Can immigration affect neighborhood effects? Lessons from Chile

How does immigration changes the opportunities of native children? I study this question focusing on 4th-grade test scores in the context of the recent migratory phenomenon in Chile, where from 2012 to 2019 immigrant population increased from near 1% to 9%. Following Chetty and Hendren's (2018) methodology, I estimate each municipality' effect on test scores using a fixed effect regression model identified by students who move across municipalities at different ages. Then I construct a shift-share instrument taking shares from the 2002 census and estimate the impact of immigrant arrivals on municipality effects. On average, I find a negative impact of foreign students on municipality effects. My estimation suggests that an increase of 1 standard deviation of immigrant students lowered students' test scores by 1.2 percentiles rank per each year spent. This effect is not directly caused by exposure to immigrant students. I show -using within school variation- that the negative impact on test scores is not explained by peer effect within class or school grade. My findings instead are consistent with an increase in socio-economic segregation across schools induced by native flights.

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1 Introduction

There is little doubt that childhood neighborhoods can affect long-term outcomes (Damm and Dustmann 2014, Chetty et al. 2016, Altonji and Mansfield 2018, Chyn 2018). We know relatively little, however, about the factors that change the quality of a neighborhood. This paper analyzes whether foreign immigration can change the movement opportunities of native children. Migratory waves can change the destination neighborhoods. The arrival of immigrants can cause natives to move to places with fewer immigrants[1], increase preferences for private schools (Betts and Fairlie 2003,Farré et al. 2015, Murray 2016), decrease preferences for redistribution (Alesina et al. 2018,Alesina et al. 2019), and reduce trust in neighbours (Putnam 2007,Alesina and Ferrara 2000)[2]. However, we cannot observe whether the neighborhood is doing better or worse for long-term outcomes because there is a selection bias induced by natives moving to avoid interaction with immigrants. The recent work of Chetty and Hendren 2018a and Chetty and Hendren 2018b developed an empirical strategy to estimate the causal effect of a neighborhood exempt from selection bias. Using this technique, I analyze whether neighborhood influence during school-age children improves or deteriorates when immigrants arrive?

The moving to opportunity experiment (henceforth MTO) found[3] that the neighborhood's effect on labor outcomes is only decisive in children and not in adults. Therefore, if we wanted to observe the neighborhood's causal effect on social mobility, we would have to wait for a child to live in a neighborhood and then look at their income at the age of 30, conditional on the parents' income. Instead, I take advantage of the national universe of test scores of 10-year-old children to analyze the impact of neighborhoods on learning during childhood, considered to be an important predictor for success later in life. My research question is how the municipality effects on test scores change once immigrants arrive. I analyze this question in Chile because it experienced a significant immigration wave - from 1% of the population in 2012 to almost 9% in 2019 - and the educational data allows to implement the methodology.

This paper evaluates the municipality causal effect on children's test scores before and after immigrants arrive. I estimate the municipality causal effect on children's test scores rank in 4th grade (10 years old) conditional on the mother education rank in two windows: before and after the large wave of immigrants. Following Chetty and Hendren (2018b) methodology, I estimate each municipality' effect using a fixed effect regression model identified by students who move across municipalities at a different age. Then I construct a shift-share instrument taking shares from the 2002 census and estimate the impact of immigrant arrivals on municipality effects. On average, I find a negative effect of foreign students on these municipality effects. The results show that a one percentile increase of immigrants lowered the municipality effect by 0.04 percentiles rank. I then show -using between cohort variation- that the negative effect on test scores is not explained by peer effect at the class, school level. Instead, a final part of the analysis shows that migrant inflows led to native flights out of the municipalities and from public to private schools. I show this resulted in socio-economic segregation, offering a plausible mechanism for the negative municipality effects.

I draw on different databases to carry out my analyses in this paper. I use the administrative data spanning from 2004 to 2020 of the Ministry of Education to know which municipality the children attended in primary school - from 1st to 8th grade- and the number of foreign students in each municipality per year. I combine this dataset with test data of children in 4th grade and their mother level of education. I complement this dataset with information at the municipality level from census and the government platform of municipal information (henceforth SINIM, Spanish acronym of the platform). With this data, I have information of around 3.5 million students test scores. Among them, 240 thousand students changed neighborhoods [4] in the years before the 7th grade between 2005 and 2020. This is the

[1] The literature documented this phenomenon when studying labor market (Borjas 2006), school choice (Cascio and Lewis 2012), or urban segregation (Crowder et al. 2011, Fernández-Huertas Moraga et al. 2017, Card et al. 2008).

[2] These papers refer to less trust in racially mixed communities. Recent papers, like Pettigrew et al. 2010, show that the negative effect is attenuated when the natives maintain contact with the immigrant population.

[3] Initially studies found null effects but Chetty et al. 2016 revisit the experiment using latter outcomes data, including earnings, college attendance, and single parenthood. There they found that kids over 13 were not affected, and under 13 were positively affected to the low-poverty neighborhood.

[4] Change of neighborhood from urban to urban school, and excluding movements to adjacent municipalities in cities

sample I hence will use for the neighborhood effect estimation.

The empirical strategy to estimate the impact of immigrants on municipality effects consists of three steps: first, I estimate municipality effects; then, I construct the shift-share instrument; and finally, I assess the impact of foreign students on municipality effects.

The municipality effects' statistical model defines students' outcomes as the sum of the municipality effect per year of exposure and family characteristics. This model assumes that 1) the impact of one year of exposure does not vary across children[5], 2) it is constant regardless of student age, and 3) disruptive costs do not vary with the children age. The municipality effect identification strategy consists of comparing students who move the same year from the same origin to the same destination but at different ages. It assumes that students' potential outcome is orthogonal to the age at which the students move.

I provide evidence in support of the statistical model assumptions introducing the exposure effects (Chetty and Hendren (2018a)). This method evaluates the impact of moving to a neighborhood where permanent residents are one percentile point higher in test scores. When estimating the exposure effect, I observe that each additional year in the municipality allows movers to converge to permanent residents by 9%. These results are comparable to those found in Chetty and Hendren (2018a) and show that childhood exposure effects are constant throughout childhood. Then, I provide evidence in support of the identification assumption taking advantage of a 2nd-grade test score and showing that children moving at different grades do not differ in this baseline test score.

I estimate municipality effects taking all the years in my sample (from 2004 to 2020). I show these estimated municipality effects correlate (as expected) positively to municipality test score value-added, and negatively with segregation.

Then, I construct a shift-share instrument that estimates immigrant inflows based on settlement patterns in 2002. This strategy assumes that arriving immigrants will seek to locate themselves in the same municipalities where immigrants were located a decade ago because they share the same network. I will group the students according to nationality and parents' level of education and assume that the networks are stronger since they share the same nationality and have a similar educational level. This predicted migration inflow strongly correlates with actual migration but is more plausibly orthogonal to any omitted variable that may drive migration inflow.

The validity of the instrument relies on one identifying assumption: municipalities that had received more immigrants in 2002 should not have been on differential trends that may affect the evolution of municipality effects once immigrants arrive. The main threat to identification is that those municipalities experienced a differential trend. I address this concern by performing a placebo test observing if municipality effects are differential comparing 2004-2008 with the 2009-2013 window. The null result confirms that it is unlikely that my instrument is related to differential trends.

The increase in immigrants was rapid, allowing to take a first-difference of municipality effects -before and after immigrant arrivals- and test if it varies with the percentiles of immigrants' arrival proportion. This strategy has the advantage of being simple and sufficient to capture the possible effect. Because immigrants arrivals started to occur from 2013 onwards, I will show results comparing the pre and post-2013 windows. Results show a negative impact on the municipality effect of around -0.04 percentiles per one percentile change of immigrants per year exposed or -.16 percentiles if they move in 1st grade (four years of exposure). Thus, a 30 percentile greater increase in immigrants - approximately one standard deviation of the shock- will lower the children's percentile score by 4.8 percentiles if they moved from 1st grade.

The second part of the paper explores the channels through which migrant inflows result in the negative municipality effect on test scores. First, I estimate immigrant peer effects using cohort and class comparison following Hoxby 2000. For the former comparison, I take advantage of the tests in the 2nd, 4th, and 6th grades. This allows to circumvent a common problem with cohort comparison when students are evaluated in different years, which can leads to omitted variable and selection bias. Similarly, the comparison of the student in the same cohort, but different classes poses the problem

compound of more than one municipality.

[5]This constant treatment effect assumption is a common simplification in works on firm effects and teacher effects.

of endogenous class formation, which will bias the results. I address this issue by providing a test of random class formation developed by Ammermueller and Pischke 2009 and discard schools that fail the test. Results show that students sharing school cohorts or classes with additional immigrants do not perform worse on the test score. In fact, students sharing the school cohort with an additional immigrant are slightly better off.

Finally, I explore whether native flight -the attempt of natives to decrease interaction with immigrants- can explain the negative neighborhood effect. First, I show a pre-trend in outcomes, meaning there is no differential trend related to immigrants' arrival. Then I test for effects during immigrants arrivals (2013-2019). I observe natives move to other municipalities to avoid immigrant interaction. The relation is approximately one to one. Also, I observe a strong effect of natives choosing private schools when exposed to a higher proportion of immigrants in the municipality. Each immigrant that arrived in a municipality induced 0.5 natives to go to private schools. Unsurprisingly, school segregation seems to move hand in hand to natives choosing private schools. I provide Theil and Atkinson segregation index for robustness check of composition bias and find similar results. This segregation provides a plausible explanation for the negative neighborhood effects resulting from the inflow of migrants: When the share of immigrants increase, tests scores of native children are affected, not by direct peer effects from the migrants on the native children, but rather as a result of native flight by the better of natives, leading to segregation and worse outcomes for the (often poorer) natives that stayed behind.

This paper builds on the idea that exposure to a neighborhood during childhood matters for educational and labour outcomes. Using different research designs, several studies have shown evidence in this line: random assignment of refugees (Damm and Dustmann 2014), housing demolition (Chyn 2018), selection correction using group characteristics (Altonji and Mansfield 2018), random assignment of housing vouchers in MTO (Chetty et al. 2016), and selection correction exploiting age at movement (Chetty and Hendren 2018a). Both the MTO experiment and Chetty and Hendren 2018a show that these effects are not perceived when adults are exposed to better neighborhoods. Also, recent literature estimates the characteristic of the neighborhood quality - defined as better social mobility- and found that good neighborhoods are less segregated, more egalitarian, and have more social capital (Chetty and Hendren 2018b, Rothwell and Massey 2015, Güell et al. 2018). While these papers shed light on what characteristics the best neighborhoods have, it remains to be understood what factors can change a neighborhood. My paper contributes by showing how immigrants in Chile can change the neighborhood effect on test scores. As such, it closely relates to Derenoncourt 2018. Because she examines whether the Great Migration in the USA had negative effects on the neighborhood effects on social mobility[6].

My paper contributes to the causal neighborhood effects literature by building on Chetty and Hendren 2018a and Chetty and Hendren 2018b methodology to study outcomes earlier in life. This methodology has promise as it can be replicated with other test score data available from cross-section administrative data, as long as you can observe children movements. I further contribute to this literature by showing that neighborhood effects may not be stable when immigrants arrive. In other words, when we encourage people to migrate for better opportunities, migration waves can make the neighborhood effect drop, decreasing the expected effect of the movement.

The second part of my paper that evaluates plausible mechanisms builds on the evidence of immigration effects on educational outcomes and neighborhood characteristics. Immigration can cause changes in neighborhood composition due to native flight. Natives can avoid interaction by moving away from neighborhoods with a high proportion of immigrants. This is well documented in different countries and context (Card et al. 2008, Fernández-Huertas Moraga et al. 2017, Crowder et al. 2011, Cascio and Lewis 2012, among others). Similarly, natives can avoid interaction by changing to schools with less immigrants, generally private schools (Betts and Fairlie 2003, Rangvid 2010, Gerdes 2013, Farre et al. 2018). This native flights can increase both school and spatial segregation, the former related to native children changing schools, and the latter related to changing residences.

The existing literature on immigration impact on education identified, most of the time, the effect of marginal exposure in school using classes or cohorts (Hoxby 2000, Ammermueller and Pischke 2009, Frattini and Meschi 2019). In general, the results of having a higher proportion of immigrants in your

[6] Using pre-settlement data as a pull factor and technological changes, among others, as a push factor, she built a shift-share instrument and finds that the Great Migration drops social mobility in receiving neighborhoods. Native flights increased policing, and incarceration rates are the most plausible explanation of this effect.

class or school cohort are mixed. Though, in general, studies find a negative impact when immigrants speak another language, or there is an important gap between the family background of natives and immigrants (Frattini and Meschi 2019, Gould et al. 2009). My paper contributes to this literature by providing an alternative to show general effects that include direct effects -like immigrants' peer effects- and indirect effects -changes in composition, segregation, resources in the school, social capital, among others-.

In the following section, I describe the context in which this wave of immigration occurs; then, in section 3, I describe the data and variables to use for my analysis. In section 4, I explain the empirical approach in detail; in section 5, I show the results, and in section 6, I test different mechanisms. Finally, in section 7, I conclude.

2 Context

2.1 Migratory wave in Chile

Since the last decade, Chile has seen an increase in the number of foreigners coming from Latin America (see figures 1 and 2). This recent increase has two triggers, the political and economic crises in the region and the restrictions to migrate to the northern developed countries (ECLAC 2019). Political and economic crises cause the emigration of their inhabitants and prevent others from emigrating to the countries in crisis. This is why it is not surprising that the wave of immigration to Chile is led by countries in crisis (Venezuela, Haiti) and countries that traditionally migrated to Chile (Colombia, Peru, and Bolivia). The immigration wave in Chile differs in its characteristics from immigration in developed countries but maintains similarities with migration in the region. Most immigrants speak the same language -Spanish- and on average, those who arrive have similar years of education than the natives (ECLAC 2019 and INE 2018).

I will analyze the dynamics of immigrants with the census and school enrollment databases. With the census data, I will define an immigrant as an individual who, at the time of birth, his/her mother resided abroad. While with the data of enrolment, I will define an immigrant as an individual that does not have the Chilean nationality[7]. Censuses since 1952 show that the foreign population increased very little until 2012 and then grew significantly by 2017, led by Haiti, Venezuela, and Rest of Latin American countries (see figure 1). In 2019 immigrants were concentrated in the northern regions (henceforth Norte Grande)[8] and in the center (henceforth Metropolitan region). Taking the population over 25 years old, immigrants were more educated than Chileans (12.6 versus 11 years if education), though these differences may be driven by different age distribution. Most immigrants are between 20 and 45 years old, so children and the elderly are relatively underrepresented. The school enrolment of foreign students shows a similar dynamic. Figure 2 shows the enrollment of immigrants in primary school and their fraction as a percentage of the total enrolment population. This figure shows that foreign enrolment started to increase from 2013 and in 7 years jumps from 1% to 5%.

2.2 Foreign arrival and firsts reactions

Attitude surveys on immigrants comparable across years are scarce in Chile. However, a recent study using an attitudes survey from 2002 and 2017 finds that the arrival of immigrants to a municipality is related to less favorable attitudes towards immigration (González et al. (2019)[9]). Additionally, while I have no data to compare with other countries, the recent 2018 bicentennial survey[10] can be informative: 75% of Chileans believe that immigration is excessive and 44% believe that there is a greater conflict with migrants (up from 38% the previous year). These levels are higher in the north of Chile - where

[7]In Chile, naturalization is based on jus solis and jus sanguinis

[8]First three regions from north to south.

[9]This relationship is found only in those pessimists of the economy.

[10]It is a project of the Pontificia Universidad Católica de Chile, whose main purpose is to obtain highly reliable and sustained information over time about the state of Chilean society in relevant and high impact topics.

more immigrants have arrived as a percentage of the population -.

Regarding native reaction as internal immigration, it seems that natives are moving out from places where immigrants arrive. Census 2017 shows that those municipalities that receive more immigrants are those where more Chileans are emigrating within Chile. In figure 3, I rank the municipalities according to immigrant arrivals, as a percentage of the Chilean population in 2017. There is a high correlation (around 70%) between foreigner arrivals and Chilean departures. This relation is not necessarily native flight - the native decision to reduce interaction with immigrants- but shows that natives composition in the municipality will change when immigrants arrive. I will discuss and provide evidence of native flights in the school system in subsection 6.2.

Education in Chile is based on a voucher system where coexist private schools, private subsidized schools - with public funding and private administration - and public schools[11]. School is compulsory for natives and foreigners, even if the lately are undocumented. The Education Ministry facilitates enrolment by providing students without ID a provisional identification. This means that many immigrants, whether documented or not, have their first interaction with the state of Chile in schools. However, foreign students do not enjoy the same rights as Chileans. Until 2016 undocumented foreign students did not receive additional benefits to the school voucher, such as free lunch or preferential voucher (an increase of 50% of the school voucher), among others (Mora Olate (2018)).

In short, we can say that most natives perceived immigration as excessive, the municipalities that received foreigners have changed their composition of neighbors -more foreigners and fewer Chileans- and that undocumented immigrants in schools receive fewer benefits vis-à-vis Chileans.

3 Data

My analysis is done with education administrative data from 2004 to 2020 and test data spanning from 2005 to 2018. I also complement this data with census and municipality-level data. The administrative data on education describe school enrolment, school municipality, and foreign status. This administrative data includes a student identifier that allows them to follow them across time and match the testing service's data. I complement this data with the testing service (Agencia de la Calidad de la Educacion) that administered national assessment of 4th-grade students every year from 2005 to 2018[12]. The assessment consists of test scores in reading, math, history or natural science, and questionnaires for students, parents, and teachers.

3.1 Variables of interest

With these databases, I will define the following variable of interest:

Cognitive score: Henceforth cog score. Variable constructed based on 4th grade students test score in math and reading[13], I do not include other subjects because they are not consistent over the years. These tests are calibrated and score with IRT and are comparable across years. This variable span from 2005 to 2018.

Mother years of education: Variable self-reported from parents questionnaire that report the number of years studied in formal education goes from 0 to 20, equivalent to no studies and Ph.D., respectively. This variable span from 2005 to 2018.

Cog score and mother education are transformed into percentile ranks because the relation between cog score and mother years of education is approximately linear if transformed to rank (see figure 5). This relation will facilitate the estimation of municipality effect (see section 4.1).

[11]For details of the education system in Chile, please see Hsieh and Urquiola (2006).

[12]For some years, they also assess other grades, but those are not yearly based.

[13]Though achievement tests are one part of the cognitive ability - named crystallized intelligence (Roberts et al. (2000)) - for simplification, I will call it cognitive.

School Municipality: Administrative variable that identifies in which municipality the student enrolled in each year. The municipalities are administrative, geographic areas with a local government whose head is the mayor. They are in charge of public schools, among other services to the community. There are 345 municipalities in Chile, with an average population of 50 thousand inhabitants, of which around 700 are 4th-grade students. Municipalities are then equivalent to approximately half of a county in the US. Most of the primary students attend the same school municipality where they live. On average, 90%[14] of them does the distribution is uneven. The extreme case is the Metropolitan region, where the capital Santiago is. Around 75% of the population attend the same school municipality where they live. Results are robust to discard the Metropolitan region from the analysis. **Foreign students change:** Information on foreign students by municipality comes from administrative enrolment data. Given the dynamic presented in the previous section, it seems natural to observe the municipality effect before and after 2013. Then to consider the exposition to immigrant students, I will use the change between 2013 to 2019 in immigrant students as a fraction of the total population:

$$\Delta F_c^{2019-2013} = \frac{F_c^{2019} - F_c^{2013}}{population_c^{2007}} \tag{1}$$

Where F_c^{2019} is the number of foreign students in primary schools in municipality c in year 2019, F_c^{2013} is the number of foreign students in primary schools in municipality c in year 2013, and $population_c^{2007}$ is the number of primary students in year 2007[15] in municipality c.

Foreign students nationality: The school principal started to report this variable from 2014. This information has not external validation. The student nationality is of interest because it will allow me a more robust instrument (Shift-share instrument) and heterogeneous analysis. Crossing the Chilean variable - validated by the civil registry - and country of origin reported by the headteacher, I can get an idea of the misreporting[16]. Figure 4 shows that the misreporting is quite generalized: of the total number of foreigners, approximately 50% report being Chilean every year. Directors may misreport because of ignorance of its student nationality or as a way to receive more benefits -given that foreigners receive less benefits-. Misreport can be a problem if it is related to the endogenous shocks that pull families into municipalities. Though I can not provide evidence in this line, I can address this concern by showing that the measurement error is very low. I do this comparing in each municipality the proportion of kids nationalities from admin data with the census in 2017[17]. Correlation is 95%, which confirms that this extrapolation has a small measurement error. The next step of this paper is to obtain the database with names and surnames from the Education Ministry and do Machine Learning with this information[18]

Because the distribution of immigrant population change is highly right-skewed, I define the percentile change as the key independent variable in the empirical analysis. Figure 6 depicts the quantile distribution of immigrant share change. The median change across municipalities was 2%, and the mean was 3.5%. As the figure shows, the distribution of immigrants was not random; they concentrated in specific municipalities.

3.2 Sample definitions

In this part, I will describe the sample definition for municipality and exposure effects. The sample definition to evaluate different mechanisms will be explained in each subsection of section 6. The sample consists of all Chilean students that took the test in 4th grade between 2005 and 2018 and moved one time during their primary school life. I restricted the data to Chileans because I focus on the impact of foreign students on them. Also, I select 4th-grade students because it is the only grade where students

[14]For this number, I am using the question from parents questionnaire in 4th grade 2009 because the administrative data has inconsistencies in the municipality of residence.

[15]I take the year 2007 because for mechanisms I will provide a DiD starting from 2007, by doing this the treatment in the main regression -the impact of immigrants on neighborhood effects- and difference in difference to study native flights will be the same.

[16]I will define misreport when the director reports Chilean, and the validation of the civil registry shows that they are foreigners.

[17]I make this two database comparable defining foreigner students in the census database as kids in age to go to primary school (between 6 and 13 years old, that were born outside of Chile (jus solis), and whose head of the households are not Chileans (jus sanguinis).

[18]I performed a Machine Learning with actual information (municipality, year of arrival, grade, rurality, type of school, gender, and priority status of the student but I did not gain more information compare to using the proportion of nationalities in each municipality.

were tested every year since 2005, and kids are more likely to enroll and live in the same municipality when younger. Then, to simplify the model, I will take only those who move only once between 1st and 6th grade. Since I want to be sure that the movements effectively change the neighborhood, I will remove from the sample those who move to adjacent municipalities in cities formed by more than one municipality. In the case of the capital Santiago, I will remove any movement within the capital. Because I need to be sure that students share the same neighborhood, I will remove those that lived in rural schools. Finally, I will also remove those moving in 6th grade to "Liceos Emblematicos", prestigious high schools because acceptance in those schools may push families to change of neighborhood - an important challenge to my main assumption that I will discuss after -.

Table 1 shows in panel A the characteristics of permanent residents and in panel B of those who move with the restricted definition mentioned above. Differences of observations between administrative variables: like regions, and test variables: cog test and mother education, comes from students that did not attend the test day. Missing observation of cog test is around 10% and is constant over the years. As you can see, the sample is around 10% of the total enrolment. Also, we can see that those that move are not too different from permanent stayers. In panel C, I restrict the sample to those observations I use to estimate the immigrant impact on the municipality effect. Using the tree steps methodology described in the next section, you can lose observations because you discard path origin-destination with a few observations, you discard disconnected setting[19], and you split the sample in two. This because you may not estimate the same municipality effects in both windows (so I do not use those estimations). As a result, panel C of tables 1 show the characteristics of the sample I use for my analysis of interest. As you can see, the sample is around 7% of the total enrolment.

4 Empirical approach

This section describes the steps and the assumptions required for the identification of the impact of immigrants on municipality effects. The main regression that I would like to estimate is the following:

$$\mu_c^{2013-2019} - \mu_c^{2005-2012} = \alpha + \beta \Delta F_c^{2013-2019} + \epsilon_c \tag{2}$$

Where $\mu_c^{2013-2019}$ represents the municipality effect estimation of municipality c using movements from 2013 to 2019, $\mu_c^{2005-2012}$ represents the municipality effect estimation of municipality c using movements from 2005-2012, $\Delta F_c^{2013-2019}$ is the increase of foreign population between the year 2013 and 2019 as a proportion of the population in the year 2007 for municipality c, as it is defined in equation 1.

The main regression estimation consists of three steps: first, estimation of municipality effect, second the construction of the immigration instrument, and third estimation of the impact of foreign immigration on the changes in municipality's effects. I will describe these steps in the following lines.

4.1 Step 1: Estimation of the municipality effects

The empirical approach of neighborhood effect estimation is based on the following statistical model (from Chetty and Hendren 2018b):

$$y_i = \sum_{m'=1}^{A} [\mu_{c(i,m')} - \kappa_{c(i,m') \neq c(i,m'-1)}] + \theta_i$$

Where y_i is the outcome of kid i in A grade, $c(i, m')$ is the neighborhood a kid i lived at grade m', μ denotes the fixed effect of exposure in neighborhood c, κ denote a disruptive cost and θ_i are the characteristics of the kid's family i.

[19] Paths that are not connected to the big network of municipality paths.

This model requires three assumptions: The neighborhood effects do not vary across children conditional on mother level of education (or in the case of mean neighborhood effect, no "essential heterogeneity"[20]). The neighborhood effects are additive and constant across grades. The disruptive effect is independent of the grade of the student movement.

The assumption that neighborhood and disruptive effects are constant across grades is not trivial. I provide evidence in support of this assumption introducing exposure effects (Chetty and Hendren 2018a). In this paper, the authors noted that students exposed to a positive (negative) place for a longer period - hence the name exposure effect - will have better (worse) results than those exposed to it for less time. To define whether one place is better than another, the authors use the outcome of students who are permanent residents. Although the exposure effect with this proxy has a selection effect, this will not be a problem if the interest is to see the change in exposure effect according to the age the child moves. The authors find that children converge at a linear rate to permanent stayers at a rate of 4 percent per year. In Appendix A, I describe the steps of this methodology, estimate the exposure effect with my data, and obtain a convergence rate of 9%. This number is similar once scaled to the outcome age (in my case, ten years old in their 23 years). These results show that childhood exposure effects are constant throughout childhood, which argues that the statistical model can be a good approximation of reality.

The empirical approach of neighborhood effects exploits the variation from comparing the outcome of students who moved between the same neighborhoods but at different ages[21]. The underlying identification assumption is that a child age at the time a family moved is orthogonal to unobserved family and student characteristics. I can formalize the empirical strategy as follow:

$$y_i = \alpha_{odps} + (4 - m_i)\mu_{od} + \epsilon_i \tag{3}$$

$$\alpha_{odps} = (\phi^0_{od} + \phi^1_{od}p + \phi^2_{od}s + \phi^3_{od}sp + \phi^4_{od}s^2 + \phi^5_{od}s^2p + \phi^4_{od}s^3 + \phi^5_{od}s^3p) \tag{4}$$

Where y_i is the educational outcome of kid i, α_{odps} is an equation that interacts an origin-destiny od fixed effect with a polynomial form of the year of change s[22] interacting with mother level of education p. $4 - m_i$ identifies the time spent in the destination before the test grade, where four comes from the test grade, and m_i is the grade when student i moves. Here μ_{od} represents the causal impact of spending an additional year in d instead of o. In other words, the municipality effect in d minus the one in o. Because μ_{od} is a difference, I will need another step to disentangle each municipality effect.

Since the estimation of each fixed effect in one step is not feasible for computational reasons, I will follow a two-step estimator from Chetty and Hendren 2018b. First, I will estimate the fixed effect of each path using equation 3. In total, there will be N_c^2 fixed effects - one per each path - to estimate, where N_c is the number of municipalities. Since we want to obtain N_c neighborhood effects, we introduce the matrix G that consists of positive or negative indicators according to destination or origin. The G matrix will have N_c^2 rows and N_c columns, one for each path and one for each municipality, respectively. This matrix will take $+1$ when the destination municipality is assigned, -1 when the origin municipality is assigned, and 0 otherwise. In this way, the matrix G will be as follows:

$$G = \begin{bmatrix} +1 & 0 & -1 \\ -1 & +1 & 0 \\ -1 & +1 & 0 \\ +1 & -1 & 0 \end{bmatrix}$$

Then I will disentangle each neighborhood effect with the following OLS:

$$\mu_{od} = G\mu_c + \eta_{od}$$

Where μ_{od} comes from equation 3 and μ_c represents the neighborhood effect at municipality level. For this regression, I will weight by the precision of each μ_{od}, which is the inverse of the standard deviation.

[20] Movements orthogonal to the heterogeneity

[21] It is important to distinguish, the movement used for this estimator comes from the internal migration of natives, not foreigners.

[22] In Chetty and Hendren 2018b they use students cohort instead of the year of change and use a quadratic form of the student cohort. Because in my context, I am worried about the implications of native flights, I will use the year of change instead of cohort and use a cubic form to allow different selection patterns in different years.

As mentioned above, equation 3 relies on the assumption that the grade at which each student moves is independent of the characteristics of the family and the child. I will provide evidence for this assumption by taking advantage of introducing a reading test in 2nd grade between 2012 and 2015. I will test that the grade at which each student moves is independent of the 2nd-grade test score. I do this by implementing equation 3 and replacing $(A - m_i)\mu_{od}$ by a dummy of the grade at move. In this way, I can test if students with the same family characteristics that move from and to the same municipality differ in 2nd-grade read test scores based on grade at the move. Table 2 show the results of the test. None of the coefficients are significant, meaning that those moving at different grades after 2nd grade do not differ in 2nd-grade test scores rank.

Concluding this subsection, I calculate the municipality effect taking all the years of my sample. With these estimates, I will run simple linear regressions and multivariate analysis with municipality characteristics like average poverty rate, segregation, and income. I will run this analysis considering only the municipalities I can estimate for the main regression. Tables 3 shows the results of these regressions. Multivariate regression shows that municipality effects are negatively related to school segregation and public education budget and positively related to municipality test score value-added measurement and percentage of students attending a rural school.

4.2 Step 2: Construction of the immigration instrument

Equation 2 identification assumption requires that the municipalities where foreign students are located be independent of changes in municipality effect over time. To deal with this problem I will provide a Shif-share instrument. This instrument used the initial networks of immigrants to deal with endogeneity related to temporal shocks that attract immigrants and affect outcomes at the same time. I will define the network of an immigrant as the share of same nationality and level of education (having or not any tertiary education). I will construct this initial network (share) with the census of 2002. Then, the construction of the instrument is standard and is calculated as follows:

$$\text{Predicted } Foreignpop_c^{2013-2019} = \frac{\hat{\Delta F_c}^{2013-2019}}{population_c^{2013}} = \hat{\Delta F_c}^{2013-2019}$$

Where $\hat{\Delta F_c}^{2013-2019}$ defines the predicted increase, which I define as follow:

$$\hat{\Delta F_c}^{2013-2019} = \sum_k z_{ck} g_k^{2013-2019}$$

Because the specification of interest is:

$$\mu_c^{2013-2019} - \mu_c^{2015-2012} = \alpha + \beta \Delta F_c^{2013-2019} + \epsilon_c$$

Then the instrument rely on the assumption:

$$E(\hat{\Delta F_c}^{2013-2019} \times \epsilon_c) = 0$$

Where z_{ck} is the initial share of students of group k in municipality c in 2002, and $g_k^{2013-2019}$ is the growth of students for group k from 2013 to 2019. Where k are 20 groups define as 10 groups of students nationality 1) Argentina ,2) Bolivia, 3) Colombia, 4) Cuba, 5) Dominican Republic, 6) Ecuador, 7) Haiti, 8) Peru 9) Venezuela, 10) Others nationalities; and mother education: 1) families where the mother did not attended higher education; 2) families where the mother attended any tertiary education. $\Delta F_c^{2013-2019}$ is the endogenous variable of increase of foreign migration from 2013 to 2019 as a proportion of the population in 2007, $\hat{\Delta F_c}^{2013-2019}$ is the instrument and $\mu_c^{2013-2019} - \mu_c^{2015-2012}$ is the first difference of municipality effect in c.

I will use the percentile rank of the endogenous variable and instrument, as mentioned in the previous section.

4.3 Step 3: Estimation of the impact of foreign students

To estimate the impact of foreign students, I will estimate the municipality effects for the window before and after the immigrant arrivals separately. Because the foreign population started to increase from

2013, I will split the windows with this year as a threshold. Once I split the sample in two, I ended up with less observation and, consequently, with fewer municipalities. Then, to make each window estimate comparable, I weight municipality effects by the municipality population, which I can estimate in both windows. So the interpretation of the coefficients is relative to the average municipality effect.

Finally, I regress this first-difference on the increase in foreign students between the time windows. Because this relation may be driven by selection -immigrants arriving in places with a positive or negative trend in municipality effect- I estimate equation 2 instrumentalizing the foreign students change with the shift-share instrument in a 2sls.

5 Results

5.0.1 Impact of immigration on municipality effects

The causal municipality effect measure the contribution of spending a year in a municipality compare to the average municipality. Then specification 3 will show the impact of a one percentile increase of foreign students on municipality effects. I provide 2sls to deal with the endogenous location of immigrants. Table 4 shows this exercise estimating municipality effects on cog score rank conditional on mother education. I estimate 227 municipalities that represent 85% of students enrolment. From panel First-stage we can see that the instrument predicts the foreign student increase. OLS panel shows a negative effect on the municipality effect. These results are similar to those in 2sls, implying that foreign students did not allocate based on the municipality effect. 2sls panel shows a negative impact on the municipality effect of around -0.04 percentiles per one percentile change of immigrants. The interpretation is as follows: a kid moving from an average municipality to a municipality with one percentile higher of immigrant students will lower its cog score by 0.04 percentiles rank. If students move in 1st grade, this will imply a drop of 0.16 percentiles rank. Thus, a 30 percentile greater increase in immigrants - approximately one standard deviation of the shock- will lowered children percentile score by 4.8 percentiles if they moved from 1st grade.

5.1 Alternative explanations

Shift-share

It might be the case that the instrument is capturing differences in trends, and therefore I get a negative effect. Since my setting looks like a DiD, I can provide parallel trends, as suggested by Goldsmith-Pinkham et al. 2018. To do this, I will perform a DiD separating the period before immigrants arrive in two and estimating municipality effects for each window. By doing this, I can test if there were a pre-trend before the arrival of immigrants. I will split the sample into two windows from 2005 to 2008 and from 2009 to 2012 (before any immigrant increase). Table 5 shows the coefficient of the parallel trend. I relax the number of observations per path to have a similar number of municipalities from 25 to 10. Coefficients are showing no differential trend. 6.2.

Another threat to the identification proposed by Jaeger et al. 2018 is when Bartik is used with a constant and homogeneous composition flow because it confuses the effect of short with long term. Fortunately, the sharp increase and the new composition of immigrants after 2014 allows us to rule out this alternative.

6 Mechanisms

This section revises different mechanisms to explain the negative impact of immigrant students on municipality effects. As mentioned, municipality effects are any effect not related to the household, so it is any effect at the school or the neighborhood level. The literature has shown that immigrants can affect student outcomes through peer effect (Hoxby 2000, Gould et al. (2009), Frattini and Meschi (2019)). Given this, it seems natural to estimate the peer effect of having more immigrant peers to test for a negative effect on native students. Additionally, the literature has shown that neighborhood quality is correlated with segregation, inequality, trust, and associativity. Given this, I will look first at the effect on native flights -because it is an indication of segregation- and then the effect on segregation directly.

6.1 Peer effect composition

One would expect that any change in peer composition can drive the effect at the neighborhood level. This impact is not necessarily peer effect, but any effect related to a change of composition of peers within the school, e.g., organization of classes, class size, peer effect, and others. To explore if the change of peer composition impacts student learning, I will exploit random variation from the allocation of immigrants across classes and the arrival of immigrants across cohorts at the school level. I include in appendix B the analysis using variation across cohort at the municipality level because these results are noisier (the units are fewer) and less standard in the literature. To run my analysis, I will complement my 4th-grade data with the introduction of a 2nd-grade test between 2012 and 2015 and the introduction of a 6th-grade test from 2013 to 2018 (2017 was not administered). Table 6 shows the years when each test was administered and their baseline (two years before). The sample definition is all the students enrolled in the 3rd and 5th grades from 2013 to 2017 (one year before the test). Because the across cohort variation and across-class variation have different comparison groups, the samples will vary between the two analyses. I will explain in detail the difference in each part.

6.1.1 Across-cohort variation

In these lines, I will describe the empirical approach and the results of using the across-cohort variation strategy. In practice, this approach estimates the impact on students outcomes in 4th and 6th grade, given that they were exposed differently to immigrants at the beginning of $t-1$: 3rd and 5th grades respectively, controlling by their baseline test score at the end of the school year t-2: 2nd and 4th grade respectively. In other words, it is like an RCT where students do a baseline test at the end of grade g. Students are treated if they have a higher fraction of immigrants in their cohort than the other cohort at the beginning of grade $g+1$. Then, I can test the effect at the end of grade $g+2$. Student characteristics may differ according to the school they choose to attend. Also, students may have different educational outcomes related to the grade they attend, e.g., repetition probability increases with grade. To take into account this heterogeneity related to school choice and the grade students attended, I add a fixed effect of school/year and a fixed effect of grade/type of school/year interacting with students characteristics as follow:

$$g_{jsgt}(X, y^B) = S^0_{jt} + S^1_{jt}X + S^2_{jt}y^B + G^0_{gst} + G^1_{gst}X + G^2_{gst}y^B_i$$

Where S_{jt} is a school- year fixed effect indicating school j and year t. G_{gst} is a grade - type of school - year fixed effect indicating grade g type of school s and year t. The types of schools are: private without vouchers, private with fees on top of the voucher, private vouchers, and public schools. y^B is the baseline test score, and X is a vector of student characteristics. Using student characteristics and baseline test is unnecessary if I show that the variation I am exploiting is random. Anyway, I will use it as a robustness check against sorting.
Thus the specification to exploit differences across-cohorts is:

$$y_i = \beta_0 + \beta_1 Frac_{gjt} + g_{jsgt}(X_i, y^B_i) + n_{gjt} + \epsilon_i \tag{5}$$

Where y_i is the outcome of a native student i, $Frac_{gjt}$ is the fraction of foreign students in year t in school j in grade g as a proportion of students in the same cell, n_{gjt} is the number of students in grade g in school j in year t. I added the number of students as a proxy of class-size.

From the calendar in table 6, we can observe that not all the grades have a baseline for all years. To compare 4th and 6th grade, we can use the years from 2014 to 2016 and 2018. I can not use the year 2018 if I want to control by baseline test. So I will provide results focusing on 2014 to 2016 and then separately results for 2018. I am determined to use 2018 because most of the variation (new arrivals) comes from 2015, so I will exploit little variation if I exclude this year from the analysis.

The strategy of equation 11 relies on independence between unobservable characteristics ϵ_i and treatment $Frac_{gjt}$ within schools/municipality. One threat to this assumption is if native students react early to more immigrants in the same grade and leave early: before the baseline year. I can test if this threat holds observing if the baseline test and student characteristics are differential according to the fraction of immigrants. Continuing with the RCT analogy, this would be like a balance test. Panel A of table 7 shows that the fraction of immigrants is not differential for the baseline test, household income rank, mother education rank, and repeat the last year when pooling students from 2014 to 2016. The girl variable is significant at a 5% level. It seems that girls are sharing more the grade with immigrants. As a robustness check, I will control by student characteristics and baseline tests to take into account this peculiarity. The number of observations differs because of non-response in the questionnaire (income and mother education). This is not problematic because the level of non-response is 2% and is not differential. Panel B of table 7 provides a balance test for the year 2018 only. Variation within the municipality level shows non-differential composition given the fraction of immigrant natives face in their cohort. The questionnaire non-response is higher for this case because I do not restrict the sample to those that answer the baseline test: questionnaire non-response is 21% but is not differential. Panel C in table 7 shows the balance test from 2014 to 2018. As expected, variation within the municipality and school level show non-differential composition given the fraction of immigrant natives students face in their cohort. The questionnaire non-response is higher in this case because I do not restrict the sample to those that answer the baseline test: questionnaire non-response is 8%[23] but is not differential.

The evidence above should rule out composition bias alternative explanation. Table 8 shows results when exploiting variation within the school. These results are robust to include student characteristics, as shown in equation 11. Results show that a higher fraction of immigrants in your cohort within school increases your test score for the period 2014 to 2016 (panel A). I find the opposite in 2018 (panel B) though it is not significant. As a result, the impact on test scores for the period 2014 to 2018 is null. It is unlikely that these results are driven by selection because the change of school or municipality and attrition is not differential.

6.1.2 Across-classes variation

This strategy compares classes with different levels of immigrants. Because I will exploit immigrant fractions across classes, I do not need to pool grades together, as I did above. This strategy is like an RCT where students do a baseline test at the end of grade g then students are treated if they have a higher fraction of immigrants in their class compared to other classes in $g+1$ and then I test the effect on them at the end of grade $g+2$. It is likely that students self-select into different schools and that each school shows different learning transitions depending on the characteristics of the students and the skills base. To allow for these fluctuations, I add a fixed effect of school-year interacting with student characteristics and skill on the baseline as follows:

$$g_{jt}(X, y^B) = S^0_{jt} + S^1_{jt}X + S^2_{jt}y^B$$

Where S_{jt} is a school-year fixed effect, y^B is the baseline test score, and X is a vector of student characteristics. Controlling by student characteristics will be key because, as I will show after, sorting of natives across classes is a relevant issue. Then, the specification is as follows:

$$y_i = \beta_0 + \beta_1 Frac_{cjt} + g_{jt}(X_i, y^B_i) + n_{cjt} + \epsilon_i \tag{6}$$

(7)

[23]The level of non-response when grouped from 2014 to 2018 (8%) is lower compared to 2018 (21%) the reason for this difference is because 2018 does not have a baseline questionnaire so I can only observe the characteristics of the students once and not twice as the rest of the years.

Where y_i is the outcome of a student i, $Frac_{cjt}$ is the fraction of foreign students in class c, school j, and year t as a proportion of students in the same cell and $+n_{cjt}$ is the class size of class c in school j and year t.

I have variation enough for the variation between classes across years with baseline test scores, so I do not need to add years without baseline -as I did with across-cohorts variation strategy. I will compare 4th-grade student outcomes from 2014 to 2017 and 6th-grade student outcomes from 2014 to 2016 and 2018 separately. This strategy relies on independence of unobservables ϵ_i and treatment $Frac_{cjt}$. To identify if class formation is independent of the fraction of foreign students, I can provide a balance test. Table 9 shows that natives that share classes with a higher fraction of immigrants have lower baseline test scores and income. This result is discouraging because it means that the variation I am exploiting has sorting immigrants or student ability (tracking).

One strategy to deal with this sorting is to flag school where I suspect classes are not formed randomly and discard them for estimation. To test this non-random allocation, I will perform a Pearson χ^2 test[24]. The Pearson χ^2 test asks whether there are more subgroups of students (immigrants, high performing, girls, or other) in a particular class than what is consistent with independence, given the number of students of the school. Formally, for each school, the test statistic is given by:

$$\begin{aligned} P &= \sum_c \sum_g \frac{(n_{cg} - \hat{n}_{cg})^2}{\hat{n}_{cg}} \\ \hat{n}_{cg} &= \frac{n_c \times n_g}{\sum_c \sum_g n_{cg}} \end{aligned} \tag{8}$$

Where n_{cg} are the number of students of subgroup g in class c, n_c the number of students in the class c and n_g the number of students of subgroup g in the school. Thus, $\hat{n}_{cg}$ is the predicted number of students of subgroup g in class c if subgroup and classes are independent. The non-random allocation can be an issue because we can confound foreign student effect with other effect related to student and class characteristics. For this, I will test the random allocation of students given foreign status and perform in the baseline test. This test will help me flag school where I suspect non-random allocation and discard them for robustness checks of my results.

I run the balance test discarding any school where I reject random allocation of immigrants and student performance using equation 8. Table 10 show this exercise. Coefficients are lower but still statistically significant. I believe balance tests did not change because immigrants have higher ability than natives within schools, so if the allocation between classes seeks to balance ability, immigrants are more likely to share with low ability students. Then the only solution left would be to control by the covariates available interacting with the school-year fixed effect as in equation 6. Table 11 shows the impact of more immigrants in your class on cog score, repeat, and dropout for 4th and 6th grade. Overall we see a null effect on education outcomes. There are, however, one statistically significant effects at 5% level for dropout and school change in 4th grade. On average, class size is around 40 students[25] these results imply that an increase of 10% of immigrants (4 students) in your class may increase the probability of school change by 0.7%. As we can see in table 12, results are similar when discarding schools doing tracking or non-random allocation of immigrants.

To sum up

Overall, results show that it is unlikely the impact I observe on municipality effects is driven by immigrants peer effect. In appendix B, the municipality peer effect estimates are noisier and non-significant. The across-cohort variation tells there is no evidence of peer effects at school or municipality level. On the other hand, the across-class variation is not as random as across-cohort, but if we control by student characteristics, we also see null effects.

[24] Ammermueller and Pischke (2009) implemented this test for the first time in this setting.
[25] Considering school with more than one class per cohort.

6.2 Native flights and segregation

In this subsection, I will study whether natives respond to immigrant arrival by reducing interaction with them. This response is named native flight. In Chile, as opposed to many developed countries, students are not assigned to public schools, so private schools are not the only option to avoid interaction with immigrants in assigned schools. Families can decide to fly from the public to private schools[26] or to fly to a municipality with relatively fewer immigrants. The consequences of these flights are an increase in the segregation of immigrant students, but- because those that move are generally those that can pay the cost of a private school or the cost of changing municipality, this can also generate socioeconomic segregation.

6.2.1 Native flights

Native flights identification poses a significant challenge. Natives can follow immigrants waves into an area when there are pull factors for both, e.g., job market. Simultaneously, impacts on housing and labor market and preferences for immigrants can motivate natives to move away once immigrants arrive. Additionally, natives can be moving even before immigrants arrive. Then I will have to take care of pre-trends, endogenous arrival of immigrants, and plausible housing and labor market effects.

In this subsection, I use aggregated data at the municipality level from all students enrolled in primary education from 1st to 6th grade from 2007 to 2019. The variables of interest in this analysis are the number of natives per municipality-year, number of natives in private schools per municipality-year, number of births per municipality-year (they are natives because of jus solis), segregation index per municipality-year. The segregation index is constructed based on the IVE, an index at the school level that identifies the fraction of vulnerable students.

To illustrate the identification problem of studying native flights, I will start with a naive approach and then modify it to overcome the identification issues. I will explain the empirical strategy of native flight to municipalities with relatively fewer foreign students. Still, it can easily adjust to doing flight to private schools by changing the dependent variable from all natives to natives in private schools. So, the naive specification evaluates if the change in natives is related to change of immigrants:

$$\Delta nat_{ct} = \alpha + A_t + \sum_{t}^{\prime} \beta^t \mathbf{1}(t' = t) \Delta mig_{ct} + \epsilon_{ct}$$

Where Δnat_{ct} is the change of natives as a proportion of the enrolment population in 2007 in the municipality c in time t, A_t is a time dummy, and Δmig_{ct} is the change of immigrants as a proportion of the population in 2007 in the municipality c in time t.

As mentioned above, the naive specification has identification problems. First, natives families can decide to move in when immigrants arrived because of positive economic shocks. To deal with the endogeneity of immigrant location, I will provide a shift-share instrument. Second, it could be that immigrants are locating in municipalities that were experiencing moving out earlier than immigrant arrivals. I will address this issue, following a DiD specification and test for pre-trend to show this is not the case. Third, they may be areas that have a different birth rate. I will address this problem by controlling by birth cohort rate[27]. This approach does not take into account the fact that families may move before birth. Because movements before birth are unlikely to be related to immigrant students, this is less of an issue.

As with any DiD, my strategy relies on the fact that treated municipalities behave similar before immigrants arrive. To provide a parallel trend, I will test if the levels are changing when immigrants

[26] In most cases, natives decide to go to private schools. However, there are exceptions like Muslim immigrants in DenmarkRangvid (2010).

[27] Since naturalization is based on jus solis, all births are Chileans. Also, I will consider that most of the students enrolled in 1st grade are six years old in April, e.g., kids enrolled in 1st grade in t have birth cohort between April in $t-7$ to March in $t-6$.

arrive. I expect that municipalities with higher exposure of immigrants have no effect until 2013, and then it must gradually increase over time. The specification is as follow:

$$nat_{ct} = \alpha + A_t + \delta\Delta mig_c + \sum_{t'=2007}^{2019} \beta^t \mathbf{1}(t' = t)\Delta mig_c + \sum_{t'=2007}^{2019} \gamma^t \mathbf{1}(t' = t) births_{ct} + \epsilon_{ct} \tag{9}$$

Where nat_{ct} is the level of natives as a proportion of the enrolment population in 2007 in the municipality c in time t, A_t is a time dummy, Δmig_c is the percentile rank transformation of the change of immigrants from 2013 to 2019 as a proportion of the population in 2007 in the municipality c. I allow β^t to change by year to test the pre-trend and observe the change in trend after immigrants arrive. Finally, $births_{ct}$ is the level of births as a proportion of the enrolment population in 2007 in the municipality c in time t. I allow γ^t to change over time to allow for differences by year in the databases, i.e., shocks that vary in time but no across municipalities. To deal with immigrant location endogeneity, I will provide a shift-share where the share is the combination of nationality and family level of education by the municipality from census 2002 for all nationalities but Haitians from 2007[28]. In this specification, I will set the baseline in 2007[29] so the coefficients β^t will show the accumulate change of natives -no explain by new births- from 2007. In the case of natives in private schools, I will not control by new births in municipalities because I can not be sure if students have a public or private school profile. Finally, the interpretation of the magnitudes in this specification is not straightforward, so I will prefer another one that I will explain in detail below.

The time span of the analysis is relevant because natives decision can show anticipation or delay. For this reason my prefer analysis to obtain the magnitudes consider 6 years window from 2007 to 2019. Also, because the increase started from 2013 my preferred specification follows a DiD structure and consider treatment as the increase of immigrants from 2013 to 2019.

$$\Delta nat_{ct} = \alpha + A_t + \delta\Delta mig_c + \sum_{t'=2009}^{2019} \beta^t \mathbf{1}(t' = t)\Delta mig_c + \sum_{t'=2007}^{2019} \gamma^t \mathbf{1}(t' = t)\Delta births_{ct} + \epsilon_{ct} \tag{10}$$

Where Δnat_{ct} is the five years change of natives as a proportion of the enrolment population in 2007 in the municipality c in time t, A_t is a time dummy, Δmig_c is the percentile rank transformation of the the change of immigrants from 2013 to 2019 as a proportion of the population in 2007 in the municipality c, and $\Delta births_{ct}$ is the five years change of births as a proportion of the enrolment population in 2007 in the municipality c in time t. As I did with the earlier specification I will provide a shift-share instrument to deal with immigrants location endogeneity.

My preferred specification uses immigrant increase percentile rank transformation because the impact of immigrants on municipality effects looks more linear in this case. However, I will add the percentage change to see if outliers drive native flight based on tipping, which is a non-linearity. For this reason, I will also show this alternative in my results.

Natives flight to municipalities with less immigrants

Figure 7 show the coefficients of equation 9 weighting by enrolment population in 2007. Panel (a) used as treatment the percentile rank change of immigrants from 2013 to 2019, while (b) the percentage change of immigrants from 2013 to 2019—both as a proportion of the population in 2007. In panel (a) and (b), we can see there is no pre-trend. This result means that the change in natives is not related to the exposure of immigrants in municipalities. In panel (a), we see a change in the slope from 2014 to 2019, showing fewer natives once immigrants arrive in each municipality. In panel (b), there is a change in the slope from 2012. This could be explained because natives started to move early on the increase of foreign students. This is likely because adult immigrants migrate first and then come with their families,

[28]This decision is because there were few Haitian in census 2002.

[29]I started from 2007 because the census-based instrument uses the location of immigrants by nationality in 2002, except Haitians, where I considered their location in 2007.

which could be a reaction from adult immigration.

Table 13 show the coefficients of equation 10. Odd columns show the result when using the percentile rank of migration change and even columns when using percentage migration change as treatment. The first two columns run analysis at the municipality level and the second two columns at the city level. The first stage panel shows that the instrument predicts the endogenous variable with enough precision. Column 1 in the OLS panel shows that immigrants follow the native movements during pre-treatment (pre-2013). This is why the instrument can be useful because it deals with positive shocks at the municipality level that may pull natives and immigrants. Column 2 does not show this pattern. Panel 2sls show the effect of immigrants on native flights. Column 1 shows that an increase of one percentile decreases the population of natives by 0.085 percentage points. A one percentile increase is associated with an average of 0.116 increase in percentage points or to a median of 0.072 percentage points. So taking the average translate into -.7 or taking the median to -1.1 natives per one immigrant arrival. Column 2 shows that a one percentage point increase in immigrants decreases the natives population by 1.2 percentage points. Column 3 in panel 2sls shows that an increase of one percentile decrease native enrolment by 0.105 percentage points. A one percentile increase is associated with an average of 0.085 increase in percentage points or to a median of 0.108 percentage points. They are so taking the average -1.2 or the median -1 native per one immigrant arrival. Column 4 shows that a one percentage point increase in immigrants decreases the natives population by 0.9 percentage points. These results show that the impact on native flights was not only driven by sorting within cities.

These results should be interpreted with caution because we do not know if the movements are to avoid exposure to immigrants or respond to the housing and labor market. In fact, because the relationship is one to one, in a world of housing supply not perfectly elastic, this effect could be fully explained by the housing market and not distaste for immigrants (Boustan (2010)).In this matter, looking at private school movements helps rule out the housing and labor market effect.

Natives flight to private schools

I can observe native flights to private school by changing the dependent variable in the equation 9 with the level of natives in private schools. Also, I will exclude the birth correction from the specification. Figure 8 show the coefficients of this modified equation 9 weighting by enrolment population in 2007. Panel (a) used as treatment the percentile rank transformation of change of immigrants from 2013 to 2019, while (b) the percentage change of immigrants from 2013 to 2019 as a proportion of the population in 2007. For both figures, we can see that from 2007 to 2013, there is a null pre-trend. This means that the change of natives to private schools is not related to the exposure to immigrants in municipalities. In panel (a), we see a change in the slope from 2012, which means that municipalities with higher exposure to immigrants show an increase of natives enrolment in private schools. In panel (b), there is no change of slope. This difference may arise because of the non-linearity of the effect.

Table 14 show the coefficients of equation 10 with a change of natives in a private school as a dependent variable and without controlling by births per municipality. Odd columns show the result when using the percentile rank of migration change and even columns when using percentage migration change as treatment. The first two columns run analysis at the municipality level and the second two columns at the city level. The first stage shows that the instrument predicts the endogenous variable with enough precision. Column 1 in the OLS panel shows that natives move to private school when immigrants move in. This is similar to the 2sls. Column 2 shows no differential variation over time. Panel 2sls show the effect of immigrants on native flights to private schools. Column 1 shows that an increase of one percentile of immigrants increases native private school enrolment by 0.03 percentage points. One percentile increase in immigrants is associated with an average of 0.12 increase in percentage points or a median of 0.07 percentage points. So taking the average translate into 0.3 or taking the median to 0.5 more natives into private schools per one immigrant arrival. Because pre-trend is positive and significant, we can observe this effect as an accelerating phenomenon, i.e., natives were moving to private schools in exposed municipalities early on immigrant arrivals, but once they arrive, the movement speeds up.

Column 2 shows no change. Column 3 shows similar coefficients to column 1, which means that this phenomenon also occurred in cities that receive more immigrants. Column 4 shows that natives were moving to private schools early on immigrant arrivals, then increase during immigrant arrival, but the coefficient is not significant.

Because native movements to private schools are unlikely to respond to the effects on the labor or housing market, we can interpret these effects as native flights. Another explanation could be a selection effect derived from native flight to municipalities with fewer immigrants. However, this explanation does not seem reasonable. After immigrant students began to arrive, there are fewer natives in the exposed municipalities, but we see that more and more students are enrolled in private schools. The most reasonable explanation for this phenomenon is that there is a proportion of natives trying to reduce interaction with natives. While it is difficult to estimate the magnitude, we can define native flights to private schools as the lower bound of the effect, i.e., 0.5 natives per one immigrant arriving.

6.2.2 School segregation

Natives flight from public to private schools can cause school segregation within a geographic area, and natives flight to areas with less immigrants can cause segregation across geographic areas. I have found evidence of both type of flights, so I would like to evaluate segregation at municipality level and at city level. In my data I do not see where each student lives but I do know where they are enrol in. Then, I will estimate the school segregation per municipality and city. To construct the segregation index I will follow Theil index (Theil (1972)) and Atkinson index (Atkinson et al. (1970), James and Taeuber (1985)) in two groups, priority students versus non-priority[30]. The calculation of the Theil Index begins with entropy at municipality level (E_c) which is defined as $P_c ln(\frac{1}{P_c}) + (1 - P_c) ln(\frac{1}{1-P_c})$ where P_c is the proportion of priority students in municipality c. Then we estimate entropy at school level (E_i) defined as $p_i ln(\frac{1}{p_i}) + (1-p_i) ln(\frac{1}{1-p_i})$ where p_i is the proportion of priority students in school i. Then, Theil index (H_c) is the average difference between the subareas' E_i and the overall E_c, expressed as a proportion of the overall E_c and weighted by the school's share of the total school population in municipality c ($\frac{N_i}{N_c}$):

$$H_c = \sum_{i \in c} \frac{N_i}{N_c} \frac{E_c - E_i}{E_c}$$

The Atkinson index (A_c is define as 1 minus the sum, over all the schools, of some weighted geometric average (w) of the percentage of priority students who attend the school (p_i).

$$A_c = 1 - \frac{P_c}{1 - P_c} \sum_{i \in c} [\frac{t_i (p_i)^w (1 - p_i)^{1-w}}{P_c T_c}]^{\frac{1}{1-w}}$$

Where t_i is the number of priority students in school i and T_c is the number of priority students in municipality c.

The Atkinson satisfy composition invariance as opposed to the Theil index. This means that the index should not change when we augmented the number of priority students in each school by a constant. To keep measures comparable across municipalities and over the years, we should set a weight that varies across groups but not across time or municipality. Frankel and Volij (2007) suggest to set weights in a baseline reference year. I will define the weight as 50% because priority students are around 50% at the baseline year 2010.

The empirical strategy is the same in equation 9 with the school segregation as a dependent variable and excluding the birth correction from the specification. Figures 9 and 10 show the coefficients weighting by enrolment population in 2007 and starting from 2010 since vulnerability index is available from this year. In both figures, panel (a) used as treatment the percentile rank transformation of change of immigrants from 2013 to 2019, while (b) the percentage change of immigrants from 2013 to 2019 as a

[30] I will use the IVE-SINAE, a social vulnerability index developed by JUNAEB, the governmental office that provides student assistance. This index reflects the vulnerabilities of students throughout their education. It focuses on two main factors: first, the risk of subsistence associated with poverty and the availability of food and shelter; and second, the risk of school dropout associated with family composition and other factors that could lead to academic dropout Cornejo et al. (2005)

proportion of the population in 2007. Figure 9 shows the coefficient for the Theil index. Both panels follow the null pre-trend from 2010 to 2013. This means that school segregation at the municipality level is not related to immigrant exposure in municipalities. In panel (a), we see a change in the slope from 2013, which means that municipalities with higher exposure to immigrants show an increase in school segregation from 2010. In panel (b), there is no change of slope. This difference may arise because of the non-linearity of the effect. Similarly, figure 10 shows the coefficient for the Atkinson index. Overall, results with Theil and Atkinson index look similar, which means that composition change over time does not drive the results.

Tables 15 and 16 show the coefficients of equation 10 with the change of the school segregation index as dependent variable[31] and without controlling by births per municipality. Odd columns show the result when using the percentile rank of migration change and even columns when using percentage migration change as treatment. The first two columns run analysis at the municipality level and the second two columns at the city level. The first stage panel for both tables is the same and shows that the instrument is a strong predictor of the change of immigrants. In table 15 (Theil), column 1 in the OLS panel shows that school segregation increase when immigrants arrive in a municipality. This is similar to the 2sls. Column 2 shows no change in school segregation overtime when immigrants move in. Panel 2sls show the effect of immigrants on school segregation. Column 1 shows that an increase of one percentile increases the school segregation index by 0.0289 percentage points. Column 2 shows no statistically significant change. Column 3 shows that immigrants accelerate school segregation at the city level. Column 4 shows that when grouping by city, immigrant arrivals affect school segregation by 0.007 per one percentage point increase of immigrants. Remember that when using percentage change of immigrants as the treatment, I found evidence of flight to municipalities with fewer immigrants but not to private schools. Hence, the significant effect in column 4 is likely driven by sorting across municipalities. Results in table 16 (Atkinson) are equivalent to those described in table 15. This shows that the segregation change in time is unlikely driven by the change in composition.

To sum up

I observe an increase in native flights leading to the rise in school segregation. In the Chilean context, the native flight is unlikely to be due to the lower quality of immigrant peers, they outperform Chileans in test scores, and their parents are more educated. Also, most immigrants speak Spanish, so there is no more resource allocation to immigrants to the detriment of natives. I also showed in the previous section that there is no evidence of peer effect.

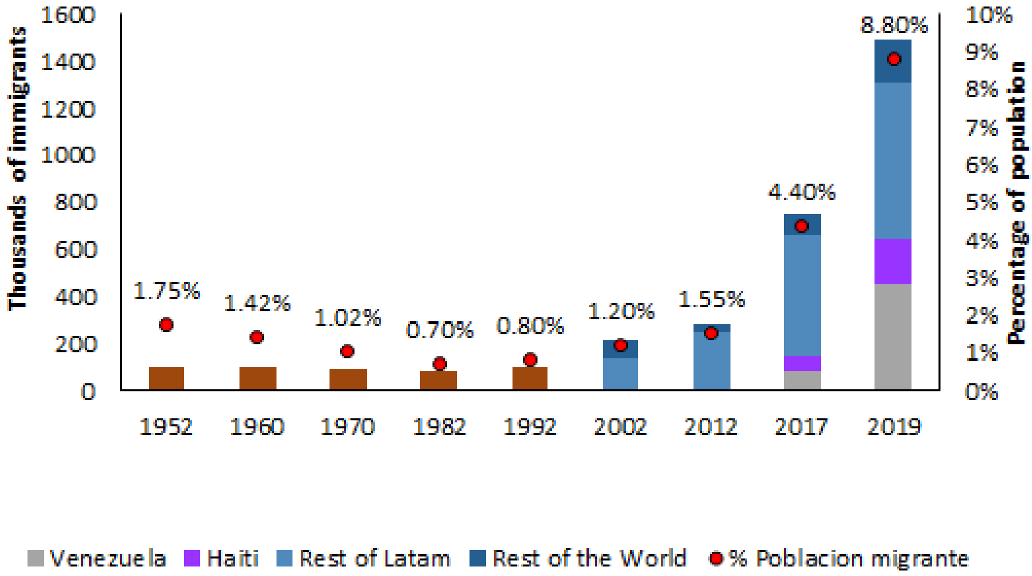
7 Conclusion

This paper estimates municipality's causal effect using children's test scores rank at 4th grade (10 years old) conditional on the mother education rank in two windows: before and after the large wave of immigrants. I found that on average there is a negative effect of foreign students on municipality effect. Additionally, the arrival of immigrants induced native flights and as a consequence increased segregation. Given the evidence that exists, it seems that this segregation caused the municipality effects to drop. However, more research have to be done to study deeply the link between increase of segregation and neighborhood effects.

[31] Because the window pre-2013 is shorter than the post, I will rescale the change in school segregation index and percentage of immigrant students as annually change.

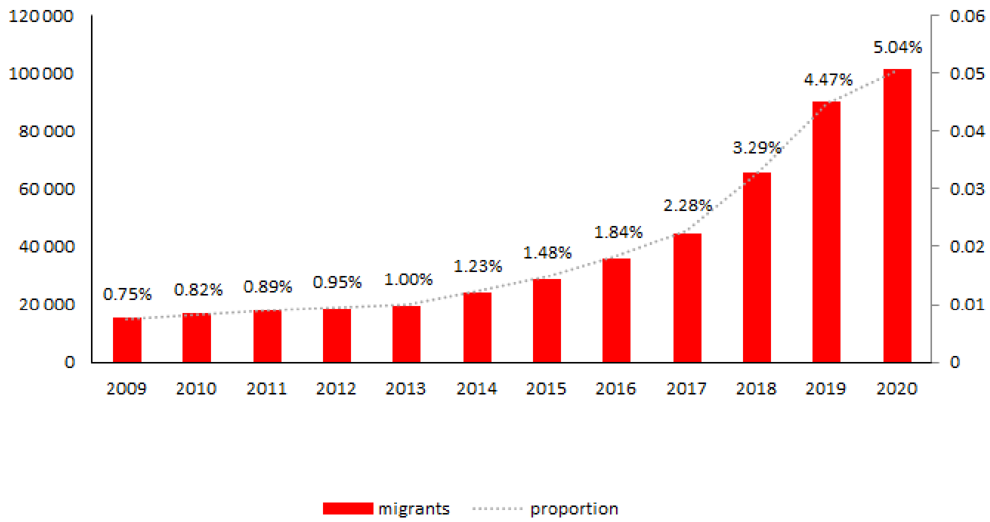
Figures and tables

Figure 1: Number and fraction of foreign population by country of origin



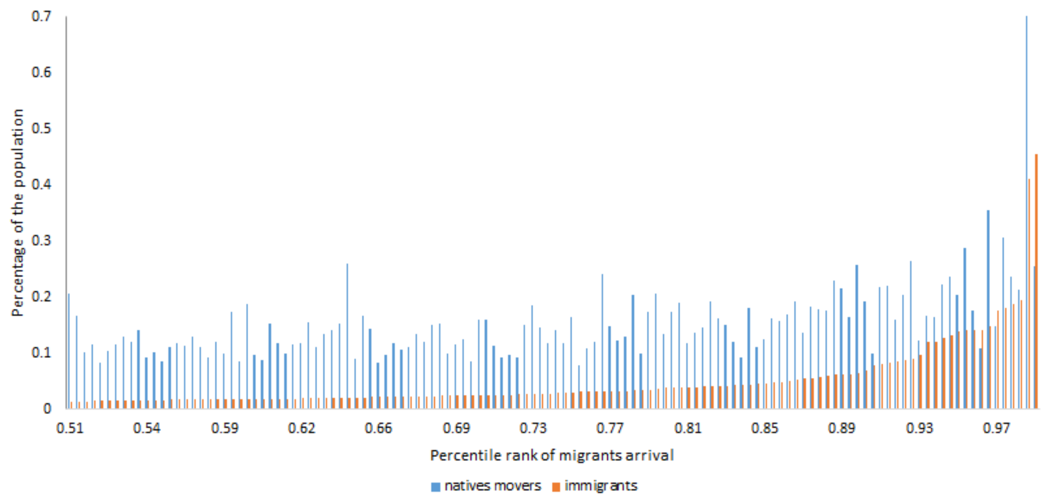
Source: Censuses 1952, 1960, 1970, 1982, 1992, 2002, 2012 and 2017. Government forecast in 2019

Figure 2: Foreign students enrolled in primary and fraction of native students.



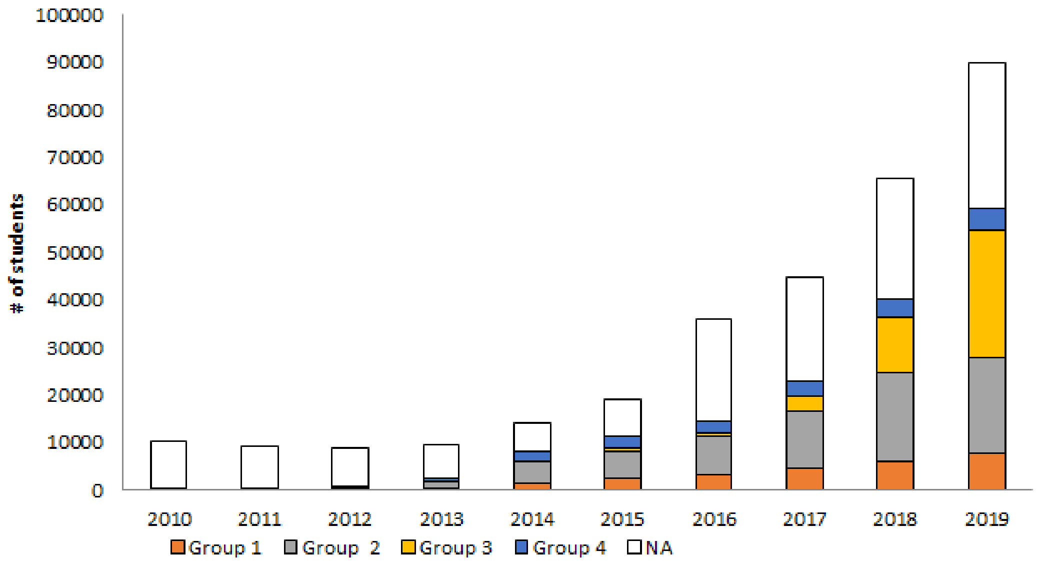
Source: Enrolment data (Education Ministry).

Figure 3: Percentage of migrants and natives movers by municipality



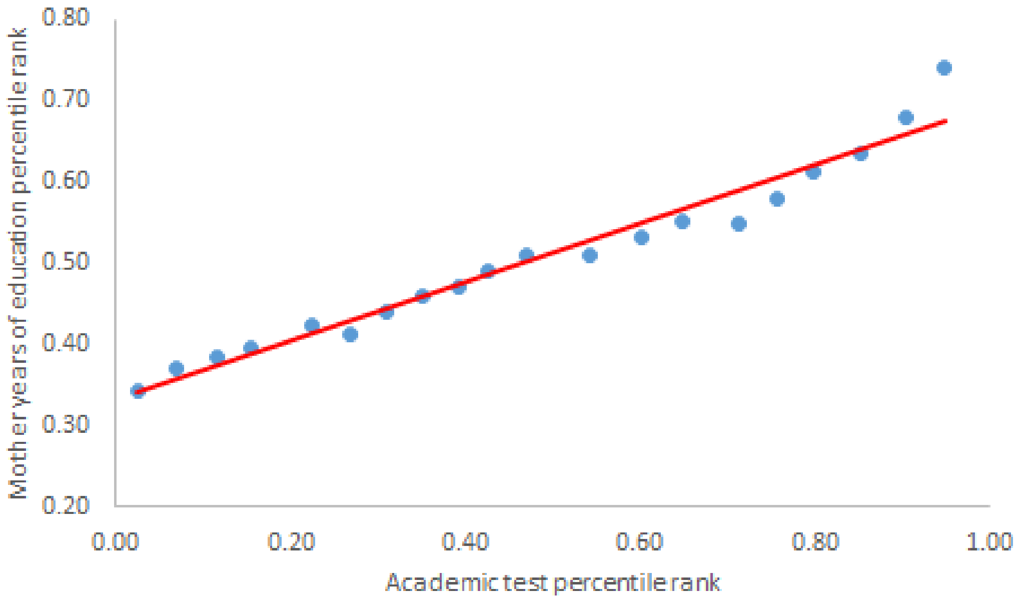
Source: Censo 2017.
Note: Movers defined as natives living in a different municipality 5 years ago. Migrants defined as being born in a foreign country.

Figure 4: Foreign students enrolled in primary by country of origin



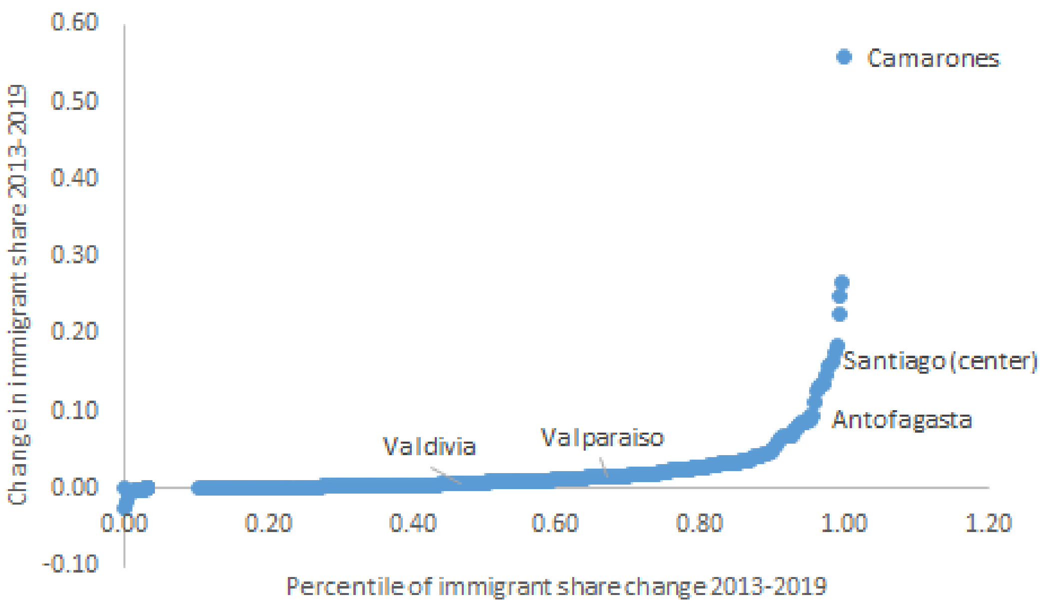
Source: Enrolment data (Education Ministry). Foreign students by country of origin based on director report. Director report started from 2014. NA when report is missing or Chilean. Group 1 is for Bolivia, Group 2 is for Peru, Haiti and Colombia, Group 3 is for Venezuela, Paraguay, Uruguay and Brasil.

Figure 5: Relationship between mother years of education percentile rank and academic test percentile rank



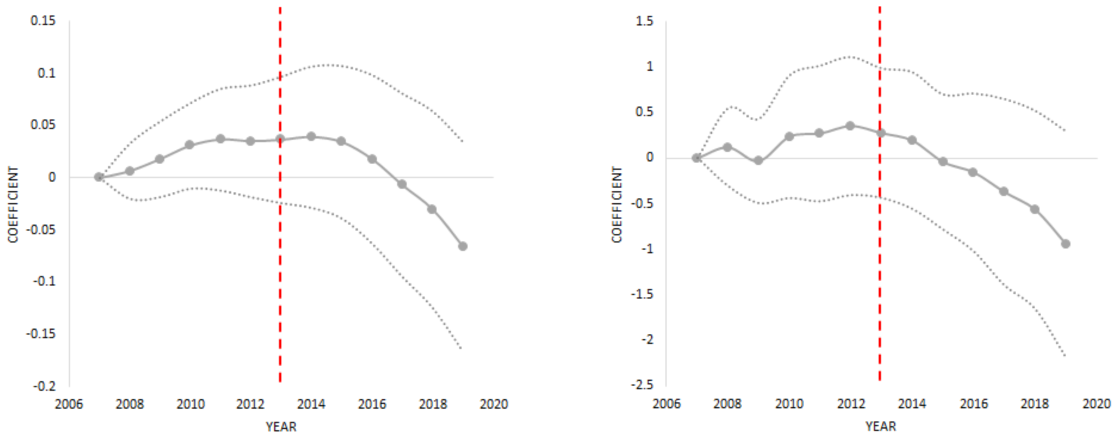
Source: This binned scatterplot depicts the relationship between the mother years of education percentile rank and the academic test percentile rank. The unit of observation a student of 4th-grade between 2005 and 2018. The right hand side variable is grouped into 20 bins (5 percentiles each). Academic test is the average between read and math.

Figure 6: Quantiles of immigrant change as a share of 2007 population, 2013-2019



Source: This figure plots the quantile function of 2013-2019 change in immigrant in municipalieties as a share of the population in 2007.

Figure 7: Impact of foreign students on natives flight to municipalities with less immigrants controlling by births



(a) Percentile rank change of immigrants

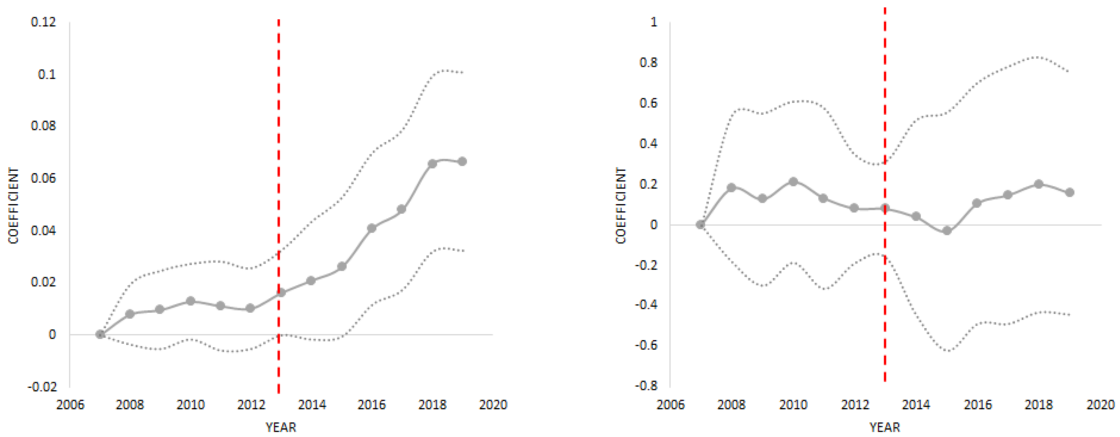
(b) Percentage change of immigrants

Plot of coefficients β^t from equation 9.

Note: Variables at municipality level and standard errors clustered at municipality level.

Because most of students enrolled in 1st grade are 6 years old in April, I will take this into account to match births with enrolment eg. kids enrolled in 1st grade in t have birth cohort between April in $t-7$ to March in $t-6$.

Figure 8: Impact of foreign students on native flight to private schools



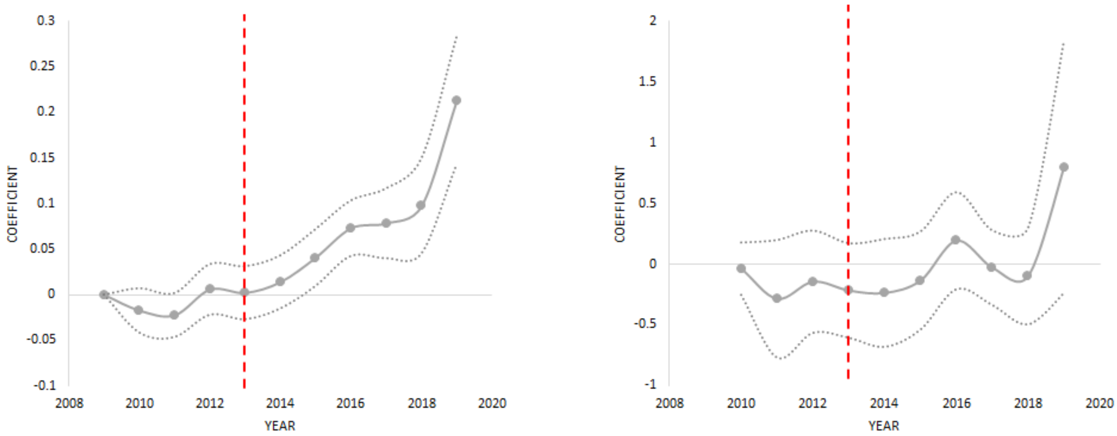
(a) Percentile rank change of immigrants

(b) Percentage change of immigrants

Plot of coefficients β^t from equation 9.

Note: Variables at municipality level and standard errors clustered at municipality level.

Figure 9: Impact of foreign students on school segregation (Theil index)



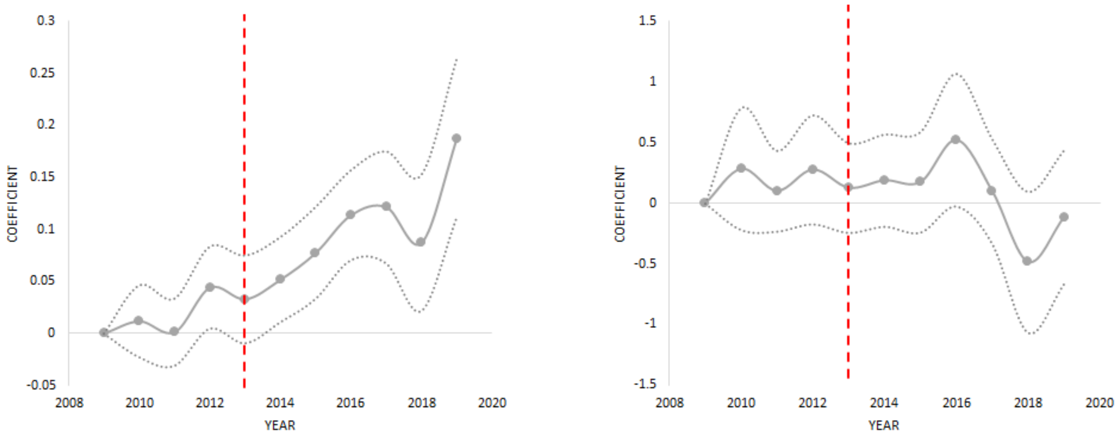
(a) Percentile rank change of immigrants

(b) Percentage change of immigrants

Plot of coefficients β^t from equation 9.

Note: Variables at municipality level and standard errors clustered at municipality level.

Figure 10: Impact of foreign students on school segregation (Atkinson index)



(a) Percentile rank change of immigrants

(b) Percentage change of immigrants

Plot of coefficients β^t from equation 9.

Note: Variables at municipality level and standard errors clustered at municipality level.

Table 1: Summary statistics for students in 4th grade: permanent residents and movers

	Mean	Std.Dev.	Obs
A: Permanent residents			
Cog test rank	0.50	0.29	2300068
Mother education rank	0.46	0.28	2400682
Norte Grande	0.07	0.26	2515084
Region Metropolitana	0.35	0.48	2515084
Rest	0.58	0.49	2515084
B: Movers restricted sample			
Cog test rank	0.50	0.29	216469
Mother education rank	0.48	0.29	231410
Norte Grande	0.08	0.28	246089
Region Metropolitana	0.22	0.41	246089
Rest	0.70	0.46	246089
C: Movers restricted sample used in the main regression			
Cog test rank	0.51	0.29	169300
Mother education rank	0.49	0.29	180637
Norte Grande	0.09	0.29	192038
Region Metropolitana	0.19	0.39	192038
Rest	0.71	0.45	192038

Note: Cog test rank defined as the rank of the average of math and reading test in 4th grade. IVE-SINAE rank is the rank of school vulnerability index. Mother education rank is the rank of years of education declared in the 4th grade parents questionnaire. Movers restricted defined as one time movers to non-adjacents municipalities that were never in rural areas and did not move to "Liceos Emblematicos".

Table 2: Selection test for movers after 2nd grade test.

	read percentile rank
move at 4th grade	-0.251
	(1.419)
move at 5th grade	-0.838
	(1.561)
move at 6th grade	-0.206
	(1.788)
Constant	52.06***
	(1.051)
Observations	26017

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Using one time movers between 3rd and 6th grade controlling by mother education.

Dependent variable is 2nd grade reading test from 2012 to 2015.

Table based on equation 3, but replacing $(A - m_i)\mu_{od}$ by a dummy of grade at move as follow:.

$$y_i = \alpha_{odps} + \sum_{m=3}^{6} \beta_m \mathbf{1}(m_i = m) + \epsilon_i$$

Where y_i is 2nd grade reading test, α_{odps} is an origin-destiny fixed effect interacting with mother levels of education and year of movement, and $\mathbf{1}(m_i = m)$ is a dummy that is 1 if grade of move is m.

Table 3: Correlation of municipality effects of 4th grade cog score conditional on mother level of education with variables of interest. Movements from the year 2005 to 2020.

	Regression estimates			Multivariate analysis		
	b	s.e.	Observations	b	s.e.	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
Test score value added	2.27***	.42	227	2.335***	0.489	227
City segregation Theil index	-1.94***	.43	227	-0.482	0.675	227
School segregation Theil index	-2.7***	.41	227	-1.222*	0.717	227
Poverty rate	2.15***	.42	227	-0.121	0.735	227
Income per household	-1.12**	.44	227	1.264	1.258	227
Rurality	2.43***	.41	227	1.104	0.692	227
Educational budget per student	.37	.44	227	-0.312	0.519	227
Health budget per capita	-.04	.45	227	-0.212	0.394	227
Neighborhood org per Capita	1.61***	.43	227	-0.237	0.533	227
Green areas per Capita	.19	.44	227	0.161	0.427	227
Fraction adults tertiary	-1.26***	.44	227	-1.195	1.512	227
Fraction of single parents	-1.44***	.43	227	0.124	0.666	227
Fraction of adults divorced	-2.52***	.41	227	-0.344	0.670	227
Fraction of adults married	1.79***	.43	227	-0.380	0.724	227
Unemployment rate	.64	.44	227	0.800	0.535	227
Crime rate	-1.3***	.44	227	-0.411	0.761	227
Slums inhabitants per capita	.14	.45	227	0.0121	0.453	227
Norte Grande	-.32	.44	227	-0.0459	0.527	227
Metropolitan Region	-2.4***	.42	227	-0.754	0.790	227

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Municipality effect estimates from students movers in 1st to 6th grade from 2005 to 2020 using method described in subsection 4.1. Column 1 reports the coefficient of regressing the neighborhood effect augmented by four (ie. spending the first four years of primary) on each covariate standardized. Column 4 reports the coefficient of regressing the neighborhood effect augmented by four (ie. spending the first four years of primary) on all covariates standardized (multivariate analysis). Residential segregation Theil index is a residential segregation index in levels of education (below and above primary) using 2017 Census as in Iceland (2004), where census track is the unit and city is the aggregation. School segregation Theil index is a school segregation index based on IVE-SINAE where school is the unit and city is the aggregation. Rurality is the average proportion of students attending rural schools between 2005 and 2019. Test score value-added is the estimated municipality fixed effect when regressing cog test rank in 4th grade students on municipality fixed effect, mother level of education rank, family income rank, indigenous dummy, and gender from 2005 to 2018. Slums inhabitants per capita is constructed based on the Slums Census of 2019 from the Housing Ministry. Norte Grande is the area compose by the first three regions from north to south. Metropolitan Region is the region of the capital Santiago. The rest of covariates Poverty rate, Income per household, Health and Educational budget, Green areas, Fraction of..., and Unemployment rate come from the survey SINIM between 2005 and 2018.

Table 4: Foreign students percentile change impact on municipality causal effect based on cog score.

	(1) First-stage	(2) OLS	(3) Reduced form	(4) Two stage least squares
$\Delta F^{\hat{w_1 - w_2}}$	0.818***		-0.0301***	
	(0.0448)		(0.0107)	
$\Delta F^{w_1 - w_2}$		-0.0339***		-0.0368***
		(0.0100)		(0.0129)
Constant	10.83***	2.338***	2.136***	2.534***
	(3.318)	(0.726)	(0.792)	(0.917)
r2	0.597	0.0485	0.0340	0.0481
N	227	227	227	227

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: Estimates of municipality effects on cog rank conditional on mother level of education.
Municipality effect estimates from on time movers in 1st to 6th grade in two windows from 2004 to 2012 and from 2013 to 2019. Movement across municipalities with more than 25 observation as in Chetty and Hendren (2018b). Municipalities fixed to common estimates between two windows and weighted by population.

Table 5: Parallel trend of municipality causal effect.

	(1) First-stage	(2) OLS	(3) Reduced form	(4) Two stage least squares
$\Delta F^{\hat{w_1 - w_2}}$	0.817***		-0.00319	
	(0.0466)		(0.0186)	
$\Delta F^{w_1 - w_2}$		0.00421		-0.00390
		(0.0175)		(0.0226)
Constant	10.94***	-0.291	0.228	0.270
	(3.464)	(1.271)	(1.380)	(1.613)
r2	0.592	0.000274	0.000139	.
N	214	214	214	214

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: Estimates of municipality effects on cog rank conditional on mother level of education.
Municipality effect estimates from on time movers in 1st to 6th grade in two windows from 2004 to 2008 and from 2009 to 2012. Movement across municipalities with more than 10 observation in order to fit the number of observations. Municipalities fixed to common estimates between two windows and weighted by population.

Table 6: Calendar of tests per year and grade and the availability of baseline

	2014	2015	2016	2017	2018
4th grade test	✓	✓	✓	✓	✓
4th grade basline (2nd grade)	✓	✓	✓	✓	X
6th grade test	✓	✓	✓	X	✓
6th grade basline (4th grade)	✓	✓	✓	✓	✓

Table 7: Balance test using across-cohort at school level for students in 4th and 6th grade

Panel A:	2014-2016				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac	0.0238	0.0155	0.00172	0.109**	0.000881
	(0.0381)	(0.0227)	(0.0229)	(0.0494)	(0.000738)
r2	0.201	0.523	0.452	0.0762	0.0455
N	1169400	1148045	1146880	1169400	1169400
N schools	7460	7452	7449	7460	7460
dependent mean	0.506	0.494	0.487	0.498	0.0000436
Panel B:	2018				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac		0.0267	0.00916	-0.0823	0.0229
		(0.0306)	(0.0274)	(0.0537)	(0.0223)
r2		0.440	0.391	0.0679	0.0478
N		383710	381827	478515	478515
N schools		6873	6871	7393	7393
dependent mean		0.447	0.435	0.489	0.0252
Panel C:	2014-2018				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac		0.0114	0.00204	0.0369	0.0151
		(0.0172)	(0.0168)	(0.0339)	(0.0152)
r2		0.504	0.444	0.0724	0.0555
N		1735235	1732150	1883484	1883484
N schools		8070	8068	8116	8116
dependent mean		0.470	0.462	0.486	0.0344

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 that took the baseline test (2014 to 2016 is the years they should take the test in 4th and 6th grade).

Panel B: Natives students test enrolled in 3th and 5th grade in 2017 (2018 is the years they should take the test in 4th and 6th grade).

Panel C: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 and 2017 (2014 to 2016 and 2018 is the years they should take the test in 4th and 6th grade).

Controlling by school-year fixed effect and grade - type of school -year fixed effect.

Baseline test is the test score in 2nd and 4th grade for 3th and 5th grade students, respectively. Income and mother is the household income and mother level of education declared transformed to percentile rank. Girl is a gender dummy if student is girl. Repeat is a dummy if students attended the same grade (3th and 5th) the year before the baseline year.

Table 8: Impact on educational outcomes using across-cohort at school level for 4th and 6th grade students

Panel A:	2014-2016						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	0.0883**	0.00235	-0.00126	0.00742	0.0115	0.00475	0.00857
	(0.0407)	(0.0187)	(0.00871)	(0.0253)	(0.0166)	(0.0370)	(0.0516)
r2	0.256	0.0404	0.0270	0.0566	0.0325	0.0721	0.0868
N	998548	1169400	1169400	1162816	1162816	1162816	1169400
N schools	6433	7460	7460	7458	7458	7458	7460
dependent mean	0.521	0.0241	0.00325	0.0470	0.0201	0.103	0.146
Panel B:	2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	-0.0553	-0.0238	0.00212	-0.0163	0.00869	-0.0273	0.0132
		(0.0448)	(0.0231)	(0.0132)	(0.0287)	(0.0172)	(0.0399)
(0.0463)							
r2	0.237	0.0413	0.0350	0.0417	0.0282	0.0839	0.0770
N	402264	478515	478515	474679	474679	474679	478515
N schools	6878	7393	7393	7387	7387	7387	7393
dependent mean	0.505	0.0227	0.00589	0.0502	0.0226	0.109	0.159
Panel C:	2014-2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	0.0145	-0.00331	0.00586	0.00524	0.0119	0.0120	0.0334
	(0.0279)	(0.0151)	(0.00838)	(0.0180)	(0.0117)	(0.0253)	(0.0348)
r2	0.256	0.0439	0.0412	0.0524	0.0310	0.0767	0.0956
N	1544625	1883484	1883484	1866928	1866928	1866928	1883484
N schools	7808	8116	8116	8115	8115	8115	8116
dependent mean	0.505	0.0288	0.00684	0.0512	0.0222	0.113	0.179

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Panel A: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 that took the baseline test (2014 to 2016 is the years they should take the test in 4th and 6th grade).
Panel B: Natives students test enrolled in 3th and 5th grade in 2017 (2018 is the years they should take the test in 4th and 6th grade).
Panel C: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 and 2017 (2014 to 2016 and 2018 is the years they should take the test in 4th and 6th grade).
Cog pc rank is an average of read and math test score. Muni change (non adj) show if student attended a different (non adjacent) municipality the year after the baseline year. Attrition show if a student was at the baseline but did not attend the test day.
Controlling by school-year and grade-type of school-year fixed effect.

Table 9: Balance test using across-class variation for 4th and 6th grade students separately

Panel A:	4th grade				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac	-0.106***	-0.0349*	0.00778	-0.0600	-0.000710
	(0.0307)	(0.0204)	(0.0211)	(0.0533)	(0.000514)
r2	0.169	0.507	0.438	0.0852	0.0236
N	500446	488846	488433	500446	500446
N schools	2220	2220	2220	2220	2220
dependent mean	0.533	0.556	0.537	0.504	0.0000300
Panel B:	6th grade				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac	-0.127***	-0.0856***	-0.0657***	-0.0754	0.0000844
	(0.0376)	(0.0199)	(0.0208)	(0.0671)	(0.000614)
r2	0.249	0.523	0.447	0.0916	0.0197
N	489610	483182	482835	489610	489610
N schools	2248	2248	2248	2248	2248
dependent mean	0.537	0.534	0.518	0.507	0.0000388

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Panel A: Natives students enrolled in 3th grade from 2013 to 2016 that took the baseline test (they should take the test in 4th grade from 2014 to 2017).
Panel B: Natives students enrolled in 5th grade from 2013 to 2015 and 2017 that took the baseline test (they should take the test in 6th grade from 2014 to 2016 and 2018).
Controlling by school-year fixed effect.
Baseline test is the test in 2nd and 4th grade for 4th and 6th grade students respectively. Income and mother is the household income and mother level of education declared transformed to percentile rank. Girl is a gender dummy if student is girl. Repeat is a dummy if students attended the same grade (3th and 5th) the year before the baseline year.

Table 10: Balance test using across-class variation: pool. Discarding non random allocation according to baseline and immigrant status.

Panel A:	4th grade				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac	-0.0454	-0.0209	0.0151	-0.0421	-0.00101
	(0.0324)	(0.0237)	(0.0254)	(0.0704)	(0.000743)
r2	0.170	0.513	0.442	0.0863	0.0236
N	446247	435839	435454	446247	446247
N schools	2188	2188	2188	2188	2188
dependent mean	0.534	0.560	0.541	0.503	0.0000336
Panel B:	6th grade				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
Frac	-0.0596*	-0.0839***	-0.0649**	-0.111	0.000138
	(0.0356)	(0.0247)	(0.0260)	(0.0796)	(0.000932)
r2	0.252	0.535	0.457	0.0935	0.0197
N	419152	413668	413369	419152	419152
N schools	2204	2204	2204	2204	2204
dependent mean	0.540	0.541	0.524	0.507	0.0000406

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Natives students enrolled in 3th grade from 2013 to 2016 that took the baseline test (they should take the test in 4th grade from 2014 to 2017).

Panel B: Natives students enrolled in 5th grade from 2013 to 2015 and 2017 that took the baseline test (they should take the test in 6th grade from 2014 to 2016 and 2018).

Controlling by school-year fixed effect.

Baseline test is the test in 2nd and 4th grade for 4th and 6th grade students respectively. Income and mother is the household income and mother level of education declared transformed to percentile rank. Girl is a gender dummy if student is girl. Repeat is a dummy if students attended the same grade (3th and 5th) the year before the baseline year.

Classes with non-random allocation of immigrants and students baseline tests were excluded.

Table 11: Impact on cog score using across-class variation

Panel A:	4th grade						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	0.0268	-0.00442	0.0102	0.0380	0.0224	0.0770**	0.0561
	(0.0277)	(0.0134)	(0.00688)	(0.0261)	(0.0187)	(0.0371)	(0.0389)
r2	0.667	0.216	0.126	0.135	0.120	0.147	0.179
N	431301	486644	486644	484309	484309	484309	486644
N schools	2220	2220	2220	2220	2220	2220	2220
dependent mean	0.550	0.0176	0.00218	0.0435	0.0192	0.0909	0.114
Panel B:	6th grade						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	0.00677	0.00520	0.0109	-0.00654	0.00137	0.0332	0.0453
	(0.0247)	(0.0153)	(0.00971)	(0.0224)	(0.0151)	(0.0353)	(0.0438)
r2	0.748	0.219	0.133	0.135	0.119	0.165	0.190
N	421833	481542	481542	479176	479176	479176	481542
N schools	2247	2248	2248	2248	2248	2248	2248
dependent mean	0.552	0.0224	0.00296	0.0396	0.0176	0.0837	0.124

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
4th grade: Natives students enrolled in 3th grade that took the baseline test for years 2013 to 2016.
6th grade: Natives students enrolled in 5th grade that took the baseline test for years 2013 to 2015 and 2017.
Cog pc rank is an average of read and math test score from years 2014 to 2017 for 4th grade and from 2014 to 2016 and 2018 for 6th grade. Muni change (non adj) show if student attended a different (non adjacent) municipality the year after the baseline year. Attrition show if a student was at the baseline but did not attend the test day.
Controlling by school-year fixed effect interacting with baseline, mother education, income, GPA and gender.
Classes with non-random allocation of immigrants and students baseline tests were excluded.

Table 12: Impact on cog score using across-class variation. Discarding non random allocation according to baseline and immigrant status.

Panel A:	4th grade						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	0.0349	-0.0130	0.00788	0.0384	0.0116	0.0767*	0.00910
	(0.0348)	(0.0162)	(0.00849)	(0.0306)	(0.0198)	(0.0438)	(0.0480)
r2	0.666	0.219	0.126	0.138	0.121	0.148	0.180
N	384585	433847	433847	431791	431791	431791	433847
N schools	2188	2188	2188	2188	2188	2188	2188
dependent mean	0.550	0.0176	0.00219	0.0436	0.0191	0.0911	0.114
Panel B:	6th grade						
	(1)	(2)	(3)	(4)	(5)	(6)	
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
Frac	-0.0171	0.000412	0.00226	-0.0242	-0.00644	0.00101	0.0337
	(0.0306)	(0.0194)	(0.0100)	(0.0282)	(0.0188)	(0.0401)	(0.0545)
r2	0.749	0.221	0.135	0.134	0.121	0.167	0.192
N	361190	412242	412242	410181	410181	410181	412242
N schools	2203	2204	2204	2204	2204	2204	2204
dependent mean	0.556	0.0222	0.00297	0.0396	0.0177	0.0839	0.124

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
4th grade: Natives students enrolled in 3th grade that took the baseline test for years 2013 to 2016.
6th grade: Natives students enrolled in 5th grade that took the baseline test for years 2013 to 2015 and 2017.
Cog pc rank is an average of read and math test score from years 2014 to 2017 for 4th grade and from 2014 to 2016 and 2018 for 6th grade.
Muni change (non adj) show if student attended a different (non adjacent) municipality the year after the baseline year. Attrition show if a student was at the baseline but did not attend the test day.
Controlling by school-year fixed effect interacting with baseline, mother education, income, GPA and gender.
Classes with non-random allocation of immigrants and students baseline tests were excluded.

Table 13: Change of natives on change of immigrants controlling by birth

	(1)	(2)	(3)	(4)
		1st stage		
$\Delta F^{\widehat{w_1-w_2}}pc$	0.794***		0.795***	
	(0.0245)		(0.0261)	
$\Delta F^{\widehat{w_1-w_2}}$		0.310***		0.467***
		(0.0267)		(0.0223)
r2	0.623	0.186	0.691	0.508
N	667	667	539	539
F	1048.5	135.5	925.9	437.5
		OLS		
$\Delta F^{w_1-w_2}pc$	0.0446**		0.0354**	
	(0.0208)		(0.0156)	
$\Delta F^{w_1-w_2}pc \times post$	-0.0438**		-0.0674***	
	(0.0201)		(0.0195)	
$\Delta F^{w_1-w_2}$		-0.152		0.0942
		(0.138)		(0.151)
$\Delta F^{w_1-w_2} \times post$		-0.193		-0.733***
		(0.136)		(0.186)
r2	0.449	0.456	0.616	0.627
N	667	667	539	539
		Reduced		
$\Delta F^{\widehat{w_1-w_2}}pc$	0.0247		0.0232*	
	(0.0199)		(0.0134)	
$\Delta F^{\widehat{w_1-w_2}}pc \times post$	-0.0494**		-0.0516***	
	(0.0197)		(0.0198)	
$\Delta F^{\widehat{w_1-w_2}}$		0.0185		0.0467
		(0.0928)		(0.0845)
$\Delta F^{\widehat{w_1-w_2}} \times post$		-0.309***		-0.260**
		(0.0922)		(0.127)
r2	0.447	0.456	0.613	0.612
N	667	667	539	539
		2sls		
$\Delta F^{w_1-w_2}pc$	0.0308		0.0281*	
	(0.0248)		(0.0168)	
$\Delta F^{w_1-w_2}pc \times post$	-0.0622**		-0.0654***	
	(0.0253)		(0.0229)	
$\Delta F^{w_1-w_2}$		0.0578		0.0972
		(0.308)		(0.190)
$\Delta F^{w_1-w_2}pc \times post$		-1.029*		-0.569***
		(0.569)		(0.189)
r2	0.00433	-0.0466	0.0220	0.0475
N	667	667	539	539

Pre and post treatment period from 2007 to 2013 and from 2013 to 2019, respectively.
Columns 1 and 2 specification at municipality level and columns 3 and 4 at city level.
Column 1 and 3 define treatment as municipality percentile rank of the change of immigrants from 2013 to 2019.
Column 2 and 4 define treatment as municipality change of immigrants from 2013 to 2019.
Regression weighted by population in 2007 and standard errors clustered at municipality or city level (when applicable).

Table 14: Change of natives in private schools on change of immigrants

	(1)	(2)	(3)	(4)
		1st stage		
$\Delta F^{\widehat{w_1 - w_2}} pc$	0.804***		0.842***	
	(0.0244)		(0.0255)	
$\Delta F^{\widehat{w_1 - w_2}}$		0.323***		0.493***
		(0.0261)		(0.0230)
r2	0.613	0.183	0.668	0.459
N	689	689	545	545
F	1086.1	153.5	1092.5	460.7
		OLS		
$\Delta F^{w_1 - w_2} pc$	0.0105		0.0125***	
	(0.00679)		(0.00410)	
$\Delta F^{w_1 - w_2} pc \times post$	0.0406***		0.0335***	
	(0.0104)		(0.00717)	
$\Delta F^{w_1 - w_2}$		0.00455		0.132***
		(0.0301)		(0.0488)
$\Delta F^{w_1 - w_2} \times post$		0.122		0.183***
		(0.0919)		(0.0649)
r2	0.147	0.0890	0.401	0.337
N	689	689	545	545
		Reduced		
$\Delta F^{\widehat{w_1 - w_2}} pc$	0.0131*		0.0122***	
	(0.00666)		(0.00428)	
$\Delta F^{\widehat{w_1 - w_2}} pc \times post$	0.0274**		0.0289***	
	(0.0116)		(0.00675)	
$\Delta F^{\widehat{w_1 - w_2}}$		0.0256		0.0715*
		(0.0382)		(0.0379)
$\Delta F^{\widehat{w_1 - w_2}} \times post$		0.00106		0.0564
		(0.0819)		(0.0426)
r2	0.121	0.0785	0.359	0.270
N	689	689	545	545
		2sls		
$\Delta F^{w_1 - w_2} pc$	0.0164*		0.0145***	
	(0.00841)		(0.00446)	
$\Delta F^{w_1 - w_2} pc \times post$	0.0343**		0.0344***	
	(0.0143)		(0.00916)	
$\Delta F^{w_1 - w_2}$		0.0824		0.150***
		(0.123)		(0.0525)
$\Delta F^{w_1 - w_2} pc \times post$		0.00341		0.118
		(0.263)		(0.0833)
r2	0.0753	0.00696	0.219	0.135
N	689	689	545	545

Pre and post treatment period from 2007 to 2013 and from 2013 to 2019, respectively.
Columns 1 and 2 specification at municipality level and columns 3 and 4 at city level.
Column 1 and 3 define treatment as municipality percentile rank of the change of immigrants from 2013 to 2019.
Column 2 and 4 define treatment as municipality change of immigrants from 2013 to 2019.
Regression weighted by population in 2007 and standard errors clustered at municipality or city level (when applicable).

Table 15: Change of school segregation (Theil index) on change of immigrants

	(1)	(2)	(3)	(4)
		1st stage		
$\Delta F^{\hat{w_1-w_2}} pc$	0.804***		0.842***	
	(0.0244)		(0.0255)	
$\Delta F^{\hat{w_1-w_2}}$		0.323***		0.493***
		(0.0261)		(0.0230)
r2	0.613	0.183	0.668	0.459
N	689	689	545	545
F	1086.1	153.5	1092.5	460.7
		OLS		
$\Delta F^{w_1-w_2} pc$	-0.00109		0.00176	
	(0.00272)		(0.00225)	
$\Delta F^{w_1-w_2} pc \times post$	0.0235***		0.0334***	
	(0.00488)		(0.00337)	
$\Delta F^{w_1-w_2}$		-0.0788		-0.127
		(0.0882)		(0.187)
$\Delta F^{w_1-w_2} \times post$		0.00248		1.147***
		(0.154)		(0.294)
r2	0.385	0.340	0.636	0.550
N	689	689	545	545
		Reduced		
$\Delta F^{\hat{w_1-w_2}} pc$	0.000361		0.00358	
	(0.00298)		(0.00250)	
$\Delta F^{\hat{w_1-w_2}} pc \times post$	0.0279***		0.0361***	
	(0.00554)		(0.00363)	
$\Delta F^{\hat{w_1-w_2}}$		-0.105		-0.0206
		(0.0741)		(0.114)
$\Delta F^{\hat{w_1-w_2}} \times post$		0.433***		0.559*
		(0.160)		(0.311)
r2	0.408	0.354	0.661	0.534
N	689	689	545	545
		2sls		
$\Delta F^{w_1-w_2} pc$	0.000450		0.00426	
	(0.00370)		(0.00298)	
$\Delta F^{w_1-w_2} pc \times post$	0.0347***		0.0429***	
	(0.00730)		(0.00506)	
$\Delta F^{w_1-w_2}$		-0.325		-0.0417
		(0.298)		(0.229)
$\Delta F^{w_1-w_2} pc \times post$		1.340		1.135***
		(0.908)		(0.397)
r2	0.0465	-0.143	0.219	0.0706
N	689	689	545	545

Pre and post treatment period from 2010 to 2013 and from 2013 to 2019, respectively.
Columns 1 and 2 specification at municipality level and columns 3 and 4 at city level.
Column 1 and 3 define treatment as municipality percentile rank of the change of immigrants from 2013 to 2019.
Column 2 and 4 define treatment as municipality annually change of immigrants from 2013 to 2019.
Dependent variable rescale for annually change in segregation index.
Regression weighted by population in 2007 and standard errors clustered at municipality or city level (when applicable).

Table 16: Change of school segregation (Atkinson index) on change of immigrants

	(1)	(2)	(3)	(4)
		1st stage		
$\Delta F^{\widehat{w_1-w_2}} pc$	0.804***		0.842***	
	(0.0244)		(0.0255)	
$\Delta F^{\widehat{w_1-w_2}}$		0.323***		0.493***
		(0.0261)		(0.0230)
r2	0.613	0.183	0.668	0.459
N	689	689	545	545
F	1086.1	153.5	1092.5	460.7
		OLS		
$\Delta F^{w_1-w_2} pc$	0.00327		0.00769**	
	(0.00405)		(0.00340)	
$\Delta F^{w_1-w_2} pc \times post$	0.0130**		0.0236***	
	(0.00584)		(0.00441)	
$\Delta F^{w_1-w_2}$		-0.0247		0.0506
		(0.132)		(0.296)
$\Delta F^{w_1-w_2} \times post$		-0.203		0.695**
		(0.183)		(0.282)
r2	0.395	0.381	0.621	0.567
N	688	688	544	544
		Reduced		
$\Delta F^{\widehat{w_1-w_2}} pc$	0.00656		0.0101***	
	(0.00428)		(0.00363)	
$\Delta F^{\widehat{w_1-w_2}} pc \times post$	0.0142**		0.0274***	
	(0.00640)		(0.00496)	
$\Delta F^{\widehat{w_1-w_2}}$		0.0621		0.0794
		(0.0833)		(0.172)
$\Delta F^{\widehat{w_1-w_2}} \times post$		-0.140		0.296
		(0.126)		(0.268)
r2	0.406	0.379	0.646	0.561
N	688	688	544	544
		2sls		
$\Delta F^{w_1-w_2} pc$	0.00816		0.0120***	
	(0.00535)		(0.00428)	
$\Delta F^{\widehat{w_1-w_2}} pc \times post$	0.0177**		0.0325***	
	(0.00801)		(0.00676)	
$\Delta F^{\widehat{w_1-w_2}}$		0.192		0.161
		(0.281)		(0.353)
$\Delta F^{\widehat{w_1-w_2}} \times post$		-0.433		0.601
		(0.454)		(0.439)
r2	0.0161	0.000454	0.121	0.0265
N	688	688	544	544

Pre and post treatment period from 2010 to 2013 and from 2013 to 2019, respectively.
Columns 1 and 2 specification at municipality level and columns 3 and 4 at city level.
Column 1 and 3 define treatment as municipality percentile rank of the change of immigrants from 2013 to 2019.
Column 2 and 4 define treatment as municipality annually change of immigrants from 2013 to 2019.
Dependent variable rescale for annually change in segregation index.
Regression weighted by population in 2007 and standard errors clustered at municipality or city level (when applicable).

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Appendix A Exposure effects

This section is based on Chetty and Hendren (2018a) paper. Exposure effect is the impact for moving to a neighborhood where permanent residents are 1 percentile point higher. Although the exposure effect will only be a noisy proxy of the neighborhood effect will serve us to evaluate how relevant student outcome varies when they exposed different period of time. This approach will also allow us to test some of the necessary assumptions to estimate the municipality effect.

Following equation 5 from Chetty and Hendren (2018a)[32]. Exposure effect is estimated as follows:

$$\begin{aligned}\Delta_{odps} &= \bar{y}_{pds} - \bar{y}_{pos} \\ y_i &= \alpha_{qosm} + \sum_{m=1}^{m=6} b_m \mathbf{I}(m_i = m)\Delta_{odps} + \theta_i\end{aligned} \tag{11}$$

Where $\bar{y}_{pds}$ is the predicted educational outcome of permanent resident students based on parents characteristics p municipality of residence d and cohort s, and $\bar{y}_{pos}$ is the predicted educational outcome of permanent resident students based on parents characteristics p municipality of residence o and cohort s. So the Δ_{odps} sign and magnitude will reflect the idea of moving to a better or worse neighborhood. In addition, y_i is educational outcome of mover student i, α_{qosm} is the fixed effect of parent characteristics q, origin o, cohort s and movement at grade m, and $\mathbf{I}(m_i = m)$ is a dummy that takes value of one if the student i moves in grade m. It is important to note that b_m varies by m so it will give us the notion of whether there are grades in which students are more sensitive to change -that can be a disruption effect as well as a treatment effect-.

We have to consider that families that move to better or worse neighborhoods are self-selected and therefore will be different in unobservables. As mentioned earlier this should not be a problem if we observe variation across age at move. The rate at which the exposure effect changes by m (henceforth convergence rate) will be unbiased under the assumption that family and students characteristics do not vary with grade at movement (assumption 4). This can be formalized as follow:

$$b_m = \frac{cov(\Delta_{odps}, y_i)}{var(\Delta_{odps})} = \beta_m + \frac{cov(\theta_i, y_i)}{var(\Delta_{odps})}$$

If we assume that $\frac{cov(\theta, y_i)}{var(\Delta_{odps})}$ do not vary with child's grade at move, then:

$$\gamma_m = \beta_{m+1} - \beta_m = b_{m+1} - b_m$$

The result of estimating equation 11 and drawing the coefficients b_m using cog score rank and mother education rank as parent characteristics can be found in figure B.1. In figure **??** I do the same but now controlling by IVE-SINAE as parent characteristics. As you can see the coefficients fall at a linear rate of approximately 9% per exposure year, ie. convergence rate of 9%. In addition, we can see that after the test the coefficients stabilize showing that around 30% is the selection effect. In Chetty and Hendren (2018a) paper they find a convergence rate of 4%, which is lower than the number I find. This difference can be explained because my convergence rate represent the effect of spending 1 year out of 10[33] in a neighborhood while in Chetty and Hendren (2018a) their convergence rate represent 1 year out of 23. So if I adjust my results to make them comparable I get $\frac{9\% \times 10}{23} = 3.9\%$, which is similar.

The result of estimating equation 11 and drawing the coefficients b_m using dropout and IVE-SINAE as parent characteristics can be found in figure **??**. In this case because the outcome is dropout I do not have observations after the test grade. As you can see the coefficients fall at a linear rate of approximately 7% per exposure year. If we scale this number with the age of the students - 1 out of 14 years- we obtain a convergence rate of 4.3% which is consistent with my findings with 4th grade students cog test score.

The linearity of the convergence rate is evidence to say that the neighborhood effect is likely to be additive and constant across grades, and disruptive effect is independent of the grade of the student.

[32] I do not add the correction of varying the treatment effect by cohort since my ability to know which school Municipality each student attended does not vary by cohort.

[33] Test score at 4th grade is on average around 10 years old.

Additionally, to gather more evidence about my identification assumption I will provide an overidentification and displacement shock test that I will explain below.

The overidentification test consists of performing placebos by varying the student's membership group. In other words, it is to be expected that a student who moves converges more to the stayers of the same subgroup of belonging than to another subgroup. It is unlikely that these patterns can be replicated by ommited variable and selection models, assuming that parents do not handle specific information about differences between subgroups to make the decision to move. To perform the overidentification test I exploit subgroup membership according to cohort with the following specification:

$$\text{Simultaneous:} \qquad y_i = \alpha_{opsm} + \sum_{s'=s-4}^{s+4} \sum_{m=1}^{6} I(m_i = m)\beta_m \Delta_{odps'} + \epsilon_i$$

$$\text{Separate:} \qquad y_i = \alpha_{opsm} + \sum_{m=1}^{6} I(m_i = m)\beta_m \Delta_{odps'} + \epsilon_i \ \forall \ s' = \{s-4, .., s+4\}$$

Both are variation of equation 11. Simultaneous add $\Delta_{odps'}$ estimated with stayers from 4 years before to 4 years after. Separate replace Δ_{odps} with $\Delta_{odps'}$ estimated with stayer from 4 years before to 4 years after.

Figure B.2 shows the convergence rate performing the equations simultaneously and separately. As you can see in panel A when you do the equation separately the convergence rate shrink as I move away from my belonging subgroup, because there is serial correlation it does not go to zero. On the other hand, in panel B you can see that if I do it simultaneously the students that move converge only to the stayers of their own cohort, being the other cohorts less relevant. This is evidence in favor of the identification assumption (4) because is unlikely that families select into neighborhoods knowing the different outcomes across cohorts.

One way to address the problem of endogeneity in the location of families is to use displacement shock. It is to be expected that those who are exposed to displacement shock (natural disaster, closure of one factory or another) will move for exogenous reasons. Since I do not have data to identify all the displacement shocks in Chile, I will identify displacement shocks as abnormal movements within each municipality. For this I construct a displacement shock indicator where I take the number of students exiting from each municipality in each year and divide it by the average exits across years of each municipality. After ranked this indicator I can see that Chaiten in 2008 (Volcano) led this indicator, which means that I am capturing external shocks. These external shocks are exogenous reasons to leave but the neighborhood to go is still endogenous. For this reason I will instrumentalize the gap with the average gap of all the students who moved during a displacement shock -instrumentalize Δ_{odps} with $E(\Delta_{odps}|c, p)$-. Finally my test consists in reducing the sample according to the distribution percentile of my displacement shock indicator and calculating the convergence rate. It is to be expected that the more restrictive is my sample, the more "pure" will be my definition of displacement shock, but I will lose observations, so my standard errors will increase. Figure B.3 shows this exercise. As you can see the convergence rate is quite stable until the end of the distribution.

Appendix B Municipality peer effect: Across-cohort variation

In this lines I will describe the empirical approach and the results of using across-cohort variation strategy within municipality. In practice, this approach estimates the impact on students outcomes in 4th and 6th grade, given that they were exposed differently to immigrants at the beginning of $t-1$: 3rd and 5th grades respectively, controlling by their baseline test score at the end of school year t-2: 2nd and 4th grade respectively. In other words is like an RCT where students do a baseline test at the end of grade g then students are treated if they have higher fraction of immigrants in their cohort compare to the other cohort at the beginning of grade $g+1$ and then I test the effect on them at the end of grade $g+2$. Students characteristics may differ according to the municipality they attend. Also, students may have different educational outcomes related to the grade they attend, eg. repetition probability is increasing with grade. To take into account this heterogeneity I add a fixed effect of municipality/year and a fixed effect of grade/type of municipality/year interacting with students characteristics as follow:

$$g_{jsgt}(X, y^B) = S^0_{jt} + S^1_{jt}X + S^2_{jt}y^B + G^0_{gst} + G^1_{gst}X + G^2_{gst}y^B_i$$

Where S_{jt} is a municipality- year fixed effect indicating municipality j and year t. G_{gst} is a grade - type of school - year fixed effect indicating grade g type of school s and year t, The types of school are: private without voucher, private with fees on top of the voucher, private voucher and public schools. y^B is the baseline test score and X is a vector of students characteristics.
Thus the specification to exploit differences across-cohorts is:

$$y_i = \beta_0 + \beta_1 Frac_{gjt} + g_{jsgt}(X_i, y^B_i) + n_{gjt} + \epsilon_i \tag{12}$$

Where y_i is the outcome of a native student i, $Frac_{gjt}$ is the fraction of foreign students in year t in municipality j in grade g as a proportion of students in the same cell, n_{gjt} is the number of students in grade g in school j in year t. I added the number of students as a proxy of class-size.

From the calendar in table 6 we can observe that not all the grades have baseline for all years. To compare 4th and 6th grade we can use years from 2014 to 2016 and 2018. I can not use, however, the year 2018 if I want to control by baseline test. So I will provide results focusing on 2014 to 2016 and then separately results for 2018. The reason why I am determined to use 2018 is that most of the variation (new arrivals) comes from year 2015 so I will exploit little variation if I exclude this year from the analysis.

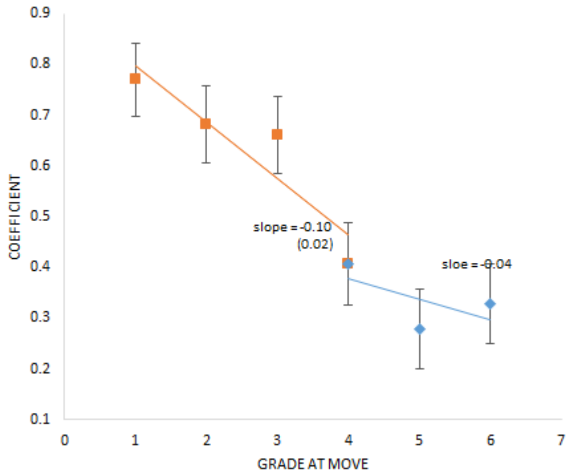
The strategy of equation 12 relies on independence between unobservable characteristics ϵ_i and treatment $Frac_{gjt}$ within schools/municipality. One threat to this assumption is if native students react early to more immigrants in the same grade and leave early: before baseline year. I can test if this threat hold observing if the baseline test and students characteristics are differential according to the fraction of immigrants. Continuing with RCT analogy this would be like a balance test. Panel A of table 17 shows that fraction of immigrants is not differential for the baseline test, household income rank, mother education rank, girl and repeat the last year when pooling students from 2014 to 2016. The point estimates, however, are very big so I will control by this characteristics when showing the results. Number of observations differ because non-response in questionnaire (income and mother education). This is not problematic because the level of non-response is 2% and is not differential. Panel B of table 17 provides balance test for year 2018 only. Variation within municipality level shows non differential composition given the fraction of immigrants natives students face in their cohort. The questionnaire non-response is higher for this case because I do not restrict the sample to those that answer the baseline test: questionnaire non-response is 21% but is not differential. Panel C in table 17 shows the balance test from 2014 to 2018. As expected, variation within municipality and school level show non differential composition given the fraction of immigrants natives students face in their cohort. The questionnaire non-response is higher for this case because I do not restrict the sample to those that answer the baseline test: questionnaire non-response is 8%[34] but is not differential.

Table 18 show results when exploiting variation within municipality controlling by students characteristics as shown in equation 11. It seems that higher fraction of immigrants in your cohort within municipality decrease your probability of change of school or municipality for all years (from panel A to C). The movement to other municipalities, however, is not significant when we discard movement to other municipality that may not imply change of neighborhood, ie. moving to adjacent municipality or within RM. The fact that natives that are treated are moving less implies that the effect on cog pc rank, repeat and dropout may be driven by selection.

[34]The level of non-response when grouped from 2014 to 2018 (8%) is lower compared to 2018 (21%) the reason for this difference is because 2018 does not have a baseline questionnaire so I can only observe the characteristics of the students once and not twice as the rest of the years.

Figure B.1: Exposure effects on test scores in 4th grade controlling by mother level of education

(a) Semi-parametric estimates



(b) Parametric estimates

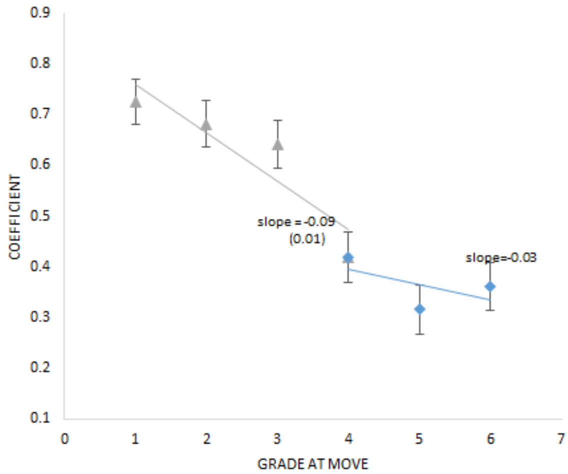


Figure B.2: Convergence rate of 4th grade test scores estimates based on cross-cohort variation

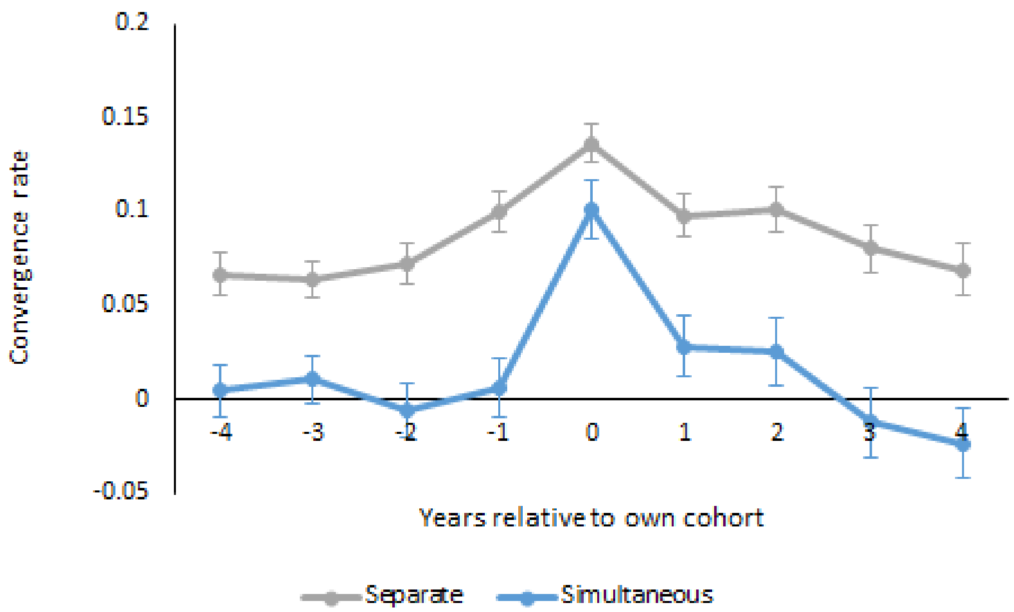


Figure B.3: Convergence rate of 4th grade test scores estimates using displacement shocks

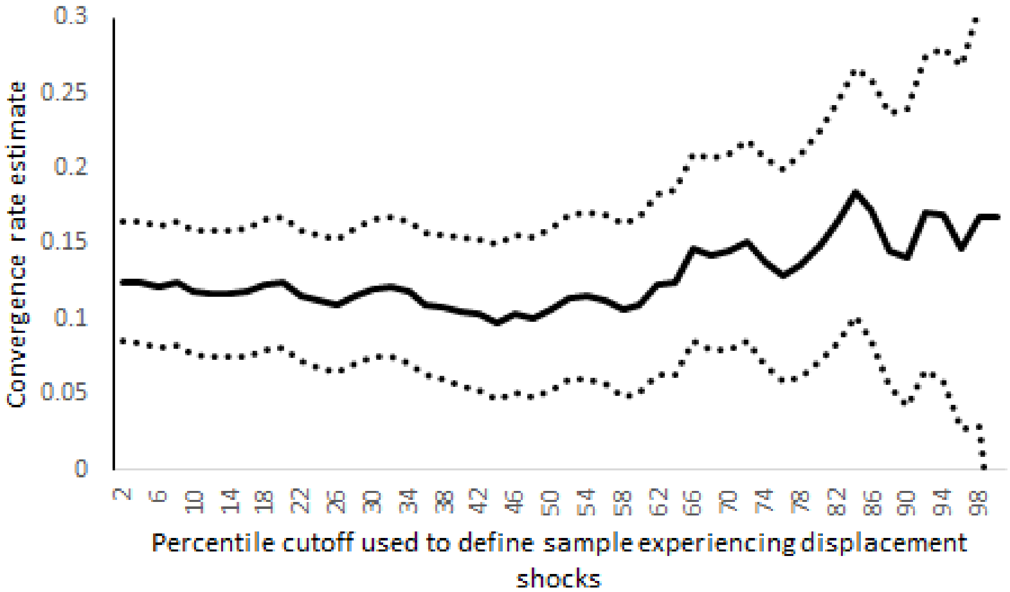


Table 17: Balance test using across-cohort at municipality level for students in 4th and 6th grade

Panel A:	2014-2016				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
FRAC	-0.0312	0.126	0.0226	0.147	0.00211
	(0.183)	(0.113)	(0.112)	(0.196)	(0.00619)
r2	0.101	0.395	0.325	0.00148	0.00101
N	1170332	1148985	1147821	1170332	1170332
dependent mean	0.506	0.494	0.487	0.498	0.0000436
Panel B:	2018				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
FRAC		0.208	0.0835	0.0312	-0.0546
		(0.129)	(0.122)	(0.208)	(0.0669)
r2		0.332	0.279	0.00152	0.00939
N		384018	382133	478829	478829
dependent mean		0.447	0.435	0.489	0.0252
Panel C:	2014-2018				
	(1)	(2)	(3)	(4)	(5)
	baseline	income rank	mother rank	girl	repeat (baseline)
FRAC		0.106	0.0535	0.143	0.0132
		(0.0825)	(0.0790)	(0.127)	(0.0526)
r2		0.383	0.320	0.00156	0.0133
N		1736590	1733504	1884779	1884779
dependent mean		0.470	0.462	0.486	0.0344

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Panel A: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 that took the baseline test (2014 to 2016 is the years they should take the test in 4th and 6th grade).
Panel B: Natives students test enrolled in 3th and 5th grade in 2017 (2018 is the years they should take the test in 4th and 6th grade).
Panel C: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 and 2017 (2014 to 2016 and 2018 is the years they should take the test in 4th and 6th grade).
Controlling by municipality-year fixed effect and grade - type of school -year fixed effect.
Baseline test is the test in 2nd and 4th grade for 3th and 5th grade students, respectively. Income and mother is the household income and mother level of education declared transformed to percentile rank. Girl is a gender dummy if student is girl. Repeat is a dummy if students attended the same grade (3th and 5th) the year before the baseline year.

Table 18: Impact on educational outcomes using across-cohort at municipality level for 4th and 6th grade students

Panel A:	2014-2016						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
FRAC	-0.0536	-0.00272	-0.00236	-0.266**	-0.158*	-0.214	0.166
	(0.172)	(0.0710)	(0.0276)	(0.105)	(0.0888)	(0.154)	(0.227)
r2	0.571	0.0367	0.00544	0.0143	0.0102	0.0162	0.0486
N	989252	1147849	1147849	1142025	1142025	1142025	1147849
ymean	0.522	0.0230	0.00267	0.0461	0.0277	0.102	0.138
Panel B:	2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
FRAC	-0.261	-0.139**	0.0119	-0.200*	-0.101	-0.299**	-0.183
	(0.223)	(0.0709)	(0.0339)	(0.109)	(0.0780)	(0.137)	(0.201)
r2	0.118	0.00618	0.00310	0.0101	0.00477	0.0107	0.0194
N	402553	478829	478829	474996	474996	474996	478829
dependent mean	0.505	0.0227	0.00589	0.0503	0.0226	0.109	0.159
Panel C:	2014-2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cog rank	repeat	dropout	muni change	muni change (non adj)	school change	attrition
FRAC	0.0406	-0.00848	-0.00201	-0.184**	-0.111*	-0.186*	-0.00221
	(0.156)	(0.0491)	(0.0201)	(0.0790)	(0.0640)	(0.107)	(0.144)
r2	0.453	0.0570	0.0152	0.0164	0.0112	0.0229	0.100
N	1468965	1701802	1701802	1692733	1692733	1692733	1701802
ymean	0.508	0.0216	0.00308	0.0480	0.0290	0.106	0.137

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Panel A: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 that took the baseline test (2014 to 2016 is the years they should take the test in 4th and 6th grade).
Panel B: Natives students test enrolled in 3th and 5th grade in 2017 (2018 is the years they should take the test in 4th and 6th grade).
Panel C: Natives students test enrolled in 3th and 5th grade from 2013 to 2015 and 2017 (2014 to 2016 and 2018 is the years they should take the test in 4th and 6th grade).
Cog pc rank is an average of read and math test score. School change show if student attended a different school the year after the baseline year. Muni change (non adj) show if student attended a different (non adjacent) municipality the year after the baseline year. Attrition show if a student was at the baseline but did not attend the test day.
Controlling by municipality-year and grade-type of school-year fixed effect.