Closer Proximity to the Subway Network Implies Lower High School Test Scores: Evidence from a Subway Expansion*

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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 24 km new subway line and the extension of an existing line in Santiago (Chile) in the mid-2000s reduced the distance between more than half of schools in the city and their nearest subway station. Estimates are derived using instrumental variables and fixed effects models that account for endogeneity in the relation between student performance and school–subway network distance. Substantial closer proximity to the subway network (5 km or more) is associated with lower test scores (11 percentage points of one standard deviation). I find evidence that some mechanisms could be an increase in the student/teacher ratio, an increase in parental hours of work and a worsening in the quality of peers of students in treated schools relative to students in control schools.

Keywords: School accessibility, subway, test scores, student achievement, transport innovations.

JEL classification: R42, H41, I29

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1. Introduction

High cognitive achievement is closely associated with outcomes such as higher future wages (Neal and Johnson 1996), higher schooling in childhood, marriage rates and not going on welfare (Herrnstein and Murray 2010). However, empirical evidence is not conclusive about the main factors affecting student achievement. Researchers have typically focused on traditional schooling inputs such as teaching quality (see, for example, Rockoff (2004)) or class size (Krueger and Whitmore 2001). Theoretically, better school accessibility could affect student supply because it decreases the student's generalised cost and effort to access certain schools. Because of spending less time and effort in commuting, if part of the additional time and effort is invested studying, students may improve their performance. In addition, better school accessibility could modify teacher supply (increasing it or decreasing it depending on whether initially most teachers live near or far from the specific schools). An increase (decrease) in teacher supply could increase (decrease) a school's value added by facilitating (making more difficult) the choice of high quality teachers by the school. Despite these links between school accessibility and student performance, 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 per cent of schools in Santiago experienced an increase in accessibility when a new 24-km subway line and six additional stations on an existing line were inaugurated in 2005. Such large and discrete change in school accessibility is currently unusual in OECD countries. Second, Chilean schools' institutional context enables school accessibility to have an effect on student outcomes through changes in school enrolment. Chile's educational system allows families to choose any school within their budget constraint (i.e. there are 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 the number of students attending the school.

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 to control for students' fixed characteristics and to avoid the contamination of my estimates with effects of changes in school composition due to better accessibility. I do this 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 the best of my 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 from 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 between the students' homes and their closest school (measuring distance 'as the crow flies') is positively related to the probability that 'young people who are on the margin of participating in post-compulsory education (according to prior attainment and family background) continue into post-compulsory education' (Dickerson and McIntosh 2013, 742). This is consistent with Falch et al.'s (2013) finding, which 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 in the second and third quartiles of prior academic achievement.

These two papers have limitations. 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 this paper does. Test scores are of interest because they could signal the impact of school accessibility not only on students with low or median prior achievement but on the whole distribution of students.

Student achievement may be affected by better urban transport accessibility because of changes in the value added provided by the students' school or by direct effects on students not mediated by the school. Several potential mechanisms operate through schools. Increased urban transport accessibility may lead to an upturn in school enrolment. Because of the importance of class size to outcomes, if this leads to greater class sizes, better urban transport accessibility may actually decrease student performance. Krueger and Whitmore (2001) find 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 per cent of a standard deviation. In a related research using teacher/student ratios as a key variable, Banerjee et al. (2007) find in data from India, that having an additional teacher in a class improved test scores by 10 per cent of a standard deviation one year after the program was over.

In addition to the mechanism of class size, better transport accessibility could affect the value added provided by schools through school competition. In an educational market with free school choice, schools that are more accessible face more competition from other schools. Card, Dooley, and Payne (2010) find a positive effect of competition on test scores (6–8 per cent of a standard deviation). On the other hand, Gibbons et al. (2008) find modest effects for faith-based voucher schools. In another study, Gibbons and Telhaj (2011) found that increased pupil mobility modestly reduces student test scores due to school disruption.

There are a number of other potential mechanisms that are not mediated by schools. Better urban transport accessibility may affect student performance through changes in neighbourhood characteristics. However, Gibbons et al. (2013) find no effect of changes in socioeconomic characteristics of neighbourhoods on students' test scores. On the other hand, increased transport accessibility increases the students' access to attractive destinations of truancy. Although the research on the destinations of truancy is extremely thin, anecdotal evidence (The Branding Brothers 2008) shows that most destinations of truancy in Santiago such as parks and movie theatres are in places served by the subway network. Hence, greater proximity to the subway network may increase truancy and this may reduce student achievement. Alternatively, greater proximity between the student's school and the subway network may imply a reduction in travel time to school for affected students. If this reduction in commuting time results in an increase in study time or in the effort at school because the students are not tired from travelling, better school accessibility could improve student achievement.

Several features in this paper are useful when estimating the impact of school accessibility on student achievement. 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 use the inauguration of new subway stations in Santiago in 2005. I first isolate the distance reduction to the subway network that is quasi-experimental due to an accidental improved connectivity. I do this by using distance reduction to the least cost path route (i.e. straight line) between Santiago's central business district and the place which the authority wanted to connect to the subway network (Puente Alto Square). This distance reduction is my instrument. Hence, conditional on a variety of controls for potential differential test score trends, my instrument is an exogenous shock to school accessibility.

This paper's second feature is demonstrating the robustness of the conclusions. The analysis is as follows. 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. Moreover, I am also able to distinguish the heterogeneous effects of school–subway network distance reduction depending on the distance from the new subway network. I also check that there is no evidence that unobservables are driving my results by carrying out a placebo test with a proposed line that had not yet been inaugurated in the post-expansion period (2006). Additionally, 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. Furthermore, 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 the former because incorporates the shape and connectivity of streets in Santiago in the distance calculation.

A final feature of this paper is that I establish my findings using administrative, individual panel data for all students in the 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 student population in Santiago, I avoid response selection and 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.

I find that school–subway network distance reductions of 4.7 km or more for students whose school ends up nearer than 2 km from the new subway stations worsen those students' scores by 11 per cent of a standard deviation. Conversely, on average, distance reductions of the same magnitude for schools at a 2 km distance or farther 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 when the outcome of interest is academic achievement. Section 3 describes the institutional context in education and data. Section 4 presents and discusses my results. Finally, Section 5 summarises this paper and presents concluding remarks.

2. Methods

2.1. Measurement Issues

The definition of transport accessibility which the British Department for Transport (2011) uses 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 this paper, the relevant day-to-day activity is students' access to nearby schools.

The British Department for Transport's definition of accessibility implies costs in terms of time, money, and effort to get from origin to destination. I call this 'destination accessibility'. Ahlfeldt (2013) uses 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, I should model the whole transport network with its different modes (walking, car, bus, subway) and car availability during different periods of the day.

Alternatively, I could assume that each individual has only two modes of transport available: subway or walking (and a combination of both modes). I call 'subway accessibility' to an indicator that is inversely proportional to the average time that each individual would take to every potential employer in the city when the only available modes of transport are subway and walking. In this paper, I would need to define all schools at a feasible commuting distance for each student. Unfortunately, the criteria for defining 'feasible commuting distance' for students requires a very specific knowledge about individuals.

A third option is to use the distance between each student's school and the nearest subway station as a proxy for access. I call this 'station accessibility'. The advantage of using station accessibility is that it does not require knowledge, data or assumptions about modes of transport other than the subway. In the context of the impact of better urban transport accessibility on property prices, Ahlfeldt (2013) finds similar results using 'destination' or 'station' accessibility. Because of data availability, in this paper I use the station accessibility definition.

2.2. Empirical Strategy

This section discusses methods for quantifying the impact of better school accessibility on student achievement. To provide a basic reference point I start by 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 by describing a simple regression model relating test scores to school accessibility proxied by proximity to the subway network. 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)):

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

In (1), y_{it} is student i's mathematics test score in period t, d_{it} is the distance between student i´s school and its nearest subway station at time t, f_i are individual and place-specific 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 proximity to the subway network on test scores. I focus in mathematics test scores—rather than language ones—because the former are more susceptible to modification by school inputs (Chetty, Friedman, and Rockoff 2011). However, in the robustness analysis (section 4.4) I show that the results using language test scores are consistent with the results using mathematics test scores.

The problem with equation (1) is that there could be unobserved individual characteristics such as students' ability, family background or the education quality provided by teachers that could be correlated both with the schools' average test score and the proximity to the subway network. 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 were the case, an estimation of equation (1) would not be feasible because some part of the f_i would be unobserved. Otherwise, omitting this unobserved part of f_i in the estimation of β would lead us to omitted variable bias.

To account for students' unobserved fixed characteristics whose effects do not change over 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 stations), models based on 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 in the mid-2000s. This subway expansion consisted of a new 24-km subway line (Line 4) that goes from the central business district to the south of Santiago, plus extensions of existing subway lines in the northern and southern peripheries of Santiago (Lines 2, 4A and 5). This massive change in transport accessibility decreased the distance from the nearest subway station for more than 50 per cent of households in Santiago. I exploit these transport innovations as well as Chile's administrative SIMCE test panel data described in Section 3.3 to identify the impact of proximity to the subway network on student achievement.

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

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

In contrast with equation (1), equation (2) does not contain individual *i*'s unobserved characteristics that are time-invariant (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 their construction (*t*=1, at the end of 2006).

Equation (2) is an explicit way of specifying a 'before and after' analysis that enables us to identify the key parameter β accounting for invariant characteristics of individuals: $\hat{\beta}$ is the fixed-effects estimator. The identifying assumption for an unbiased estimate of the effect of closer proximity to the subway network on academic achievement is that, conditional on individuals' invariant characteristics, the change in unobservables for a student ($\varepsilon_{i1} - \varepsilon_{i0}$) must be uncorrelated with the distance reduction to the subway network ($d_{i1} - d_{i0}$). This assumption could be violated if, between the baseline and post-subway expansion periods, differential shocks on test scores could have affected students who would experience different magnitudes of distance reduction to the subway network. For example, the identifying assumption would be violated if students who would experience a large distance reduction to the subway network in the mid-2000s experienced a sustained improvement in their academic achievement before and after the opening of the new subway stations relative to the change in academic achievement for students who would not experience such a distance reduction to the subway network due to reasons that are unrelated to closer accessibility to the subway network.

One way of relaxing the identifying assumption is to assume that the change in unobservables affecting academic achievement is uncorrelated with the distance reduction to the subway network only for students of similar baseline characteristics. To implement this assumption, in equation (3), I control for several baseline characteristics of students. These controls allow the fixed effects estimator to compare the outcomes of specific students not with the whole sample, but only with those students with similar baseline characteristics. All regressions include the linear and quadratic terms of continuous variables and dichotomous variables for discrete characteristics. After controlling for these baseline characteristics, the empirical specification is as follows:

$$y_{i1} - y_{i0} = (d_{i1} - d_{i0})\beta + (g_1 - g_0) + x'_{i0}\gamma + (\varepsilon_{i1} - \varepsilon_{i0}) \qquad (i = 1, \ldots, N), \qquad (3)$$

where x'_{i0} is a vector that contains all previously mentioned baseline characteristics.[1]

A more general specification allows for the possibility that a distance reduction to the subway network for a student in a school that ends up at a certain threshold distance (e.g. walking distance) from a subway station could have a larger impact than the same distance reduction for a student whose school ends up several kilometres away from the subway network. To allow for such flexibility, in the spirit of Gibbons and Machin (2005), I interact the distance from the subway network with an indicator function that takes value one when the student's school is at a maximum threshold from the new subway stations and zero otherwise. 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 (walking) distance used by Gibbons and Machin (2005) and Ahlfeldt (2013). Defining the indicator function as $h_{it} = I(d_{it} \leq threshold\ distance)$, where $I(\ldots)$ equals one when the condition in parentheses is true and zero otherwise, I have

$$\begin{aligned}(y_{i1} - y_{i0}) = {} & (d_{i1} - d_{i0})h_{i1}\beta_1 + (d_{i1} - d_{i0})(1 - h_{i1})\beta_2 + x'_{i0}\gamma + (g_1 - g_0) \\ & + (\varepsilon_{i1} - \varepsilon_{i0})\end{aligned} \qquad (4)$$

In equation (4), β_1 is the impact of closer proximity to the subway network on student test scores.

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[1] Including x'_{i1} in the equation in first differences is equivalent to incorporating $h_{it}x'_{it}$ in the levels equation where $h_{it} = I\{t = 1\}$ is an indicator function that takes value one during the first period, zero otherwise.

Equations (3.1) through (3.4) assume that the effect of distance reduction to the subway network $(d_{i1} - d_{i0})$ on academic achievement is linear (i.e. the marginal effect is the same for units which experience a one or a ten kilometre distance reduction). However, there are no theoretical reasons to assume that such effect is linear. One way for allowing non-linear effects is to categorise units 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:

$$(y_{i1} - y_{i0}) = \sum_j c_j\, h_{i1}\beta_{1j} + \sum_j c_j\,(1 - h_{i1})\beta_{2j} + x'_{i0}\gamma + (g_1 - g_0) + (\varepsilon_{i1} - \varepsilon_{i0}) \qquad (5)$$

In (5), c_j are dummy variables, one for each of the j non-reference categories of distance reduction.

Despite the controls for observables and time-invariant unobservables, equation (5) could still be biased by time-variant unobservables. For example, if the lobbying power of a neighbourhood organisation in Santiago improved in the early 2000s and this specific neighbourhood association was effective in advocating to both bring the subway close to the community and improve the quality of schools, this could bias the estimates from equation (5). To avoid such bias we would need an (exogenous) shock that affected test scores only by affecting the placement of the new subway stations (not through other channels; this is the exclusion restriction of an instrumental variable).

The interpretation of the coefficient of interest in equation (5) is the intention to treat effect for a national planner who has no control over associated investments. Most of the investments that could have occurred around the new stations such as improvements to parks, streets, and lighting are decided by local governments. Local governments in Chile are elected separately from the central government, so the decisions of the former are autonomous with respect to the decisions of the latter. Other investments such as commercial investment are partly decided by the local governments through each municipality's land use planning and partly by the private firms who decide their own location. Although it would be interesting to explore whether additional investment around the new subway stations are relevant mechanisms for the effect of better urban transport accessibility on student achievement, to my knowledge, there is no dataset with the information of park improvements, commercial investment or other relevant infrastructure investment in Santiago during the mid-2000s.

3. Chile's institutional context in education and data

3.1. Chile's educational context

Since one relevant hypothesised channel for the impact of school accessibility on student achievement is through interactions between schools via changes in school enrolment or competition for teachers, it is relevant to describe Chile's institutional context in education. During the 2000s, the Chilean education system was structured as an educational market where schools competed for greater student enrolment. In my sample in the Santiago urban area (the area within 20 km of Santiago's 2006 subway network) there were 1,435 schools in 2004. At that time, 52 per cent (742 schools) were administered by a private institution and received a per-student subsidy from the government ('voucher schools'), 35 per cent (502) of schools were directly administered by the local government ('municipal schools'), and 13% (191 schools) were administered by a private institution receiving no subsidy from the government ('private schools').[2]

Since 1981 and during the 2000s, 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 student attendance. Second, voucher schools were enabled to select students from the applying pool of students and could charge families an additional top-up fee . Third, school entry was a relatively unregulated process with practically no administrative barriers for new schools (Gallego and Hernando 2008). Fourth, families were free to choose any school within their budget constraint (i.e. there were no catchment areas). Despite the non-existence of catchment areas, the median distance between the residences of students in fourth (primary) grade in 2002 and their school was 1.9 km (Gallego and Hernando 2009). Fifth, as a cap on oversubscription, Chilean law mandated that the maximum class size could be 45 students. Oversubscribed municipal schools selected students using academic criteria and voucher and private schools used academic and other criteria. For instance, faith-based schools could take into account the family's religious participation, and international (e.g. British) schools could take into account a family's cultural background.

A crucial contextual characteristic of Chile's educational market is the schools' funding mechanisms. Municipal and voucher schools' budget constraints in Chile during 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

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

larger schools with good pupil 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. The transport system in Santiago and its major expansion in the mid-2000s

In the early 2000's, the period before the expansion of Santiago's subway network, the transport network was crucial for most Santiago citizens' daily activities. In 2001, there were 13.1 million trips taken in Santiago, 71 per cent of which were motorised (the rest of the trips were made on foot) (SECTRA 2002). Of the motorised trips, 46 per cent of the trips were made by bus, 41 per cent by car, 12 per cent by subway, and 11 per cent in taxi or shared taxi (author's estimates based on SECTRA 2002 data).

Therefore, the two main modes in Santiago's public transport system in the early 2000s were bus and subway. The subway network covered the densest part of the city in terms of population, and was a fast and reliable transport system. A master plan dating from 1968 had established the construction of five subway lines in 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). Fig. 1 shows a map of Santiago's subway network in 2001 (panel A) with lines 1, 2, and 5. Panel B shows Santiago's subway network in the city centre. Lines 1, 2, and 5 are in red, yellow and green. Fig. 1 shows that Santiago's subway network in the early 2000s did not serve the population in the metropolitan periphery. This was especially true for Santiago's population in the city's southeast, an area that would be served in the mid-2000s by the blue line (Line 4) in panel B. The population in the city's southwest would be served in the early 2010s by the extension of the green line (Line 5).

As with any rapid transit system, Santiago's subway system was fast because it was not subject to congestion. In addition, Santiago's subway had predictable wait times (with timetables being adhered to), and was a safe means of transport.

Panel A: Subway network in 2001. Source: Metro de Santiago (2014)

Panel B: Subway network in 2012. Source: Google Maps.

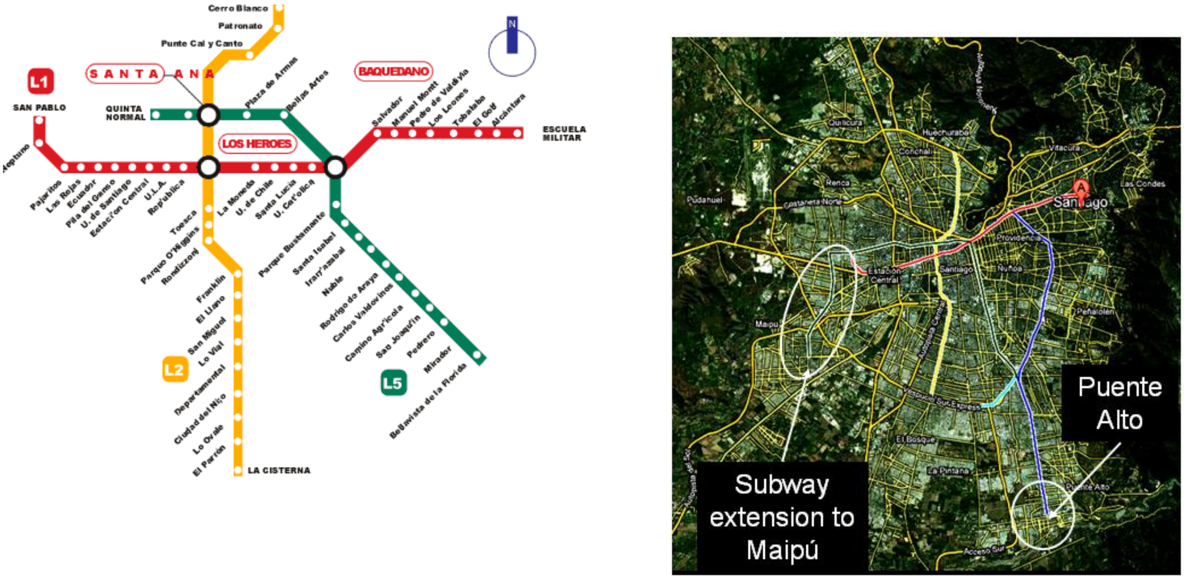

Fig. 1. Santiago's subway network

By 2001, the bus network covered the whole city of Santiago including its metropolitan periphery, and had a high share of the city's trips on public transport. Pinochet's military dictatorship (1973–1989) implemented a bus system that had no barriers of entry to new operators. During the 1990s, the newly elected democratic governments of Chile's centre-left Concertación, put out to tender the routes that crossed the city centre. By the late 1990s, there were almost 4,000 bus operators, most of which owned just one bus (Gschwender 2005).

However, the bus network was subject to several problems. It was slow during peak-times, had unpredictable waiting times, and was a dangerous and relatively unpleasant means of transport (Gschwender 2005). First, at peak times, buses were subject to high levels of congestion. This is a characteristic shared by any transport system without exclusive lanes. Second, waiting times were unpredictable. Although the individual operators associated to form bus lines, because of the atomised structure of ownership, buses competed even within the same lines. This competition implied that, 'it was normal to see two or even three buses from the same line travelling together, 'fighting' to catch passengers in the next stop' (Gschwender 2005, 5). Hence, this competition increased bus bunching, making waiting times unpredictable. Third, there was a high probability of accidents involving buses. This was because of the incentives for drivers to go above the speed limits because of the competition between buses, and the fact that drivers often worked long hours because labour laws were not enforced. (see the explanation of 'the war for the fare' by Johnson, Reiley and Muñoz 2005). Fourth, buses in Santiago deteriorated rapidly because of a lack of preventive maintenance. The atomised structure of ownership along the same routes implied a lack of professional management or preventive maintenance schemes for buses (Gschwender 2005). On the other hand, one positive aspect of Santiago's bus system was that the routes were extremely long, so most commuters did not need to make transfers (Gschwender 2005). Hence, in the early 2000s, though it was limited in geographic coverage, the subway network had superior attributes relative to the bus network in terms of speed, safety, and quality of service.

At the beginning of 2001, there were two competing projects to extend Santiago's subway network. One alternative was to extend the subway network to Maipú (in Santiago's southwest); the other alternative, was to extend it to Puente Alto (in Santiago's southeast) (Radio Cooperativa 2001). Each of these two municipalities in the city's metropolitan periphery had a large population (around 500,000) not served by the subway network.

In May 2001, the Chilean government announced the construction of subway Line 4, a 24-km subway line running from Providencia, located 5 km east of Santiago's central business district, to Puente Alto (see Panel B in Fig. 1). In December 2001, the exact locations of the stations were announced. The new subway line was inaugurated in two phases; the first in November 2005 and the second in March 2006. Before this date, many citizens living in Santiago's most unserviced areas in the southeast of the city (Puente Alto) had more than four-hour round trip commutes each day to get to jobs and 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 to this large expansion of the system, between September 2004 and November 2005 Line 2, which runs in the north-south direction, also experienced a (small) extension of the line and the addition of six new subway stations.

The opening of the subway Line 4 to Puente Alto and the extension of Line 2 took place between September 2004 and March 2006. This was the greatest expansion of Santiago's subway network since the 1970s and implied an increase in urban transport accessibility whose impact on academic achievement I evaluate in this paper.

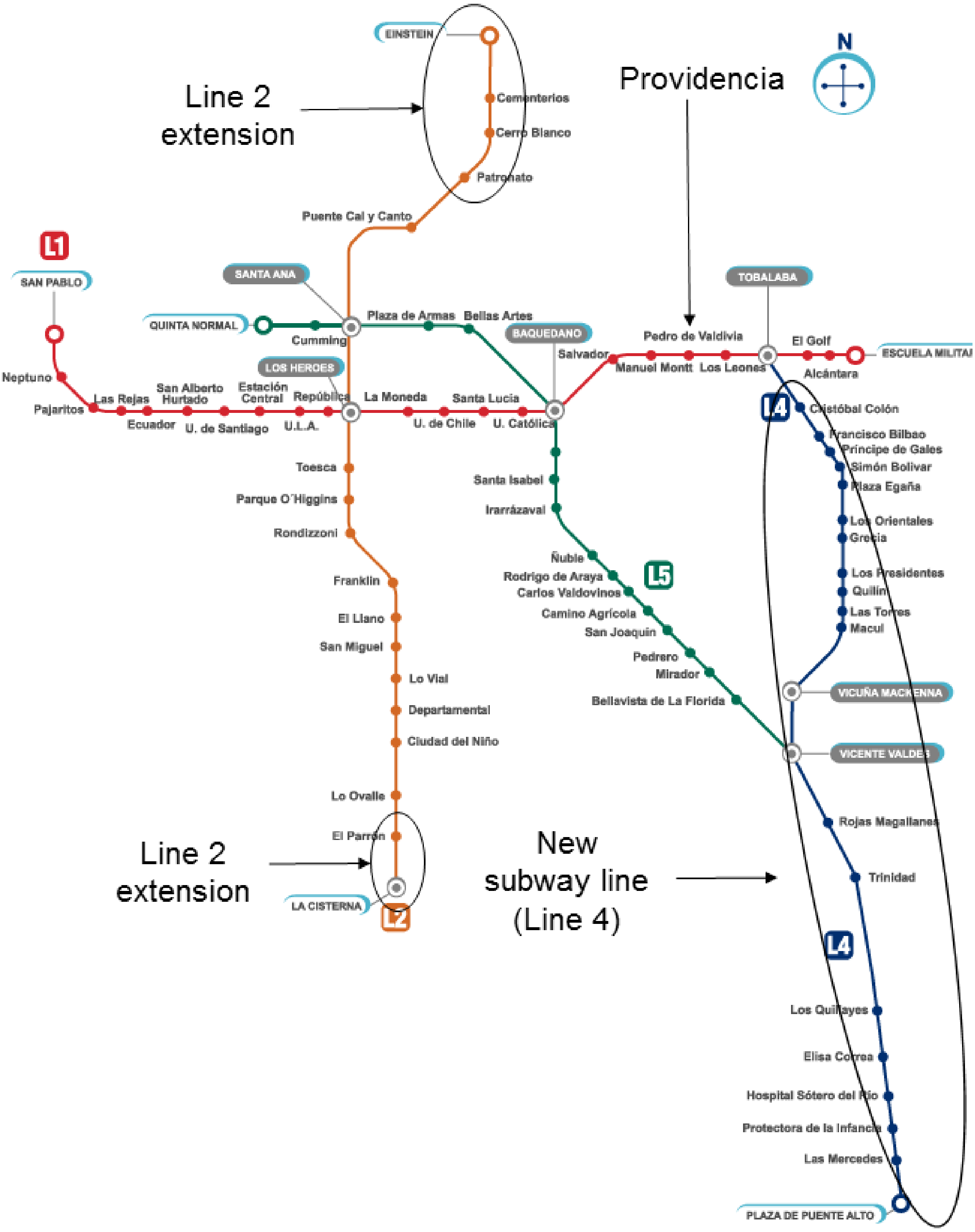

Fig. 2. Santiago post-subway expansion (July 2006) subway map. Note: Stations inaugurated between September 2004 and March 2006 highlighted with black circles. Source: Metro de Santiago.

In November 2005, Chile's President Lagos announced the 14-km extension of subway Line 5 to Maipú (Atina Chile! 2005) (See this extension in Fig. 1, Panel B.). This extension was inaugurated in February 2011. In section 4 I use the extension of Line 5 to Maipú as a 'placebo experiment' for Santiago's subway expansion in the mid-2000s. One characteristic of this extension that makes it suitable as a placebo experiment is that this was a proposed subway line in the early 2000s that was inaugurated after my post-expansion data (2006). Another characteristic is that the destination of both proposed subway extensions, the municipalities of Puente Alto and Maipú, share similar characteristics in terms of their location in Santiago's metropolitan periphery and their large population with limited access to Santiago's subway network during the early 2000s. These two facts provided the mayors of Maipú and Puente Alto great bargaining power for lobbying the central government's authorities for the subway to pass through their municipalities.

3.3. Data

I use three main sources of data. First, Chile's SIMCE dataset contains an individual panel with test scores in eighth and tenth grades for students who were in eighth grade in 2004. Both in 2004 (for eighth graders) and in 2006 (for tenth graders), the SIMCE test was taken in November. This dataset contains language and mathematics test scores in both grades, as well as eighth grade social science and natural science test scores, and household income. SIMCE is Chile's standardized test that, during the period of study, was taken every year in fourth grade and some years in eighth or tenth grades.

Second, I then merged the SIMCE test information with the schools' georeferenced addresses and other administrative information such as the schools' type of administration (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. Third, I use the addresses of each subway station in Santiago. The pre-expansion subway network includes those stations that were inaugurated before the date of the baseline test (this is, on or before October 2004). The post-expansion subway network includes the pre-expansion subway network and all the subway stations inaugurated on or before the beginning of the academic year in 2006 (this is, between November 2004 and March 2006). 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.

Ideally, to offer a more comprehensive view of the effects of better transport accessibility on academic achievement, this study would have benefitted from having the individual addresses of the students.

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 subsample ("positive distance reduction" or "treated" subsample). The eighth grade pre-intervention average SIMCE score of students in schools in urban Santiago whose schools did not (did) experience a distance reduction was between 39–43 per cent (12–17 per cent) of a standard deviation above the national mean. By contrast, the average size of class of students in eighth grade in non-treated and treated schools is quite similar: 35.4 and 35.6 respectively.[3]

[3] The average size of class is measured using the registry of students who took the SIMCE test in 2004.

Table 1. Descriptive statistics of students in schools urban Santiago in the pre-intervention period

	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	0.43	1.03	0.17	0.98
Language	0.40	0.98	0.15	0.97
Social Science	0.39	0.99	0.12	0.98
Natural Science	0.39	1.03	0.14	0.99
Average size of class	35.4	7.1	35.6	6.9
Average size of cohort	125.80	0.13	93.76	0.54
Household median income (2004 USD)	421		252	
Type of Administration				
Municipal	19.7%	39.8%	18.1%	38.5%
Municipal Corporation	15.6%	36.3%	18.2%	38.6%
Voucher	46.5%	49.9%	53.5%	49.9%
Private	18.1%	38.5%	10.3%	30.4%
Average minimum school-subway network distance in 2004 (km)	4.23	3.95	5.96	3.41
Proportion of students in schools at a maximum distance of 2 km from the 2006 subway network	40.7%	49.1%	36.5%	48.1%
Distance reduction (km)	0		2.47	1.58
Categories of students in positive-distance-reduction schools				
0 km< distance reduction ≤ 1.6 km			30.4%	
1.6 km< distance reduction ≤ 2.3 km			33.5%	
2.3 km< distance reduction ≤ 4.7 km			26.2%	
4.7 km< distance reduction			9.8%	

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 student level and considered students who took the SIMCE test in mathematics in 2004 and 2006. Zero (positive) distance reduction subsample refers to those students whose school 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 students in schools at a maximum distance of 20 km from the 2006 subway network with no missing values in all the described variables.

Monthly household median income is higher in the untreated subsample (US$421 per month) than in the treated subsample (US$252). Voucher schools represent a 7 percentage points higher proportion in the treated subsample compared to the untreated subsample. Conversely, private schools represent a 7.8 percentage point lower proportion in the untreated compared to the treated subsample. 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 for type of school in my preferred specification in Section 4.3. 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 6.0 km respectively). The average distance reduction experienced by students in treated schools was 2.5 km.

4.2. Fixed effects estimates

In this 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 (–1.186) suggests that, for each kilometre of distance reduction to the subway network, the average school test score worsens by 1 per cent of a standard deviation. After accounting for differential school trends depending on school pre-treatment characteristics (size of each school's eighth grade cohort, mathematics, language, natural and social science SIMCE average score, income category of each household, and the student's school type of administration), the coefficient on distance reduction in column (2) in Table 2 (–1.289) does not change significantly in magnitude.[4]

[4] An ad-hoc t-test shows that the difference between both coefficients is not statistically significant.

Table 2. The effect of school–subway distance reduction on mathematics test scores: linear model

Dependent variable: individual change in standardised test score 2004 to 2006	(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)	-1.186*** (0.345)	-1.289*** (0.311)		
Distance reduction (km) | distance ≤ 2 km			-1.288*** (0.272)	-1.265*** (0.456)
Distance reduction (km) | distance > 2 km			-1.291** (0.490)	-0.910 (0.719)
Baseline characteristics				
Number of students in same school and grade in (log)	No	Yes	Yes	Yes
Language, natural and social science quintile	No	Yes	Yes	Yes
Household income	No	Yes	Yes	Yes
School type of administration	No	Yes	Yes	No
Municipality x School type of administration	No	No	No	Yes
Proximity to the old subway network	No	No	No	Yes
Observations	68,160	67,026	67,026	67,026
R-squared	0.002	0.021	0.021	0.033

Notes: The table reports regression coefficients and standard errors multiplied by 100 to give the % effect of a one-km change in distance reduction to the subway network. The dependent variable is post-treatment (2006, 10th grade) minus pre-treatment (2004, 8th grade) individual difference in standardised average language test scores; hence, this is a fixed-effects 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 individual level. 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 15 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 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 municipality level in all regressions. Sample restricted to schools at a maximum distance of 20 km from Santiago's 2006 subway network. The largest distance reduction is 10.5 km. All regressions include an intercept (not shown). *** p<0.01, ** p<0.05, * p<0.1.

The estimates in columns (3) and (4) correspond to the model specified in equation (4). 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 significance and similar magnitude (–1.288 and –1.291 respectively). This suggests that the effect of distance reduction on mathematics test scores is homogeneous in school–subway post-treatment distance. 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 2 km from the subway network (see column (4) in Table 2) remains stable at –1.3 per cent of a standard deviation per kilometre (coefficient equal to -1.265). By contrast, the distance reduction effect for schools that end up farther than 2 km from the subway network turns statistically insignificant (coefficient equal to –0.910).

When estimating equation (4) 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 student-fixed-effects framework). Equation (5) allows for non-linearities by using categories of distance reduction as treatment variables. In Table 3, I used five categories. Students in the first category are those whose school did not experience a distance reduction to the nearest subway station after the 2005 subway expansion (667 schools; 46 per cent 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) between 1.6 and 2.3 km inclusive, (4) between 2.3 and 4.7 km inclusive, and (5) more than 4.7 km.[5] In the regressions, the first category is the reference category.

Non-linear estimates suggest that the causal effect of a large school–subway distance reduction (larger than 4.7 km) for students in schools that ended up at a maximum distance of 2 km from the subway network is to worsen test scores in a policy-relevant way (see Table 3). The point estimates in column (1) show significant negative effects for the first (coefficient of –9.758), second (–7.461), and fourth –4.952 distance reduction categories: a worsening between 5.0 and 9.6 per cent of a standard deviation compared to those schools that 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 2's notes for a detail of these characteristics.)

[5] Google maps approximates distances to 100 m.

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

	(1)	(2)	(3)	(4)
Dependent variable: individual change in standardised test score 2004 to 2006	Basic model	As (1) plus student covariates	As (2), plus heterogeneity in school-subway distance	As (3), plus spatial controls
4.7 km< distance reduction	-9.578***	-10.48***		
	(2.441)	(2.260)		
2.3 km< distance reduction ≤ 4.7 km	-7.461**	-7.959**		
	(3.252)	(3.101)		
1.6 km< distance reduction ≤ 2.3 km	-5.294*	-5.207*		
	(3.120)	(2.990)		
0 km< distance reduction ≤ 1.6 km	-4.952**	-4.929**		
	(2.332)	(2.106)		
0-km distance reduction (reference category for coefs. in columns (1) and (2))	0	0		
	(0)	(0)		
School-subway distance ≤ 2 km				
4.7 km< distance reduction			-12.08***	-11.14**
			(3.247)	(4.913)
2.3 km< distance reduction ≤ 4.7 km			-3.530	2.278
			(2.935)	(3.362)
1.6 km< distance reduction ≤ 2.3 km			-2.881	2.485
			(4.789)	(4.259)
0 km< distance reduction ≤ 1.6 km			0.000660	-2.339
			(2.843)	(3.363)
0-km distance reduction			2.099	-7.021
			(1.890)	(5.315)
School-subway distance > 2 km				
4.7 km< distance reduction			-5.997**	-2.428
			(2.531)	(3.142)
2.3 km< distance reduction ≤ 4.7 km			-12.10***	-5.988
			(3.628)	(5.675)
1.6 km< distance reduction ≤ 2.3 km			-4.838	-1.990
			(3.663)	(4.179)
0 km< distance reduction ≤ 1.6 km			-6.190**	3.046
			(2.319)	(2.278)
0-km distance reduction (reference category for coefs. in columns (3) and (4))			0	0
			(0)	(0)
Predetermined covariates (2004)				
Sex	No	Yes	Yes	Yes
Number of students in same school and grade (log)	No	Yes	Yes	Yes
Quintile group of baseline score in language, social science, and natural science	No	Yes	Yes	Yes
Household income	No	Yes	Yes	Yes
School type of administration	No	Yes	Yes	No
School has secondary school.	No	Yes	Yes	No
Municipality x Type of administration	No	No	No	Yes
Proximity to the old subway network	No	No	No	Yes
Observations	68,160	67,026	67,026	67,026
R-squared	0.003	0.021	0.022	0.034

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

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 from the new subway stations. The size of the coefficient on the fifth category of distance reduction in column (3) is –12.08. The interpretation of this coefficient is the treatment effect for students in schools nearer than 2 km from the new subway stations that experienced a distance reduction larger than 4.7 km. Hence, controlling for all relevant covariates, test scores of students who before the inauguration of the new subway stations were in the latter group of schools worsened in 12.1 per cent of a standard deviation compared to students in schools that did not experience a distance reduction to the subway network.

Table 3, column (4) incorporates spatial controls: 42 dummy variables, one for each non-reference municipalities and 12 dummy variables for each kilometre from the pre-treatment subway network. The estimates in column (4) imply that the effect on test score of closer proximity to the subway network for students in schools that experienced more than 4.7 km of distance reduction and ended up nearer than 2 km from the new subway stations is –11 per cent of a standard deviation (coefficient of –11.14; see Table 3, column (4)). On average, students in schools that ended up farther than 2 km from the new subway stations and experienced large distance-to-the-subway-network reductions did not experience a significant change in their test scores 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 closer 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. Instrumental variable estimates

If the location of the subway stations inaugurated in the mid-2000s in Santiago was correlated with changes in the same period in student outcomes, this would bias my estimates. Because of the spatial controls in column (4) of Table 2 and Table 3, the previously described bias would arise if the location of the new subway stations within each municipality was correlated with changes in student outcomes. This would occur if, for example, neighbourhoods which were experiencing a decrease in material conditions during the mid-2000s would be more effective than neighbourhoods which were experiencing an increase in material conditions during the same period in lobbying mayors and the central government to bring the subway route closer to them. In fact, descriptive statistics in Table 1 show that students whose school experienced closer proximity to the subway network were, on average, poorer and performed worse in academic terms relative to students whose school did not experience closer proximity to the subway network during the mid-2000s.

According to the studies which justified the creation of Santiago's subway line 4, the purpose of this line was to connect downtown Santiago with Puente Alto Square (Sectra and Metro de Santiago 2001). According to this same study, Puente Alto was the fastest growing and largest (in terms of population) municipality in the Greater Santiago Area. If the purpose of the authority was to connect Puente Alto Square with Santiago's downtown, then one could argue that the closer proximity to the subway network experienced by neighbourhoods between downtown Santiago and Puente Alto Square was an unintended consequence from the point of view of the planner. If this were true, one could also argue that unobservable characteristics of the places that improved their connectivity to the subway network are inconsequential to the choice of route for the new subway line in Santiago. Exploiting this to solve a potential endogeneity between the placement of transport infrastructure and the potential outcomes around this new infrastructure when identifying an unbiased effect of transport infrastructure is what Redding and Turner (2014) have called the "inconsequential units approach".

The inconsequential units approach was pioneered by Chandra and Thompson (2000) when examining the impact of highway construction on the level of economic activity of counties in the USA. By focusing in non-metropolitan (rural) counties that received a highway after 1969, the authors claim that the fact that these counties improve their connectivity to the highway network is accidental and therefore there should be no correlation between unobservables of these counties and the probability of improved connectivity.

To implement the inconsequential units approach I construct a hypothetical least cost path route (i.e. straight line) between Santiago's central business district and Puente Alto Square (see Fig. 3). In this framework, to estimate the causal effect of improved access to the subway network on student outcomes, I isolate the distance reduction to the subway network that is quasi-experimental due to an "accidental" improved connectivity. Hence, my instrument is the distance reduction to the hypothetical subway network excluding the target to be connected (Puente Alto Square).

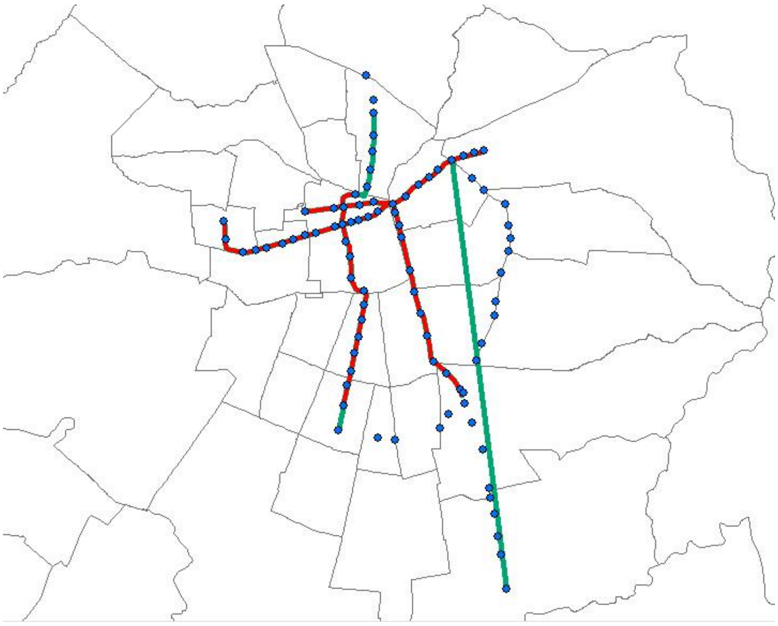

Fig. 3. Santiago's pre-expansion (2001) subway line plus straight lines to destination subway stations of the expansion in the mid-2000s.

Then, I regress student performance on this quasi-experimental variation of proximity to the subway network. In other words, I instrument the potentially endogenous regressor "distance reduction to the subway network" with the exogenous regressor "distance reduction to the least cost path route between Santiago's central business district (CBD) and Puente Alto Square". Due to the potential endogeneity between the placement of the new subway stations and the change in test scores, I expect that the IV estimates could be lower than the OLS ones.

Table 4. The effect of school-subway distance reduction on mathematics test scores: instrumental variables.

Instrument: distance reduction to the pre-intervention subway network plus the least-cost path route between Tobalaba subway station and Puente Alto Square.	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
Panel A: Two-Stage Least Squares. Dependent variable: individual change in standardised test score 2004 to 2006				
Distance reduction (km)	-1.981***	-2.042***		
	(0.590)	(0.539)		
Distance reduction (km) | distance ≤ 2 km			-2.158***	-2.116***
			(0.716)	(0.817)
Distance reduction (km) | distance > 2 km			-1.952***	-0.873
			(0.715)	(0.904)
Panel B: First Stage. Dependent variable: distance reduction to the subway network				
Distance reduction to straight line B (km)	119.2***	119.0***		
	(7.594)	(7.182)		
Distance reduction to straight line B (km) | distance ≤ 2 km			91.49***	91.88***
			(7.369)	(13.21)
Distance reduction (km) straight line B | distance > 2 km			122.9***	85.96***
			(8.304)	(14.74)
Kleibergen-Paap Wald F-statistic	249.014	272.149	45.636	12.845
F-statistic	10.99	296.87	1425	2363.7
Baseline characteristics				
Number of students in same school and grade in (log)	No	Yes	Yes	Yes
Language, natural and social science quintile	No	Yes	Yes	Yes
Household income	No	Yes	Yes	No
School type of administration	No	No	No	Yes
Proximity to the old subway network	No	No	No	Yes
Observations	62,364	61,307	61,307	61,307
R-squared	0.002	0.020	0.020	0.028

Notes: As for Table 3. The table reports regression coefficients and standard errors multiplied by 100 to give the % effect of a one-unit increase in the key variable. The dependent variable in Panel A (second stage in the two-stage least squares regression) is post-treatment (2006, 10th grade) minus pre-treatment (2004, 8th grade) individual difference in standardised average language test scores. The endogenous regressor in Panel A is distance reduction to the subway network. The instrument in Panel A is distance reduction to the straight line between Tobalaba station (in Santiago's central business district) and Puente Alto Square. The dependent variable in Panel B is distance reduction to the nearest subway station. See notes in Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 shows the results of my instrumental variable estimates. Column (1) shows the results using the straight line between Tobalaba subway station and Puente Alto Square as the hypothetical non-endogenous route joining the central business district and Puente Alto. These are the two end-stations in Santiago's subway Line 4. According to this specification, the effect of school–subway distance reduction on mathematics test score is –2.1 percentage points of a standard deviation per kilometre (coefficient of –2.116). Because this calculated effect is lower than the effect calculated in Table 2 (the effect in Table 2 was –1.3 per standard deviation), I interpret that my OLS estimates suffer from an upward bias. Hence, whenever possible, the next sections use IV estimates.

4.4. Robustness analysis

4.4.1 Robustness to a pre-existing test score trend correlated with future subway lines: placebo subway line

The common trends assumption would be violated if an unobserved shock between the pre-subway expansion and post-subway expansion tests would affect the academic achievement of treated and control students in different magnitudes. For example, during 2006, secondary school students in Chile carried out the largest student demonstrations in Chile's last three decades (BBC News 2006). During 2006, almost 800,000 secondary students in Chile used strikes and demonstrations as a way of demanding a better quality of schools. The strikes, some of them lasting for months, may have had a significant effect on academic achievement. On the other hand, most of the new the new stations that opened in the mid-2000s were located along the main streets. Hence, if the proportion of students participating in strikes in a school correlated with the distance between the school and the nearest main street, the student strikes and demonstrations in 2006 could have biased my estimates.

A placebo experiment may falsify the previous concern. As I describe in detail in Section 3.2 on Santiago's transport system, in 2001, a potential subway line to Maipú was competing with the line to Puente Alto for the central government's funding. Because the line to Maipú was not built at the time, I use it as a placebo subway line. Table 5 shows that the coefficients on all distance reduction categories are non-significant and extremely low in practical terms (all of them are smaller than five per cent of a standard deviation). Hence, there is no evidence that unobserved shocks like the student demonstrations affected treated students differently from control students and, thus, could be driving the significance of the results in my preferred specification (Table 5.3 column (4)).

Table 5. The effect of school–placebo subway distance reduction on mathematics test scores: nonlinear models

Dependent variable: individual change in standardised test score 2004 to 2006	
School–placebo distance ≤ 2 km	
4.7 km< distance reduction to placebo	0.179
	(3.742)
2.3 km< distance reduction to placebo ≤ 4.7 km	-0.700
	(3.615)
1.6 km< distance reduction to placebo ≤ 2.3 km	2.099
	(3.325)
0 km< distance reduction to placebo ≤ 1.6 km	2.222
	(4.668)
0-km distance reduction to placebo	-4.572
	(5.550)
School–placebo distance > 2 km	
0 km< distance reduction to placebo ≤ 1.6 km	-0.435
	(2.363)
1.6 km< distance reduction to placebo ≤ 2.3 km	-1.182
	(4.199)
2.3 km< distance reduction to placebo ≤ 4.7 km	-0.316
	(4.424)
4.7 km< distance reduction to placebo ≤ 10.7 km	2.921
	(3.330)
0-km distance reduction (reference category)	0
	(0)
Observations	61,993
R-squared	0.033

Notes and covariates: As for Table 3. In this table, 'placebo' stands for 'placebo subway network'. I exclude treated students from this sample. Treated students are those who experienced a distance reduction from the subway network larger than 4.7 km and ended up nearer than two km from the subway network.

4.4.2 Robustness to spatial correlation between the regression errors: permutation test

In this section, I also analyse the robustness of the results to different assumptions about spatial correlation between the students' test scores. In my preferred specification (Table 5.3, 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 tests derive from Fisher's (1935) exact test and have been further developed by researchers like Welch (1990) and 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 5.3). 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 5.3, column (4).

Only 1.3 per cent of the estimated coefficients are larger in absolute value than the ones in Table 5.3, column (4). This robust p-value is to be compared to the p-values implicit in column (4) in Table 5.3 obtained under parametric assumptions (1.5 per cent). Hence, regardless of the regression errors' spatial correlation, there is an extremely small probability of obtaining the results in my preferred specification (Table 5.3, column (4)) if the null hypothesis that there is no impact of better school accessibility on student test scores is true.

4.4.3 Robustness to a spurious correlation: use of language test scores

Despite the high statistical significance in the effect in column (4) of Table 6, it could be argued that the effect is only attributable to the natural variability in the data. If this were the case, we should not observe an effect of closer proximity between schools and the subway network when using another outcome variable. The specifications that generated Table 6 are identical to the ones that generated my preferred results (Table 4, where I apply an instrumental variables approach) but use language test scores as dependent variable. In Table 6, we also observe a significant effect of closer proximity between a school and the subway network. For each kilometre of closer proximity to the subway network there is a decrease in test scores of 1.7 percentage points of a standard deviation (coefficient of –1.741). This is evidence that the results in Table 4 are not due to a spurious correlation between closer proximity to the subway network and changes in test scores.

Table 6. The effect of school-subway distance reduction on language test scores: instrumental variables

	(1) Basic model	(2) As (1) plus school covariates	(3) As (2), plus heterogeneity in school-subway distance	(4) As (3), plus spatial controls
Panel A: Two-Stage Least Squares. Dependent variable: individual change in standardised test score 2004 to 2006				
Distance reduction (km)	-0.753	-0.797*		
	(0.529)	(0.421)		
Distance reduction (km) | distance ≤ 2 km			-1.123	-1.741***
			(0.689)	(0.565)
Distance reduction (km) | distance > 2 km			-0.543	0.0509
			(0.518)	(0.674)
Panel B: First Stage. Dependent variable: distance reduction to subway line				
Distance reduction to straight line B (km)	119.2***	119.0***		
	(7.594)	(7.182)		
Distance reduction to straight line B (km) | distance ≤ 2 km			91.49***	91.88***
			(7.369)	(13.21)
Distance reduction (km) straight line B | distance > 2 km			122.9***	85.96***
			(8.304)	(14.74)
Kleibergen-Paap Wald F-statistic	246.78	272.115	45.504	12.8
F-statistic	1.98	4800.54	72299	59552.35
Baseline characteristics				
Number of students in same school and grade in (log)	No	Yes	Yes	Yes
Language, natural and social science quintile	No	Yes	Yes	Yes
Household income	No	Yes	Yes	No
School type of administration	No	No	No	Yes
Proximity to the old subway network	No	No	No	Yes
Observations	62,247	61,392	61,392	61,392
R-squared	0.000	0.192	0.192	0.196

Notes: See notes in Table 4. *** p<0.01, ** p<0.05, * p<0.1.

4.5. Why does school–subway network distance matter?

The school–subway network distance reduction could affect student test scores through at least five mechanisms. First, 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 7 shows the effect of school–subway network distance reduction on the number of students per grade in each school. The dependent variable in Table 7 is the number of students in tenth grade in each school in the post-treatment period (2006) minus the number of students in tenth 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 subway stations in 2005 when students in tenth grade took the SIMCE test. As in all previous analyses, my preferred specification is depicted in column (4).

Table 7. 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: As for Table 3. Regressions are run at the school level. *** p<0.01, ** p<0.05, *p<0.1.

Controlling for all relevant covariates, schools that experienced a large reduction in their distance from the subway network had an average increase of nine more students in tenth 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 families choosing to change schools on achievement. Changing schools implies adaptation costs and, potentially, higher commuting times if the changes are to schools farther from the students' homes. Hanushek, Kain, and Rivkin (2004) conclude that the effect of families choosing to change schools on achievement is modest and negative (around 1 per cent 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 A1 in the Appendix depicts the results of a regression of students' 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 schools.

A third mechanism through which closer school–subway network proximity could have affected academic achievement is through changes in the parents' labour market outcomes. For example, if parents' hours of work increased, this could have affected students' test scores negatively. Appendix 2 shows the effect of municipality–subway distance reduction on employment status. The impact of school–subway network distance on employment status and hours of work (particularly women's) is positive but non-statistically significant (see column 2 in Tables A2.2 and A2.3). The policy or economic significance of these estimates is considerable (7 percentage points in employment status and 23 hours of work per week). On the other hand, on average, using a two-kilometre distance threshold implies that the effect of closer school–subway network proximity on labour earnings is negative, but not statistically significant.

Hence, changes in the labour market are potentially relevant mechanisms through which closer proximity to the subway network implied lower test scores. The fact that the size of the previously discussed coefficients is not statistically significant could be due to low power of my estimates due to a high spatial aggregation of the data (see Chapter 4 in Asahi (2015)). Future work with individual addresses could improve the precision of these estimates.

A fourth mechanism through which closer proximity to the subway network could have affected test scores is through changes in peers experienced by treated individuals. Table 8 shows the change in the 2004 test score of each student's classmates between 2004 and 2006. This change in test scores is a proxy for whether the ability of each student's peers improved between 2004 and 2006[6]. Column (3) in Table 7 shows that this improvement is not statistically significant. Hence, there is no evidence supporting the hypothesis that peer effects were a relevant channel for the effect of the treatment. However, the caveat of this finding is that the fact of whether a student moved or stayed at their school during the 2004–2006 period could have been affected by the treatment status. The reason is that closer proximity to the subway network could have induced some students to move to another school, while inducing other students to remain at their primary school (due to fewer slots in some schools).[7]

Table 8. Descriptive statistics exploring peer effects channel

Dependent variable: change in the 2004 test score of students' classmates between 2004 and 2006	(1) Control students [s.d]	(2) Treated students [s.d.]	(3) Diff. (2)–(1) (s.e.)
Stayers	4.30	1.03	-3.27
	[13.03]	[18.63]	(3.17)
Movers	12.54	13.00	0.46
	[50.08]	[49.25]	(4.5)

Notes: The table reports regression coefficients and standard errors multiplied by 100 to give the % association in terms of standard deviations of Chile's SIMCE test. Sample constrained to the one in the main specification (Table 3, column 4). *** p<0.01, ** p<0.05, * p<0.1

A fifth mechanism through which closer proximity to the subway network could have affected academic achievement is through the effect of better or worse teachers. Unfortunately, to my knowledge, during the studied period there are no datasets with measures for teacher quality. An (imperfect) proxy for teacher quality could be the value-added provided by schools. Because I want to avoid value added measures being contaminated by a potential effect of closer proximity to the subway network, I use the 'contextual average student performance' estimated with 2004 data in Asahi 2015's section 2.3.2 as a proxy for each school's value added. A problem of this measure is that it is only feasible to calculate for schools with eighth grade. Out of the 655 schools with students in the sample of my preferred specification (the one that generated Table 4, column

[6] The reason why I use pre-intervention (2004) test scores and not post-intervention ones is that the latter ones could be affected by the effect of closer proximity to the subway network.

[7] This possibility is compatible with the fact that, on average, the treatment did not increase the probability that a treated student would move to another school during the 2004–2006 period.

(4)), it was not feasible to calculate the 'contextual average student performance' for a 22 per cent of these schools (145 schools).

Table 9 shows the change in the 2004 'contextual average student performance' for students who moved to another school between eighth and tenth grade. Column (3) shows that students in the control group experienced a greater increase in the proxy for their school's value added (coefficient of 12.35 percentage points of one standard deviation) relative to students in the treated group (coefficient of 4.57). However, this difference is not statistically significant at conventional levels. Hence, there is no conclusive evidence supporting the hypothesis that the effect of closer proximity to the subway network was due to treated students moving to worse schools relative to control students. As in the previous paragraph, a caveat for this conclusion is that whether a student moved to another school between 2004 and 2006 could have been affected by closer proximity between their school and the subway network.

Table 9. Descriptive statistics exploring teacher effects channel

Dependent variable: change in the 2004 "contextual average student performance" (proxy for value added) of the student's school between 2004 and 2006	(1) Control students [s.d]	(2) Treated students [s.d.]	(3) Diff. (2)–(1) (s.e.)
Movers	12.35	4.57	-7.78
	[51.13]	[53.57]	(5.1)

Notes: As for Table 8 . *** p<0.01, ** p<0.05, * p<0.1.

5. Summary and Conclusions

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

This paper carefully addresses the identification of the impacts of better school accessibility on academic achievement. First, I use a detailed individual administrative test score dataset with information before and after the transport innovation, and thus avoid 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 account for a potential endogeneity between the location of the new subway stations and academic achievement by exploiting the distance reduction to the least cost path route between the central business district and the subway line's destination; this type of distance reduction is a plausibly exogenous variation in distance to the subway network. Fourth, I carry out robustness checks to unobserved differential shocks to treated and control student test scores and spatial correlation between the regression errors.

My main finding is that there is a large negative effect of school–subway distance reduction on test scores. Students in schools that experience a large decrease (of more than 4.7 km) in the distance from the nearest subway station, and ended up at a walking distance from the subway network, had average test scores that were lower by some 11 per cent of a standard deviation compared to test scores of students in schools that did not experience a distance reduction to the subway network.

The magnitude of this finding is large. 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 per cent confidence intervals of the average effect of educational programs was less than 9 per cent of a standard deviation.

I also find evidence that the negative effect of distance reduction on test scores is due to an increase in the number of students in schools that were now significantly closer 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.

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Appendix 1

Table A1
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: As for Table 3. Individual-level probit regressions. Dependent variable: whether students who took the 2004 test in eighth grade were in the same school in 2006 in tenth grade. Sample restricted to students whose schools had both primary and secondary levels. *** p<0.01, ** p<0.05, * p<0.1.

Appendix 2: The effect of municipality–subway distance reduction on employment status

Table A2.1
The effect of municipality–subway distance reduction on employment status using a distance threshold of two kilometres: linear models

Dependent variable. Columns (1) through (3): employment status in 2001; columns (4) through (6): 2006–2001 employment status	(1)	(2)	(3)	(4)	(5)	(6)
	Cross-section association			Individual fixed effects		
	Basic model	As (1) plus predetermined covariates	As (2), plus heterogeneity in school-subway distance	Basic model	As (4) plus predetermined covariates	As (4), plus heterogeneity in school-subway distance
Proximity to the nearest subway station (km)	0.0884 (0.116)	-0.167 (0.116)		0.572 (0.492)	0.299 (0.694)	
Proximity to the nearest subway station (km) | distance ≤ 2 km			0.139 (0.129)			0.232 (0.790)
Proximity to the nearest subway station (km) | distance > 2 km			-0.146 (0.113)			0.467 (1.245)
Control variables (2001)	No	Yes	Yes	No	Yes	Yes
Observations	2,511	2,464	2,464	2,500	2,453	2,453
R-squared	0.000	0.966	0.966	0.000	0.362	0.362

Notes: As for Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.2
The effect of municipality–subway distance reduction on employment status using a distance threshold of two kilometres: nonlinear models

Dependent variable: change in employment status 2001 to 2006	(1) All individuals	(2) Women	(3) Men
Post-treatment municipality–subway distance ≤ 2 km			
2 km< distance reduction	2.722	7.173	-2.204
	(4.138)	(5.761)	(4.764)
0 km< distance reduction ≤ 2 km	-1.301	-0.290	-1.569
	(4.275)	(6.145)	(5.010)
0-km distance reduction	4.430	5.416	-3.164
	(3.929)	(5.327)	(4.647)
Post-treatment municipality–subway distance > 2 km			
2 km< distance reduction	1.039	3.168	-1.087
	(3.056)	(5.234)	(3.601)
0 km< distance reduction ≤ 2 km	-5.269*	-6.271	-5.324
	(2.877)	(4.921)	(3.673)
0-km distance reduction (reference category)	0	0	0
	(0)	(0)	(0)
Observations	2,453	1,361	1,092
R-squared	0.366	0.341	0.502

Notes: As for Table 2. *** p<0.01, ** p<0.05, * p<0.1

Table A2.3
The effect of municipality–subway distance reduction on hours of work using a distance threshold of two kilometres: nonlinear models

Dependent variable: change in monthly hours of work 2001 to 2006	(1) All individuals	(2) Women	(3) Men	(4) As in (1) restricting sample to employed in both periods
Post-treatment municipality–subway distance ≤ 2 km				
2 km< distance reduction	8.793	23.73	-1.289	10.96
	(10.37)	(14.07)	(11.61)	(11.62)
0 km< distance reduction ≤ 2 km	10.92	24.54*	-4.643	18.92
	(8.357)	(13.70)	(7.277)	(13.78)
0-km distance reduction	22.08	27.78	-7.728	31.89
	(15.05)	(20.00)	(10.94)	(22.14)
Post-treatment municipality–subway distance > 2 km				
1 km< distance reduction	6.284	11.88	1.037	-4.971
	(7.153)	(10.41)	(10.28)	(8.750)
0 km< distance reduction ≤ 2 km	-6.702	-2.736	-11.63	9.406
	(5.478)	(9.276)	(6.991)	(9.391)
0-km distance reduction (reference category)	0	0	0	0
	(0)	(0)	(0)	(0)
Observations	2,078	1,210	868	744
R-squared	0.273	0.290	0.391	0.206

Notes: As for Table 2. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.4

The effect of municipality–subway distance reduction on individual income from work using a distance threshold of two kilometres: nonlinear models

Dependent variable: change in monthly individual labour earnings 2001 to 2006 (in 2001 US$)	(1) All individuals	(2) Women	(3) Men
Post-treatment municipality–subway distance ≤ 2 km			
2 km< distance reduction	-33.32	47.05	-105.8*
	(36.14)	(39.05)	(58.12)
0 km< distance reduction ≤ 2 km	15.21	11.01	-1.094
	(36.93)	(41.53)	(60.88)
0-km distance reduction	32.77	8.794	40.67
	(52.60)	(34.91)	(93.22)
Post-treatment municipality–subway distance > 2 km			
2 km< distance reduction	-4.268	-66.18	91.78*
	(25.66)	(32.03)	(33.04)
0 km< distance reduction ≤ 2 km	-7.275	-42.81	32.47
	(25.66)	(32.03)	(33.04)
0-km distance reduction (reference category)	0	0	0
	(0)	(0)	(0)
Observations	2,464	1,366	1,098
R-squared	0.136	0.270	0.187

Notes: As for Table 2. *** p<0.01, ** p<0.05, * p<0.1.