE average score, income category of each school's median household, school's type of administration,), the coefficient on distance reduction in column 2 in Table 2 (–1.041) does not change significantly in magnitude.

<Table 2 near here>

The estimates in columns (3) and (4) correspond to the model specified in equation (3). This specification allows for heterogeneous effects of distance reduction on test scores depending on whether the distance between the school and the post-treatment subway network is less-or-equal or more than 2 km. The coefficients on distance reduction in Column (3) in Table 2 for schools at a distance both smaller-or-equal and larger than 2 km are of the same magnitude and significance (–1.081 and –1.022 respectively). This suggests that the effect of distance reduction on mathematics test scores is homogeneous in school-subway post-treatment distance. However, once I add spatial controls (school administration types in each municipality and proximity to the pre-treatment subway network fixed effects), the distance reduction effect on mathematics test scores for schools that end up at a maximum distance of two kilometres from the subway network (see column (4) in Table 2) increases in absolute terms to –1.5% of a standard deviation per kilometre. In contrast, the distance reduction effect for schools that end up farther than 2 km from the subway network turns statistically insignificant (coefficient equal to –0.751).

When estimating equation (3) for obtaining the results in Table 2 I assume that the effect of the treatment (distance reduction) on test scores is linear; an alternative way to analyse the results is to allow for non-linear effects of distance reduction on test scores (still under a school-fixed effects framework). Non-linearities can be introduced into equation (3) by using categories of distance reduction as treatment variables. I used five categories. Schools in the first category are those who did not experience a distance reduction to the nearest subway station after the 2005 subway expansion (667 schools, a 46% of all schools). The other four categories are formed by dividing those schools that experienced a positive distance reduction into quartile groups. There are approximately 360 schools in each group. To be precise, the five categories of distance reduction are (1) null, (2) between 0.1 and 1.6 km inclusive[3], (3) between 1.6 and 2.3 km inclusive (third category), (4) between 2.3 and 4.7 km inclusive, and (5) between 4.7 and 10.7km. In the regressions, the first category is omitted.

Non-linear estimates suggest that the causal effect of a large school-subway distance reduction (between 4.7 and 10.7km) for schools who ended up at a maximum distance of 2 km from the subway network is to worsen test scores in a policy-wise relevant way (see Table 3). The point estimates in column (1) show significant negative effects for the third, fourth and fifth distance reduction categories: a worsening between 5.2% and 7.9% of a standard deviation compared to those schools who did not experience a distance reduction and were always farther than 2 km from the subway network). Controlling for school differential test score trends according to pre-treatment school characteristics does not change the results in qualitative terms (see column (2) in Table 3)). (See Table 3 notes for a detail of these characteristics.)

<Table 3 near here>

As in Table 2, the specification in Table 3, column (3), allows for heterogeneity in the treatment effect. I allow such heterogeneity by interacting the distance reduction categories with the distance to the new subway stations. The size of the coefficient on the fifth category of distance reduction in column (3) is –9.827. The interpretation of this coefficient is the treatment effect for schools nearer than 2 km from the new subway stations who experienced a distance reduction larger than 4.7 km. Hence, controlling for all relevant covariates, test scores of students that before the inauguration of the new tube stations were in the latter group of schools worsened in 9.8% of a standard deviation compared to students in schools that did not experience a distance reduction.

Table 3, column (4) shows my preferred estimates. Compared to column (3) these incorporate spatial controls: 42 dummy variables for municipalities and 12 dummy variables for each kilometre from the old subway network. The estimates in column (4) imply that the effect on test score of great proximity to the subway network for schools that experienced more than 4.7 km of distance reduction and ended up nearer than 2 km from the new subway stations is –15% of a standard deviation (see Table 3, column (4)). Schools that ended up farther than 2km from the new subway stations that also experienced large distance-to-the-subway-network reductions did

[3] I used Google Maps to calculate the distance between subway stations and schools. Google maps approximates distances to 100m.

not experience a significant change in their test score after the inauguration of the new subway stations. Hence, the negative effect of better transport accessibility on test scores is driven by those schools that ended up nearer than 2 km from the new subway stations. (All coefficients in the post-treatment school–subway distance greater than 2 km category are non-significantly different from zero.)

4.3. Robustness analysis

In this section, I analyse the robustness of the results to different assumptions about spatial correlation between the students' test scores. In my preferred specification (Table 3, column (4)), I cluster standard errors at the municipality level. However, the regression errors could also be correlated across adjacent municipalities.

To consider the impact of spatial correlation between the regression errors I implement a permutation test of the treatment variable coefficient's standard error that is exact regardless of the presence of spatial correlation of the regression errors (and sample size). This type of robustness check to spatial correlation between the regression errors is similar to the one applied by Abadie and Dermisi (2008). To implement such a test I first produce 10,000 random permutations of the treatment variable (categories of distance reduction for column (4) in Table 3). Each permutation forces the null hypothesis—that the treatment is uncorrelated with the dependent variable—to be true by delinking the treatment and dependent variables. Second, I run the regression depicted in equation (4) with each permuted set of treatment variables. Third, I calculate the proportion of the permuted treatment variable coefficients that are greater in absolute value than the estimate calculated using the actual treatment ($\widehat{\beta_{1,5}}$). This proportion is a robust version of the p-value calculated under parametric assumptions in Table 3, column (4).

Only 1.3% of the estimated coefficients are larger in absolute value than the one calculated in Table 3, column (4). This robust p-value is to be compared to the p-values implicit in column (4) in Table 3 obtained under parametric assumptions (1.5%). Hence, regardless the regression errors' spatial correlation, there is an extremely small probability of obtaining the results in my preferred specification (Table 3, column (4)) if the null hypothesis that there is no impact of better school accessibility on student test scores is true.

4.4. Does the school–subway network distance really matter?

One way in which increased school accessibility could have had a non-causal impact on student outcomes is through changes in student dropout and repetition rates. This could have induced sample selection where worse performing students could have decreased their likelihood of dropping out from high school due to the accessibility improvement. My estimates in section 4.2 are an intent-to-treat calculation where the students' post-treatment test scores are always attached to their pre-treatment school. Note that the new subway stations were inaugurated during the students' first year in high school (9^{th} grade). Hence, if better school accessibility increased the chance that students with worsening performance took the post-treatment (2006) test, this could induce a biased negative impact of distance reduction on individual test scores.

Two reasons for a student that took the pre-treatment test for not taking the post-treatment test are dropping out from high school or repeating a grade. Table A1 in Appendix 1 shows that there is no evidence that distance reduction had an effect on the probability that a student who took the SIMCE test in 2004 would also take the test in 2006. Hence, there is no indication supporting the hypothesis that the negative effect of better accessibility on test scores was due to a decrease in dropout and repetition rates among the treated students. Therefore, I find no evidence of a non-causal explanation underlying my results.

4.5. Why does school-subway network distance matter?

The school–subway network distance reduction could affect student test scores through at least two mechanisms. Firstly, schools that experienced large reductions in school–subway network distance could have received more students due to better accessibility after the inauguration of the subway stations compared to schools that did not experience such accessibility improvement. This, in turn, leads to an increase in the student–teacher ratio and to disruption for non-moving students in the treated schools. Both factors are associated with worse test scores. (See, for example, Krueger (1999) for the effect of smaller classes on student performance and Gibbons and Telhaj (2011) for the effect of pupil mobility and school disruption on test scores.)

Table 4 shows the effect of school–subway network distance reduction on the number of students per grade in each school. The dependent variable in Table 4 is the number of students in 10th grade in each school in the post-treatment period (2006) minus the number of students in 10th grade in the same school in a pre-treatment year (2003). I used 2003 as the pre-treatment year because this is the closest year before the inauguration of the tube stations in 2005 when students in 10th grade took the SIMCE test. As in all previous analyses, my preferred specification is depicted in column (4).

<Table 4 near here>

Controlling for all relevant covariates, schools that experienced a large reduction in their distance to the subway network had an average increase of nine more students in 10th grade compared to schools that did not experience any distance reduction. Hence, there is evidence that one of the mechanisms through which the reduction in school–subway network distance affected test scores negatively is via an increase in the number of students per grade in the treated schools compared to the number of students per grade in the control group. In addition, this increase in the number of students per grade in the treated schools most likely implied disruption to the incumbent pupils in those schools.

A second mechanism through which a reduction in school–subway network distance could have affected test scores is through the effect of own moves on achievement. School movements imply adaptation costs and, potentially, higher commuting times if the movements are to schools farther from the students' homes. Hanushek, Kain, and Rivkin (2004) conclude that the effect of own moves on achievement is modest and negative (around 1% of a standard deviation in terms of the annual gain in mathematics achievement).

I find no evidence that the school–subway network distance reduction experienced by some schools implied a higher probability that students in those schools would move to another school. Column (4) in Table A2 depicts the results of a regression of own movement (whether the student changed school after the inauguration of the subway stations) on distance reduction categories. In this regression, the coefficients on large distance reductions are not statistically significant and have a low absolute value in practical terms. Therefore, most likely, the negative impact of distance reduction on test scores is not driven by an increase in the probability that students in treated schools would change school.

5. Summary and Conclusions

The main purpose of this paper was to establish whether improvements in school accessibility have a causal effect on test scores. This is a key policy question because many developing countries are investing resources in improving their urban transport network and the consequences for human capital accumulation have not been considered.

This paper addresses an understudied research question. First, I use a detailed individual administrative test score data set with information before and after the transport innovation that avoids selection bias and changes in school composition by calculating an intent-to-treat effect. Second, I account for potential biases in my fixed effect estimates by controlling for test score differential trends in relevant dimensions. Third, I carry out robustness checks to spatial correlation between the regression errors. This combination of techniques is generalisable to almost any study working with panel data.

My main finding is that there is a large negative effect of school-subway distance reduction on test scores. Fixed effects analyses lead me to conclude that schools that experienced a large decrease of more than 4.7 km of distance to the nearest subway station and ended up at walking distance from the subway network (2 km) had average test scores that were some 15% of a standard deviation lower.

The magnitude of this estimate is policy-relevant. In a review of 18 randomised evaluations reporting test score outcomes in developing countries, Kremer et al. (2013) reported that the upper bound of all 90% confidence intervals of the average effect of the programs was less than 9% of a standard deviation.

I also found evidence that the negative effect of distance reduction on test scores was due to an increase in the number of students in schools that experienced a large decrease in distance to the subway network. Understanding the channels through which better school accessibility affects student performance is of key importance if policy makers wish to avoid undesired effects of new transport infrastructure on human capital accumulation. Future research should investigate the relevance of other potential channels such as teacher performance, eviction of students and their families due to an increase in rents after the inauguration of the new subway stations and peer effects due to changes in school composition.

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Table 1

Descriptive statistics of schools in urban Santiago

	Zero distance reduction sub-sample		Positive distance reduction sub-sample	
	Mean	s.d.	Mean	s.d.
Number of schools	667		768	
Number of students	45,103		49,980	
Average standardised SIMCE 2004 scores				
Mathematics	33.6%	70%	6.6%	5.8%
Language	30.2%	60%	4.5%	5.2%
Social Science	28.9%	60%	2.4%	5.3%
Natural Science	29.3%	66%	3.9%	5.6%
Average number of students in same school and grade who took the SIMCE test in 2004.	66.7	62.9	64.1	43.7
Household median income (2004 USD)	421.0		252.1	
Type of Administration				
Municipal	19.7%		19.5%	
Municipal Corporation	15.7%		15.1%	
Voucher	46.5%		56.1%	
Private	18.1%		9.2%	
Minimum school-subway network distance in 2004 (km)	4.24	3.96	6.70	3.65
Proportion of schools at a maximum distance of 2 km from the 2006 subway network	41%	49%	42%	49%
Distance reduction (km)	0		3.47	2.79
Categories of positive-distance-reduction schools				
0 km< distance reduction ≤ 1.6 km			25.5%	
1.6 km< distance reduction ≤ 2.3 km			27.9%	
2.3 km< distance reduction ≤ 4.7 km			22.3%	
4.7 km< distance reduction ≤ 10.7 km			24.3%	

Notes: The pre-intervention and post-intervention years are 2004 and 2006 respectively. Test scores are measured as z-scores standardised at the national level with a mean of zero and a standard deviation of one. Statistics are at the school level and (except for the "students in same school" variable) are weighted by the number of students enrolled in 2004 who also took the SIMCE test in 2006. Zero (positive) distance reduction sub-sample refers to those schools who did not (did) experience a school-subway network distance reduction due to the subway stations inaugurated in 2005. The sample is restricted to those schools at a maximum distance of 20 km from the 2006 subway network with no missing values in all the described variables.

Table 2

The effect of school-subway distance reduction on mathematics test scores: linear model

Dependent variable: 2006-2004 mathematics average test score	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
Distance reduction (km)	−1.013***	−1.041**		
	(0.288)	(0.182)		
Distance reduction (km) | distance ≤ 2 km			−1.081**	−0.751
			(0.185)	(0.846)
Distance reduction (km) | distance > 2 km			−1.022**	−1.497**
			(0.239)	(0.563)
Number of students in same school and grade in 2004 (log)		6.249***	6.245**	6.105***
		(1.066)	(1.082)	(1.475)
Mathematics, language, natural and social science 2004 average school score fixed effects	No	Yes	Yes	Yes
Household median income fixed effects	No	Yes	Yes	Yes
School type of administration fixed effects	No	Yes	Yes	No
Municipality x School type of administration fixed effects	No	No	No	Yes
Proximity to the pre-treatment subway network fixed effects	No	No	No	Yes
R-squared	0.014	0.387	0.387	0.482

Notes: The table reports regression coefficients and standard errors multiplied by 100 to give the % effect of a one point change in explanatory variables. The dependent variable is post-treatment (2006, 10th grade) minus pre-treatment (2004, 8th grade) school average difference in standardised average language test scores; hence, this is a fixed effect estimate. Test scores are measured as z-scores standardised at the national level with a mean of zero and a standard deviation of one. Regressions are run at the school level and are weighted by the number of students who took the SIMCE test in the same school and grade in 2004. To get an intent-to-treat effect I assign students to their initial school even if the student changed school between initial and final periods. Distance reduction means distance reduction between the school and the nearest subway network because of the new stations between final and initial periods in kilometres. There are 29 categories of household median income; these categories are calculated obtaining the household median income in each school. Municipalities in the (urban) studied area are 42 and school type of administration categories are four (municipal, municipal corporation, voucher and private schools). Proximity to the old subway network fixed effects is a set of 12 dummy variables; one for each km of school-subway distance (plus an omitted category). Robust standard errors in parentheses clustered at the type of administration level in regressions (2) and (3) and at the Municipality level in regression (4). Sample restricted to schools at a maximum distance of 20 km from the new subway network. All regressions include an intercept (not shown). Sample size = 1,435 schools. *** p<0.01, ** p<0.05, * p<0.1.

Table 3
The effect of school-subway distance reduction on mathematics test scores: nonlinear models

Dependent variable: 2006-2004 mathematics average test score	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
0 km distance reduction (reference category)	0	0		
0 km< distance reduction ≤ 1.6 km	−3.653	−4.246		
	(2.372)	(3.981)		
1.6 km< distance reduction ≤ 2.3 km	−5.233**	−7.261***		
	(2.092)	(1.147)		
2.3 km< distance reduction ≤ 4.7 km	−7.751***	−6.826**		
	(2.181)	(1.870)		
4.7 km< distance reduction ≤ 10.7 km	−7.886***	−8.957***		
	(2.631)	(1.136)		
Post-treatment school-subway distance > 2 km				
0 km distance reduction (reference category)			0	0
0 km< distance reduction ≤ 1.6 km			−4.255	−0.672
			(3.995)	(2.787)
1.6 km< distance reduction ≤ 2.3 km			−6.513***	−5.138
			(0.851)	(3.757)
2.3 km< distance reduction ≤ 4.7 km			−8.252**	−6.188
			(1.446)	(4.380)
4.7 km< distance reduction ≤ 10.7 km			−4.524***	−3.444
			(0.401)	(4.828)
Post-treatment school-subway distance ≤ 2 km				
0 km distance reduction			3.328**	−2.522
			(0.627)	(5.074)
0 km< distance reduction ≤ 1.6 km			−0.141	−1.166
			(3.840)	(3.174)
1.6 km< distance reduction ≤ 2.3 km			−4.819	−2.483
			(2.805)	(4.457)
2.3 km< distance reduction ≤ 4.7 km			−3.629	−2.598
			(1.766)	(4.037)
4.7 km< distance reduction ≤ 10.7 km			−9.827**	−14.86**
			(1.851)	(5.857)
Number of students in same school and grade in 2004 (log)		6.030***	6.324***	6.131***
		(0.972)	(0.929)	(1.461)
Pre-treatment average school score in language, maths, natural and social science controls	No	Yes	Yes	Yes
Household median income fixed effects	No	Yes	Yes	Yes
School type of administration fixed effects	No	Yes	Yes	No
Municipality x Type of administration fixed effects	No	No	No	Yes
Proximity to the old subway network fixed effects	No	No	No	Yes
R-squared	0.021	0.396	0.402	0.486

Notes: See notes in Table 2. Distance reduction categories are five: one zero-distance reduction school (reference) category and four categories divided along quartiles of students in the non-zero distance reduction schools.

Table 4

The effect of school-subway distance reduction on the size of each school's cohort: nonlinear models

Dependent variable: students in each 10th grade cohort in each school in 2006 minus students in 2003	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
0 km distance reduction (ref. category)	0	0		
0 km< distance reduction ≤ 1.6 km	-13.81**	-14.47*		
	(5.404)	(6.523)		
1.6 km< distance reduction ≤ 2.3 km	-13.70	-14.36*		
	(8.819)	(6.640)		
2.3 km< distance reduction ≤ 4.7 km	0.621	-1.156		
	(4.100)	(1.682)		
4.7 km< distance reduction ≤ 10.7 km	9.450**	6.942**		
	(4.298)	(2.075)		
School-subway distance > 2 km				
0 km distance reduction (ref. category)			0	0
0 km< distance reduction ≤ 1.6 km			-17.57	-15.34*
			(9.946)	(8.552)
1.6 km< distance reduction ≤ 2.3 km			-15.27	-20.81
			(12.88)	(14.90)
2.3 km< distance reduction ≤ 4.7 km			4.777***	3.803
			(0.736)	(4.206)
4.7 km< distance reduction ≤ 10.7 km			5.103	4.959
			(2.456)	(5.814)
School-subway distance ≤ 2 km				
0 km distance reduction			1.503	2.444
			(4.814)	(9.123)
0 km< distance reduction ≤ 1.6 km			-9.769	-6.112
			(6.583)	(7.770)
1.6 km< distance reduction ≤ 2.3 km			-11.10	-6.382
			(5.781)	(5.349)
2.3 km< distance reduction ≤ 4.7 km			-5.233	-7.422
			(7.592)	(5.218)
4.7 km< distance reduction ≤ 10.7 km			9.998***	9.105**
			(1.010)	(3.886)
Quintile of number of students in same school and grade in 2003 fixed effects	No	Yes	Yes	Yes
Quintile of average school score in language and maths in 2003 fixed effects	No	Yes	Yes	Yes
School type of administration fixed effects	No	Yes	Yes	Yes
Proximity to the old subway network fixed effects	No	No	No	Yes
R-squared	0.030	0.240	0.245	0.257

Notes: The table reports regression coefficients and standard errors multiplied by 100 to give the % effect of a one-point change in explanatory variables. The dependent variable is post-treatment (2006, 10th grade) minus pre-treatment (2003, 10th grade) average number of students in each cohort in each school. Regressions are run at the school level. Distance reduction means distance reduction between the school and the nearest subway network because of the new stations between final and initial periods in kilometres. Distance reduction categories are five: one zero-distance reduction school (ref.) category and four categories divided along quartiles of students in the non-zero distance reduction schools. Proximity to the old subway network fixed effects is a set of 12 dummy variables; one for each km of school-subway distance (plus an omitted category). Robust standard errors in parentheses clustered at the type of administration level in regressions (2) and (3) and at the Municipality level in regression (4). Sample restricted to schools at a maximum distance of 20 km from the 2006 subway network. All regressions include an intercept (not shown) and have a sample size equal to 690 schools. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Table A1

The effect of school-subway distance reduction on the probability of remaining in high school: nonlinear models

Dependent variable: student remains in high school	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
0 km distance reduction (reference category)	0	0		
0 km< distance reduction ≤ 1.6 km	–0.0870**	–0.0126		
	(0.0437)	(0.0477)		
1.6 km< distance reduction ≤ 2.3 km	–0.132***	0.0618***		
	(0.0356)	(0.0227)		
2.3 km< distance reduction ≤ 4.7 km	–0.0824*	0.00930		
	(0.0433)	(0.0311)		
4.7 km< distance reduction ≤ 10.7 km	–0.166***	–0.0388		
	(0.0430)	(0.0312)		
School-subway distance ≤ 2 km				
0 km distance reduction (ref. category)			0	0
0 km< distance reduction ≤ 1.6 km			0.0582*	0.165***
			(0.0346)	(0.0515)
1.6 km< distance reduction ≤ 2.3 km			0.0490*	0.107*
			(0.0271)	(0.0605)
2.3 km< distance reduction ≤ 4.7 km			0.0270	0.133*
			(0.0407)	(0.0679)
4.7 km< distance reduction ≤ 10.7 km			–0.00263	0.120
			(0.0431)	(0.0797)
School-subway distance > 2 km				
0 km distance reduction			–0.0163	0.204
			(0.0315)	(0.133)
0 km< distance reduction ≤ 1.6 km			–0.155	0.00855
			(0.104)	(0.0686)
1.6 km< distance reduction ≤ 2.3 km			0.0951**	0.170***
			(0.0409)	(0.0572)
2.3 km< distance reduction ≤ 4.7 km			–0.0152	0.0378
			(0.0441)	(0.0655)
4.7 km< distance reduction ≤ 10.7 km			–0.0739*	0.0352
			(0.0422)	(0.0802)
Number of students in same school and grade in 2004 (log)		0.102***	0.104***	0.106***
		(0.0197)	(0.0195)	(0.0196)
Individual score in language, maths, natural and social science in 2004 fixed effects	No	Yes	Yes	Yes
Household income fixed effects	No	Yes	Yes	Yes
School type of administration fixed effects	No	Yes	Yes	No
Municipality x Type of administration fixed effects	No	No	No	Yes
Proximity to the old subway network fixed effects	No	No	No	Yes
Observations	93,798	83,668	83,668	83,668

Notes: See notes in Table 2. Individual-level probit regressions. Dependent variable: whether students who took the test in 8th grade also took the test in 10th grade. Robust standard errors in parentheses clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.

Table A2

The effect of school-subway distance reduction on the probability of remaining in the same school: nonlinear models

Dependent variable: student remains in same school	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
0 km distance reduction (reference category)	0	0		
0 km< distance reduction ≤ 1.6 km	-0.365***	-0.243***		
	(0.0825)	(0.0743)		
1.6 km< distance reduction ≤ 2.3 km	-0.515***	-0.170**		
	(0.0986)	(0.0701)		
2.3 km< distance reduction ≤ 4.7 km	-0.146*	0.00315		
	(0.0878)	(0.0589)		
4.7 km< distance reduction ≤ 10.7 km	-0.395***	-0.105		
	(0.0858)	(0.0707)		
School-subway distance > 2 km				
0 km distance reduction (reference category)			0	0
0 km< distance reduction ≤ 1.6 km			-0.164**	-0.188
			(0.0778)	(0.126)
1.6 km< distance reduction ≤ 2.3 km			-0.144*	-0.315**
			(0.0852)	(0.159)
2.3 km< distance reduction ≤ 4.7 km			0.0977	0.126
			(0.0771)	(0.149)
4.7 km< distance reduction ≤ 10.7 km			0.122	0.202
			(0.0806)	(0.180)
School-subway distance ≤ 2 km				
0 km distance reduction | distance > 2 km			0.147**	-0.337
			(0.0659)	(0.211)
0 km< distance reduction ≤ 1.6 km			-0.197	-0.549***
			(0.142)	(0.194)
1.6 km< distance reduction ≤ 2.3 km			0.0622	-0.130
			(0.0956)	(0.145)
2.3 km< distance reduction ≤ 4.7 km			0.0466	-0.113
			(0.0806)	(0.149)
4.7 km< distance reduction ≤ 10.7 km			-0.138	-0.0738
			(0.0937)	(0.181)
Number of students in same school and grade in 2004 (log)		0.196***	0.209***	0.143***
		(0.0356)	(0.0336)	(0.0315)
Individual score in language, maths, natural and social science in 2004 fixed effects	No	Yes	Yes	Yes
Household income fixed effects	No	Yes	Yes	Yes
Municipality x Type of administration fixed effects	No	No	No	Yes
Proximity to the old subway network fixed effects	No	No	No	Yes
School type of administration fixed effects	No	Yes	Yes	No
Observations	47,849	41,348	41,348	41,283

Notes: See notes in Table 2. Individual-level probit regressions. Dependent variable: whether students who took the 2004 test in 8th grade were in the same school in 2006 in 10th grade. Sample restricted to students whose school had both primary and secondary levels.

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