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Can immigration affect skills based neighborhood effect? Lessons from Chile

Using the recent migratory phenomenon in Chile this paper evaluates the impact of exposure to foreign students on causal municipality effect. I do so by estimating municipality's causal effect on children's test scores rank at 4th grade (10 years old) in two windows: before and after the large wave of immigrants. Following Chetty and Hendren's (2018) methodology I estimate each municipality' effect using a fixed effect regression model identified by students who move across municipalities at different ages. I found that on average there is a negative effect of foreign students arrival on municipality effect. My estimation suggest that an increase of 1 standard deviation of immigrant students, lowered students test scores by 1.8 percentiles rank per each year spend. This effect can not be explained by peers effect at the school level, and it is likely driven by segregation induced by natives flight.

Juan Diego Luksic[1]

Paris School of Economics

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[1]Electronic address: `juan.luksic@psemail.eu`

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1 Introduction

Migratory waves can change the environment of destination neighbourhoods. Literature point out that the arrival of immigrants can cause natives to move to places with fewer immigrants[1], choose private schools (Betts and Fairlie (2003),Farré et al. 2015, Murray 2016), decrease preferences for redistribution (Alesina et al. (2018),Alesina et al. 2019), participation in social groups (Alesina and La Ferrara (2000) and trust in neighbours (Putnam (2007),Alesina and Ferrara (2000))[2]. On the other hand, recent literature estimate the determinants 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). Also, one of the most important lessons from the experiment Moving to Opportunity is that individuals are mostly affected to neighborhood quality if exposed during childhood. Recently, Ellora Derenoncourt JMP link these two literature by showing that the Great Migration in the USA had negative effects on social mobility due to native flights, increased policing and incarceration rate. Inspired by this idea, in this paper I want to assess whether recent immigration to Chile is changing the neighborhood quality. As this is a recent phenomenon I can not look directly to social mobility, but I can look at outcomes during childhood that correlate with future income. Thus, I will study how the effect of the neighborhood on test scores changed after the arrival of immigrants.

The estimation of change in the neighborhood effect product of the arrival of immigrants pose an identification challenge. At the same time that immigrants arrive, native flights occur: natives who move to neighborhoods with less immigrants. This sorting bias the neighborhood effects estimate if the strategy is based on comparing residents over time. In addition, as natives move away from immigrants this bias increases in time, therefore the comparison of the neighborhood effect in long term becomes more difficult. To solve this problem I will estimate neighborhood effects (Chetty and Hendren (2018b)) using movers. Exploiting the variation of the age of those who move between municipalities I will be able to estimates effects safe from sorting. In this way, under certain assumptions that I will describe below, I will be able to estimate unbiased estimates of municipality effect across time.

This paper evaluates the causal effect of the municipality on child learning before and after immigrants arrive using the recent migratory phenomenon in Chile. I do so by estimating municipality's causal effect on 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. 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 different age. I found that on average there is a negative effect of foreign students on municipality effect. My estimation suggest that a 1 percentile increase of immigrants, lowered municipality effect by 0.06 percentiles rank. I complement this result estimating municipality effects based on high school dropout. I find null result in this case. Finally, I show -using within school variation- that the negative effect on test score is not explained by peer effect at the school level. Conversely, it seems that natives flights and segregation can explain this results.

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-, the number of foreign students in each municipality per year, and high school dropout. I combine this dataset with test data to know children test score in 4th grade, and mother level of education. I complement this dataset with information at municipality level from censuses -2002 provide me the shares for shift-share strategy- and the govenmental database of municipal information (hencefort SINIM, Spanish acronym of the platform). I will only use those who made a neighborhood change for sure, so the sample is around 170 thousands students that move during primary school between 2004 and 2020.

[1]The literature have 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]This effect is attenuated when the natives maintain contact with the immigrant population.

The empirical strategy consists of three steps: 1) the estimation of municipality effects, then 2) the construction of the instrument for foreign students arrival and 3) the estimation of the impact of foreign students arrival on the changes in the municipality's effects. The estimation of municipality effects requires to show that spending time in a municipality determines test scores, otherwise we could just be exploiting noise. For this I will introduce the exposure effects from Chetty and Hendren (2018a) that evaluates the impact of moving to a neighborhood where permanent residents are 1 percentile point higher. When estimating the exposure effect I observe that each additional year in the neighborhood allows movers to converge to permanent residents by 9%, these results are comparable to those found in Chetty and Hendren (2018a) and means that the neighborhood effects are relevant to determine test scores. I estimate municipality effects comparing movers of the same origin and destiny but that spend different period of time in each place. This identification requires that students potential outcome is orthogonal to the age at which the students move. I provide evidence for this assumption taking advantage of the short introduction of a 2nd grade test score and show that children moving at different grades do not differ in this baseline test score. Finally I estimate municipality effect and observe correlation with variables of interest.

The construction of the instrument of change in foreign students is done following a shift-share as is common in migration literature. 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. For this reason I will group the students according to family nationality and level of education and assume that the networks are stronger since they share the same nationality and have a similar educational level. This strategy seems to work since the instrument is quite predictive of the future location of immigrants although I cannot rule out that my exclusion restriction will be met. In this line, I provide a placebo tests to show that at least my instrument is not related to any trend before immigrants arrival.

The estimation of the impact of foreign students arrival on the changes in the municipality effects is done with a first difference using with municipal estimates before and after the arrival of immigrants. Since the increase in immigrants was rapid, this strategy has the advantage of being simple and sufficient to capture the effect. Since the increase of immigrants started to occur from 2013 onwards I will show results comparing the pre and post 2013 windows. Results show a negative impact on municipality effect of around -0.06 percentiles per one percentile change of immigrants. Thus, a 30 percentile greater increase in immigrants - approximately 1 standard deviation of the shock- will lowered children percentile score by 7.2 percentiles if they moved from 1st grade. Although it would be interesting to see years later the sample becomes smaller and the results more imprecise. This results are robust if I replace mother education by an administrative vulnerability index. When estimating municipality effects using dropout results are null.

Finally, this paper explores different explanations for the negative effect of immigrants on neighborhood effects. I study if there is any effect related to change in peer composition using within school variation comparing cohorts (Hoxby (2000), Gould et al. (2009)) and classes (Frattini and Meschi (2019),Ammermueller and Pischke (2009)). All my findings using both methods show little evidence of peer effect. Then, I observe the native flight and segregation using a DiD strategy and instrumenting immigrant students change with a shift-share. There is evidence of natives moving away and of natives sending their kids to private schools more often once immigrants arrive into a Municipality. This natives flight seems to increase segregation across schools. Because municipality effects are sensitive to school segregation this seems like a plausible venue.

In the following section I will describe the context in which this wave of immigration occurs, then in section 3 I will describe the data and variables to use for my analysis. In section 4 I will describe in detail the empirical approach, in section 5 I will show the results and in section 6 I will 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. 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 not only cause the emigration of their inhabitants, but also 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), as well as by 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 data bases. With the data of the census 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[3]. Censuses since 1982 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 2017 immigrants were concentrated in the northern regions (henceforth Norte Grande)[4] and in the center (henceforth Metropolitan region). Considering population over 25 years old, immigrants were more educated than Chileans (12.6 versus 11 years if education), though this 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 show similar dynamic. Figure 2 shows the enrollment of immigrants in primary school and their fraction as percentage of 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)[5]). Additionally, while I have no data to compare with other countries, the recent 2018 bicentennial survey[6] can be informative: 75% of Chileans believe that immigration is excessive and 44% believe that there is greater conflict with migrants (up from 38% the previous year). These levels are higher in the north of Chile - where more immigrants have arrived as a percentage of the population - than in the rest of the country.

Regarding natives 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 the arrival of migrants as a percentage of the Chilean population in 2017. As can be seen there is a high correlation (around 70%) between the arrival of foreigners and the departure of Chileans. This relation is not necessarily native flight - the natives decision to reduce interaction with immigrants- but shows that municipalities natives composition will change when immigrants arrive. I will discuss and provide evidence of natives flight in the school system in subsection 6.2.

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

[4]First 3 regions from north to south.

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

[6]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.

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[7]. School is compulsory for natives and foreigners even if the lately are undocumented. The Education Ministry facilitate 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 (increase of 50% of the school voucher), among others (Mora Olate (2018)).

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

3 Data

The core of my analysis is done with education administrative data spanning 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, foreign status, and IVE-SINAE. A social vulnerability index developed by JUNAEB, the governmental office that provides student assistance. This administrative data includes an student identifier that allows to follow them across time and match with the data of the testing service. 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[8]. The assessment consist of test scores in reading, math and history or natural science; and questionnaires for students, parents and teachers.

3.1 Variables of interest

With these database 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[9], I do not include other subjects because they are not consistent over years. These tests are calibrated and score with IRT and are comparable across years. This variable span from 2005 to 2018.
High school dropout: Binary variable based on administrative data that takes the value of 1 if a students was enrolled at the end of primary education (8th grade) but was not enrolled in the next three years of high school. Because this variable requires to match with data three years ahead the time span is from 2004 to 2017.
Mother years of education: Variable self-reported from parents questionnaire that report the number of years studied in formal education, it goes from 0 to 20 equivalent to no studies and PhD. This variable span from 2005 to 2018.
IVE-SINAE: This school 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). This variable spans from 2004 to 2020, however, because these variables change significantly before 2007, I will assign the IVE-SINAE in 2007 to the schools prior to this year. Because I am interested in the vulnerability of the student along their school life, I will assign to each student the average IVE-SINAE during all primary (from 1st to 8th grade).
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

[7]For details of the education system in Chile please see Hsieh and Urquiola (2006).
[8]For some years they also assess other grades but those are not yearly based.
[9]Though achievement test are one part of the cognitive ability - named crystallized intelligence (Roberts et al. (2000)) - for simplification I will call it cognitive.

use the change between 2013 to 2019 in immigrant students as a fraction of the total population:

$$\Delta F_c^{y_1-y_2} = \frac{F_c^{y_2} - F_c^{y_1}}{population_c^{y_1}} \tag{1}$$

Where $F_c^{y_1}$ is the number of foreign students in primary school in municipality c in year y_1 (2013), $F_c^{y_2}$ is the number of foreign students in primary school in municipality c in year y_2 (2019), and $population_c^{y_1}$ is the number of primary students in year y_1 in municipality c.

Finally, all the variables of interested are transformed to percentile ranks because the relation between outcomes (dropout, cog score) and controls (mother years of education and IVE-SINAE) are approximately linear if transformed to rank. Change of foreign students also is transformed to percentile ranks because impact on municipality effect and native flight is also approximately linear if transformed to rank.

3.2 Sample definitions

The dataset of students consists of all children that 1) were enrolled in primary school, 2) took the test in 4th grade between 2005 and 2017 or where enrolled in 8th grade between 2004 to 2017 (dropout), 3) are Chilean. I will focus on 4th grade because is the only grade where students were tested every year since 2005, and also because kids are more likely to enrol and live in the same municipality when younger. I will focus in school dropout of 8th grade students because this is the last grade of primary education so most dropout occurs after this grade. I restricted the data to Chileans because I will focus on the impact of foreign students on them. For reasons that I will discuss in the next section, I will divide the database between those who move and those who stay. Tables 1 and 2 shows in the panel A the characteristics of those who do not move and in the panel B those who do, for the sample of 4th grade and 8th grade students respectively. 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 are effectively a change in the neighborhood, I am going to remove from the sample those who move to adjacent municipalities. Because I need to be sure that students share same place I will remove those whoever were in a rural school. Finally, I will remove also those moving in 6th grade to "Liceos Emblematicos", prestigious high school because acceptance in those schools may push families to change of neighborhood - an important challenge to my main assumption that I will discuss after -. As a result, the panel C of tables 1 and 2 show the characteristics of my two samples. Differences of observations between administrative variables: dropout, IVE-SINAE and 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 years. I will use these samples to estimate municipality effect in the next section.

4 Empirical approach

I am providing municipality effects to estimate the impact of immigrants on educational outcomes because they can capture effects at school and neighborhood level, and because they can deal with composition effect. This section describes the steps in the empirical approach and the assumptions required for identification.

The intuition behind empirical strategy can be well illustrated with an example. Let's take a set of similar children who move in 1st and 2nd grade from the municipality of Valparaiso to Antofagasta in the year 2012. If I make the difference in test score in 4th grade between those who moved in 1st minus in 2nd I get how much you gain by being one more year in Antofagasta versus Valparaiso. How much it contributes to spend a year in a municipality I will call it municipality effect, therefore this difference will be the municipality effect of Antofagasta minus the one of Valparaiso. Then if I do the same exercise 5 years later I will get the municipality effect of Valparaiso and Antofagasta 5 years later. In this way I can construct a first difference between the municipality effect before and after the start of immigrants arrivals. Then, I can evaluate if the increase of immigrants impact the municipality effect with the following specification:

$$\mu_c^{w_2} - \mu_c^{w_1} = \alpha + \beta \Delta F_c^{y_1 - y_2} + \epsilon_c \tag{2}$$

Where $\mu_c^{w_2}$ represents the municipality effect estimation of municipality c using window w_2, $\mu_c^{w_1}$ represents the municipality effect estimation of municipality c using window w_1, $\Delta F_c^{y_1-y_2}$ is the increase of foreign population between the year y_1 and y_2 as a proportion of the population in year y_1 for municipality c, as it is defined in equation 1.

The estimation of the impact of foreign students on municipality effect 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 neighborhood effect method was introduced in Chetty and Hendren 2018b and its ultimate goal was to estimate the causal effect of place by dealing with composition/selection effect using movers. They estimated each neighborhood effect exploiting the time exposure to a neighborhood comparing the outcome of kids who moved in the same origin-destiny path[10] but at different ages. The underlying assumption is that the age of a child at the time a family moved is orthogonal to unobserved family and student characteristics. In practice this variation is exploited by comparing outcome of students who moved between the same neighborhoods but at different ages.

Chetty's empirical strategy is motivated by a finding in Chetty and Hendren (2018a), estimating exposure effects. They realize that students who are exposed to a positive (negative) place for the longer period of time - hence the name exposure effect - will have better (worse) results than those who are 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 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% and 7% when observing cog rank and dropout, respectively. These numbers are similar once scaled to the age at which the outcomes are measured (in my case 10 years in the their paper 23). This finding will be to motivates the use of the neighborhood effect methodology in my context.

The empirical approach of neighborhood effect estimation is based on the following statistical model:

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

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.

This model requires three assumptions: 1) the neighborhoods effects do not vary across children conditional on mother level of education (or in case of mean neighborhood effect no "essential heterogeneity"[11]), 2) the neighborhoods effects are additive and constant across grades, and 3) disruptive effect

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

[11] Movements orthogonal to the heterogeneity

is independent of the grade of the student. The based on this statistical model Chetty and Hendren (2018b) propose the following specification:

$$\begin{aligned} y_i &= \alpha_{odps} + (A - m_i)\mu_{od} + \epsilon_i \\ \alpha_{odps} &= (\alpha^0_{od} +^1_{od} p + \phi^0_{od}s + \phi^1_{od}s^2 + \phi^1_{od}sq + \phi^1_{od}s^2p) \end{aligned} \tag{3}$$

Where y_i is the educational outcome of kid i, α_{odps} is an equation that interacts an origin-destiny od fixed effect with year of change s[12] and parents characteristics p, A is the grade of the outcome, 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 require a two-step estimator. 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 will take the value of $+1$ when the municipality of destination is assigned, -1 when the municipality of origin 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 precision of each μ_{od}, which is the inverse of the standard deviation.

Given that we will use movers for identification of equation 3 we must add a fourth assumption: 4) the grade at which each student moves is independent of the characteristics of the family and the child. The linearity of the decline in exposure effect described in Appendix A serves as evidence for Assumptions 2 and 3. Also, I will provide evidence for assumption 4 taking advantage of the introduction of 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 grade at move. In this way I can test if students with same family characteristics that move from and to the same municipality differs in 2nd grade read test score based on grade at move. Table 6 show the results of the test. Column 1 and 2 uses the sample used for 4th grade cog rank -movements from 1st to 6th grade- conditional on mother education and IVE-SINAE respectively, column 3 uses the sample for high school dropout -movements from 1st to 7th grade- conditional on IVE-SINAE. The fact that none of the coefficients are significant means that those moving at different grades after 3rd grade do not differ in 2nd grade test scores.

Concluding this subsection I calculate the municipality effect taking all the years of my sample and cross-validate whether these estimates correlate with the variables in the same way that the literature has found. Tables 3 and 4 show the result of regress municipality effect on municipal variables using cog score but controlling by mother education and IVE-SINAE respectively. Municipality's effect is augmented by four to consider the effect of moving in 1st grade and spend four years of primary, eg. in table 3 moving in 1st grade from an average municipality to a municipality with one sd higher in school segregation Theil index lower the outcome of a kid by 3 percentiles rank conditional on the same mother level of education. Tables 3 and 4 show that segregation is detrimental for educational outcomes.

[12] In Chetty and Hendren (2018b) they use students cohort instead of year of change. Because immigration may cause natives flight it seems more reasonable in my setting to use year of change instead.

Surprisingly, municipalities with higher poverty rate have better municipality effect, this correlation could be explained by rurality or government transfers. Test score value added and municipality effect is positively correlated which is reassuring of picking the right variation. The bottom line message is that these municipal effects are capturing relevant variation though we must analyze with caution since is the add-on of each municipality conditional on mother education or IVE-SINAE.

Table 5 show the result of regress municipality effect on municipal variables using high school dropout controlling by IVE-SINAE respectively. Municipality's effect is augmented by four to consider the effect of moving in 1st grade and spend four years of primary, eg. in table 3 moving in 1st grade from an average municipality to a municipality with one sd of residential segregation increase dropout probability by 1 percentage points conditional on the IVE-SINAE. Correlation between municipality effect using cog rank and dropout is low (-17%), but goes in the right direction. There are several reasons that can explain no strong correlation. This can be because of measurement error, municipalities with good primary schools are not necessary those with good high schools, or because the outcomes are very different: dropout is an outcome where students at the lower tail of cog distribution are at risk, while it does not tell us much for students above the median.

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 (above or below tertiary education). I will construct this initial network (share) with the census of 2002. Now to estimate the flow of immigrants by nationality and education I will use the students administrative data. Nationality comes from students' school principal declaration and mother level of education comes from SIMCE parents questionnaire. Because there is a proportion of students that never respond the SIMCE questionnaire and never have a declared nationality I will assume that they are similar or that the bias is not related to endogenous shocks. In other words, the instrument can have measurement error but not bias that threat the exclusion restriction assumption. Though I can not provide evidence in this line if I show that the measurement error is very low it will be enough to say that this is a minor problem. I can do this comparing the estimates with the census in 2017. So, with the administrative students data I estimate the distribution of immigrants students in primary school by nationality and education in 2017 and compare it with the census[13]. Correlation is 95% which confirms that this extrapolation have small measurement error. Then, the construction of the instrument is standard and is calculated as follows:

$$\text{Predicted } Foreignpop_c^{y_1-y_2} = \frac{\Delta\hat{F_c}^{y_1-y_2}}{population_c^{y_1}} = \Delta\hat{F_c}^{y_1-y_2}$$

Where $\Delta\hat{F_c}^{y_1-y_2}$ defines the predicted increase, which I define as follow:

$$\Delta\hat{F_c}^{y_1-y_2} = \sum_k z_{ck} g_k^{y_1-y_2}$$

Because the specification of interest is:

$$\mu_c^{w_2} - \mu_c^{w_1} = \alpha + \beta \Delta F_c^{y_1-y_2} + \epsilon_c$$

Then the instrument rely on the assumption:

$$E(\Delta\hat{F_c}^{y_1-y_2} \times \epsilon_c) = 0$$

Where z_{ck} is the initial share of students of group k in municipality c in 2002, and $g_k^{y_1-y_2}$ is the growth of students for group k from y_1 (2013) to y_2 (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,

[13]To make this two database comparable I define 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).

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. $\Delta F_c^{y_1-y_2}$ is the endogenous variable of increase of foreign migration from y_1 to y_2 as a proportion of the population in y_1, $\Delta \hat{F_c}^{y_1-y_2}$ is the instrument and $\mu_c^{w_2} - \mu_c^{w_1}$ is the first difference of municipality effect in c.

I will use the percentile rank of the endogenous variable and instrument because the relation between the outcomes first difference of municipality effect and natives flight is approximately linear in this case.

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 immigrants arrival separately. Because foreign population started to increase from 2013 I will split the windows with this year threshold. Once I split the sample in two I end up with less observation and consequently with less number of municipality effects than those observed when I use all the years (tables 3 to 5). Then, to make each window estimates comparable I weight municipality effects by the population of the municipalities I can estimate in both windows. So the interpretation of the coefficients are what is the municipality effect compare to the average municipality effect compose of the same municipalities.

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 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 is the contribution of spending a year in a specific municipality compare to the average municipality. Then specification 2 will show what is the impact of a 1 percentile increase of foreign students on municipality effects. I provide 2sls with a window before and after 2013. Table 7 shows this exercise estimating municipality effects on cog score rank conditional on mother education and IVE-SINAE. I estimate around 100 municipalities that represent 80% of students enrolment and 94% of immigrant students enrolment. From panel First-stage we can see that the instrument predict the foreign students variation. OLS panel shows a negative effect on municipality effect, these results are similar to those in 2sls implying that foreign students did not allocate based on municipality effect. 2sls panel is showing a negative impact on municipality effect of around -0.06 percentiles per one percentile change of immigrants. The interpretation is as follow: a kid moving from an average municipality to a municipality with 1 percentile higher of immigrant students will lower its cog score by 0.06 percentiles rank. So, if a students move in 1st grade this will imply a drop of 0.24 percentiles rank. Thus, a 30 percentile greater increase in immigrants - approximately 1 standard deviation of the shock- will lowered children percentile score by 7.2 percentiles if they moved from 1st grade.

Table 8 show the results on municipality effect on dropout. I am estimating 25 municipality effects in this case. This municipalities represent 30% of students enrolment and 53% of immigrant students enrolment. First stage panel show that the instrument is predictive of students immigration change. OLS panel shows a null effect equivalent to the 2sls implying that immigrant students did not allocated based on municipality effects. This null result show that municipalities more exposed to immigrants are not changing in terms of dropout. Still we must be cautious, first this data considers outcomes until 2017, so it does not take into account students exposed to a larger proportion of immigrants, and second the effect observed in lower primary takes longer time to be reflected in high school dropout, eg. kids affected 4th grade may have an effect on dropout observable 7 years after in 11th grade.

5.1 Alternative explanations

Shift-share

It could be that my 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 arrival in two and estimating municipality effects for each window. By doing this I can test if there were a pre-trend before arrival of immigrants. I will split the sample in two windows from 2004 to 2008 and from 2009 to 2013 (before any increase of immigrants). Table 9 shows the coefficient of the parallel trend. Coefficients are showing no differential trend for municipality effects based on cog rank controlling by mother education and for municipality effects based on dropout controlling by IVE-SINAE. On the contrary, municipality effects based on cog rank controlling by IVE-SINAE is showing a positive trend. This is showing that immigrants located in municipalities that were experiencing a positive trend. This positive trend is a problem because it shows that the municipalities most exposed to immigrants would not have the same trend as the less exposed ones. While I cannot deal directly with this problem, I believe I have the necessary evidence of parallel trend with the null trend result in the other two outcomes as well as the evidence of parallel trend for native flight and segregation in section 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 to rule out this alternative.

6 Mechanisms

This section revise different mechanism to explain the negative impact of immigrant students on municipality effects. As mentioned, municipality effects are any effect not related to the household, so we can think of it as any effect at the school or at the neighborhood level. The literature has shown that immigrants can affect student outcomes through peer effect (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 the quality of the neighborhood 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 the effect at neighborhood level can be driven by any change in peers composition. This is not necessarily peer effect but any effect related to a change of composition of peers within the school or municipality, eg. organization of classes, class size, peer effect, and others. To explore if the change of peer composition has an impact on student learning I will exploit random variation from the allocation of immigrants across classes and the arrival of immigrants across cohorts at the school and municipality level. These strategies consider composition effects but at different levels so we may have different effects that are not necessary inconsistent. 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 10 shows the years when each test was administered and their baseline (two years before). These database will allow me to estimate the effect of immigrants exploiting across-cohort and across-classes variation.

6.1.1 Across-cohort variation

In this lines I will describe the empirical approach and the results of using across-cohort variation strategy. To describe the empirical strategy I will talk about schools, but this method is applicable for variation within municipality (where it says school replace by 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 in 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 school they choose to 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 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:

$$g_{jsgt}(y_i^B) = S_{jt}^0 + S_{jt}^1 X_i + S_{jt}^2 y_i^B + G_{gst}^0 + G_{gst}^1 X_i + G_{gst}^2 y_i^B$$

Where S_{jt} is a school/municipality - year fixed effect indicating school/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_i^B is the baseline test score and X_i is a vector of students characteristics. Using students characteristics is not necessary and my prefer specification do not include them, anyway I will use it for robustness check against sorting.
Thus the specification to exploit differences across-cohorts is:

$$y_i = \beta_0 + \beta_1 Frac_{gjt} + g_{jt}(X_i, y_i^B) + n_{gjt} + n_{gjt}^2 + \epsilon_i \tag{4}$$

Where y_i is the outcome of a native student i, $Frac_{gjt}$ is the fraction of foreign students in year t in school/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 and n_{gjt}^2 is the square number of students. I added the number of students as a proxy of class-size.

From the calendar in table 10 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 10 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 tables 11 and table 12 shows that fraction of immigrants is not differential for the baseline test and students characteristics at school and municipality level when pooling students from 2014 to 2016. 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 tables 11 and table 12 provides balance test for year 2018 only. 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 21% but is not differential. Panel C in tables 11 and 12 show 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%[14] but is not differential.

Now that I am confident my results should not be driven by composition I can show results of equation 10. Table 13 show results when exploiting variation within municipality. These results are robust to include students characteristics as shown in equation 10. 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

[14]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.

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. Table 14 show results when exploiting variation within school. These results are robust to include students characteristics as shown in equation 10. These coefficients are more precise compare to within municipality strategy. This is not because of number of observations, but cohort composition at school level explain more variation. Results show that higher fraction of immigrants in your cohort within school increase 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 result, the impact on test score for period 2014 to 2018 is null. It is unlikely that these results are driven by selection because change of school or municipality is not differential. The effect on change of school within municipality and school is not consistent. This may imply that results at municipality level can be related to the capacity of the system to absorb students. If there are more immigrants in my cohort my possibilities to be accepted in other school are lower so I change less of school or of municipality (when adjacent). The positive impact on cog rank within school versus null effect within municipality may be explained by the imprecise estimation within municipality.

6.1.2 Across-classes variation

This strategy estimates the impact on students outcomes in a specific grade, given that they were exposed differently to immigrants in their classes in $t-1$, and controlling by their test score at the baseline $t-2$. Because I will exploit differences of fraction of immigrants across classes I do not need to pool grades together like I did above. 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 class compare 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:

$$g_{jt}(X_i, y_i^B) = S_{jt}^0 + S_{jt}^1 X_i + S_{jt}^2 y_i$$

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

$$y_i = \beta_0 + \beta_1 Frac_{cjt} + g_{jt}(X_i, y_i^B) + \epsilon_i \tag{5}$$

(6)

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.

For the variation between classes I have variation enough across years with baseline test score so I do not need to add years without baseline -as I did with across-cohorts variation strategy-. I will compare separately outcomes of 4th grade students from 2014 to 2017 and outcomes of 6th grade students from 2014 to 2016 and 2018. This strategy relies on independence of unobservables ϵ_i and treatment $Frac_{cjt}$. To identify if classes formation are independent of the fraction of foreign students I can provide a balance test. Table 15 shows that natives that share classes with higher fraction of immigrants have lower baseline test score and income. This is discouraging because it means that the variation I am exploiting has sorting of immigrants or students 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[15]. The Pearson χ^2 test asks whether there are more subgroup of students (immigrants, high per-

[15] Ammermueller and Pischke (2009) implemented this test for the first time in this setting.

form, 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:

$$P = \sum_c \sum_g \frac{(n_{cg} - \hat{n}_{cg})^2}{\hat{n}_{cg}}$$
$$\hat{n}_{cg} = \frac{n_c \times n_g}{\sum_c \sum_g n_{cg}} \tag{7}$$

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 the effect of foreign students with other effect related to student and class characteristics. For this I will test random allocation of students given foreign status and perform in the baseline test. The results of this test will help me to 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 students performance using equation 7. Table 16 show this exercise. Coefficients are lower but still statistically significant. I believe balance tests did not change radically 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 school-year fixed effect as in equation 5. Table 17 shows the impact of more immigrants in your class on cog score, repeat, and dropout for 4th and 6th grade. Overall we see null effect on education outcomes. There are, however, one statistically significant effects at 5% level for dropout and school change in 4th grade. Because class-size are on average around 40 students[16] this results imply that an increase of 10% of immigrants (4 students) in your class may increase the probability of change of school by 0.7%, As we can see in table 18 results are similar when discarding schools doing tracking or non-random allocation of immigrants.

To sum up

Overall, results are showing that it is unlikely the impact I am observing on municipality effects is driven by immigrants peer effect. The across-cohort variation tell there is no evidence of effects related to peer composition 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 students characteristics we also see null effect.

6.2 Native flights and segregation

In this subsection I will study whether natives respond to the arrival of immigrants student 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 school are not the only option to avoid interaction with immigrants in assigned schools. Families can decide to fly from public to private schools[17] or to fly to a municipality with relatively less immigrants. The consequences of this flights are increase in the segregation of immigrant students, but because those that move are generally those that can pay the cost of a private schools or the cost of changing of municipality this can generate also socioeconomic segregation.

6.2.1 Native flights

Native flights identification pose an important challenge. Natives can follow immigrants waves into an area when there are pull factors for both, eg. job market. At the same time, impacts on housing and labor market and preferences for immigrants can motivate natives to move away once immigrants arrive.

[16] Considering school with more than one class per cohort.

[17] In most of the cases is natives that decide to go to private schools, though there are exceptions like Muslims immigrants in DenmarkRangvid (2010).

Additionally, natives can be moving even before immigrants arrival. Then I will have to take care of pre-trends, endogenous arrival of immigrants and plausible housing and labor market effects.

For the analysis in this subsection I will look at yearly aggregated data at municipality level of all students enrolled in primary education from 1st to 6th grade from 2007 to 2019. To illustrate the identification problem of studying native flights I will starts with a naive approach and then modify it to overcome the identification issues. I will explain the empirical strategy looking at native flight to municipalities with relatively less foreign students, but it can easily adjust to do flight to private school by changing the dependent variable from all natives to natives in private school. So, the naive specification evaluate if change in natives is related to change of immigrants:

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

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 immigrants 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 immigrants arrival. For this I will follow a DiD specification and test for pre-trend to show this is not the case. Third, they may be areas that have different birth rate, then I will control by birth cohort rate[18]. This approach does not take into account the fact that families may move before birth. Because movements before birth is unlikely to be related to immigrant students this is less of an issue.

As any DiD my strategy relies in the fact that treated municipalities behave similar before immigrants arrival and then after there is an effect that grows in time. To provide parallel trend I will test if the levels are changing at the same time that immigrants are arriving. This will allow me to test if my strategy is correct where I expect that municipalities that will have higher exposure of immigrants have no effect until 2013 and then it must increase gradually over time. The specification is as follow:

$$nat_{ct} = \alpha + A_t + \delta\Delta mig_c + \sum_{t'=2007}^{2019} \beta^t \mathbf{1}(t' = t)\Delta mig_c + \sum_{t'=2007}^{2019} \gamma^t \mathbf{1}(t' = t) births_{ct} + \epsilon_{ct} \tag{8}$$

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, ie. shocks that vary in time but no across municipalities. To deal with immigrants location endogeneity I will provide a shift-share where the share is the combination of nationality and high-low skills immigrants family location by municipality from census 2002 for all nationalities but Haitians that are from 2007[19]. In this specification I will set the baseline in 2007[20] 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 school 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 are not straightforward, so I will prefer another one which I will explain in detail below.

[18]Since naturalization is based on jus solis all births are Chileans. Also, most of students enrolled in 1st grade are 6 years old in April, I will take this into account eg. kids enrolled in 1st grade in t have birth cohort between April in $t-7$ to March in $t-6$.

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

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

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.

$$\Delta nat_{ct} = \alpha + A_t + \delta \Delta mig_c + \sum_{t'=2009}^{2019} \beta^t \mathbf{1}(t' = t) \Delta mig_c + \sum_{t'=2007}^{2019} \gamma^t \mathbf{1}(t' = t) \Delta births_{ct} + \epsilon_{ct} \tag{9}$$

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 immigrants increase percentile rank transformation because the impact of immigrants on municipality effects looks linear in this case. However, I will add the change in percentage as well to see outliers drive native flight based on tipping, which is basically a non-linearity. For this reason when I present the results I will also show this alternative.

Natives flight to municipalities with less immigrants

Figure 4 show the coefficients of equation 8 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. In panel (a) and (b) we can see there is no pre-trend. This mean 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, this is showing that there are less 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. It is likely this is happening because adults immigrants migrate first and then come with their families, so this could be a reaction from adults immigration.

Table 19 show the coefficients of equation 9. Odd columns show the result when using the percentile rank of migration change and even columns when using percentage migration change as treatment. Also first two columns do analysis at municipality level and second two columns at city level. The first stage panel shows that the instrument predict the endogenous variable with enough precision. Column 1 in OLS panel shows that immigrants follows natives movement during pre-treatment (pre 2013). This is why the instrument can be useful because deals with positive shocks at municipality level that may pull natives and immigrants. Column 2 does not show this pattern. Panel 2sls show the effect of immigrants on native flight. Column 1 shows that an increase of one percentile decrease population of natives by 0.085 percentage points. A one percentile increase is associated to 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 points increase in immigrants decrease natives population by 1.2 percentage points. Column 3 in panel 2sls shows that an increase of one percentile decrease population of natives by 0.105 percentage points. A one percentile increase is associated to an average of 0.085 increase in percentage points or to a median of 0.108 percentage points. So taking the average -1.2 or the median -1 native per one immigrant arrival. Column 4 shows that a one percentage points increase in immigrants decrease 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 as a response to the housing and labor market. In fact because the relation is one to one, in a world of housing supply not perfectly elastic this effect could be fully explained by housing market and not distaste for immigrants (Boustan (2010)).In this matter, look at movement to private schools helps to rule out the housing and labor market effect.

Natives flight to private schools

I can observe natives flight to private school by changing the dependent variable in the equation 8 with the level of natives in private schools. Also I will exclude the birth correction from the specification. Figure 5 show the coefficients of this modified equation 8 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 immgrants 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 null pre-trend. This mean 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 immigrant show an increase of natives enrolment in private schools. In panel (b) there is no change of slope. This difference may arise because of non-linearity of the effect.

Table 20 show the coefficients of equation 9 with change of natives in private school as 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. Also first two columns do analysis at municipality level and second two columns at city level. The first stage shows that the instrument predict the endogenous variable with enough precision. Column 1 in 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 flight to private schools. Column 1 shows that an increase of one percentile increase enrolment of natives in private schools by 0.03 percentage points. A one percentile increase is associated to an average of 0.12 increase in percentage points or to 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, ie. natives where moving to private schools in exposed municipalities early on the arrival of immigrants but once they arrive the movement speed up. Column 2 shows no change. Column 3 shows similar coefficients than column 1, which means that this phenomenon occurred also in cities that receive more immigrants. Column 4 shows that native where moving to private schools early on the arrival of immigrants, then increase during immigrants arrival but the coefficient is not significant.

Because natives movement to private schools may not be a response 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 say the lower bound are the effect on those increasing their participation in private schools, ie. 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[21]. 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 does not change when we augmented the number of priority students in each school by a constant. To keep measures comparable across municipalities and over 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 8 with the school segregation as dependent variable and excluding the birth correction from the specification. Figures 6 and 7 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 proportion of the population in 2007. Figure 6 shows the coefficient for the Theil index, both panels follow null pre-trend from 2010 to 2013. This mean that the schools segregation at municipality level is not related to the exposure to immigrants in municipalities. In panel (a) we see a change in the slope from 2013,which means that municipalities with higher exposure to immigrant show an increase in school segregation from 2010. In panel (b) there is no change of slope. This difference may arise because of non-linearity of the effect. Similarly, figure 7 shows the coefficient for the Atkinson index. Overall, results with Theil and Atkinson index looks similar which means that composition change over time is not driving the results of the Theil index.

Tables 21 and 22 show the coefficients of equation 9 with change of the school segregation index as dependent variable[22] 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. Also first two columns do analysis at municipality level and second two columns at city level. The first stage panel for both tables is the same and is showing that the instrument is a strong predictor of the change of immigrants. In table 21 (Theil) column 1 in OLS panel shows that school segregation increase when immigrants arrive into a municipality. This is similar to the 2sls. Column 2 shows no change of school segregation over time when immigrants move in. Panel 2sls show the effect of immigrants on school segregation. Column 1 shows that an increase of one percentile increase school segregation index by 0.0289 percentage points. Column 2 shows no statistically significant change. Column 3 shows that immigrants accelerate the schools segregation at city level. Column 4 shows that when grouping by city immigrants arrival has an effect on school segregation by 0.007 per one percentage point increase of immigrants. Remember that when using percentage change of immigrants as treatment

[21] 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)

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

I found evidence of flight to municipalities with less immigrants but not to private schools. Hence, the significant effect in column 4 is likely driven by sorting across municipalities. Results in table 22 (Atkinson) are equivalent to those described in table 21. This show that the segregation change in time is unlikely driven by change in composition.

To sum up

Though both mechanism are a form of peer effect, they can be more than that. School segregation can show preferences for interact with other, trust in neighborhoods, preferences for public goods, and others. It could show also identity,,,

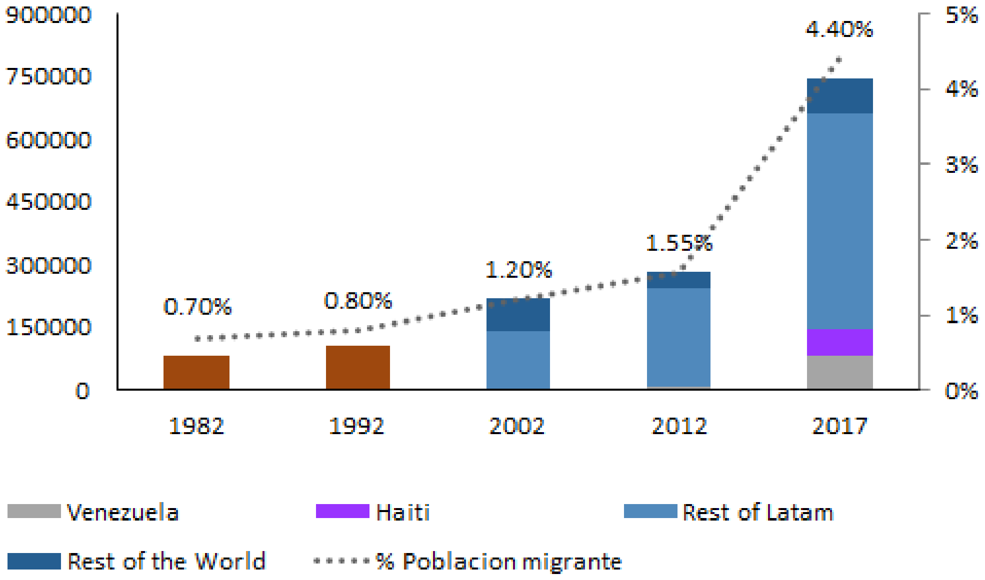
In the Chilean context, natives flight is unlikely to be due to the lower quality of public schools, immigrants outperform Chileans in test score and their parents are more educated within public schools. Also, most immigrants speak Spanish so there is no more allocation of resource to immigrants to the detriment of natives. On the other hand, native flight can be explained by housing and labor market and preferences.

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.

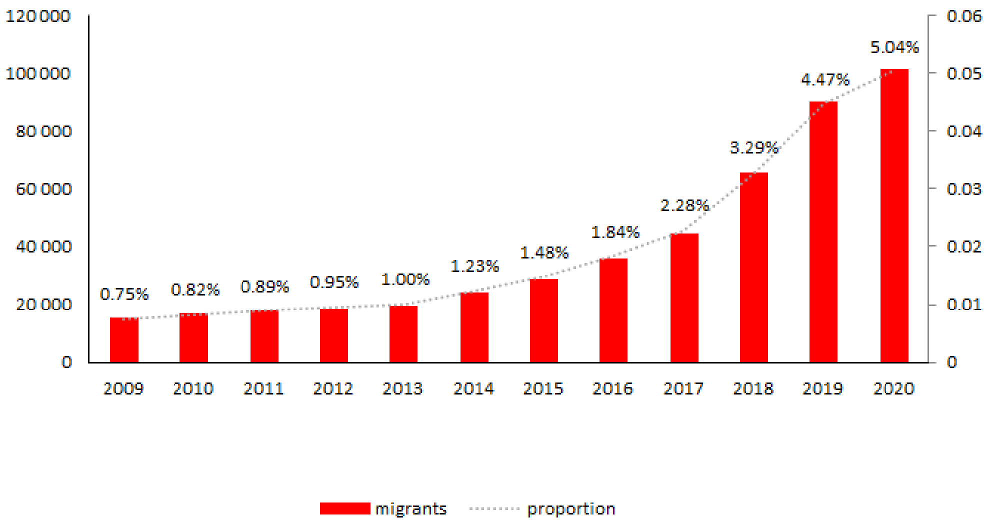
Figures and tables

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



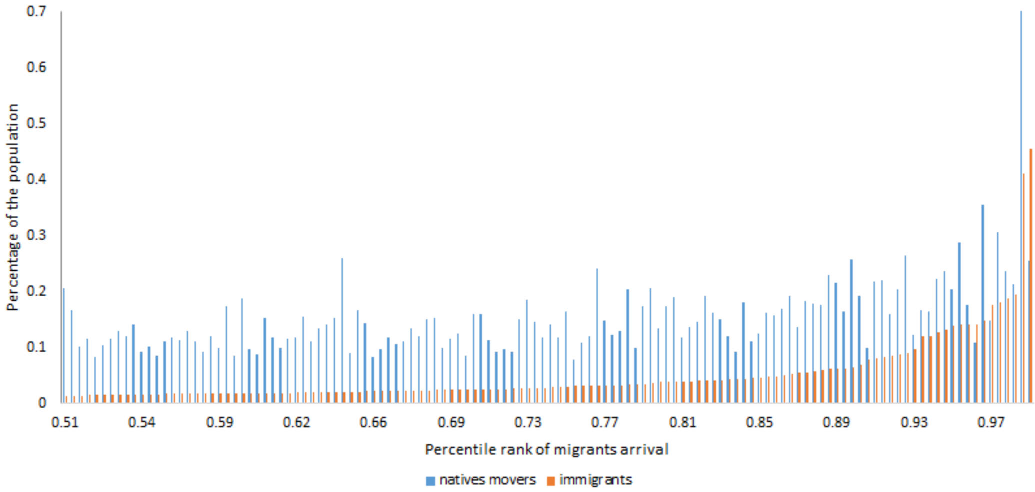
Source: Censuses 1982, 1992, 2002, 2012 and 2017.

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



Source: Enrolment data (Education Ministry).

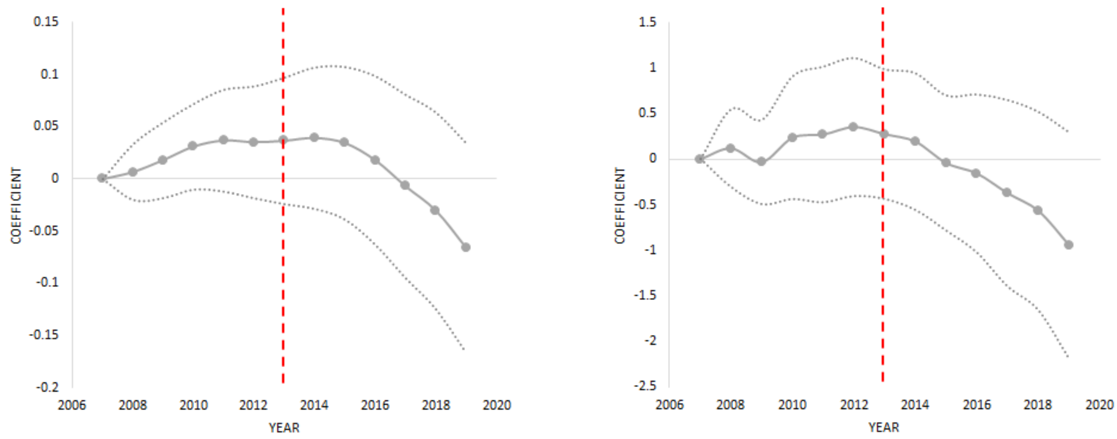
Figure 3: Percentage of migrants and natives movers by municipality



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

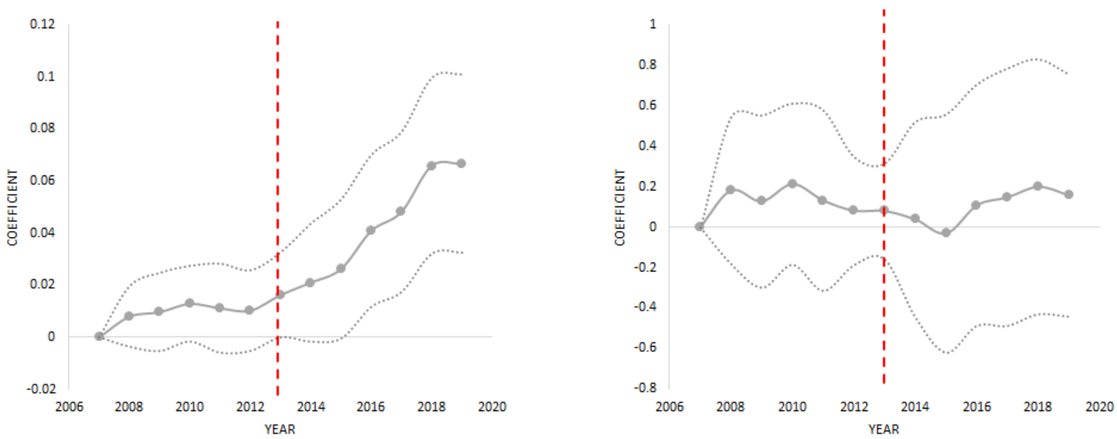


(a) Percentile rank change of immigrants

(b) Percentage change of immigrants

Plot of coefficients β^t from equation 8.
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 5: Impact of foreign students on native flight to private schools

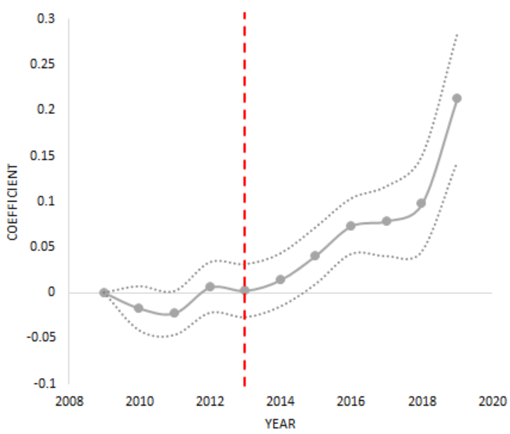


(a) Percentile rank change of immigrants

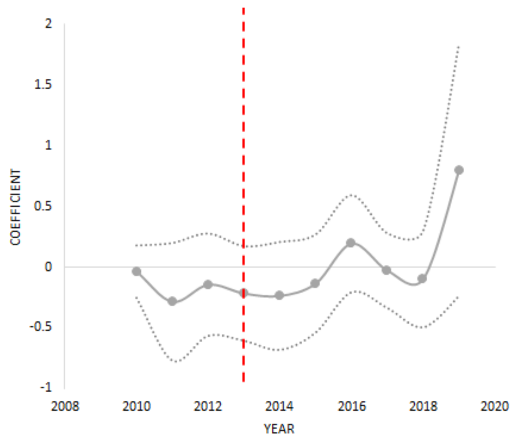
(b) Percentage change of immigrants

Plot of coefficients β^t from equation 8.
Note: Variables at municipality level and standard errors clustered at municipality level.

Figure 6: Impact of foreign students on school segregation (Theil index)



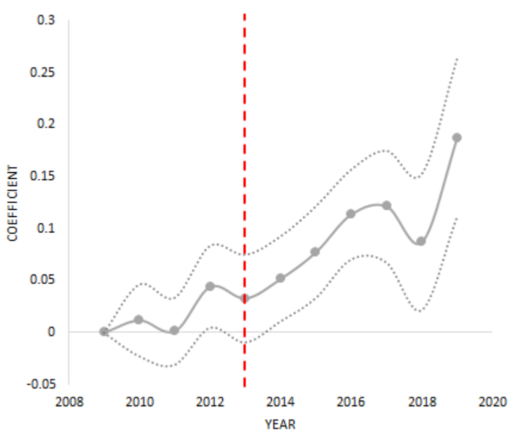
(a) Percentile rank change of immigrants



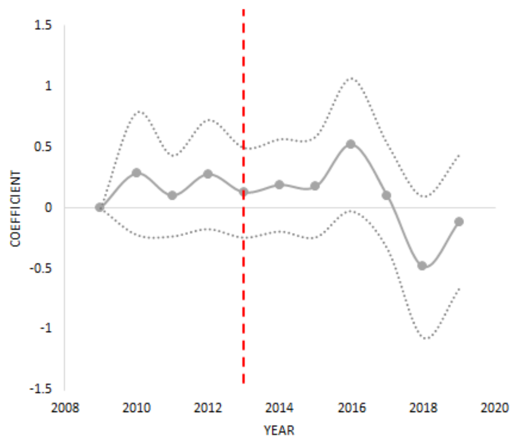
(b) Percentage change of immigrants

Plot of coefficients β^t from equation 8.
Note: Variables at municipality level and standard errors clustered at municipality level.

Figure 7: 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 8.
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
IVE-SINAE rank	0.50	0.30	2515084
Norte Grande	0.07	0.26	2515084
Region Metropolitana	0.35	0.48	2515084
Rest	0.58	0.49	2515084
B: Movers			
Cog test rank	0.50	0.29	679567
Mother education rank	0.50	0.29	722300
IVE-SINAE rank	0.50	0.28	782845
Norte Grande	0.06	0.23	782845
Region Metropolitana	0.48	0.50	782845
Rest	0.46	0.50	782845
C: Movers restricted			
Cog test rank	0.53	0.29	179345
Mother education rank	0.54	0.28	186474
IVE-SINAE rank	0.42	0.26	198947
Norte Grande	0.08	0.28	198947
Region Metropolitana	0.48	0.50	198947
Rest	0.43	0.50	198947

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: Summary statistics for students in 8th grade: permanent residents and movers

	Mean	Std.Dev.	Obs
A: Permanent residents			
Dropout	0.12	0.32	2761562
IVE-SINAE rank	0.50	0.29	2761562
Norte Grande	0.07	0.25	2761562
Region Metropolitana	0.36	0.48	2761562
Rest	0.58	0.49	2761562
B: Movers			
Dropout	0.16	0.37	740828
IVE-SINAE rank	0.49	0.28	740828
Norte Grande	0.06	0.23	740828
Region Metropolitana	0.48	0.50	740828
Rest	0.46	0.50	740828
C: Movers restricted			
Dropout	0.13	0.33	170495
IVE-SINAE rank	0.41	0.26	170495
Norte Grande	0.07	0.26	170495
Region Metropolitana	0.47	0.50	170495
Rest	0.46	0.50	170495

Note: Dropout defined as students not enrolled in high school in 11th grade. IVE-SINAE rank is the rank of school vulnerability index. 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 3: Correlation of municipality effects of 4th grade cog score conditional on mother level of education with variables of interest.

	Regression estimates		
	b	s.e.	Observations
	(1)	(2)	(3)
Residential segregation Theil index	-1.08*	.59	199
School segregation Theil index	-2.06**	.64	199
Poverty rate	2.06***	.6	199
Income per household	-.35	.57	199
Rurality	1.52**	.74	199
Test score value added	2.18***	.59	199
Educational budget per student	.77	.62	199
Health budget per capita	.02	.65	186
Neighborhood org per Capita	.88	.64	199
Green areas per Capita	.66	.66	199
Fraction adults tertiary	-.22	.56	199
Fraction of single parents	-.52	.62	199
Fraction of adults divorced	-2.08**	.67	199
Fraction of adults married	.99	.81	199
Unemployment rate	1.35**	.59	199
Crime rate	-.48	.55	199
Slums inhabitants per capita	.14	.58	199
Norte Grande	.2	.54	199
Metropolitan Region	-1.96***	.54	199

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 2004 to 2019 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. 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: Correlation of municipality effects of 4th grade cog score conditional on IVE-SINAE with variables of interest.

	Regression estimates		
	b	s.e.	Observations
	(1)	(2)	(3)
Residential segregation Theil index	-1.69**	.7	202
School segregation Theil index	-3.09***	.76	202
Poverty rate	4.06***	.68	202
Income per household	-1.46**	.68	202
Rurality	3.86***	.85	202
Test score value added	2.77***	.71	202
Educational budget per student	1.17	.74	202
Health budget per capita	.08	.79	189
Neighborhood org per Capita	2.23***	.75	202
Green areas per Capita	.26	.8	202
Fraction adults tertiary	-1.41**	.67	202
Fraction of single parents	-.69	.75	202
Fraction of adults divorced	-3.08***	.8	202
Fraction of adults married	1.25	.98	202
Unemployment rate	2.1***	.71	202
Crime rate	-1.2*	.66	202
Slums inhabitants per capita	.35	.7	202
Norte Grande	-.53	.66	202
Metropolitan Region	-2.69***	.65	202

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 2004 to 2019 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. 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 5: Correlation of municipality effects of high school dropout conditional on IVE-SINAE with variables of interest.

	Regression estimates		
	b	s.e.	Observations
	(1)	(2)	(3)
Residential segregation Theil index	1.08**	.45	147
School segregation Theil index	-.01	.51	147
Poverty rate	-.26	.44	147
Income per household	.09	.37	147
Rurality	-.23	.62	147
Test score value added	.03	.43	147
Educational budget per student	.38	.39	147
Health budget per capita	-.03	.5	139
Neighborhood org per Capita	-.12	.45	147
Green areas per Capita	.1	.49	147
Fraction adults tertiary	.29	.37	147
Fraction of single parents	-.26	.44	147
Fraction of adults divorced	-.56	.5	147
Fraction of adults married	.06	.57	147
Unemployment rate	.29	.44	147
Crime rate	.15	.37	147
Slums inhabitants per capita	.19	.41	147
Norte Grande	.4	.36	147
Metropolitan Region	.17	.38	147

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 2004 to 2019 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. 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 6: Selection test for movers after 2nd grade test.

	read rank	read rank	read rank
move at 3rd grade	0	0	0
	(.)	(.)	(.)
move at 4th grade	-0.144	0.0436	0.557
	(1.450)	(1.352)	(1.981)
move at 5th grade	0.482	0.265	0.0111
	(1.593)	(1.496)	(1.658)
move at 6th grade	0.153	0.0656	-0.349
	(1.859)	(1.762)	(1.990)
move at 7th grade			-0.922
			(2.326)
Constant	53.04***	52.79***	54.13***
	(1.053)	(0.990)	(1.791)
Observations	14010	14695	14299

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

Note: Column one and two using one time movers between 3rd and 6th grade controlling by mother education and IVE-SINAE respectively.

Column three one time movers between 3rd and 7th grade controlling by IVE-SINAE.

2nd grade test from 2012 to 2015.

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

$$y_i = \alpha_{odps} + \beta_m \mathbf{1} m_i + \epsilon_i$$

Table 7: Foreign students percentile change impact on municipality causal effect based on cog score.

	(1)	(2)
	First-stage	
$\Delta F^{\widehat{w_1 - w_2}}$	0.776***	0.767***
	(0.0750)	(0.0723)
Constant	15.18**	15.89***
	(5.861)	(5.629)
F-stat	107.0	112.4
	OLS	
$\Delta F^{w_1 - w_2}$	-0.0626***	-0.0466**
	(0.0208)	(0.0207)
Constant	4.713***	3.392**
	(1.588)	(1.574)
r2	0.0842	0.0476
	Reduced form	
$\Delta F^{\widehat{w_1 - w_2}}$	-0.0538**	-0.0384*
	(0.0227)	(0.0221)
Constant	4.166**	2.852
	(1.775)	(1.719)
r2	0.0538	0.0288
	Two stage least squares	
$\Delta F^{w_1 - w_2}$	-0.0584***	-0.0645***
	(0.0191)	(0.0195)
Constant	4.391***	4.865***
	(1.468)	(1.495)
r2	0.102	0.103
N	112	114

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

Note: Column 1 estimates municipality effects on cog rank conditional on mother level of education. Column 2 estimates municipality effects on cog rank conditional on IVE-SINAE.

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 8: Foreign students percentile change impact on municipality causal effect based on high school dropout.

	(1)
	First-stage
$\Delta F^{\hat{w_1 - w_2}}$	0.906***
	(0.199)
Constant	5.470
	(17.35)
F1	20.63
	OLS
$\Delta F^{w_1 - w_2}$	-0.0221
	(0.0471)
Constant	1.836
	(3.983)
r2	0.00952
	Reduced form
$\Delta F^{\hat{w_1 - w_2}}$ -0.0574	
	(0.0611)
Constant	4.953
	(5.317)
r2	0.0370
	Two stage least squares
$\Delta F^{w_1 - w_2}$	-0.0634
	(0.0667)
Constant	5.300
	(5.621)
r2	.
N	25

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

Note: Column 1 estimates municipality effects on cog rank conditional on mother level of education. Column 2 estimates municipality effects on cog rank conditional on IVE-SINAE.

Municipality effect estimates from on time movers in 1st to 7th grade in two windows from 2004 to 2012 and from 2013 to 2017. Movement across municipalities with more than 50 observation. Municipalities fixed to common estimates between two windows and weighted by population.

Table 9: Parallel trend of municipality causal effect.

	Two stage least squares		
	Cog test rank	Cog test rank	Dropout
	(1)	(2)	(3)
$\Delta F^{w_1-w_2}$	0.0709	0.193***	0.0193
	(0.0522)	(0.0459)	(0.0238)
Constant	-5.146	-15.21***	-1.563
	(4.090)	(3.588)	(2.007)
r2	0.0922	0.0543	.
N	83	85	25

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: Column 1 estimates municipality effects on cog rank conditional on mother level of education. Column 2 estimates municipality effects on cog rank conditional on IVE-SINAE. Column 3 estimates municipality effects on dropout conditional on IVE-SINAE
Municipality effect estimates from on time movers in 1st to 7th grade in two windows from 2004 to 2012 and from 2013 to 2017. Movement across municipalities with more than 25 observation as in Chetty and Hendren (2018b) for cog test rank and more than 50 observations for dropout. Municipalities fixed to common estimates between two windows and weighted by population.

Table 10: 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 11: 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_gma	-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
N_clust	7874	7871	7869	7874	7874
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_gma		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
N_clust		7181	7177	7707	7707
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_gma		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
N_clust		8222	8223	8230	8230
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 12: 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_clust	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_clust		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_clust		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 13: 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_gma	-0.142	-0.0201	-0.00452	-0.248**	-0.104	-0.234	0.00414
	(0.193)	(0.0691)	(0.0257)	(0.106)	(0.0769)	(0.148)	(0.226)
r2	0.132	0.00657	0.00247	0.00955	0.00685	0.0103	0.0220
N	999481	1170332	1170332	1163749	1163749	1163749	1170332
N_clust	6980	7874	7874	7874	7874	7874	7874
dependent mean	0.521	0.0241	0.00325	0.0470	0.0201	0.104	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_gma	-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
N_clust	7167	7707	7707	7704	7704	7704	7707
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_gma	-0.155	-0.0606	0.0346	-0.157**	-0.0801	-0.199**	-0.0882
	(0.138)	(0.0517)	(0.0351)	(0.0725)	(0.0532)	(0.0925)	(0.147)
r2	0.132	0.00764	0.00479	0.0102	0.00641	0.0110	0.0277
N	1546061	1884779	1884779	1868235	1868235	1868235	1884779
N_clust	8108	8230	8230	8230	8230	8230	8230
dependent mean	0.505	0.0288	0.00684	0.0512	0.0222	0.113	0.180

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.

Table 14: Impact on educational outcomes using across-cohort at school level for 4th and 6th grade students

Panel A:	2014-2018						
	(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_clust	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_clust	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_clust	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 15: 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_clust	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_clust	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 16: 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_clust	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_clust	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 17: 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_clust	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_clust	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 18: 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_clust	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_clust	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 19: 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 20: 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 21: Change of school segregation (Theil 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		
$\Delta F^{w_1-w_2}pc$	-0.00109		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		
$\Delta F^{\widehat{w_1-w_2}}pc$	0.000361		0.00358	
	(0.00298)		(0.00250)	
$\Delta F^{\widehat{w_1-w_2}}pc \times post$	0.0279***		0.0361***	
	(0.00554)		(0.00363)	
$\Delta F^{\widehat{w_1-w_2}}$		-0.105		-0.0206
		(0.0741)		(0.114)
$\Delta F^{\widehat{w_1-w_2}} \times post$		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 22: 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		
$\Delta F^{\widehat{w_1-w_2}}pc$	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)[23]. Exposure effect is estimated as follows:

$$\begin{aligned} \Delta_{odps} &= \bar{y}_{pds} - \bar{y}_{pos} \\ y_i &= \alpha_{qosm} + \sum_{m=1}^{m=6} b_m \mathbf{I}(m_i = m)\Delta_{odps} + \theta_i \end{aligned} \tag{10}$$

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 10 and drawing the coefficients b_m using cog score rank and mother education rank as parent characteristics can be found in figure A.1. In figure A.2 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[24] 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 10 and drawing the coefficients b_m using dropout and IVE-SINAE as parent characteristics can be found in figure A.3. 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.

[23] 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.

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

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. 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:

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

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

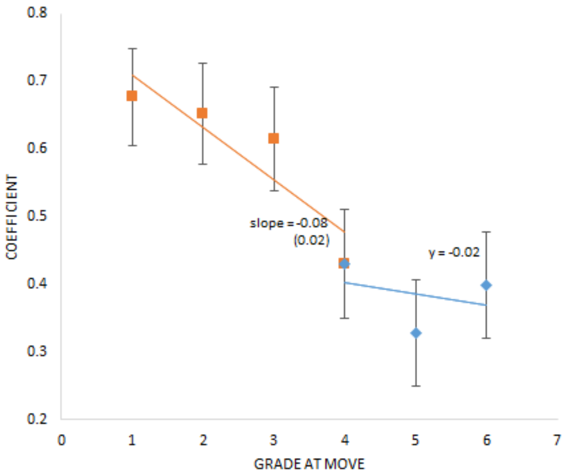
Both are variation of equation 10. Simultaneous add $\Delta_{odps'}$ estimated with stayers from 4 years before to 4 years after. Separate replace Δ_{odps} with $\Delta_{odps'}$ estimated with stayer from 4 years before to 4 years after.

Figure A.4 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 A.5 shows this exercise. As you can see the convergence rate is quite stable until the end of the distribution.

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

(a) Semi-parametric estimates



(b) Parametric estimates

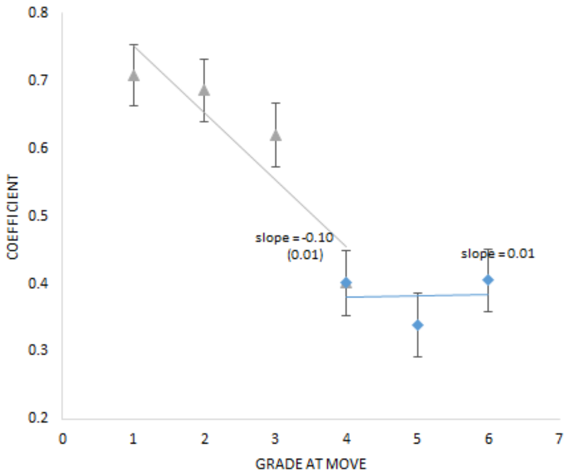
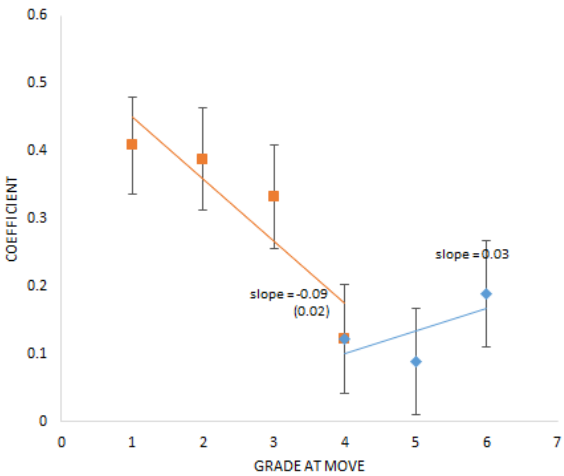


Figure A.2: Exposure effects on test scores in 4th grade controlling by IVE-SINAE

(a) Semi-parametric estimates



(b) Parametric estimates

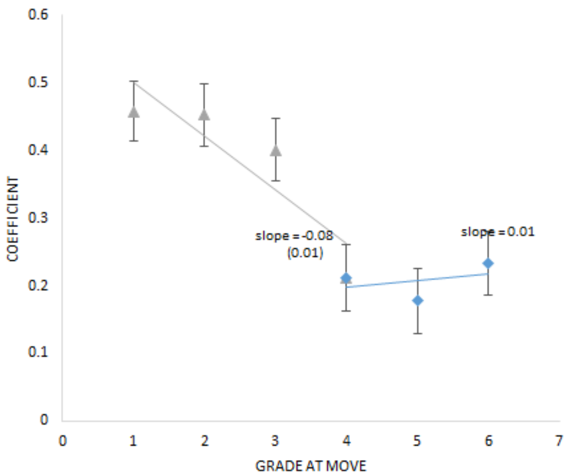


Figure A.3: Exposure effects on dropout in 8th grade controlling by IVE-SINAE

(a) Semi-parametric estimates

(b) Parametric estimates

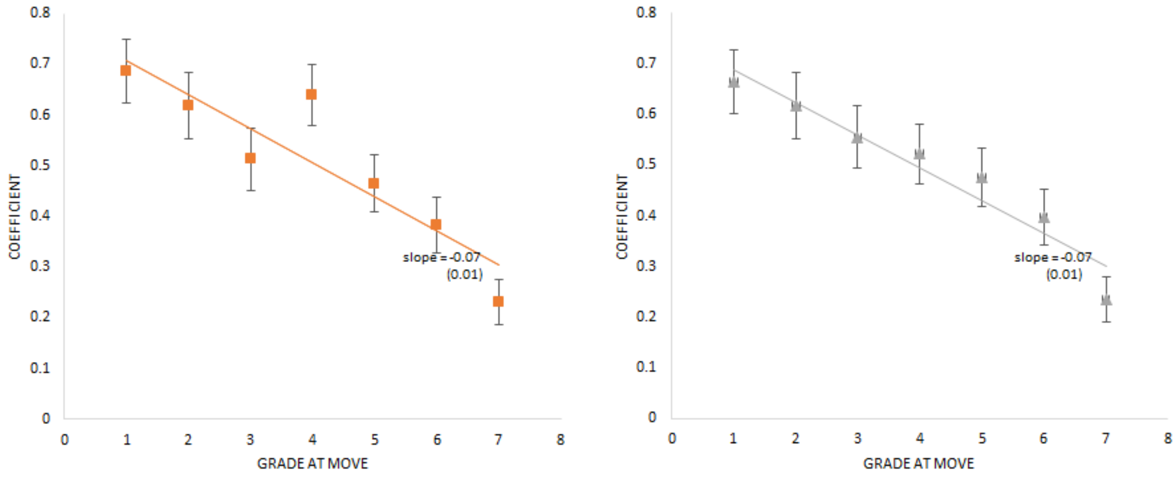


Figure A.4: Convergence rate of 4th grade test scores estimates based on cross-cohort variation

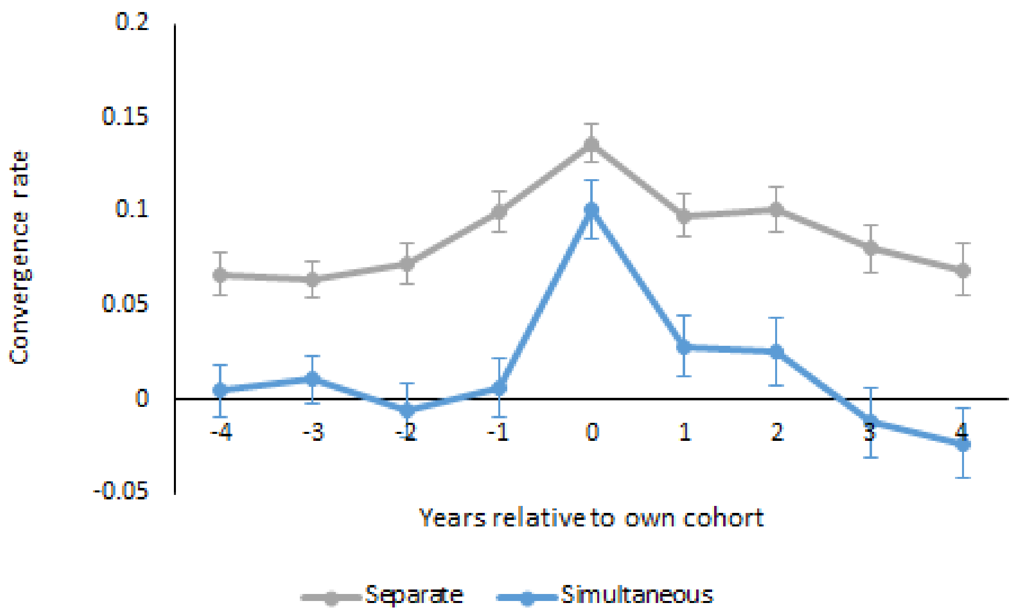


Figure A.5: Convergence rate of 4th grade test scores estimates using displacement shocks