ABSTRACT

Title of Dissertation: ESSAYS ON THE ECONOMICS OF EDUCATION

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All over the world, governments are increasingly introducing reforms intending to promote school integration and equal access within the educational systems. Many aim to provide more school opportunities to families and include education vouchers, open enrollment, inter-district choice, and centralized assignment policies, among others. This dissertation consists of three essays that seek to provide evidence for the design of effective and inclusive education policies.

Although many of these reforms implicitly or explicitly concern peer effects, -as they affect the composition of classrooms and schools-, they are often not fully internalized in their design [Hoxby, 2000, Heckman, 1999]. Hence, in Chapter 2, I develop a dynamic model of social interactions at school to study the impact of peers on student outcomes. I take advantage of the dynamics of the model to deal with the main identification issues that have been documented in the literature. Then, I use this model to estimate the impact of classroom peers on students' academic achievement by using student-level administrative data from Chile.

I find that social effects are positive but only significant for math. When exploring heterogeneous

effects, I find that exposure to opposite-gender peers leads to higher academic performances in math for boys but not for girls. This result could be related to gender stereotypes that affect girls' attitudes and self-perceptions toward math. I also find that higher achievers experience the largest effects from high-ability peers. Conversely, students with low initial achievement levels appear to benefit less and may even experience adverse effects from top ability students. This suggests that some degree of tracking should be preferred to thoroughly mixed or random classrooms.

In the following two chapters, I analyze the effects of two large-scale school choice reforms aimed at reducing social segregation across schools by influencing enrollment patterns. In Chapter 3, I study the impact of a targeted voucher program on a series of cognitive, non-cognitive, and behavioral outcomes, ranging from student test scores to aggressive behavior (bullying). I exploit a national targeted voucher policy implemented in Chile, which increased the funding for disadvantaged students by 50%. Using a difference-in-differences strategy, I confirm previous findings that this policy did not significantly improve students' test scores. However, I show that the program led to meaningful improvements in students' non-cognitive and behavioral outcomes, for both low- and high-SES students. Finally, I provide evidence of the primary channel behind my results: schools participating in the policy have used the additional resources to hire more learning support staff, especially from the psycho-social area.

My last chapter, written in collaboration with Sergio Urzúa and Shanjukta Nath, investigates whether adopting a centralized school admission system can alter within-school socio-economic diversity. We examine a centralized school admission system that the Chilean government introduced in 2016 as the central component of a major education reform aimed at promoting social inclusion and reducing the high levels of school segregation. We use a difference-in-differences strategy, exploiting the sequential introduction of the reform across regions to quantify its heterogeneous impact on segregation. We find no impact of the new policy on school segregation. However, we document that the reform increased within-school segregation in areas with high levels of pre-existing residential segregation and with higher provision of private education.

ESSAYS ON THE ECONOMICS OF EDUCATION

by

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Dedication

To Javier, because this journey would not have been possible without you.

A mis papás, por apoyarme siempre.

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Chapter 1: Introduction

There is consensus that education is a fundamental tool through which society can affect inequality of opportunities and helps alleviate large and persistent income disparities across generations. All over the world, governments are increasingly introducing reforms aimed at promoting school integration and equal access within the educational system.[1] This dissertation explores how economics can help with the design of more effective and inclusive education policies.

Over the past few decades, many countries have implemented reforms that provide more school opportunities to families [OECD, 2019]. Common examples are education vouchers, open enrollment, inter-district choice and centralized assignment policies [Chingos and Monarrez, 2020]. These interventions, implicitly or explicitly, concern peer effects. For instance, if high- and low-ability students are not affected in the same way by their peers, changes in the composition of schools may have consequences on both equity and the academic outcomes of the students. In addition, many of these reforms are large-scale policies that are likely to affect all students, even those not directly benefiting from the reform. For example, non-targeted students could experience changes in their classmates' characteristics or outcomes that impact their own achievement. Despite this, social effects are often not fully internalized in the design of these policies [Hoxby,

[1] There is evidence that school desegregation policies in the United States have resulted in better educational outcomes for minority groups as reflected by higher academic achievement, lower drop out rates, and higher graduation percentages [Guryan, 2004, Hanushek et al., 2009, Johnson, 2011].

2000, Heckman, 1999].

A better understanding of the magnitude and nature of social interactions at school would help inform the policy debate. Hence, in the Chapter 2, I develop a dynamic model of social interactions at school to study the impact of classmates in academic achievement. Then, in the following two chapters, I analyze the effects of two large-scale school choice reforms aimed at reducing social segregation across schools by influencing enrollment patterns. The first reform is a school voucher targeted to low income students that expanded the school choice set at which they could enroll and provided schools an incentive to serve disadvantaged students. The second reform is a centralized admission system that eliminated supply-side choice for state-sponsored schools by requiring all families to list (unlimited) school preferences through a centralized web application platform.

Peer effects in education may provide inherent opportunities for social welfare-enhancing interventions [Hoxby, 2000]. However, the identification and estimation of social effects is a difficult task, especially in a world of non-experimentally derived observations where classrooms are the result of active choices. First, an individual and her peers simultaneously influence each other, a "reflection problem " that hinders the identification [Manski, 1993]. Second, individuals self-select into peer groups based on observed and unobserved characteristics, and third, there are confounding effects due to common institutional environment shared by all members of the peer group, such as teacher ability.

In the second chapter of this dissertation, I show that extending the standard linear model of social interactions estimated in the network literature to a dynamic setting broadens the possibilities for identification of peer effects. More specifically, I show that the reflection or simultaneity problem can be solved by taking advantage of the dynamics of the model and exploiting instruments that are internally generated by the model. Importantly, unlike previous efforts, this set of instruments does not require the existence of partially overlapping peers (e.g., friendship links within the classroom). Thus, social effects are identified even when students interact in groups. This is a relevant contribution to the literature, as in many situations researchers lack data on interactions within groups and have to specify groups at the level of the grade, classroom, or roommates [Hanushek et al., 2003, Burke and Sass, 2013, Sacerdote, 2001]. To account for self-selection into the network, the longitudinal dimension of the model allows me to include student fixed effects so that peer effects are identified from within-student variation in the network over time. Finally, the inclusion of school and teacher fixed effects allow me to control for the presence of correlated effects, such as teacher ability and school unobserved characteristics.

I use this model to estimate the impact of classroom peers on students' achievement. To this end, I use longitudinal student-level administrative data from Chile. I find that social effects are positive but only significant for math. In particular, one standard deviation increase in the average math test scores of classmate peers increases own test scores by 0.09 standard deviation. Moreover, my results indicate that peer effects estimates tend to be overestimated when not dealing with the main confounding issues. In particular, they reinforce the importance of controlling for unobserved teacher quality. When exploring heterogeneous effects, I find that exposure to opposite-gender peers leads to higher academic performances on math for boys but not for girls. This result could be related to gender stereotypes that affect girls' attitudes and self-perceptions toward math. I also find that higher achievers experience the largest effects from high-ability peers. Conversely, students with low initial achievement levels appear to benefit less and may even experience adverse effects from top ability students. This suggests that some degree of tracking should be preferred to completely mixed or random classrooms.

In my third chapter, I study the effect of a targeted voucher program on a series of cognitive, non-cognitive, and behavioral outcomes, ranging from student test scores to aggressive behavior (bullying). I exploit a national targeted voucher policy implemented in Chile, which increased the funding for disadvantaged students by 50%. The purpose of this targeted voucher policy (SEP) was to expand the student choice set at which low-income students could enroll and acknowledge the fact that educating low-income students is costly by giving schools an incentive to serve disadvantaged students. Eligible schools had to sign up for the policy and agree not to charge out-of-pocket tuition to priority students, which also expanded the set of school choice set at which low-income students could enroll at no cost. Using administrative data for the universe of 8th graders and a difference-in-differences strategy, I confirm previous findings that this policy did not significantly improve students' test scores [Feigenberg et al., 2019, Aguirre, 2020]. However, I show that the program led to meaningful improvements in students' non-cognitive and behavioral outcomes. Few prior studies have examined the effects of school vouchers on other student outcomes beyond test scores, and they have focused mainly on their impact on educational attainment [Angrist et al., 2006, 2002, Chingos and Peterson, 2015, Cowen et al., 2013].

In particular, I find that the targeted voucher policy is associated with an increase in academic self-efficacy and motivation and the students' expectations of attending higher education. Also, the policy reduced the probability of experiencing peer discrimination, the frequency (prevalence) of bullying suffered, and the occurrence of violent events in the school. These effects are large and economically significant in general. For example, the expectation of students to attend higher education increased between one to two percentage points in schools that signed up for the policy (from an average of 86% before the reform), the probability of suffering bullying decreased by three percentage points (from an average of 54% pre-reform), and the percentage of students reporting being discriminated against decreased by 5 percentage points (from an average of 73% pre-reform). Finally, I provide evidence of the primary channel behind my results: schools participating in the policy have used the additional resources to hire relatively more learning support staff, especially from the psycho-social area.

My last chapter, written in collaboration with Sergio Urzúa and Shanjukta Nath, investigates whether adopting a centralized school admission system can alter within-school socio-economic diversity. We examine a centralized school admission system that the Chilean government introduced in 2016 as the central component of a major education reform aimed at promoting social inclusion and reducing the high levels of school segregation. The new school admission system (SAS) was undertaken between 2017 and 2019, and it came to replace the country's widely studied decentralized system [Epple et al., 2017, Hsieh and Urquiola, 2006, Mizala and Romaguera, 2000].[2] We exploit its sequential introduction of the reform across regions to quantify its heterogeneous impact on segregation. The empirical analysis is carried out using administrative data and a difference-in-difference strategy. We find no impact of the new policy on school segregation. However, the richness of the data allows us to go beyond the average impact and explore the heterogeneous effects suggested by the theory. Specifically, we document that SAS increased within-school segregation in areas with high levels of pre-existing residential segregation. School districts with higher provision of private education experienced an uptick in school segregation as well. The migration of high-SES students to private schools emerges as a driver.

All in all, we can extract several lessons from this dissertation. The second chapter suggests

[2] The previous (decentralized) student admission process was highly unregulated until 2008. Most private-voucher schools (privately managed schools but financed by government vouchers) selected students based on parents' interviews, entry exams, and/or required proof of income.[OECD, 2010].

that policy reforms could be improved by recognizing the effects that social interactions play in education, especially the role peers play in affecting outcomes of low-ability and disadvantaged students. By internalizing peer effects in the educational production function, one can potentially take full advantage of school and class assignment policies as tools to improve student academic and non-academic outcomes. My following two chapters suggest that a look at test scores alone is almost surely not sufficient to capture the full impact of the policies and sheds light on the importance of reforms being adapted to the local market conditions. For instance, in contexts with high levels of residential segregation, disadvantaged students are surrounded by worse quality schools and weaker peer groups. Thus, centralized admission systems could be problematic as schools are locally consumed goods. In these circumstances, providing support for families in navigating the system and transportation to schools is much more likely to have an integrating impact [Chingos and Monarrez, 2020].

Chapter 2: Dynamic Social Interactions at School

2.1 Introduction

Social interactions in education have received attention from researchers for decades. The notion that the influence of classmates and friends is an important determinant of student outcomes has fueled an extensive literature that continues to expand [see Sacerdote, 2011 and Epple and Romano, 2011 for a comprehensive literature review]. Understanding the nature and magnitude of these effects is critical for designing effective most educational policies.

However, the identification and estimation of social effects have proved to be challenging. First, an individual and her peers simultaneously influence each other, which is known as the "reflection problem" [Manski, 1993]. The reflection problem hinders the identification of endogenous effects from exogenous effects, i.e., how an individual's outcome is affected by her peers' outcome versus her peers' characteristics. Another difficulty is the separation of social effects from other confounding effects due to common institutional environment or unobserved shocks faced by all peer group members (e.g., teacher effects). Third, endogeneity may exist due to individuals' self-selection into the network. In addition to these conceptual problems, the estimation of social interactions also faces substantial data limitations, as most data sets do not provide detailed information about an individual's reference group [Lin, 2010].

These obstacles limited our understanding of peer effects and have hindered our scope for internalizing social effects in the design of education policies. Recently, the fast-growing interest in networks has expanded the identification possibilities. In particular, work by Bramoullé et al. [2009], Calvó-Armengol et al. [2009] and De Giorgi et al. [2010] show that the reflection problem can be solved by exploiting the static structure of the network in contexts of partially overlapping groups of peers, i.e., when peers of peers are not necessarily peers, and linear models.[1]

In this paper, I develop a model of social interactions at school and show that extending the standard linear model to a dynamic setting broadens the possibilities for identification of peer effects. More specifically, I show that the reflection or simultaneity problem can be solved by taking advantage of the dynamics of the model and exploiting instruments that are internally generated by the model. Importantly, this set of instruments does not require the existence of partially overlapping peers (e.g., friendship links within the classroom). Instead, one can rely on the evolution of the students' social network and draw on instruments such as the predetermined characteristics of the past classmates of a student's current classmates. Those characteristics impact individual outcomes only through their effect on current classmates' outcome. Thus, social effects are identified even when a student is assumed to be equally affected by all other members in their classroom and no one outside of it. This is a convenient and necessary assumption in many situations due to the lack of data on interactions within groups.[2]

To account for the presence of correlated effects, such as teacher ability and school unobserved characteristics, I include school and teacher fixed effects. To deal with the fact that classrooms do not form randomly but are themselves the result of individual choices, I furthermore include

[1]A common example or partially overlapping peers in the education literature are friendship links in the classroom. Most of the papers exploiting this variation use datasets from the National Longitudinal Study of Adolescent to Adult Health, which collects information of a sample of individuals and their friends at school [Bramoullé et al., 2009, Calvó-Armengol et al., 2009, Patacchini et al., 2017].

[2]In the educational context, many studies have to specify groups at the level of the grade, classroom or roommates [Hanushek et al., 2003, Burke and Sass, 2013, Sacerdote, 2001].

student fixed effects to account for time-invariant unobserved individual characteristics that influence both the network formation and the student achievement. In this way, the effect of social interactions on educational outcomes will then be identified from within-student variation in the network over time.

I use this model to estimate the impact of classroom peers on students' achievement. To this end, I use longitudinal student-level administrative data from Chile. Chile's administrative data offers several attractive elements for studying social interactions. The most important feature is that one can follow students throughout their schooling history and construct their entire network of current and historical classmates. The data also contains detailed information on teachers and schools. My final sample consists of approximately 260,000 students of the high school graduating classes of 2018 and 2019. A highlight of these cohorts is that they took the national standardized exams three times: in 4th grade, 8th grade, and 10th grade.

After dealing with the issues of the reflection problem, correlated effects and sorting into the network, I find that social effects are positive but only significant for math. In particular, one standard deviation increase in the average math test scores of classmate peers increases own test scores by 0.09 standard deviation. My results indicate that peer effects estimates tend to be overestimated when not dealing with the main confounding issues. In particular, they reinforce the importance of controlling for unobserved teacher quality. The magnitude of these effects is generally smaller than those previously found in the literature. For example, the estimated social effects in Hanushek et al. [2003] range from 0.15 to 0.24 standard deviation in math test scores, and about 0.25 standard deviation in Lin [2010].

However, I find some sizable and significant social effects when exploring heterogeneous models. For example, peer influences differ for male and female students. In math, for instance, exposure to opposite-gender peers leads to higher academic performances for boys but not for girls. This result could be related to gender stereotypes and girls having a positive effect on the learning environment[Hoxby, 2000, Lavy and Schlosser, 2011]. Also, the evidence suggests that higher achievers experience the largest effects from high ability peers. Contrarily, students with low initial achievement levels appear to benefit less, and may even experience negative effects, from top ability students.

An important limitation of my setting is that students are assumed to be equally affected by all other members in the same classroom. I acknowledge that individuals may be more likely to be predominantly influenced by some of their classmates [Lin, 2010]. For example, a high-achieving classmate might be a role model or a competitor. In my data, I observe classmates, but I lack information on the strength of their links. Hence, I allow for higher-order spatial lags, letting different subgroups have differential social impacts. This helps me to capture different forms of proximity between students.

The paper contributes to the literature in different ways. First, it adds to the peer effects literature that exploits the structure (intransitivity) of the network to identify social effects [Bramoullé et al., 2009, De Giorgi et al., 2010], an approach that has borrowed from the spatial statistics literature [see Kelejian and Prucha, 1998, Lee, 2003]. I extend the standard linear social interactions model to a dynamic setting and show that allowing for time variation in the students' network expands the possibilities for identification of peer effects. More specifically, I show that the dynamics enable the identification even in the absence of partially overlapping peers. This is important because there are not many surveys that collect detailed information on network links (e.g., AddHealth data set), and collecting high-quality network data might be prohibitively costly.

I also contribute to the literature of peer effects in education. In general, this research focuses on reduced-form specifications in which students' achievement are assumed to be a function of contemporaneous inputs only [Calvó-Armengol et al., 2009, Patacchini et al., 2017, Del Bello et al., 2015]. In this paper, I consider a dynamic model in which a student chooses how much effort to exert in class each period. These effort decisions, which determine achievement, are influenced not only by current family inputs and social interactions at school, but also by those of the past. As pointed out by Hanushek et al. [2003], a value-added specification that takes into account the history of inputs is a more convincing specification in peer estimation because of omitted variable bias.[3]

The paper is organized as follows. Section 2.2 presents a dynamic model of social interactions at school. Section 2.3 reviews the main identification problems and discusses how the dynamics of the model can aid with these issues. Section 2.4 discusses the data and describes the final sample. Sections 2.5 and 2.6 demonstrate the empirical framework and present my estimation results on student academic achievement, respectively. Concluding remarks are provided in section 2.7.

2.2 Model of dynamic social interactions at school

Social interactions are present if individual behavior is affected by reference group behavior, characteristics, or both [Manski, 1993]. To study the role of social interactions at school, I consider a model in which students are optimizing agents whose effort decisions are influenced by their peers, and these decisions determine achievement, as in Fruehwirth [2013]. In my setting,

[3] Hanushek et al. [2003] argue that omitted variable bias is critical in peer effects estimation because peer groups tend to have similar experiences over time. If many omitted historical factors are common to the peer group, then it will create correlation between contemporaneous peers characteristics and the error term, which could make current peer appear important when they are not.

however, student's effort decision not only depends on contemporary inputs but also on those from the past [Hanushek et al., 2003]. This is relevant because ignoring history could lead to inconsistent estimates of social effects due to omitted variable bias.[4]

Since students interact mostly with other students in their classroom, both academically and socially, we can think of a student's peer group as her classmates in a particular period of time. Peers in a classroom can be seen as a source of motivation, inspiration and direct interaction in learning. The classroom is an important peer group definition, as it is something that can be manipulated by policy makers, parents, teachers, and principals.

Let consider T periods of time ($t = 1, \cdots, T$) and n students ($i = 1, \cdots, n$). Define $d_{ij,t} = 1$ if individuals i and j are classmates in year t, and $d_{ij,t} = 0$ otherwise. Let $\sum_{j=1}^{n} d_{ij,t} = n_{i,t}$ be the number of peers of i in period t and define the $n \times n$ adjacency matrix $\mathbf{G}_t = (g_{ij,t})$, with $g_{ij,t} = d_{ij,t}/n_{i,t}$. Hence, $\mathbf{G}_t$ is row normalized (i.e., its row sums take value one), which implies that social effects operate through a weighted sum over peers, with weights summing to one. The links are undirected, so $g_{ij,t} = g_{ji,t}$ (i.e. $\mathbf{G}_t$ is symmetric), and $g_{ii,t} = 0$, meaning that individuals are not linked to themselves. I discuss endogenous peer group formation in the next section.

Given the network structure, each period, student i chooses how much effort e_{it} to exert in class, which includes behavioral choices that affect her engagement, such as attention, attendance, time spend on classroom assignments, among others. Student's payoff depends on the effort profile $\mathbf{e}_t = (e_{1t}, \cdots, e_{nt})$ and the underlying network, and is defined as:

[4] Omitted variable bias is critical in peer effects estimation because peer groups tend to have similar experiences over time. If many omitted historical factors are common to the peer group, then it will create correlation between contemporaneous peers characteristics and the error term [Hanushek et al., 2003]. This could make current peers appear important when they are not.

$$U_{it}\big(\mathbf{e}_t, \mathbf{G}_t\big) = \big(\pi_{it} + \lambda \sum_{j=1}^{n} g_{ij,t} e_{jt}\big) e_{it} - \frac{1}{2} e_{it}^2. \tag{2.1}$$

This linear-quadratic utility function, with a cost-benefit structure, is relatively standard in games on networks [Calvó-Armengol et al., 2009, Jackson and Zenou, 2015, Patacchini and Zenou, 2012]. The benefit component is modeled as a linear function of own effort level e_{it} with the term $(\pi_{it} + \lambda \sum_{j=1}^{n} g_{ij,t} e_{jt})$ representing the return to effort. The component $\lambda \sum_{j=1}^{n} g_{ij,t} e_{jt}$ represents the impact of the aggregate effort of i's peers on her own "productivity". A student may benefit directly from the effort of her classmates if they work hard and collaborate with each other. We can also think that a student works hard based on whether others do so. The term π_{it} represents ex-ante individual heterogeneity in the return to effort, and is given by

$$\pi_{it} = \alpha_0 + \alpha_1 X_{it} + \alpha_2 \sum_{j=1}^{n} g_{ij,t} X_{jt} + \alpha_3 S_t + \alpha_4 e_{i,t-1}, \tag{2.2}$$

where X_{it} and S_t denote individual and school characteristics, respectively. Students are also affected by the socioeconomic characteristics of the peers, $\sum_{j=1}^{n} g_{ij,t} X_{jt}$. For instance, we can think that the classmates' parental educational level influences students' aspirations and thus, affects students' utility from effort [Durlauf and Ioannides, 2010]. Hence, the utility function is such that peers may affect student's utility through their characteristics (contextual or exogenous effects), $\sum_{j=1}^{n} g_{ij,t} X_{jt}$, and through their own effort (endogenous peer effects), $\sum_{j=1}^{n} g_{ij,t} e_{jt}$.

Importantly, the effort in the previous period, $e_{i,t-1}$, is taken to be a sufficient statistic for student effort in all prior periods. Hence, the "productivity" of the students' effort depends not only on current inputs (including social interactions) but also on those of the past. This value-added specification is considered to be more convincing than that based only on contemporary inputs [see, for example, discussion in Hanushek et al., 2003, Todd and Wolpin, 2003 and Burke and Sass, 2013]. The main assumptions it imposes on the underlying production technology are that the input coefficients are geometrically declining with time distance from the achievement measure and that the $U_t(\cdot)$ function is the same for every period (i.e., $U_t(\cdot)$ is non-time-varying). As an example, suppose we are looking at student achievement in 4th grade (t=4). Thus, the endogenous effect from peers in 3rd grade on the student achievement in 4th grade is bigger than the endogenous effect from peers in 2nd grade because the input in 3rd grade is closer in its distance from the achievement measure. In addition, the impact of peers in 3rd grade on achievement in 4th grade is the same as the effect of peers in 4th grade on student achievement in 5th grade because the inputs are equally distant from the achievement measure.[5]

Each student maximizes her utility 2.1 and the best response function for each student i is given by:

$$e_{it}^* = \alpha_0 + \lambda \sum_{j=1}^{n} g_{ij,t} e_{jt} + \alpha_1 X_{it} + \alpha_2 \sum_{j=1}^{n} g_{ij,t} X_{jt} + \alpha_3 S_t + \alpha_4 e_{i,t-1}^*. \tag{2.3}$$

In this game on networks, a pure strategy Nash equilibrium involves everyone choosing their effort level $e_{it} > 0$ simultaneously according to the best response 2.3. Let $\mu(\mathbf{G}_t)$ denote the spectral radius of $\mathbf{G}_t$. It is possible to show that if $\lambda\mu_1(\mathbf{G}_t) < 1$, then the game has a unique (and interior) Nash equilibrium in pure strategies [Jackson and Zenou, 2015]. For details, see Appendix A.1.1.

Because effort is not observable, this linear best response function cannot be estimated directly. Let us assume the achievement production function of student i depends on her own and

[5] See Todd and Wolpin [2003] for a more detailed discussion on the assumptions of value-added specifications.

her peer's effort level, individual and peer characteristics, as well as classroom and school inputs. If the achievement production function is linear and monotonically increasing in effort, the game in effort maps into a game in achievement that is observable in the data (see Appendix A.1.2). Hence, assuming a standard linear achievement production function, with separable inputs, the achievement equilibrium can be expressed as:

$$Y_{it}^{*} = \alpha + \delta \sum_{j=1}^{n} g_{ij,t} Y_{j,t} + \beta X_{i,t} + \gamma \sum_{j=1}^{n} g_{ij,t} X_{j,t} + \eta S_t + \sigma Y_{i,t-1}^{*} + u_{it}, \tag{2.4}$$

which resembles the standard production functions with peer effects estimated in the literature [Bramoullé et al., 2009, Calvó-Armengol et al., 2009, De Giorgi et al., 2010], with the exception that it takes into account the entire history of relevant inputs in the form of a value-added specification.

2.3 Identification

The identification of the dynamic model of peer effects (equation 2.4) poses several challenges. First, i's achievement and peer achievement are simultaneously determined, which makes it impossible to separately identify endogenous effects -how an individual's outcome is affected by her peers' outcome- from contextual or exogenous effects -influences of peers' exogenous characteristics- in the usual linear model. This is the reflection problem suggested by Manski [1993]. Second, social interaction models are susceptible to endogeneity concerns due to endogenous network formation and correlated effects.

Endogenous network formation arises because of the absence of random allocation of students across schools. If the variables that drive the process of school choice are not fully observable by the researcher and also affect the learning process, then these unobservables may be major sources of bias. Correlated effects arise because students in the same classroom tend to behave similarly because they face a shared environment or common shocks (e.g., teacher effects). Hence, we can think of the error term as being of the following form:

$$u_{it} = \mu_i + \mu_k + \mu_s + \epsilon_{it}. \tag{2.5}$$

The three terms in the right-hand side capture unobserved individual time-invariant characteristics μ_i (e.g. ability), as well as unobserved teacher, μ_k, and school time-invariant effects, μ_s. For example, if high ability students tend to associate with high ability peers, then the social interaction coefficient will be overestimated due to omitted ability bias. In addition, the social interaction coefficient may reflect the effects of common institutional environments or group shocks. A common example are teachers effects. A good teacher tends to improve the performance of the entire class, so no controlling for teacher quality will also overestimate the effect of social interactions.

In this section I discuss how I take advantage of the dynamics of the model to deal the main identification issues: reflection problem, correlated effects and sorting into networks.

2.3.1 Dealing with the Reflection Problem

The reflection problem arises because the student achievement and her peer group achievement are jointly determined. This simultaneity problem hinders the identification of endogenous peer effects from exogenous peer effects in the standard contemporaneous linear-in-means model when individuals interact in groups (e.g., their classroom) [Manski, 1993]. More specifically, social effects are not identified in this setting because the expected group mean outcome is a linear function of the group mean characteristics.[6,7] Hence, any correlations between an individual's outcome and her reference group's mean outcome may simply reflect the effects of the group mean characteristics [Lin, 2010].

Recent papers, including Lin [2010], Bramoullé et al. [2009], Calvó-Armengol et al. [2009], De Giorgi et al. [2010], analyze the identification of social effects in a context of contemporary partially overlapping peers, that is, when peers of my peers are not necessarily my peers. Thus, some peers of peers only affect an individual indirectly though her peers, and the characteristics of peers of peers may serve as instruments for peers' behavior of an individual. I adapt this strategy to a more general setting. By exploiting the dynamic evolution of the students' social network, I construct excluded instruments.

For illustrative purposes, let's initially assume no correlated effects or self-selection into classrooms. For simplicity, I suppress the constant (assume all variables are in deviation from means) and omit the the school and teacher covariates in period t, $\mathbf{S}_t$. Hence, in matrix notation, Equation 2.4 can be expressed as:

$$\mathbf{Y}_t = \delta \mathbf{G}_t \mathbf{Y}_t + \beta \mathbf{X}_t + \gamma \mathbf{G}_t \mathbf{X}_t + \sigma \mathbf{Y}_{t-1} + \mathbf{u}_t. \tag{2.6}$$

[6] In a simple contemporaneous linear-in-means case when students interact in groups and the student is included when computing the mean, such that $\mathbf{G}_t^2 = \mathbf{G}_t$, the expected mean outcome of peers $E(\mathbf{GY})$ is given by $\alpha/(1-\beta)\boldsymbol{\iota} + (\gamma+\delta)/(1-\beta)\mathbf{GX}$, meaning that is perfectly collinear with the mean characteristics of the group [see Bramoullé et al., 2009 for a more detailed discussion].

[7] Technically, it is possible to identify the parameters in Equation 2.6 as long as individuals interact in groups of different sizes, as discussed in Lee [2007] and Bramoullé et al. [2009]. However, the identification will be weak if group sizes are very similar, very large, or if there is a separate group size impact on the outcome [Gibbons et al., 2015].

Since $|\delta| < 1$, $(\mathbf{I} - \delta\mathbf{G}_t)$ is invertible, the reduced form of Equation 2.6 can be written as:

$$\mathbf{Y}_t = (\mathbf{I} - \delta\mathbf{G}_t)^{-1}(\beta\mathbf{I} + \gamma\mathbf{G}_t)\mathbf{X}_t + \sigma(\mathbf{I} - \delta\mathbf{G}_t)^{-1}\mathbf{Y}_{t-1} + (\mathbf{I} - \delta\mathbf{G}_t)^{-1}\mathbf{u}_t. \tag{2.7}$$

In a setting where students interacts in groups, groups have equal size m, and the student is excluded from her peer group (i.e., leave-me-out type mean), it can be shown that $(\mathbf{I} - \delta\mathbf{G}_t)^{-1} = \rho_1\mathbf{I} + \rho_2\mathbf{G}_t$, with $\rho_1 = \frac{(m-1)(1-\delta)+\delta}{(m-1+\delta)(1-\delta)}$ and $\rho_2 = \frac{\delta(m-1)}{(m-1+\delta)(1-\delta)}$ [Kuersteiner, Prucha and Zeng, 2022]. See appendix A.1.3 for more details. Replacing this into Equation 2.7, yields:

$$\mathbf{Y}_t = (\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)(\beta\mathbf{I} + \gamma\mathbf{G}_t)\mathbf{X}_t + \sigma(\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)\mathbf{Y}_{t-1} + (\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)\mathbf{u}_t. \tag{2.8}$$

After a few manipulations, and replacing $\mathbf{G}_t^2 = \lambda_0\mathbf{I} + \lambda_1\mathbf{G}_t$ where $\lambda_0 = \frac{1}{m-1}$ and $\lambda_1 = \frac{m-2}{m-1}$, we have:

$$\mathbf{Y}_t = \tilde{\beta}\mathbf{X}_t + \tilde{\gamma}\mathbf{G}_t\mathbf{X}_t + \sigma(\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)\mathbf{Y}_{t-1} + (\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)\mathbf{u}_t, \tag{2.9}$$

where $\tilde{\beta} = \rho_1\beta + \rho_2\gamma\lambda_0 = \frac{\beta(m-1)-\delta\beta(m-2)+\delta\gamma}{(m-1+\delta)(1-\delta)}$ and $\tilde{\gamma} = \rho_1\gamma + \rho_2(\beta + \gamma\lambda_1) = \frac{(m-1)(\delta\beta+\gamma)}{(m-1+\delta)(1-\delta)}$. Iterating backwards, we obtain:

$$\begin{aligned}\mathbf{Y}_t = \sigma^t \prod_{h=0}^{t-1}[\rho_1\mathbf{I} + \rho_2\mathbf{G}_{t-h}]\mathbf{Y}_0 + \tilde{\beta}\mathbf{X}_t + \tilde{\gamma}\mathbf{G}_t\mathbf{X}_t + \sigma(\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)(\tilde{\beta}\mathbf{X}_{t-1} + \tilde{\gamma}\mathbf{G}_{t-1}\mathbf{X}_{t-1}) \\ + \sigma^2(\rho_1\mathbf{I} + \rho_2\mathbf{G}_t)(\mathbf{I} + \sigma\mathbf{G}_{t-1})(\tilde{\beta}\mathbf{X}_{t-2} + \tilde{\gamma}\mathbf{G}_{t-2}\mathbf{X}_{t-2}) + \dots\end{aligned} \tag{2.10}$$

Therefore, by taking the conditional expectation and pre-multiplying by $\mathbf{G}_t$, it is possible to show that the endogenous regressor $\mathbf{G}_t\mathbf{Y}_t$ can be expressed in terms of a set of internally generated excluded instruments:

$$
\begin{aligned}
E(\mathbf{G}_t\mathbf{Y}_t) &= \sigma^t\mathbf{G}_t\prod_{h=0}^{t-1}[\rho_1\mathbf{I}+\rho_2\mathbf{G}_{t-h}]\mathbf{Y}_0+\tilde{\gamma}\lambda_0\mathbf{X}_t+(\tilde{\beta}+\tilde{\gamma}\lambda_1)\mathbf{G}_t\mathbf{X}_t \\
&\quad+\sigma(\rho_2\lambda_0\mathbf{I}+(\rho_1+\rho_2\lambda_1)\mathbf{G}_t)(\tilde{\beta}\mathbf{X}_{t-1}+\tilde{\gamma}\mathbf{G}_{t-1}\mathbf{X}_{t-1}) \\
&\quad+\sigma^2(\rho_2\lambda_0\mathbf{I}+(\rho_1+\rho_2\lambda_1)\mathbf{G}_t)(\rho_1\mathbf{I}+\rho_2\mathbf{G}_{t-1})(\tilde{\beta}\mathbf{X}_{t-2}+\tilde{\gamma}\mathbf{G}_{t-2}\mathbf{X}_{t-2})+\ldots
\end{aligned}
\tag{2.11}
$$

From this expression we see that the possible set of instruments for $\mathbf{G}_t\mathbf{Y}_t$ corresponds to the linearly independent columns of $\{\mathbf{G}_t\mathbf{X}_{t-j}, j=1,2\ldots\}$, $\{\mathbf{G}_t\mathbf{G}_{t-j}\mathbf{X}_{t-h}, j=1,2\ldots,h=j,j+1\ldots\}$, $\{\mathbf{G}_t\mathbf{G}_{t-j}\mathbf{G}_{t-h}\mathbf{X}_{t-k}, j=1,2\ldots,h=j+1,j+2\ldots,k=h,h+1,\ldots\}$, and so on. For instance, if $T=3$, then the candidate instruments for $\mathbf{G}_t\mathbf{Y}_t$ are $\mathbf{G}_t\mathbf{X}_{t-1}$, $\mathbf{G}_t\mathbf{X}_{t-2}$, $\mathbf{G}_t\mathbf{G}_{t-1}\mathbf{X}_{t-1}$, $\mathbf{G}_t\mathbf{G}_{t-1}\mathbf{X}_{t-2}$, $\mathbf{G}_t\mathbf{G}_{t-2}\mathbf{X}_{t-2}$, and $\mathbf{G}_t\mathbf{G}_{t-1}\mathbf{G}_{t-2}\mathbf{X}_{t-2}$. These variables are uncorrelated with $\mathbf{u}_t$ but correlated with the endogenous regressor $\mathbf{G}_t\mathbf{Y}_t$.

Hence, the dynamics provide instruments beyond the quadratic moments $(\mathbf{G}_t^2\mathbf{X}_t, \mathbf{G}_t^3\mathbf{X}_t, \cdots)$ that have been used in the literature so far (e.g., characteristics of contemporaneous friends of friends). Even if students interact in groups and the group sizes are the same, social effects can be identified, either if there is time variation in $\mathbf{G}_t$ or in $\mathbf{X}_t$ over time.[8]

An interesting set of instruments is $\{\mathbf{G}_t\mathbf{G}_{t-j}\mathbf{X}_{t-h}; j=1,2\ldots; h=j,j+1\ldots\}$, which represents the average past characteristics of previous classmates of student i's peers. The exogenous characteristics of the links of (time) distance greater than one are valid instruments for the endogenous effects. This is because they are uncorrelated with the classroom current shocks but correlated with the mean outcome of student i's current peers, $\mathbf{G}_t\mathbf{Y}_t$, through endogenous

[8]Notice that even if students interact in groups and there is no variation in groups over time, there are still instruments when there is variation in $\mathbf{X}_t$ (e.g.,$\mathbf{G}_t\mathbf{X}_{t-1}$, $\mathbf{G}_t\mathbf{X}_{t-2}$, etc.). However, if there is no meaningful variation in the students' characteristics over time, then lagged peer characteristics may not be exogenous to current student achievement, due to the cumulative nature of the education production function.

interactions. For example, let's consider the simple case illustrated in Section 2.1. Students 2, 3, 4 and 5, in red dots, are part of a current classroom (connected by a solid line), whereas students 1, 6, 7, 8, 9, in grey dots, reflect their past classmates (connected with dashed lines). Let's assure student 1 was classmate with student 2 in the previous period $t-1$, but is not part of student 2's current classroom in t. Then, student 1 would not be affected by the current group shock but has an impact on student 2's achievement through the education production function described above. The logical chain that guarantees the relevance of the instrument is that the characteristics of student 1 have a direct impact on her own effort, which in turn impacts the effort, and therefore the outcome, of student 2 through endogenous interactions.

Thus, instead of exploiting the contemporaneous distance in the network (i.e., the quadratic moments of the adjacency matrix), as has been the common approach in the literature, one can exploit the history of past interactions of students (i.e., the lagged adjacency matrices). Note that this strategy requires the existence of intransitivity in classrooms peers if there is no time variation in the characteristics of the students. Figure 2.2 shows a simple example of two small classrooms in two consecutive periods. The first case displays a scenario with no students switching classrooms between periods $t-1$ and t, in which case there is no variation for identification. Case 2 displays a situation in which one student switches classroom between the two periods. In this case, the pre-determined characteristics of students 5 and 7 can be used as instruments for student 6's endogenous outcome. Finally, the third case shows a situation with two switches, so we can have identifying variation in both groups of students. The strength of this instrument depends on the level of intransitivity (i.e., class turnover) of the classrooms peers.

2.3.2 Dealing with Endogenous Network Formation and Correlated Effects

Social effects are also contaminated by correlated effects and sorting into the network. These issues are reflected in the error term in equation 2.5. An important source of correlated effects are common factors that influence achievement within a peer group and may bias peer effects estimation. In the educational context, the main suspects are common institutional environment such as teacher ability and school unobserved characteristics.[9]

To account for those potentially time varying correlated effects I include teacher and school fixed effects. In addition, the instrumental variable approach defined above also aids with this issue because none of the past classmates of student i's peers are affected by the same group shock as i, because they were classmates with i's peers in the past.

A further concern is that peer groups do not form randomly but are themselves the result of individual choices made by parents, teachers and/or school principals. Given the absence of random allocation of students across schools and classrooms, a student and her network tend to have similar preferences, which may confound the estimation.

To address this issue I that take advantage of the dynamic feature of my setting and include student-level fixed effects, which account for all student and family characteristics that do not vary over time and affect both the learning process and the network formation (for example, parental attitudes towards school and peers). This helps to attenuate the endogeneity problem due to non-random selection into schools from unobservable variables affecting class formation and achievement that are time-invariant within the period of study. Due to data limitations, most

[9] It is noteworthy that omitted correlated effects can bias social effects estimation even under random assignment because the correlated factor promotes similarity of outcomes within groups regardless of how the peer group is selected.

of the existing papers use network fixed effects to address this issue (Bramoullé et al., 2009; Calvó-Armengol et al., 2009; Lin, 2010; Patacchini et al., 2017b). The underlying assumption is that the unobservable factors that drive the network formation are common to all individuals belonging to the same network. However, if there are individual-level unobservables that drive both network formation and outcomes choices, this strategy might not be enough to control for endogeneity biases.[10]

Ultimately, in the model with individual fixed effects, social interactions are identified from within-student variation in its network over time. Because of this, it is important to understand the factors behind the sorting of students across schools and classrooms. I discuss this in more detail below.

2.3.2.1 Exogenous mobility?

Student mobility across schools and classrooms is the main source of identification in my model, as shown in Figure 2.2. If a student i experiences no change in her peer group throughout her schooling years, then that student will not contribute to the estimation of peer effects, given the student fixed-effects structure. Therefore, an important question is what drives school mobility.

School switches are the result of an active choice, and those who switch schools could be different from those who do not. Sorting based on student ability or school characteristics create no bias since we condition on time-invariant student and school observables and unobservables. For instance, high ability students could be more likely to engage on school search and therefore

[10] An even more flexible approach to deal with the issue of general endogenous group formation that I'm planning to explore in the near future is to use predicted students networks based on predetermined dyadic characteristics to construct the instruments.

be more likely to move to high quality schools over time. That type of sorting does not create any concerns for the identification in this setting.

Other sorts of endogenous mobility could be more problematic. School mobility will be correlated with the achievement gain shock u_{it} if students move because of family events that affect both their educational achievement and their mobility, such as unemployment or family break-ups. If students who experience negative family events move to worse schools, resulting in a particular weak peer group, the estimates of social effects are likely to be overestimated.

In my empirical application, I take advantage of an institutional feature of my context that helps mitigate the endogenous mobility bias in measuring peer effects: compulsory school mobility. Most schools in Chile provide either primary or secondary education, meaning that a significant number of students are forced to switch schools at the end of primary education. These "structural switches" help mitigate the endogenous mobility bias in measuring peer effects.

2.4 Data

The identification strategy described above requires data on students' social networks and their achievement on a standardized metric in multiple grades. In addition, it needs student, teacher, and school identifiers to be able to control for other confounding effects. Chile's administrative data fulfill these requirements and offer additional attractive elements for studying social effects in education.

The Ministry of Education in Chile provides information on the school and classroom of each student, which allows to follow all students throughout their schooling history and construct their entire network of current and historical classmates. Also, students are assessed across several grades through an array of national standardized tests (known as SIMCE tests). These exams are administered at the end of the school year and are marked by the national educational authority. Importantly, as part of the process, parents must answer comprehensive questionnaires that provide me with relevant background information such as parental education and household income.

In addition, the data offers rich administrative records at the student, teacher, and school level. The student-level records include information on school enrollment, attendance rate, and demographic characteristics such as gender and age. The teacher database contains information on the gender and age of the teacher, the subject taught, the number of years she has been in the educational system, the number of years she has been teaching at the specific school, and educational background information of the teacher. Teachers can be linked to each specific classroom because school and class identifiers are provided. Finally, it provides information on school characteristics.

A relevant feature of the Chilean context is that student mobility levels are high. In my dataset, more than 70% of students have switched schools within the sample. This high mobility is mostly driven by an institutional characteristic of the schooling system. In Chile, compulsory education lasts 12 years and is organized in two cycles: primary education (grades 1 to 8), and secondary education (grades 9 to 12). However, most schools provide either primary or secondary education, forcing a significant number of students to switch schools at the end of primary education, or 8th grade.

2.4.1 Sample and Descriptive Statistics

I focus on two consecutive cohorts of students who entered 1st grade in 2007 and 2008 and were expected to graduate from high school in 2018 and 2019, respectively. A highlight of those cohorts is that they took the standardized exams three times: in 4th, 8th, and 10th grade. This allows me to construct a panel of students with three time periods ($T = 3$).

Of about 200,000 students who participated in SIMCE in 4th grade in each cohort, I observe 65% of them again in the 8th and 10th grade SIMCE. I lose students who were grade-retained, left the formal schooling system and those who did not take the standardized tests in both 8th and 10th grade. I remove special and adult education from my sample and drop classes with less than five students.

My final sample consist of 261,874 students across both cohorts. As mentioned before, an attractive feature of my context is the high school mobility. Between 4th and 10th grade, 70% of the students in my sample have switched schools at least once. An important proportion of those switches is explained by compulsory mobility: at least 40% of 8th graders are forced to switch schools at the end of the academic year to continue their education because their schools do not offer secondary education.

To exploit this source of variation in the students' peer group I focus the empirical analysis on the transition between primary and secondary education. Table 2.1 summarizes the main outcomes and characteristics of the students and their peers in 8th and 10th grade. Table 2.2 reports characteristics of teachers, including the age, the number of years in the educational system, the number of years teaching in the current school, the percentage who have a university degree, among others. Teachers are predominately females, although the presence of male teachers increases in high school. I observe each teacher with an average of three groups of students.

2.5 Econometric Specification

Following the discussion in Section 2.2, my main econometric specification can be written as:

$$Y_{isk,t} = \alpha + \delta \sum_{j=1}^{n} g_{ij,t} Y_{jsk,t} + \beta X_{i,t} + \gamma \sum_{j=1}^{n} g_{ij,t} X_{j,t} + \eta S_{sk,t} + \sigma Y_{i,t-1} + \phi_i + \phi_s + \phi_k + u_{isk,t}, \quad (2.12)$$

where $Y_{isk,t}$ is the test score (math or reading) of student i in school s with teacher k in grade $t = \{4, 8, 10\}$; $X_{i,t}$ is a vector of socioeconomic background characteristics at time t (e.g. gender and family income), and $S_{sk,t}$ is a set of school and classroom inputs at t (e.g. class size and gender composition in the classroom). Recalling from Section 2.2, $g_{ij,t}$ corresponds to the elements of the adjacency matrix $\mathbf{G}_t$, which keeps track of the links. The terms ϕ_i, ϕ_s and ϕ_k account for individual, school and teacher fixed effects, respectively. Finally, $u_{isk,t}$ represents a random error. Throughout the analysis, I cluster the standard errors at the school level, as this accounts for serial correlation [Bertrand et al., 2004]. The key coefficient is δ, which captures the endogenous network effects.

As discussed in Section 2.3, the estimation of this model has at least three fundamental challenges: the reflection problem, the sorting problem and the presence of omitted variables or correlated effects. To deal with the reflection problem, I use the exogenous characteristics of the past classmates of the current classmates. Hence, I follow a 2SLS, where $\mathbf{G}_t\mathbf{Y}_t$ is instrumented in the second stage by $\mathbf{G}_t\mathbf{G}_4\mathbf{X}_4$. The term $\mathbf{G}_t\mathbf{G}_4\mathbf{X}_4$ represents the (mostly) predetermined characteristics of the peers in 4th grade of the current peers of student i.

I focus on peers' peers in 4th grade because those students will likely have similar demographics as i's peers but may have gone to different schools/classes through the years. Exploiting a very recent period such as $t-1$ encounters the risk that a high percentage of the past classmates of student i's classmates at $t-1$ were also her classmates. To deal with the issues of sorting and correlated effects, I include individual, school, and teacher fixed effects. I also take advantage of structural school switches between primary and secondary education.

Heterogeneous Effects Even though it is likely that all class members interact in some regards, some peers will be more influencing in affecting behavior and achievement. Therefore, I also estimate higher order spatial lags models of peer effects to capture different forms of proximity between student [Drukker et al., 2019].

First, I allow the influence of peers to be different for male and female students. I also let male and female students be subjected to distinct effects from both male and female peers, potentially leading to four kinds of endogenous effects. This lead to non-symmetric $\mathbf{G}_t$ matrices, as shown below. Thus, I estimate:

$$\begin{aligned} Y^a_{isk,t} =& \alpha^a + \delta^a_M \sum_{j=1}^{n} g^M_{ij,t} Y_{j,t} + \delta^a_F \sum_{j=1}^{n} g^F_{ij,t} Y_{j,t} + \beta X_{i,t} + \gamma \sum_{j=1}^{n} g_{ij,t} X_{j,t} + \eta S_{s,t} \\ & + \sigma Y^a_{i,t-1} + \phi_i + \phi_s + \phi_k + u^a_{isk,t}, \end{aligned} \tag{2.13}$$

were $a = \{female, male\}$, and δ^a_M and δ^a_F capture the endogenous effects from male and female peers, respectively, for both female and male students

Second, I allow the peer coefficient to vary by own achievement level, as in Burke and

Sass [2013], Carrell et al. [2013, 2009]. Specifically, I estimate separate peer coefficients for each third of the (baseline) test score distribution. To allow for even more complex sets of peer influences, I allow for the achievement level of each student type (low, middle and high achievers) to be influenced by their low, middle and high achiever peers. This specification can be used to ask other more specific questions such as, are top students good influences on all students in the classroom? I analyze this below.

2.6 Results

I begin by presenting the results from the linear-in-means specification and estimate different versions of Equation 2.12. I report only the coefficient estimates of the variable of interest, i.e. the effect on current achievement of the mean achievement of current classroom peers, but the complete set of coefficients can be found in the appendix.

Table 2.3 and 2.4 display the endogenous effects for math and reading test scores, respectively. The first four columns show the OLS results, accounting for various levels of fixed effects. The next four columns show the same coefficient by two stages least squares method (2SLS). Following the literature, I standardize the test scores (at the grade-cohort level) to have a mean of zero and a standard deviation of one.

All specifications include time-varying individual socioeconomic characteristics and contextual variables, classroom inputs and the previous test score. More precisely, regressions include an indicator for female, dummies of household income level, a series of variables that capture the proportion of peers with different levels household income, previous individual achievement, class size and the share females in the classroom. The excluded instruments exploited in the

2SLS strategy are dummies for mother's and father's education, a dummy if the mother is present, a dummy if the father present, number of books in the home, indigenous ethnicity, parental expectations of child attending HE and university, and attendance to school meetings, in 4th grade for peers' peers. These are standard variables in the literature in the educational context (see Lee et al., 2021, Patacchini et al., 2017, Bramoullé et al., 2009, among others).[11]

OLS estimates (column 1) suggest positive and highly significant endogenous effects, for both math and reading test scores. However, we know that under the reflection problem the mean outcome of the peers is a linear function of the group mean characteristics, so any correlation between an individual's outcome and her reference group's mean outcome may simply reflect the effects of the group mean characteristics [Lin, 2010].[12]

The 2SLS estimates (columns 5-8) are smaller than those reported under OLS, for both math and reading test scores, which reassures the idea that OLS overestimate the true endogenous network effect due to the simultaneity problem. The magnitude of the estimates decreases significantly once the student fixed effects are taken into account. For instance, Table 2.3 indicates that in the case of math, the inclusion of the student fixed effect reduces the magnitude of the endogenous peer effect from 0.49 to 0.24 standard deviation. This result corroborates the idea that in most cases there is a positive correlation between the students' unobservables and the academic achievement, so the parameters representing social interactions effects will tend to be overestimated.

The school fixed effects, which capture inputs that are likely not to vary significantly across classrooms within schools, also play an important role. We can think of the school fixed effects

[11] For example, Bramoullé et al. [2009] use dummies for parental level of education, dummies for mother/father present, female indicator, race indicator, parents' participation in the labor market.

[12] Lee [2007] demonstrates that theoretically both endogenous and exogenous effects can be identified as long as group sizes are no constant, but the identification will be weak if group sizes are large (average group size of 52).

as capturing the impact of the principal, or even physical inputs such as desks and computer. Removing teacher fixed effects proves to be crucial as well. The estimates confirm the intuition that social effects are generally biased upward when teacher quality is not accounted for. Since good teacher will tend to improve the performance of the whole class, not accounting for teacher unobservables could make peer effects to appear more important.

Column (8) of Tables 2.3 and 2.4 displays the estimates of social interactions after dealing with the main identification issues: reflection problem, correlated effects and sorting into the network. The magnitudes of the effect are small and only significant for math. In particular, I find that one standard deviation increase in the average math test scores of peers increases own test scores by 0.09 standard deviation. In other words, if the average performance of the peers increases by 0.25 standard deviation, own test score increases by 0.02 standard deviation. Again, all specifications include time-varying individual socioeconomic characteristics and contextual variables, classroom inputs and the previous test score. The reported Cragg-Donald F-statistics suggest the IVs are informative.

These estimated effects are generally smaller than those previously found in the literature. For example, Hanushek et al. [2003] find that the estimated social effects range from 0.15 to 0.24 in math test scores, and Lin [2010] finds that the estimated endogenous effect coefficient is 0.247 after controlling for group unobservables.

Heterogeneous Effects I relax the linear-in-means specification and allow for higher order spatial lags to explore heterogeneous peer effects. Table 2.5 reports the results on gender for the preferred specification: using 2SLS strategy and including individual, school and teacher fixed effects. Columns (1)-(2) and (5)-(6) report the endogenous social effects separately for male and female students, respectively. The findings for math indicate that only male students are significantly influenced by their classmates. In fact, one standard deviation increase in peers' average math test scores increases boys' test scores by 0.21 standard deviation. In reading, however, I do not find any significant effect.

When allowing the peer influence to be different for males and females, as in equation 2.13, I find that in math, exposure to opposite-gender peers leads to higher academic performances for boys but not for girls. More concretely, one standard deviation increase in girls' average test scores increases boys' test scores in 0.19 standard deviations. In reading, on the contrary, both boys and girls experiences positive peer effects from their same-gender classmates, with estimated peer effects between 0.25-0.29 standard deviations.

These results have to be interpreted in the Chilean context, which displays one of the largest gender achievement gaps in math favoring boys and the smallest achievement gap favoring girls in reading [OECD, 2020]. In this setting, given that probably boys and girls differ significantly in perceptions about their own ability in math, an increase in their female peers performance might push boys to increase their performance as well. In addition, there is evidence that female presence improve the classroom behavior and create a better atmosphere for learning [Hoxby, 2000, Lavy and Schlosser, 2011]. The situation for girls is different. The estimates indicate that male performance has a negative impact on females, even in reading test scores, although these are not statistically significant. Gender stereotypes could prevent girls to benefit from peer effects from their boys classmates. This result could be related to gender attitudes and self-perceptions toward math, and by the evidence that boys are more disruptive in class and lower the quality of student-teacher interactions.

Next, I explore the effect of social interactions depending on the achievement level of the students and their classmates. In particular, I estimate separate peer coefficients for each one-third on the (baseline) student academic achievement distribution. Table 2.6 reports the coefficient of interest for low, medium, and high achievers. The magnitude of the endogenous effect for students in the top third of the academic ability distribution is significantly higher than that estimated for students in the bottom and the middle. A one-standard-deviation increase in peer achievement raises achievement of a high-ranked student by 0.35 standard deviations in math and 0.38 standard deviations in reading. On the contrary, low and middle ability students may even experience a negative impact from an increase in the average ability of their peer group.

Figure 2.3 shows the estimates when allowing the achievement level of each student type (low, medium, and high) to be influenced by low-, medium-, and high-achieving peers. These estimates have to be interpreted with caution because the F-statistics are, in general, low. The results suggest that higher achievers experience the largest effects from high ability peers, and that low and medium achievers do not benefit from high achieving classmates. Low achievers even seem to be negatively affected by the average test scores of the top students in their class.

These results are in line with the finding reported by Burke and Sass [2013] and Carrell et al. [2013]. For instance, Burke and Sass [2013] find that students with low initial achievement levels appear to benefit less from an increase in the average ability of their peers than do students with higher initial scores. Carrell et al. [2013] also find that low-ability students perform worse when placed into peer groups with a high fraction of high-ability peers. This is also consistent with Figure 2.4, which shows that lower ability students experience even more negative effects as the share of top students in their classroom increases. The opposite is true for high achievers.

A potential explanation behind these findings could be that classroom composition affect teacher expectations and the teaching pace [Kiss, 2018]. For example, if ability differential in a class are considerable and teachers target the top students, low achievers might feel overwhelmed, negatively affecting their achievement. Also, high-achieving peers could be intimidating to those more distant from their abilities and thus inhibit learning efforts from low achievers [Winston et al., 2004]. Hence, if the goal is to raise achievement among the lowest scorers, they might not be placed in classrooms with a high fraction of top students.

2.6.1 Robustness Tests

In what follows, I perform a series of exercises to test the robustness of my results. First, I restrict the analysis to ”structural” moves. Thus, I limit the sample to students in 8th grade attending a school that does not offer secondary education. The idea is to leave out the impact of moves due to events such as family relocation and other factors that could bias the estimates.

As discussed above, some forms of school mobility could be problematic. For example, consider the case of a negative family event, such as unemployment, that, on the one hand, affects the child’s academic performance and, on the other, the child has to relocate to a worse school with a weaker peer group. In that case, the change in the peer groups will be correlated with the student time-varying shock and will confound the estimates. The results of this analysis are reported in Panel A of Table 2.7. The estimates are comparable to those of columns (5)-(8) of Tables 2.3 and 2.4, although the magnitudes and significance are a little bit higher for math test scores.

By focusing only on compulsory movers, we exploit the variation for those whom variations in peer groups are close to random shocks. In my sample, out of the 53% school switches between 8th and 10th grade, 40% were compulsory moves. There is evidence compulsory movers can provide a more credibly exogenous source of mobility than non-compulsory movers [Kramarz et al., 2015]. However, the external validity of this exercise might be limited if compulsory movers are not representative of the overall population of students. Hence, I compare the characteristics of the students who are forced to change schools with those who are not.

Table 2.8 reveals that students who attend a school that does not offer secondary education have lower performance in standardized tests, less educated parents, lower household income, lower parental expectations that they will attend higher education, and have a higher probability of belonging to an indigenous group. For instance, only 3% of the mothers of those students who were forced to change schools have a university degree, versus 15% of the mothers of those students who were not forced to change. Furthermore, in the first group, 86% of parents believe that their children will obtain a higher education degree, versus 95% in the second group. All the differences are significant at the 1% level.

Second, I perform the analysis at the (within-school) grade level. The results, displayed in Panel B, are smaller in magnitude, and the significant effect in math disappears when accounting for student, school, and teacher fixed effects. A natural interpretation for this result is that the classroom facilitates learning interactions in a way that non-classroom do not. However, as discussed by Burke and Sass [2013], there is also more likely to obtain spurious peer effects at the classroom level than at the grade level due to correlated effects that the inclusion of teacher fixed effects cannot capture.

Finally, I modified the set of excluded instruments by including the characteristics of teachers in 4th grade (my baseline year). I include the gender and the age of the teacher, the number of years in the educational system, the number of teaching hours in the contract, and dummies indicating the type of higher education institution in which the professor obtained his degree. The results are shown in Panel C of Table 2.7. The estimates are robust to the inclusion of the teacher characteristics to the set of instruments.

2.7 Conclusion

Over the past few decades, many countries have implemented reforms that, implicitly or explicitly, concern peer effects. However, the identification challenges limit our scope for fully internalizing the effect of social interactions in the design of these policy interventions. In this paper, I study the role of social interactions at school on student achievement and show that a dynamic model can aid with the main identification issues. In particular, I show that extending the standard linear model to a dynamic setting allows for the identification of social effects even when students interact in groups (e.g., classrooms). This is especially relevant in situations that lack data on interactions within groups.

To deal with the reflection problem, I rely on the evolution of the students' social network and exploit excluded instruments that are internally generated by the model, such as the predetermined characteristics of the past classmates of a student's current classmates. Those characteristics impact individual outcomes only through their effect on current classmates' outcome. To account for the presence of correlated effects, such as teacher ability and school unobserved characteristics, I include school and teacher fixed effects. To deal with the fact that classrooms do not form randomly but are themselves the result of individual choices, I include student fixed effects. In this way, I account for time-invariant unobserved individual characteristics that influence both the network formation and the student achievement. Therefore, the effect of social interactions on educational outcomes will then be identified from within-student variation in its network over time.

I empirically apply this strategy to longitudinal student-level administrative data from Chile. After dealing with the issues of reflection problem, correlated effects and sorting into the network, I find that social effects are positive but only significant for math. In particular, one standard deviation increase in the average math test scores of classmate peers increases own test scores by 0.09 standard deviation. My results indicate that peer effects estimates tend to be overestimated when not dealing with the main confounding issues. In particular, they reinforce the importance of controlling for unobserved teacher quality.

I find some sizable and significant social effects when exploring heterogeneous models. For instance, I find that exposure to opposite-gender peers leads to higher academic performances on math for boys but not for girls. More concretely, one standard deviation increase in girls' average test scores increases boys' test scores in 0.19 standard deviations. In addition, my results suggest that higher achievers experience the largest effects from high ability peers, and that low and medium achievers do not benefit from high achieving classmates. Low achievers even seem to be negatively affected by the average test scores of the top students in their class.

As a word of caution, it is important to recognize that social interactions may operate differently in different contexts. It is impossible to know if the results of this study are generalizable to different contexts, so consistent policy implications are hard to extract.

Tables and Figures

Table 2.1: Students' descriptive statistics

	8th graders				10th graders			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Mean	SD	Min	Max	Mean	SD	Min	Max
Outcome Variables								
Math test scores	273.26	47.12	140.05	402.69	278.17	62.63	96.59	428.04
Reading test scores	252.70	49.74	112.21	373.24	257.93	51.50	116.78	401.64
Background Variables								
Female	0.52	0.50	0.00	1.00	0.52	0.50	0.00	1.00
Indigenous ethnicity	0.14	0.34	0.00	1.00	0.13	0.34	0.00	1.00
Mother's education: university	0.10	0.30	0.00	1.00	0.10	0.30	0.00	1.00
Father's education: university	0.12	0.33	0.00	1.00	0.12	0.33	0.00	1.00
Ln(income)	12.90	0.88	10.82	14.65	13.02	0.84	10.82	14.65
Parental expect. HE	0.91	0.28	0.00	1.00	0.93	0.26	0.00	1.00
Parental expect. University	0.76	0.43	0.00	1.00	0.83	0.37	0.00	1.00
Contextual Variables								
Peer's gender (female)	0.52	0.19	0.00	1.00	0.52	0.23	0.00	1.00
Peer's indigenous ethnicity	0.13	0.16	0.00	1.00	0.13	0.16	0.00	1.00
Peer's mother education: university	0.10	0.15	0.00	1.00	0.10	0.14	0.00	1.00
Peer's father education: university	0.12	0.17	0.00	1.00	0.11	0.16	0.00	1.00
Peer's Ln(income)	12.92	0.67	10.82	14.65	13.03	0.62	10.82	14.65
Peer's Parental expect. HE	0.90	0.12	0.00	1.00	0.92	0.09	0.00	1.00
Peer's Parental expect. University	0.74	0.20	0.00	1.00	0.82	0.17	0.00	1.00

Table 2.2: Teachers' descriptive statistics

	Math teachers				Reading teachers			
VARIABLES	Mean	SD	Min	Max	Mean	SD	Min	Max
8th grade								
Female	0.59	0.49	0	1	0.80	0.40	0	1
Age	41.33	12.38	21	83	40.83	12.08	22	79
N. of years in the educational system	13.90	12.32	0	64	13.86	12.04	0	59
N. of years in the school	8.02	9.31	0	46	8.07	9.17	0	45
% with degree in education	0.97	0.18	0	1	0.99	0.12	0	1
% with a degree from a university	0.92	0.28	0	1	0.93	0.25	0	1
Teaching hours	32.80	7.94	0	44	32.55	7.90	0	44
10th grade								
Female	0.50	0.50	0	1	0.73	0.44	0	1
Age	38.93	12.77	20	83	36.60	11.40	20	78
N. of years in the educational system	13.26	11.91	0	54	12.26	10.77	0	51
N. of years in the school	7.74	9.07	0	45	7.22	8.75	0	47
% with degree in education	0.91	0.29	0	1	0.98	0.15	0	1
% with a degree from a university	0.95	0.22	0	1	0.98	0.15	0	1
Teaching hours	33.36	8.27	0	44	32.35	8.58	0	44

Table 2.3: Estimation results: Math

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS
Endgenous effect δ	0.545***	0.420***	0.364***	0.116***	0.486***	0.241***	0.164***	0.09**
	(0.004)	(0.003)	(0.004)	(0.005)	(0.010)	(0.011)	(0.035)	(0.042)
Individual FE		✓	✓	✓		✓	✓	✓
School FE			✓	✓			✓	✓
Teacher FE				✓				✓
Observations	495,170	495,170	495,170	495,170	495,170	495,170	495,170	495,170
R-squared	0.633	0.215	0.156	0.126	0.632	0.199	0.145	0.125
Cragg-Donald Wald F statistic	-	-	-	-	3369.7	773.8	154.5	207.9

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the school level. All regressions include contextual variables. Excluded instruments include the following characteristics of peers' peers in 4th grade: dummies for mother's and father's education, a dummy if mother is present, a dummy for father present, number of books in the home, indicator of indigenous ethnicity, parental expectations of student attending HE and university in the future, and parental attendance rate to school meetings.

Table 2.4: Estimation results: Reading

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS
Endgenous effect δ	0.577***	0.487***	0.446***	0.277***	0.435***	0.228***	0.159***	0.07
	(0.004)	(0.003)	(0.004)	(0.005)	(0.015)	(0.019)	(0.054)	(0.067)
Individual FE		✓	✓	✓		✓	✓	✓
School FE			✓	✓			✓	✓
Teacher FE				✓				✓
Observations	484,451	484,451	484,451	484,451	484,451	484,451	484,451	484,451
R-squared	0.493	0.237	0.187	0.147	0.489	0.211	0.165	0.140
Cragg-Donald Wald F statistic	-	-	-	-	1877.0	336.8	63.9	71.3

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the school level. All regressions include contextual variables. Excluded instruments include the following characteristics of peers' peers in 4th grade: dummies for mother's and father's education, a dummy if mother is present, a dummy for father present, number of books in the home, indicator of indigenous ethnicity, parental expectations of student attending HE and university in the future, and parental attendance rate to school meetings.

Table 2.5: 2SLS estimation results, by gender

	Math				Reading			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Male	Female	Male	Female	Male	Female	Male	Female
Endgenous effect δ	0.212***	-0.016			0.147	-0.009		
	(0.060)	(0.060)			(0.096)	(0.092)		
Endgenous effect δ - Females			0.188***	0.088			-0.001	0.285***
			(0.068)	(0.063)			(0.096)	(0.096)
Endgenous effect δ - Males			0.096	-0.031			0.253***	-0.049
			(0.061)	(0.056)			(0.087)	(0.070)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
School FE	✓	✓	✓	✓	✓	✓	✓	✓
Teacher FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	236,370	256,691	203,553	214,596	231,972	250,378	199,420	210,042
R-squared	0.113	0.134	0.113	0.110	0.129	0.143	0.126	0.136
Cragg-Donald Wald F statistic	103.5	97.3	21.9	23.2	37.6	35.6	12.4	9.4

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the school level. All regressions include contextual variables. Excluded instruments include the following characteristics of peers' peers in 4th grade: dummies for mother's and father's education, a dummy if mother is present, a dummy for father present, number of books in the home, indicator of indigenous ethnicity, parental expectations of student attending HE and university in the future, and parental attendance rate to school meetings.

Table 2.6: 2SLS estimation results, by achievement level

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Low	Middle	High	Low	Middle	High
Endgenous effect δ	-0.217***	-0.258***	0.349***	-0.125	-0.170	0.378**
	(0.085)	(0.070)	(0.075)	(0.112)	(0.106)	(0.158)
Individual FE	✓	✓	✓	✓	✓	✓
School FE	✓	✓	✓	✓	✓	✓
Teacher FE	✓	✓	✓	✓	✓	✓
Observations	118,729	169,577	201,427	119,907	166,314	192,874
R-squared	0.170	0.192	0.169	0.175	0.192	0.165
Cragg-Donald Wald F statistic	47.2	63.8	65.5	21.5	26.2	14.4

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the school level. The columns report separate coefficients for each third of the (baseline) test score distribution. All regressions include contextual variables. Excluded instruments include the following characteristics of peers' peers in 4th grade: dummies for mother's and father's education, a dummy if mother is present, a dummy for father present, number of books in the home, indicator of indigenous ethnicity, parental expectations of student attending HE and university in the future, and parental attendance rate to school meetings.

Table 2.7: Robustness exercises

	Math				Reading			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
A. Sample that was forced to move								
(Weighted) Endgenous effect	0.460***	0.282***	0.180***	0.178***	0.378***	0.249***	0.057	0.087
	(0.011)	(0.018)	(0.042)	(0.053)	(0.016)	(0.027)	(0.062)	(0.077)
Observations	197,183	197,181	196,777	196,101	192,476	192,475	192,047	191,385
R-squared	0.548	0.236	0.148	0.136	0.451	0.243	0.148	0.141
Cragg-Donald Wald F statistic	2053.6	235.09	109.13	104.11	943.02	124.7	47.8	44.79
Individual FE		✓	✓	✓		✓	✓	✓
School FE			✓	✓			✓	✓
Teacher FE				✓				✓
B. Analysis by grade (within-school) level								
Endgenous effect	0.484***	0.237***	0.222***	0.037	0.417***	0.185***	0.067	0.052
	(0.005)	(0.010)	(0.050)	(0.078)	(0.008)	(0.019)	(0.097)	(0.107)
Observations	495,170	495,170	495,170	495,170	484,451	484,451	484,451	484,451
R-squared	0.629	0.196	0.144	0.124	0.477	0.191	0.143	0.135
Cragg-Donald Wald F statistic								
Individual FE		✓	✓	✓		✓	✓	✓
School FE			✓	✓			✓	✓
Teacher FE				✓				✓
C. Adding teacher characteristics to the set of instruments								
Endgenous effect	0.500***	0.247***	0.169***	0.098**	0.442***	0.248***	0.160***	0.079
	(0.006)	(0.011)	(0.035)	(0.041)	(0.009)	(0.019)	(0.055)	(0.067)
Observations	492,824	492,822	492,816	492,812	482,185	482,183	482,175	482,167
R-squared	0.632	0.200	0.146	0.126	0.489	0.215	0.165	0.141
Cragg-Donald Wald F statistic								
Individual FE		✓	✓	✓		✓	✓	✓
School FE			✓	✓			✓	✓
Teacher FE				✓				✓

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

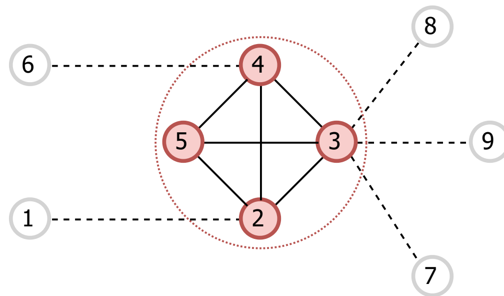
Notes: Standard errors are clustered at the school level. All regressions include contextual variables.

Table 2.8: Characteristics of students depending on whether they are forced to switch schools at the end of primary education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Forced to move					Not forced to move				
VARIABLES	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Math test scores	255.76	43.21	140.05	402.69	102,704	284.91	46.01	140.84	402.69	154,232
Reading test scores	241.09	48.05	114.07	373.24	101,859	260.40	49.35	112.21	373.24	153,338
Father's education: university	0.03	0.17	0.00	1.00	104,937	0.19	0.39	0.00	1.00	156,93
Mother's education: university	0.03	0.16	0.00	1.00	104,937	0.15	0.36	0.00	1.00	156,932
Ethnicity	0.18	0.38	0.00	1.00	89,545	0.11	0.31	0.00	1.00	131,463
Parental expect. HS	0.14	0.35	0.00	1.00	104,937	0.05	0.22	0.00	1.00	156,932
Parental expect. University	0.65	0.48	0.00	1.00	104,937	0.84	0.37	0.00	1.00	156,932
Parental expect. HE	0.86	0.35	0.00	1.00	104,937	0.95	0.22	0.00	1.00	156,932
Class Size	22.36	7.59	5.00	44.00	104,937	27.77	7.49	5.00	45.00	156,932
Ln(income)	12.51	0.70	10.82	14.65	91,975	13.17	0.88	10.82	14.65	133,230

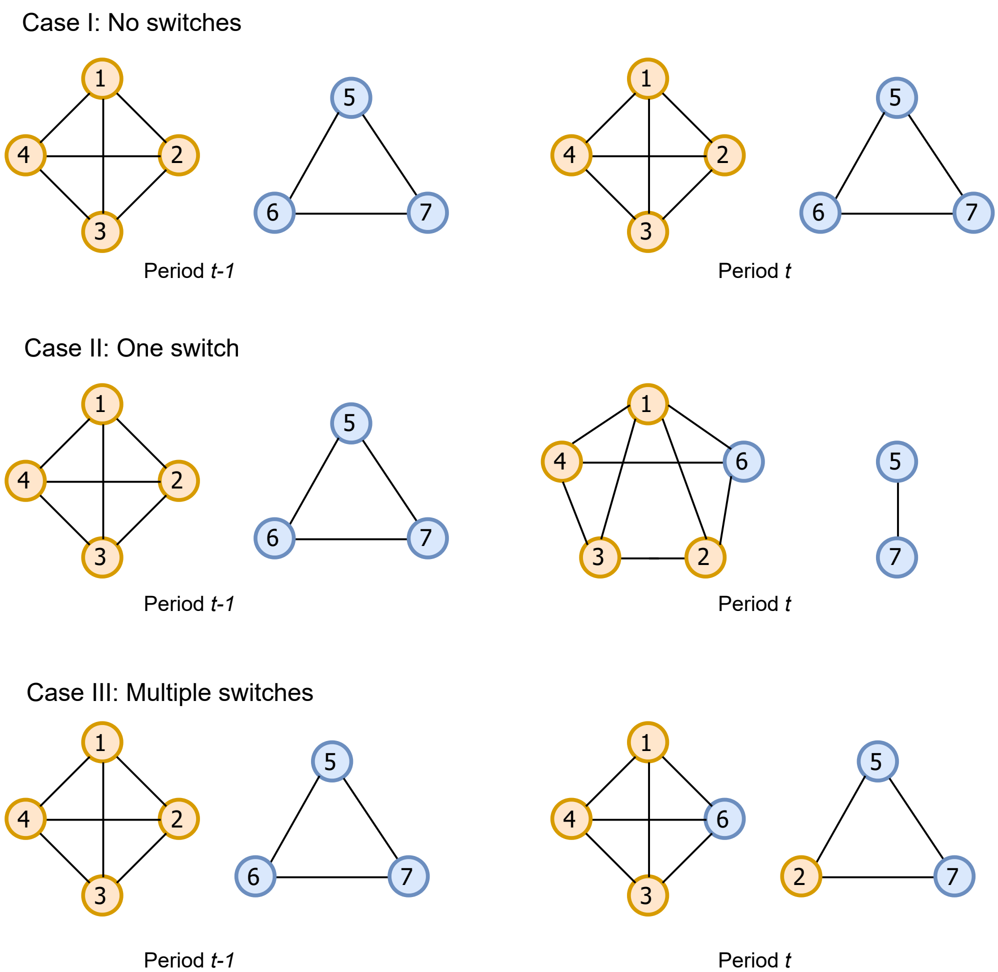
Notes: The table displays the characteristics of students who attend a school that does not offer secondary education (i.e., forced to move to a different school at the end of 8th grade) and students who attend a school that does provide secondary education.

Figure 2.1: Identification Strategy



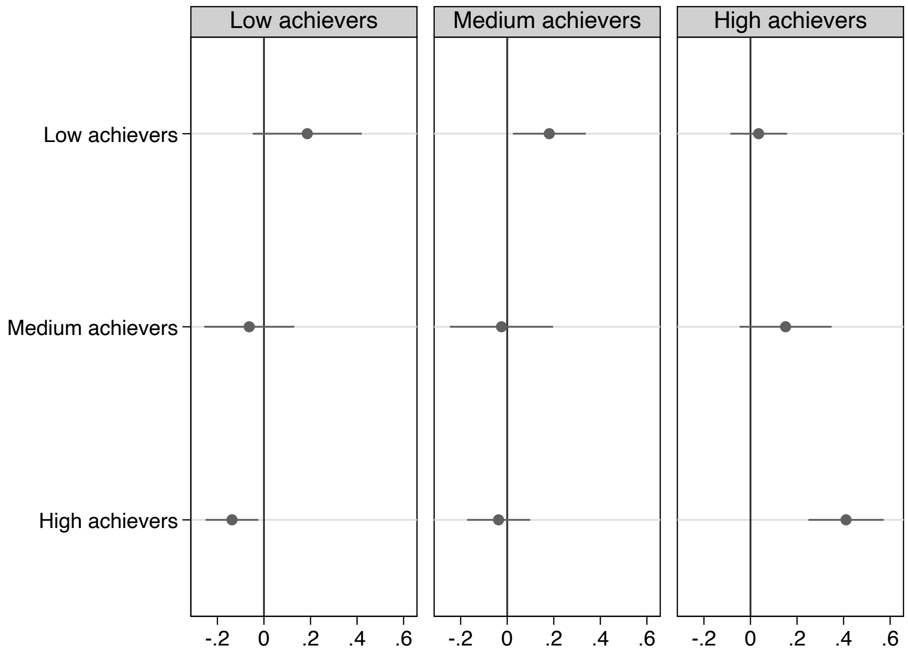
Notes: The figure illustrates an example where students 2, 3, 4 and 5, in red dots, are part of a current classroom (connected by a solid line), whereas students 1, 6, 7, 8, 9, in grey dots, reflect their past classmates (connected with dashed lines).

Figure 2.2: Identification in a dynamic model

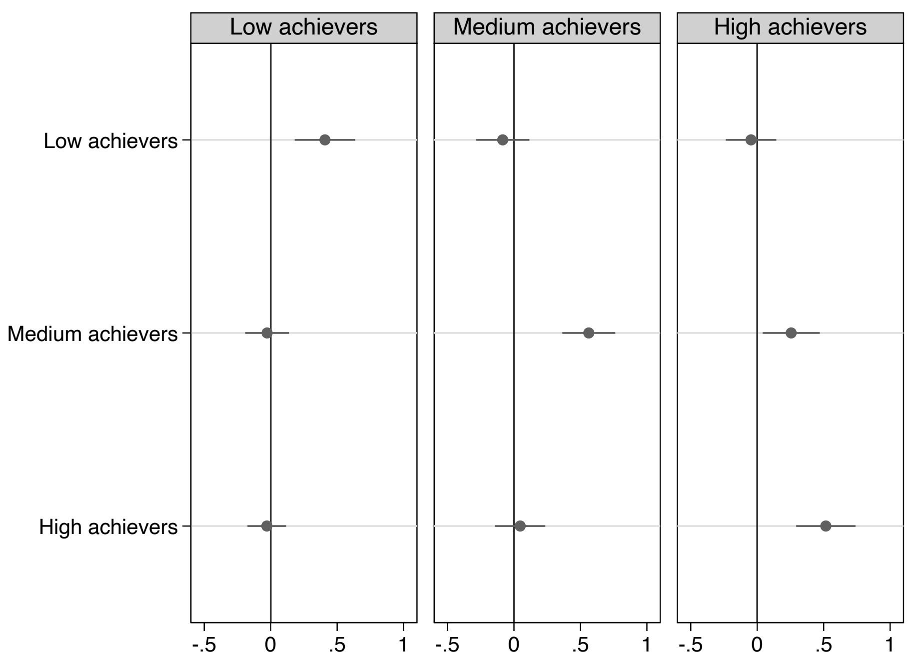


Notes: This figure shows a simple example of two small classrooms in two consecutive periods to illustrate the importance of intransitivity in classrooms peers.

Figure 2.3: Endogenous peer effect, by achievement level



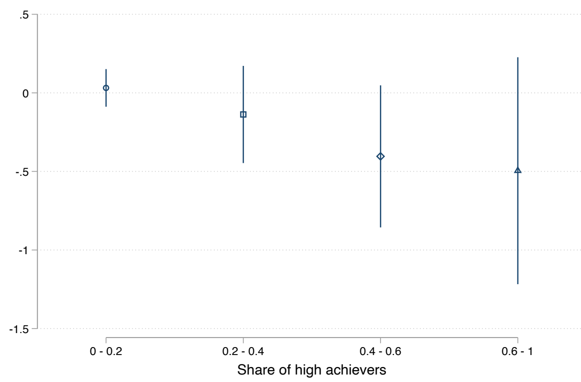
(a) Math



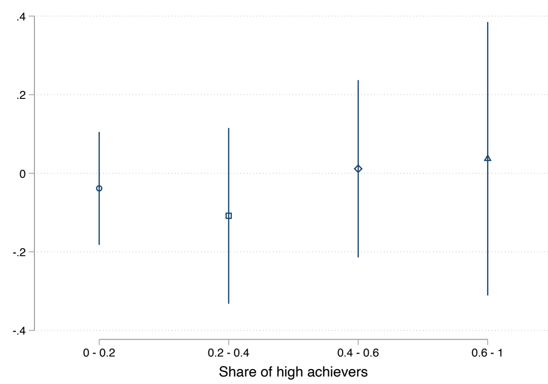
(b) Reading

Notes: This figure shows the endogenous effects estimates when allowing the achievement level of each student type (low, medium, and high) to be influenced by low-, medium-, and high-achieving peers.

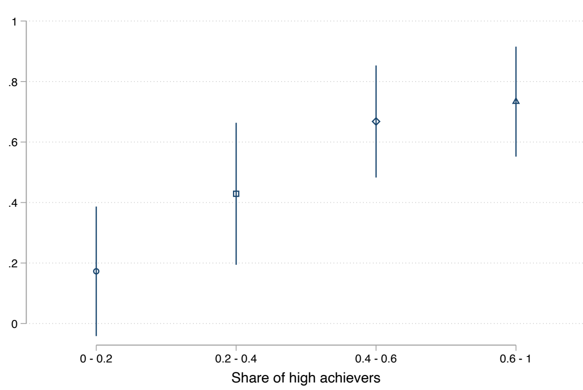
Figure 2.4: Social effects in Math, by achievement level and classroom composition



(a) Low achievers



(b) Medium achievers



(c) High achievers

Notes: The figure displays the estimates of endogenous peer effects in math for low, medium and high achievers, depending on the share of top students in their classroom. The estimates are shown as dots with their associated confidence intervals in horizontal lines.

Chapter 3: Do Targeted School Vouchers Improve Educational Outcomes? It Depends on What We Ask

3.1 Introduction

Many education reformers and economists believe school choice can help enhance education quality by forcing schools to compete for students [Milton, 1962, Hoxby, 2003]. To the extent that match quality between a school and a student is important, school choice programs may also yield benefits by promoting better matches between schools and students [Cullen et al., 2006]. School vouchers are one of the most common forms of school choice programs, although they vary greatly in dimensions including their eligibility, their source of funding, and the criteria for private school participation [Epple et al., 2017]. Frequently, they target students from low-income households, students attending failing schools, or students with disabilities, among others.

The literature on school vouchers is extensive and has generally focused on evaluating the impact on student achievement, almost always defined as scores on standardized tests.[1] However, a look at test scores alone is almost surely not sufficient to capture the full impact of school voucher programs. Non-cognitive skills have been shown to be critically important as they have substantial influence on labor market outcomes and degree attainment [Heckman et al.,

[1] Epple et al. [2017] review the empirical evidence on voucher programs in Unites States, India, Sweden, Canada, Colombia, and Chile, among other countries

2006, Heckman and Rubinstein, 2001]. Empirical research has also found that young children's behavioral disorders can negatively affect their future outcomes [Nagin and Tremblay, 1999], and disruptive students harm the behavior and learning of their peers [Carrell and Hoekstra, 2010, Carrell et al., 2018].

In this paper, I study the effect of a targeted school voucher program on a series of cognitive, non-cognitive and behavioral outcomes, ranging from academic achievement to aggressive behavior (bullying). For this, I exploit a national targeted voucher policy implemented in Chile, which increased the funding for disadvantaged students by 50%. The purpose of this targeted voucher policy (SEP) was to give schools an incentive to serve disadvantaged students. Eligible schools had to sign up for the policy and agree not to charge out-of-pocket tuition to priority students, which also expanded the set of school choice set at which low-income students could enroll at no cost.[2] The program was implemented gradually, starting with 4th graders in 2008 and adding a new grade each year.

There are several channels through which the policy may affect the outcomes of disadvantaged students, and probably those of non-disadvantaged students as well [Aguirre, 2020]. First, schools must spend the resources coming from the policy on actions committed to in an Educational Improvement Plan, which has a strong emphasis on the school climate.[3] Second, the targeted voucher provides schools an incentive to compete for low-income students, and hence improving those attributes their parents place the highest value on. Parents consider the school climate and behavior as an important factor when choosing a school, and ascribe less importance to academic

[2] Participating schools where prohibited to reject low-income students based on their background characteristics.

[3] The Educational Improvement Plan includes curriculum management, school leadership, student life, and resource management. Schools have to allocate the targeted voucher to the implementation of the Educational Improvement Plan's actions, with particular emphasis on disadvantaged students, and to promote technical-pedagogical assistance to improve students' school performance with low academic performance.

achievement.[4] Third, the policy expanded the student choice set at which low-income students could enroll at no cost. This could have led to better educational outcomes if students switch to better schools.

For the analysis, I gather data from the universe of 8th-grade students in Chile, for the period 2011-2015. As the policy was gradually implemented, disadvantaged 8th-grade students could be affected by the policy as early as 2012, and only if they attended a school that signed up for it. I focus on 8th-graders (approximately 250,000 individuals each year) because, since 2011, they must answer a detailed survey that allows me to construct measures of non-cognitive outcomes, such as self-efficacy and motivation, and measures of behavioral outcomes like violence and bullying.

Although only low-income students attending participating schools were eligible for the policy, SEP's benefits accrue to all students if participating schools improve the overall educational experience. Therefore, to account for the fact that schools might have improved students' outcomes regardless of their socioeconomic background, I first employ a difference-in-differences empirical approach at the school level. In other words, I compare students who attend a school that signed up for the program (i.e., the treatment group) and those who attend a school that did not sign up (i.e., the control group) before and after the policy was in place. Second, I explore whether the policy has had a differential impact on low-SES students, for whom the policy was aimed.

To test for the validity of my identifying assumptions, I perform several falsification tests and sensitive analysis. Furthermore, since school participation is voluntary and also there could

[4] Surveys applied in Chile show that parents ascribe less importance to academic achievement than they do to school reputation, having a pleasant school climate and having a safe school environment [OECD, 2016]. Evidence in the US also suggests that parents' choices are highly influenced by non-academic reasons, including a safe school environment Erickson [2017], Weiher and Tedin [2002]. This is particularly true for low-income families, which often value school academic quality less than high-income families.

be general equilibrium effects -schools that have never participated in the program may not be a good control group if the reform has increased the competition among all schools-, I perform an analysis at the *market* level, exploiting the variation in the share of disadvantaged students at local education markets. The idea behind this exercise is that some markets are more affected by the program depending on their share of low-income students [Sánchez, 2018a, Neilson, 2013].

I find evidence that the targeted policy positively affected several non-cognitive and behavioral outcomes, such as the students' self-efficacy, bullying, peer discrimination, and the occurrence of violent events in the school, among others. Specifically, I find that the SEP policy is associated with an increase in the general index of academic self-efficacy and motivation of 0.12 standard deviation and an increment of one percentage point in the student perceived probability of attending higher education. Also, the policy had an effect of reducing the probability of experiencing peer rejection (exclusion) by five percentage points, of decreasing the frequency (prevalence) of bullying by 0.1 standard deviation (three percentage points), and it is associated with a reduction in the occurrence of violent events in the school. I find no statistical significant effects on standardized test scores. These results are robust to a variety of sensitivity analyses and do not vary substantively across student subgroups.

When it comes to the economic mechanisms behind my results, I cannot disentangle between the three different channels through which the targeted school voucher could have impacted students' outcomes. Schools might be considering this voucher as extra resources and allocating it to actions committed in the Educational Improvement Plan; the voucher could be incentivizing schools to compete for disadvantaged students, hence improving attributes valued by the parents; or the policy could be expanding the school choice set at which low-income students could enroll at no cost. However, I provide evidence of one mechanism that probably drives my results. The schools participating in the policy have used the SEP funding to hire relatively more learning support staff, especially from the psycho-social area, such as psychologists, psycho-pedagogues, speech therapists, and social workers. I also provide suggestive evidence that my results are not due to a change in the composition of students due to school switching.

The paper contributes to the literature in several ways. Only a few prior studies have examined the effects of school vouchers on other student outcomes beyond test scores, and they have focused mainly on their impact on educational attainment [Angrist et al., 2006, 2002, Chingos and Peterson, 2015, Cowen et al., 2013]. I add to the literature by providing evidence on the effects of a large-scale voucher reform on relevant non-academic outcomes, such as self-efficacy, bullying and violence. Behavioral disorders, such as bullying, might have serious negative long-term consequences by disrupting cognitive and non-cognitive skill development [Ammermueller, 2012, Brown and Taylor, 2008, Sarzosa and Urzúa, 2015, Eriksen et al., 2014]. To the extent that they exert a direct negative impact on self-esteem and other non-cognitive skills, educational and labor market outcomes are also affected through this channel [Heckman, 2008, Waddell, 2006].

Second, this paper also speaks to the literature that studies the role of increasing public funding for schools serving disadvantaged students [Lafortune et al., 2018, Jackson et al., 2015]. Empirical studies tend to estimate effects on test scores rather than behavioral outcomes or non-cognitive skills, and there have been relatively few studies examining the impact of non-instructional funding. Reback [2010] examines the importance of student-to-staff ratios for non-instructional staff by exploiting discontinuities in Alabama's financing system for school counselors and finds that greater counselor subsidies reduce disciplinary events such as weapon-related incidents and student suspensions, but do not strongly affect mean student test scores.

Carrell and Carrell [2006] exploit within-school variation in counselor-student ratios in elementary schools in Alachua County, Florida, and find that greater availability of counselors reduces the disciplinary incidents, especially among black male students and students from low-income families.

Finally, this paper contributes to the literature analyzing the impact of the 2008 Chilean reform on educational outcomes. There are a number of papers looking at the effect of this policy on test scores [Feigenberg et al., 2019, Aguirre, 2020, Neilson, 2013, Correa et al., 2014], but to the best of my knowledge this is the first paper to look at non-academic outcomes in this setting. While most of these papers have found a positive effect on students' test scores, two recent papers question these results. Feigenberg et al. [2019] argue that changes in parental education and household income could account for much of the decline in the achievement differential between low and high socioeconomic status students observed in the data for the period analyzed. On the other hand, Aguirre [2020] implement an regression discontinuity design exploiting the fact that eligibility for the program is a discontinuous function of a socioeconomic ranking. Her findings reject the idea that eligible students chose schools with higher test scores or average SES, or that eligibility students are doing better than non-eligible students in math and language test scores.

As targeted school vouchers continue to be implemented across the world, my results provide relevant insights not only from an economic perspective but also from a public policy perspective. It reminds us that it is important to explore the effects of a policy beyond test scores, as the full impact of the policy may not be entirely captured.

3.2 Institutional Background

In the early 1980s, Chile's military regime undertook a comprehensive structural reform of the educational system, establishing a nationwide flat voucher paid directly to schools, on a per-student basis. As a result, three types of schools were established: (a) public schools, managed by local municipalities and financed by vouchers; (b) private-voucher schools, privately managed but financed by vouchers and; (c) private non-voucher schools, in which the private sector provides both funding and administration.[5] By 2015, public schools represented approximately 40 percent of total enrollment, private voucher schools represented approximately 51 percent of total enrollment and the private non-voucher sector the 8 percent of total enrollment. In 2015, the average monthly voucher for each students in primary education amounted $94 US dollars, approximately.

Until 2015, few restrictions were imposed to the private-voucher sector. Private voucher schools were allowed to select their students and, as of 1993, were allowed to charge out-of-pocket tuition to complement the state voucher.[6] Private voucher schools that charged add-on fees to parents suffer from a reduction of the voucher, which was progressive according to the amount of co-payment charged. The public sector is way more constrained: they are required to admit all students (unless they are oversubscribed) and are not allowed to charge add-on fees to parents.[7]

[5] The reform also involved a decentralization of the public school system, by transferring the administration of public schools from the central government to local municipalities. Before these reforms Chile had a centralized education system where the vast majority of schools were publicly financed and managed directly by the Ministry of Education.

[6] The average co-payment amounted US $30.23 in private-voucher schools and US $ 4 respectively in public schools (in nominal terms), by 2014.

[7] Public schools can charge add-on fees to parents in the secondary level only if all parents agree, but only about 6% of public schools charge add-on fees.

3.2.1 The Targeted Voucher

In 2008, with the aim of reducing the existing school segregation in the country, the Chilean government passed the Law 20,248 of *Subvención Escolar Preferencial* (SEP). The SEP policy introduced a targeted voucher for every disadvantaged student enrolled in participating schools, which increased the resources for low-income students in approximately 50%. In addition, participating schools received a subsidy depending on the percentage of disadvantaged students enrolled. These extra resources were available to public and private voucher schools that signed up for the policy. Schools that signed up had to agree not to charge out-of-pocket tuition to priority students, although they could continue to charge out-of-pocket tuition to non-priority students. In addition, they had to present an educational improvement plan to the Ministry of Education, with comprehensive education measures that the school would have to undertake to improve students outcomes, and agree to provide detailed accounting of the use of the SEP funds.

Private voucher schools that decided not to participate in this policy still receive the regular flat voucher, but could not receive the targeted voucher for each priority student. The classification of priority students depend on an income-based index and was initially reserved for approximately the poorest 40% of the population.[8]

The implementation of the program was a gradual process. The Law went into effect for 1st to 4th grades in 2008, and a new grade was added each year. In 2008, 98 percent of public schools and 50 percent of private voucher schools had joined the program, as showed in Figure 3.1. Private voucher schools that decided to participate charged lower prices, had students with

[8] Eligibility considered several dimensions, but the two most main ones were to belong to the lowest 33% of the income distribution according to the governmentś ranking of socioeconomic status called *Ficha de Proteccion Social* or to belong to the social program for poor families called *Chile Solidario*.

lower levels of maternal education and had lower test scores conditional on student characteristics than non-participants prior to 2008 [Feigenberg et al., 2019]. Figure 3.2 displays the monthly per-student subsidy for 8th graders.

In October 2011, the targeted voucher amount was further increased by 21% and the use of SEP resources was made more flexible, specifically allowing the hiring of *educational assistants* or learning support staff, such as psychologists, social workers, speech therapists, etc. The amendment to the law explicitly states:

"[The school] may hire teachers, educational assistants and the staff needed to improve the technical-pedagogical capabilities of the institution, and for the preparation, development, monitoring and evaluation of the Educational Improvement Plan. In addition, and with the same purpose, it may increase the working hours of teachers, educational assistants and other staff, as well as increase their salaries".

I take advantage of the Educational Improvement Plan's context.

3.2.1.1 Educational Improvement Plan

The Educational Improvement Plan includes four areas: curriculum management, school leadership, student life and resource management. Some specific actions that can be developed in each of these areas are:

- *Curriculum management*: Schools could carry out measures to strengthen the school education project, improve pedagogic practices, support students with special needs, and improve student evaluation systems.

- *School leadership*: Schools could, for example, prepare and train school management teams, strengthen the value and civic formation of students, and involve the school in the community.

- *Student life*: Schools were required to take measures to provide psychological support and social assistance to students and their families and improve the school climate, hiring suitable personnel for the achievement of the mentioned actions.

- *Resource management*: Schools could take actions to improve teacher performance; strengthen those areas of the curriculum in which students obtain unsuccessful results; design and implement evaluation systems for teachers; implement performance incentives for management teams, teachers, and administrative personnel (which must be linked to the goals established in the Educational Improvement Plan); and foster instruments to support educational activities, such as a library, computers, and Internet access.

Evidently, the Educational Improvement Plan has a strong emphasis on improving the school climate.

3.3 Data

My primary sources of information come from the Ministry of Education and the *Agencia de Calidad de la Educación*, the government's agency in charge of conducting national standardized examinations. I have individual-level administrative data on all students enrolled in primary education, along with the registry of all schools in Chile with precise records on their characteristics, and the SEP program eligibility and participation for each student and school.

In addition, I have access to data from the Education Quality Measurement System (*Sistema de Medición de Calidad de la Educación* or SIMCE). The SIMCE is a battery of national standardized exams that is generally administered multiple times during the students' educational history, either in second, fourth, eighth, or tenth grade. In addition, along with standardized tests, all students and their parents must fill out questionnaires that make it possible to build students' socioeconomic indicators, such as mother's education, father's education, and household income.

Crucial to my paper, in 2011, the students' questionnaire expanded to include questions that capture students' personal and social development. These new questions allow me to construct non-academic outcomes such as students' academic self-efficacy, school motivation and other school climate measures. Since the program's implementation was gradual, 8th-grade students were first affected by the reform in 2012, which makes this group ideal for my analysis as I would have non-academic outcomes for one year pre-reform.Most of the studies that evaluate the effect of this policy on test scores focus on 4th grade students [Feigenberg et al., 2019, Neilson, 2013, Correa et al., 2014]. However, the students' questionnaire for 4th graders is very limited and does not include most of the non-academic outcomes exploited in this paper. Hence, data limitations force me to move away from 4th graders.

Unique school and students identifiers make it possible to combine the various administrative data sets of Chilean students and schools for the years 2011-2015 to form a repeated cross-section sample for the entire universe of 8th grade students, approximately 250,000 students per year. Therefore, I have 8th graders' outcomes one year before the reform and three years after the reform.[9] The timing also coincides with the new regulations introduced in 2011, that increased the targeted voucher amount and encouraged the hiring of learning support staff.

In addition, the Ministry of Education provides data on all teachers and learning support staff, which in Chile are known as *educational assistants*. Educational assistants (EA) fulfill the

[9]The SIMCE exam was administered to the 8th grade students in 2011, 2013, 2014 and 2015. In 2012 the exam was not administered.

role of support to teachers and are vital for the school's proper functioning.

Outcomes Measures First, I look at the effect of the SEP policy on academic outcomes. Following the existing literature, I exploit the comparability of the SIMCE tests across years and normalized them by the corresponding mean and standard deviation from 2011 [Feigenberg et al., 2019, Neilson, 2013, Navarro-Palau, 2017]. I focus on the average of the normalized math and Spanish tests scores.

Second, I take advantage of the questionnaires that students must answer as part of the SIMCE exam to construct measures of non-cognitive and behavioral outcomes. The student questionnaire includes about 30 to 45 questions each year addressing various aspects of their school, their learning environment, their interests and learning strategies, among others. For example, students are asked the frequency to which they experience bullying (either physical, social, verbal, or cyber bullying) on a 5-point scale ranging from "never" to "every day". In particular, I focus on nine outcomes, which are summarized in Table 3.1.[10] They include academic self-efficacy of the student (both general and specific to math), student's expectation of attending higher education, the frequency, and prevalence of bullying experienced by students, among others.

3.3.1 Descriptive Statistics

From the original sample of approximately 885,000 8th grade students attending either a public or a private voucher, and responding SIMCE test between 2011-2015, 78.5% answered all the questions necessary for constructing the ten outcomes analyzed in this paper. I drop private non-voucher schools and their students, as the SEP reform applies only to public and

[10] Table B.1 in the Appendix has more detail of the questions used to construct each variable.

private-voucher schools. The sample is further reduced to approximately 590,000 students when I exclude those students without family background characteristics (i.e. students whose parents did not answer the parents' questionnaire), and those students attending schools that initially joined the program but later withdrew.

Table 3.2 presents summary statistics for the key variables. Panel A reports the means and standard deviations for each of the outcome measures described above, as well as demographic characteristics of 8th grade students attending SEP and non-SEP in 2011, one year before the reform was implemented for 8th graders.[11] Panel B presents the same statistics for the post-reform period (2013-2015).

In Panel A, students attending a non-SEP school perform better in both language and math SIMCE exams, have significantly higher expectations of entering higher education, and on average have higher levels of general self-efficacy and lower levels of academic insecurity (or lack of academic confidence). On the contrary, students enrolled in SEP schools suffer more situations of discrimination within the school, experience bullying more frequently, and are more likely to suffer physical, social, verbal or cyber bullying.

Students attending SEP schools in 2011 also report a higher frequency of violent acts in their schools, have parents less educated parents, and have a lower household income on average. This is not surprising considering that, as documented in section 3.2.1, schools that joined the program charge lower out-of-pocket tuition to parents at baseline, have a higher proportion of low-SES students and have lower test scores.

From the comparison of Panels A and B, one conclude that while the gap in test scores and

[11] Although the policy was not yet implemented for 8th graders in 2011, it was already in place for students in 7th grade and below.

socioeconomic characteristics between students enrolled in SEP and non-SEP schools remained after the reform, the differences on the non-academic outcomes decreased substantially. The next section attempts to test this hypothesis more directly and uncover causality using a difference-in- differences strategy. In Section 3.6 I seek to provide evidence on an important channel through which the reform could have impacted non-academic outcomes related to school climate. Specifically, I show that SEP schools use the policy's additional resources to hire learning support staff that support teachers in their work and provide support for students within schools.

3.4 Methodology

To estimate the causal effect of the SEP policy, I use a repeated cross-section sample for 8th-grade students from 2011 to 2015, containing both individual and school-level information. The policy's implementation was gradual, and 8th graders were first affected only in 2012, so in 2011 no 8th-grade student was eligible. Participation in the reform was not mandatory; schools could decide whether to participate in the SEP reform or not and if not, they could join in subsequent years.

Even though only low-income students were eligible for the targeted voucher, the benefits of SEP accrue to all students if participating schools use the additional resources to improve the overall educational experience for all the students in that cohort. Therefore, as a first approach I define the treatment at the school-level and implement a difference-in-differences approach using a two-way fixed effect model. Specifically, I estimate the following empirical model:

$$Y_{ist} = \gamma_s + \lambda_t + \delta SEP_{st} + X_{ist}\eta + \epsilon_{ist} \tag{3.1}$$

where Y_{ist} is the outcome of interest for student i in school s in year t, γ_s is a school fixed effect, λ_t is a year fixed effect, and SEP_{st} is a binary program indicator that equals one if school s participates in the program for 8th graders at time t and equals zero otherwise. X_{ist} is a vector of individual level demographic controls and the term ϵ_{ist} represents the error term. I cluster the standard errors at the school level. The coefficient of interest is δ, as it captures the effect of the targeted voucher reform.

Equation (3.1) compares the outcomes of 8th graders who where exposed to the SEP program versus outcomes of 8th graders who where not exposed to the program, while also accounting for a general cohort effect and for individual level demographic characteristics. Including individual level covariates such as mother's education, father's education and household income, not only reduce the variance of ϵ_{ist}, which may reduce the standard errors of the estimate of β, but also address the concerns that my estimates reflects changes in socioeconomic conditions contemporaneous with the implementation of the policy.

In addition, to analyzing the time variation in the outcomes gains, I implement a more flexible approach by including different policy indicators for each year of the treatment period.

$$Y_{ist} = \gamma_s + \lambda_t + \alpha + \sum_{t=2011}^{2015} \delta SEP_{st} + X_{ist} + \epsilon_{ist}. \tag{3.2}$$

Still, since the policy was specifically aimed to disadvantaged students, as a second approach I estimate equation (3.1) distinguish the impact of the SEP policy on students with high and low SES. This results into a more robust analysis because it controls for two kinds of potentially confounding trends: changes in outcomes of low-SES students across SEP and non-SEP schools (that would have nothing to do with the policy) and changes in outcomes of all students attending a SEP school (possibly due to other school policies that affect everyone's outcome). I estimate a model of the form:

$$Y_{ist} = \gamma_s + \lambda_t + \beta SEP_{st} + \zeta LowSES_{ist} + \delta(SEP_{st} \times LowSES_{ist}) + X_{ist}\eta + \epsilon_{ist}, \quad (3.3)$$

where $LowSES_{ist}$ is a binary variable that takes the value 1 if the individual i attending school s on year t is a low-SES student. All the other variables are defined as in (1). The coefficient of interest is δ, as it captures the effect of SEP on students receiving the voucher.

The main assumption needed for the model to yield valid causal estimates is that the trend in the outcomes would be the same for students in SEP and non-SEP schools in the absence of the policy, after accounting for cohort specific effects and individual level demographic characteristics. Previous studies had documented parallel trends in test scores for this setting [Feigenberg et al., 2019, Correa et al., 2014]. Since the adoption of the policy was staggered over time, standard parallel trends visualization will not suffice as schools switched their treatment status over time (see Figure 3.1). And even though most of the students in my data began to be treated the first year of the policy implementation, I only have non-academic outcomes since 2011, which leaves me with only one pre-treatment period. However, I provide validations of the research design through a series of placebo and robustness tests.[12]

First, I check for differences in pre-trends using the leads and lags test. I include m lags or post-treatment effects and q leads or anticipatory effects [Angrist and Pischke, 2008, Cunningham, 2021]. Any evidence of significant coefficients for the lead treatment indicators

[12] Recent papers have shown that the two-way fixed effects estimator could be biased when there is differential timing and heterogeneous treatment effects over time [see, for instance, Goodman-Bacon, 2021 and Callaway and Sant'Anna, 2021]. I wrote this chapter before this new evidence, but I plan to compute the Bacon Decomposition and the Callaway and Sant'Anna estimator.

is a threat to the parallel trend assumption. Next, following Angrist and Pischke [2008], as an alternative way to examine the robustness of the difference-in-differences strategy, I include school specific parametric time trends to the list of controls to allow SEP and non-SEP schools to follow different trends.

Another way to test the assumption of equal trends is to perform a placebo test using a fake treatment group. Hence, since private non-voucher schools were not affected by the program, I use private non-voucher schools as a fake treatment group expecting to find zero impact of the policy. This is not an ideal exercise because private non-voucher schools differ substantially from public and private voucher schools in a series of aspects that goes beyond the administration of the school, and they do not compete for the same students and resources [Contreras et al., 2010a]. However, although the results of this analysis must be interpreted with caution, it still can gives us good insights.

The SEP reform eliminated the out-of-pocket tuition to priority students in the participating schools, which could have incentivized them to switch schools. A large sorting of students between schools can lead to the impression that any effect found is not due to higher investment by schools but due to a change in the socioeconomic composition of students. As Correa et al. (2014) point out, the sign of the bias generated by the sorting of students depends on the type of sorting induced by the program. If participating schools with higher copayment rates (which initially enrolled middle- and higher-income students) began to enroll priority students, then the socioeconomic composition of the treated schools should worsen. Hence, a difference-in-differences methodology would under-estimate the potential positive effects of the reform. Even though including family background individual level covariates partially address this concern, I explore this further by analyzing the evolution over time of the average enrollment in treatment and control groups, as well as the trend in the socioeconomic indicators.

A potential threat to the identification is that the SEP policy may have been introduced simultaneously with other municipality-level policies that affected students's behavioral outcomes or school environment. For example, it could be the case that municipalities with a greater proportion of SEP schools are investing more in reducing violence or crime. I address this concern by including a yearly measure of crimes known to the police in each municipality.

Another concern one may have is that even though 8th graders were not affected directly by the SEP reform in 2011, some of them could have been attending a school were the reform was already in place for younger cohorts. If SEP schools use the resources from the policy to improve the educational experience for all students in the establishment, even for students in those grades that are not yet eligible, then 8th graders will be affected by the policy even in 2011. To deal with this matter, I run the difference-in-differences regression only for the subgroup of students attending schools that joined the SEP policy after 2011.

Finally, one could be worried with the school-level analysis mainly for two reasons: schools decide whether to participate or not in this policy, and there could be general equilibrium effects. The fact that participation is voluntary may cast doubt on the assumption of parallel trends. Also, schools that never participated in the SEP policy may not be a good control group if the reform has had general equilibrium effects. If the reform increased the competition among schools, students attending non-SEP schools might also benefit from the policy. In fact, Neilson [2013] finds that the SEP policy leads to increased competition in the poorest neighborhoods, pushing all schools to improve their academic quality.

To overcome those issue, I pursue an alternative methodology following Sánchez [2018a] and Neilson [2013], and exploit the variation in the share of disadvantaged students at local educational markets. In Chile's educational system, time and travel costs allow for the existence of differentiated local school markets at the municipality level. This is true despite the fact that students can freely choose schools regardless of their location of residence.[13] In my sample, about 83% of 8th grade students attend a school that is located in the same municipality of their municipality of residence.

The idea behind this analysis is that some municipalities are more affected by the program than others depending on their share of disadvantaged students. For example, a municipality with no disadvantaged students is probably not be affected by the program, and a market where all students are disadvantaged is (potentially) fully affected by the program. To avoid any endogenous migration issues, I define the percentage of disadvantaged students in the municipality of residence the year before the introduction of the program for 8th grade students.[14]. Thus, I estimate an empirical model of the form:

$$Y_{imt} = \gamma_m + \lambda_t + \delta(post_t \times intensity_m) + X_{imt}\eta + \epsilon_{imt}, \tag{3.4}$$

where Y_{imt} is the outcome for student i in municipality of residence m before in 2011 in period t, γ_m is a fixed effect for municipality of residence in 2011, λ_t is a year fixed effect, $intensity_m$ is the intensity of the treatment (i.e. share of disadvantaged students in the municipality of residence one year before the reform was in place for 8th graders) and $Post_t$ is a post-reform indicator. ϵ_{imt} is the error term, and the standard errors clustered at municipality of residence in 2011.

[13]Previous research has found that primary school students avoid traveling long distances to go to school, with the average student traveling less than 2.78 km (1.7 mi) (Gallego and Hernando, 2009; Chumacero et al., 2011).

[14]About 85% of students live in the same municipality between 2007 and 2011

3.5 Results

My main estimates of Equation 3.1 are reported in Table 3.3. To facilitate a clear interpretation of the coefficient of interest, all non-dichotomous variables were standardized to have a mean of 0 and a standard deviation of one, given that they are measured on different scales.[15] This allows me to interpret the estimated coefficients as standard deviation. The binary outcomes, such as the student's expectations of attending higher education and the prevalence of exclusion and bullying, are not standardized, so that the coefficient of interest can be interpreted as the effect of the policy on the probability of being bullied, for example.

Column 1 of Table 3.3 reports results for test scores. The estimated coefficient indicates that being enrolled in school that participated in the SEP policy leads to a a decrease of 0.01 standard deviation in the SIMCE score, which is not significant at conventional levels. However, the SEP policy is associated with significant improvements in all non-academic outcomes, except self-efficacy in math. The magnitudes of the estimates are also economically significant. For instance, the SEP policy is associated with an increase in the general index of academic self-efficacy and motivation of 0.12 standard deviation, a decrease in academic insecurity (or lack of academic confidence) of 0.12 standard deviation, and an increment of one percentage points in the students' perceived probability of attending higher education. Also, the policy had an effect of reducing the probability of experiencing peer rejection (exclusion) by 5 percentage points, of decreasing the frequency (prevalence) of bullying by 0.09 standard deviation (3 percentage points), and it is associated with a reduction in the occurrence of violent events in the school.

[15]Based on the existing literature, I exploit the comparability of the SIMCE tests across years and normalized them by the corresponding mean and standard deviation from 2011 and focus on the average of the normalized math and Spanish tests scores. For the non-academic outcomes that are non-dichotomous, I standardized them to have mean 0 and standard deviation 1, for facilitating the comparison between them.

Table 3.3 reports the main estimates of Equation 3.3. The coefficients on the interaction of $LowSES_{ist}$ and SEP_{st} tell us the effect of the SEP policy on the students (potentially) receiving the voucher. While the impact on test scores is not significant, low-SES students improved in almost all non-academic outcomes, evidencing that disadvantaged students significantly benefited from the policy. For instance, even though the SEP reform did not impact the math self-efficacy index in general, it did affect the index for low-SES students. Moreover, the coefficient on SEP_{st} reveals that there are indeed spillover effects on high-SES students attending SEP schools.

3.5.1 Threads to identification and robustness tests

As I mentioned in the previous section, I provide additional validations of the research design through a series of placebo and robustness tests. First, I check for differences in pre-trends by including m lags (post-treatment effects) and q leads (anticipatory effects). More specifically, I run a model of the form:

$$Y_{ist} = \gamma_s + \lambda_t + \sum_{t=-q}^{-1} \gamma_t SEP_{st} + \sum_{t=0}^{m} \delta_t SEP_{st} + X_{ist}\eta + \epsilon_{ist}. \tag{3.5}$$

Figure 3.3 provides a visualization of the results of this analysis for the main outcomes. We can see that most of the pre-treatment coefficient are nearly zero and, with the exception of the test scores, they immediately shoots up (or down) after the treatment. This is particularly true for the expectation of attending higher education, academic self-efficacy and academic insecurity. The idea of this exercise is to show that, prior to the treatment, SEP and non-SEP schools were similar with regards to non-academic outcomes, but they diverge significantly post-treatment.

In Table 3.5 I include school specific parametric time trends to the list of controls to allow

SEP and non-SEP schools to follow different trends. The results show that the estimated effects of interest are almost unchanged by the inclusion of theses trends.

Table 3.6 reports the results of a placebo test using private non-voucher schools as a fake treatment group. The results show no impact of the policy in most of the non-academic outcomes. Although a positive impact on self-efficacy is observed, it is also observed that the reform had a positive impact on the academic insecurity index, which is not consistent.

In addition, to examine whether the reform led to any noticeable compositional change in the control and treatment groups, in Table 3.7 I explore the evolution over time of the average enrollment in treatment and control groups, as well as the trend in the family background characteristics in the student body. Consistent with Correa et al. [2014], I do not find any remarkable compositional changes in the control and treatment groups between 2011 and 2015. Even though socioeconomic indicators of treated schools increased to a lesser extent than in control schools, which could give some support to the idea that the socioeconomic composition of the treated schools worsened, the magnitudes are small. Moreover, Aguirre [2020] shows that being eligible for a targeted voucher had no impact on parents' school choices, therefore it is not expected a great sorting among priority students product of the reform.

Since there could be concerns that my estimates reveal changes at the municipality level rather than at the school level if, for example, municipalities with a larger proportion of SEP schools are investing more to reduce violence or crime rates. I deal with this issue by controlling for a yearly measure of crime rates known to police for each municipality, in addition to controlling for family background characteristics. Table 3.8 displays the results on this analysis. Again, results remain robust.

In Table 3.9 I estimate the main specification for the subgroup of students attending schools that did not participate in the SEP policy in 2011. The sample size is significantly smaller because most of the schools that joined the SEP policy did so before 2011. Although the sample size is significantly smaller (most of the schools that joined the SEP policy did so before 2011), the results remain almost unchanged.

Finally, Table 3.10 reports the results of the market-level analysis. As I pointed out in the previously, I define each municipality as an individual market and compare the changes in outcomes after the implementation of the policy for 8th graders. This empirical approach allows us to better capture competitive forces. This market-level analysis confirms the positive and statistically significant effects of targeted vouchers on most of the non-academic outcomes.

Thus, together, these results support the idea that the targeted voucher reform positively affected non-academic outcomes that had not been evaluated before in the literature. The results confirm that a look at test scores alone is almost surely not sufficient to capture the full impact of the voucher program. However, a question that remains is: what are SEP schools doing to improve school climate indicators? In the next section I explore a potential mechanism behind my results.

3.6 Potential Mechanism

In this section, I seek to provide evidence on a primary channel through which the reform could have impacted the school environment. The Law 20,248 states that educational establishments must allocate all the SEP resources to implement the Educational Improvement Plan. The law emphasizes promoting “special technical-pedagogical assistance to improve students’ school performance with low academic performance”.

As I mentioned in section 3.2.1, in 2011, the SEP regulation was amended to eliminate some of the restrictions on the use of the funds, explicitly allowing the hiring of learning support staff and other professionals using the SEP resources. Schools can now dedicate up to 50% of the additional resources for hiring additional staff or educational assistants, as they are know in Chile. Educational assistants support teachers in their work and provide support for students within schools. They can provide professional, technical, administrative, or auxiliary assistance.[16]

I obtained the registry of all the education assistants in each school in Chile since 2007 to check if educational assistants' hiring increased in that period.[17] Figure 3.4 presents visual evidence of the number of education assistants in SEP schools versus non-SEP schools between 2007 and 2015. Since the implementation of the reform in 2008, the number of assistants increased notably in SEP schools compared to non-SEP schools, leading to a significant difference by order of 4.2 assistants in 2015.

To formally analyze if the number of assistants changed during this period, I estimate a difference-in-differences regression of the form:

$$EA_{st} = \sum_{2007}^{2015} (SEP_{st} \times \lambda_t)\delta_t + \lambda_t + \epsilon_{st} \tag{3.6}$$

where EA_{st} is the ratio of education assistant over the total enrollment in school s in year t, λ_t is a year fixed effect, and SEP_{st} is the binary program indicator that equals one if school s participates in the program at time t and equals zero otherwise. The term ϵ_{ist} represents the error term, and the standard errors are clustered at the school level. The parameter δ captures the

[16]Examples include Receptionist, Gardener, Secretary, Accountant, Computer Support, Librarian, Laboratory Manager, Psychologist, Psychopedagogue, Speech Therapist, Nurse, Paramedic, among others.

[17]It corresponds to the total number of education assistants in each school by June 30 of each year.

impact of the SEP reform in the ratio of EA.

Figure 3.5 plot the coefficients of the regression. It is clear that after the SEP reform the ratio of educational assistants in the school started to increase. This result may be associated with the new regulations introduced in that year, which made the SEP resources more flexible.

Since 2010, it is also possible to distinguish in the data the main function of the educational assistant. I group those who perform as Psychopedagogue, Psychologist, Speech Therapist, Social Worker, Nurse, Paramedic, Nutrition Manager, Kinesiologist, Occupational Therapist, Differential Assistant and School Inspector, and named them "technical-pedagogical" assistants. Figure 3.6 displays the ratio of technical-pedagogical assistants over the total number of assistants in the school. It provides suggestive evidence that the proportion of technical-pedagogical assistants increased over time after the change in regulations in 2011.

All in all, I have documented that SEP schools have invested relatively more resources in hiring learning support staff, and specifically professionals of the psycho-social area (speech therapists, psycho-pedagogues, psychologists, differential educators, among others) than non-SEP schools. To further examine if such a pattern can, at least partially, explain the results I found in this paper, I estimate two more difference-in-differences regressions. First, a specification including the interaction between the binary program indicator and the ratio of educational assistants over the total enrollment in the school, and second, including that same variable with the ratio of technical-pedagogical assistants over total enrollment. If my hypothesis is true, I should find that SEP schools with a greater ratio of EA/technical-pedagogical assistants over total enrollment have better non-academic outcomes.

Therefore, I run the following difference-in-differences regression:

$$y_{ist} = \beta SEP_{st} + \zeta RatioEA_{st} + \delta(SEP_{st} \times RatioEA_{st}) + \gamma_s + \lambda_t + \eta X_{ist} + \epsilon_{ist}, \qquad (3.7)$$

were $RatioEA_{st}$ is defined as the fraction of educational assistants of technical-pedagogical assistants over the total enrollment in school s in time t, and the other variables are defined as above. Results, which are displayed in Table 3.12, support the main hypothesis. Students attending schools that have invested relatively more resources in hiring technical-pedagogical assistants experience a greater positive effect on the non-academic outcomes considered. All the interaction coefficients are significant and have the expected signs. For instance, an increase of 0.1 in the ratio of education assistants in the school translates into an increase in self-efficacy levels of 0.11 standard deviation after the policy (or 0.17 standard deviation if the increase is specifically in the ratio of assistants in the technical-pedagogical area).

3.7 Conclusions

Many policymakers and researchers view school choice-based interventions as among the most promising strategies for improving educational outcomes, but the evidence, which has mainly focused on assessing the impact on students' achievement, is mixed and has not been fully examined. In this paper, I present new evidence on the impact of a targeted school voucher policy on a series of non-conventional outcomes, ranging from student's test scores to aggressive behavior (bullying). I use a difference-in-differences strategy to estimate the program's causal effect not only on students' test scores but in a range of outcomes that have not been previously explored by the literature.

I confirm previous findings that this policy did not significantly improve students' test scores [Feigenberg et al., 2019, Aguirre, 2020]. However, I show that the program led to meaningful improvements in students' non-cognitive and behavioral outcomes. In particular, I find that the targeted voucher policy is associated with an increase in academic self-efficacy and motivation and the students' expectations of attending higher education. Also, the policy reduced the probability of experiencing peer discrimination, the frequency (prevalence) of bullying suffered, and the occurrence of violent events in the school. These effects are large and economically significant in general. For example, the expectation of students to attend higher education increased between 1 to 2 percentage points in schools that signed up for the policy (from an average of 86% before the reform), the probability of suffering bullying decreased by 3 percentage points (from an average of 54% pre-reform), and the percentage of students reporting being discriminated against decreased by 5 percentage points (from an average of 73% pre-reform). Finally, I provide evidence of the primary channel behind my results: schools participating in the policy have used the additional resources to hire relatively more learning support staff, especially from the psycho-social area.

These findings highlight the importance of not focusing only on students' test scores when evaluating the impact of school choice. My results also highlight the importance of understanding how parents and families choose schools, considering that one of the main mechanisms through which a voucher reform impacts educational outcomes is that it provides school incentives to compete for the enrollment of students. If families choose schools predominantly for non-academic reasons, systematic academic gains would not be expected, which is exactly what I find.

Tables and Figures

Table 3.1: Definition of outcomes

Outcome	Description
Test Scores	Average of the normalized math and language SIMCE tests scores.
Academic self-efficacy and motivation	Students' perceptions of their aptitudes and abilities to learn, as well as their attitudes towards learning and academic achievement. Is standardized to have a mean of 0 and a standard deviation of 1.
Academic insecurity	Measure of the student's lack of academic confidence and motivation towards learning. Is standardized to have a mean of 0 and a standard deviation of 1.
Math academic self-efficacy	Students' perceptions of their aptitudes and abilities to learn math, and their attitudes towards learning and academic achievement in that subject. Is standardized to have a mean of 0 and a standard deviation of 1.
Student's expectation of Higher Education	Student's perceived probability of attending higher education. Takes the value of 1 if the student believes she will complete a higher education degree, and 0 otherwise.
Exclusion	Binary variable that indicates if the student has reported feeling left out, either because of sexual orientation reasons, for being an immigrant, because of gender, among other reasons.
Violence Type 1	Captures the frequency of robberies, malicious rumors , fights and threats and/or harassment among students in the school. Is standardized to have a mean of 0 and a standard deviation of 1.
Violence Type 2	Captures the frequency of assaults with bladed weapons, assaults or threats with firearms, and vandalism by students. Is standardized to have a mean of 0 and a standard deviation of 1.
Frequency of bullying (physical, verbal, social and cyberbullying)	Captures the frequency of bullying experienced by students, either physical (hitting or breaking your stuff), verbal (insulting you, mocking or threatening you), social (isolating yourself, speaking badly of you or humiliating yourself in front of others), or cyber bullying (threatening you, humiliating you or making fun of you for text or Internet messages, such as networks social, emails, etc.)". Is standardized to have a mean of 0 and a standard deviation of 1.
Prevalence of bullying	Binary variable that reflects the prevalence of bullying experienced by students. It takes the value 1 if the student reports having suffered bullying, either physical, verbal social or cyber bullying, and 0 otherwise.

Source: Own elaboration based on 8th-grade SIMCE questionnaires, years 2011, 2013, 2014 and 2015.

Table 3.2: Summary statistics before and after the SEP reform for 8th graders, by school status

	Non-SEP School				SEP School			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Mean	SD	Min	Max	Mean	SD	Min	Max
2011: pre-reform								
Language score	275.2	48.32	104.0	375.7	248.5	47.72	105.0	375.7
Math score	284.4	48.81	135.3	395.7	250.5	45.13	135.7	395.7
HE expectations	0.957	0.204	0	1	0.858	0.349	0	1
General self-efficacy	3.276	0.416	1	4	3.242	0.431	1	4
Math self-efficacy	2.679	0.808	1	4	2.693	0.768	1	4
Academic insecurity	2.101	0.676	1	4	2.246	0.709	1	4
Exclusion	0.663	0.473	0	1	0.731	0.444	0	1
Violence type 1	2.475	0.836	1	5	2.637	0.900	1	5
Violence type 2	1.367	0.514	1	5	1.511	0.650	1	5
Freq. of bullying	1.405	0.671	1	5	1.509	0.794	1	5
Prevalence of bullying	0.502	0.500	0	1	0.538	0.499	0	1
Father's education	13.79	3.783	0	22	10.05	3.824	0	22
Mother's education	13.77	3.317	0	22	10.33	3.479	0	22
Ln(income)	13.29	0.931	10.82	14.65	12.24	0.757	10.82	14.65
2013-2015: Post reform								
Language score	268.8	48.94	111.3	373.2	242.1	49.99	107.5	373.2
Math score	292.1	45.22	134.7	402.7	254.6	45.79	134.8	402.7
HE expectations	0.946	0.225	0	1	0.867	0.340	0	1
General self-efficacy	3.042	0.437	1	4	3.051	0.452	1	4
Math self-efficacy	2.698	0.789	1	4	2.712	0.726	1	4
Academic insecurity	2.116	0.645	1	4	2.171	0.660	1	4
Exclusion	0.684	0.465	0	1	0.683	0.465	0	1
Violence type 1	2.221	0.768	1	5	2.362	0.839	1	5
Violence type 2	1.279	0.393	1	5	1.394	0.528	1	5
Freq. of bullying	1.345	0.568	1	5	1.378	0.626	1	5
Prevalence of bullying	0.482	0.500	0	1	0.480	0.500	0	1
Father's education	14.72	3.951	0	22	10.56	4.003	0	22
Mother's education	14.65	3.355	0	22	10.94	3.573	0	22
Ln(income)	13.61	0.836	10.82	14.65	12.53	0.736	10.82	14.65

Notes: The descriptive statistics where calculated on the final sample of the 640,519 students.

Table 3.3: Effects of the SEP reform

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Frequency of bullying	(10) Prevalence of bullying
SEP dummy (SEP_{st})	-0.01	0.01***	0.12***	-0.12***	0.02	-0.05***	-0.03**	-0.05***	-0.09***	-0.03***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301
R-squared	0.30	0.07	0.10	0.05	0.04	0.02	0.13	0.12	0.04	0.03

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data of 8th grade students for the period 2011-2015. I report the estimated coefficient on the dummy variable that equals one if the school participates in the program at time t and zero otherwise. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Table 3.4: Effects of the SEP reform, interacted by low-SES indicator

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
SEP dummy	-0.01	0.01***	0.09***	-0.10***	-0.00	-0.04***	-0.03*	-0.05***	-0.07***	-0.02***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Low-SES	-0.05***	-0.03***	-0.06***	0.08***	-0.05***	0.02***	-0.00	0.00	0.02***	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
SEP×Low-SES	-0.00	0.01***	0.06***	-0.04***	0.04***	-0.03***	-0.02**	-0.01	-0.04***	-0.02***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301
R-squared	0.30	0.07	0.10	0.05	0.04	0.02	0.13	0.12	0.04	0.03

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: I report the estimated coefficient on the dummy variable that equals one if the school participates in the program at time t and zero otherwise, an indicator of low-SES and its interaction. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Table 3.5: Robustness test: School specific time tends

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
SEP dummy	-0.02	-0.01***	0.12***	-0.10***	0.01	-0.03***	-0.04**	-0.01	-0.09***	-0.02**
	(0.02)	(0.00)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301
R-squared	0.32	0.89	0.12	0.06	0.06	0.71	0.16	0.15	0.05	0.51

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: I report the estimated coefficient on the interaction between the dummy for the period after the introduction of the program and the fake treatment variable. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Table 3.6: Placebo test with a fake treatment group

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
SEP dummy	-0.06***	0.01***	0.07***	0.09***	0.01	0.07***	-0.01	0.01	0.02	0.01
	(0.02)	(0.00)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	139,983	139,983	139,983	139,983	139,983	139,983	139,983	139,983	139,983	139,983
R-squared	0.29	0.03	0.12	0.04	0.04	0.02	0.11	0.08	0.03	0.02

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data for students one year before and three years after the introduction of the SEP reform. I report the estimated coefficient on the interaction between the dummy for the period after the introduction of the program and the fake treatment variable. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Table 3.7: Composition of the enrollment and socioeconomic variables in the treatment and control groups

	(1) 2011	(2) 2013	(3) 2014	(4) 2015
Enrollment (treatment)	39.54	37.94	38.74	37.21
Enrollment (control)	50.99	51.32	54.45	54.89
Mother's years of education (treatment)	9.33	9.69	10.50	9.96
Mother's years of education (control)	12.52	13.37	13.96	13.55
Father's years of education (treatment)	9.11	9.44	10.23	9.44
Father's years of education (control)	12.53	13.27	14.1	13.33
Ln income (treatment)	12.05	12.25	12.35	12.41
Ln income (control)	12.9	13.2	13.26	13.33

Table 3.8: Robustness test: Controlling for municipal crime index

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
SEP dummy	-0.01	0.01***	0.12***	-0.12***	0.02	-0.05***	-0.03**	-0.05***	-0.09***	-0.03***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Crime rate	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301	586,301
R-squared	0.30	0.07	0.10	0.05	0.04	0.02	0.13	0.12	0.04	0.03

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data for students one year before and three years after the introduction of the SEP reform. I report the estimated coefficient on the dummy variable that equals one if the school participates in the program at time t and zero otherwise. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Crime rates control is the municipal rate of "crimes known to the police", every 100,000 inhabitants. Standard errors are clustered at the school level.

Table 3.9: Effects of the SEP reform, subgroup of students attending non-SEP schools in 2011

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
SEP dummy	0.01	0.01*	0.03**	-0.08***	-0.01	-0.04***	-0.02	-0.04**	-0.05***	-0.01
	(0.02)	(0.00)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	153,325	153,325	153,325	153,325	153,325	153,325	153,325	153,325	153,325	153,325
R-squared	0.29	0.04	0.11	0.04	0.04	0.02	0.13	0.12	0.03	0.02

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data for students one year before and three years after the introduction of the SEP reform. Sample consist of subgroup of students attending schools that did not participate in the SEP policy in 2011. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Table 3.10: Effects of the SEP reform, market-level analysis

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
post×intensity	-0.23***	0.03***	0.36***	0.02	0.11***	-0.10***	0.01	0.13***	-0.02	-0.06***
	(0.05)	(0.01)	(0.04)	(0.05)	(0.04)	(0.02)	(0.04)	(0.05)	(0.04)	(0.02)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515
R-squared	0.18	0.04	0.07	0.03	0.01	0.00	0.04	0.02	0.01	0.00

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data for students one year before and three years after the introduction of the SEP reform. The treatment variable correspond the municipality's share of priority students. I report the estimated coefficient on the interaction between the dummy for the period after the introduction of the program and the treatment variable. Standard errors are clustered at the school level.

Table 3.11: Effects of the SEP reform, market-level analysis interacted by low-SES indicator

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
post×intensity	-0.28***	0.01	0.21***	0.18***	0.03	-0.01	0.11**	0.25***	0.10***	-0.04**
	(0.05)	(0.01)	(0.04)	(0.06)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.02)
Low SES	-0.11***	-0.04***	-0.05***	0.11***	-0.01*	0.03***	0.03***	0.07***	0.05***	0.00
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
(post×intensity)×	0.06***	0.02***	0.17***	-0.18***	0.09***	-0.11***	-0.12***	-0.14***	-0.15***	-0.03***
Low-SES	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515	640,515
R-squared	0.18	0.04	0.07	0.03	0.01	0.01	0.04	0.02	0.01	0.00

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: difference-in-differences regressions use data for students one year before and three years after the introduction of the SEP reform. The treatment variable correspond the municipality's share of priority students. I report the estimated coefficient on the interaction between the dummy for the period after the introduction of the program and the treatment variable. Standard errors are clustered at the school level.

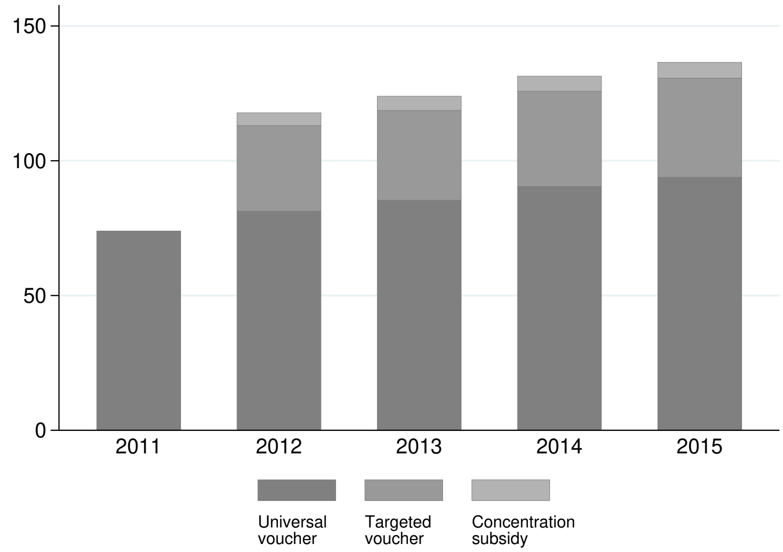
Table 3.12: Effects of the SEP reform, interacted with the ratio of EA over total enrollment

	(1) Test scores	(2) HE expect.	(3) Academic self-efficacy	(4) Academic insecurity	(5) Math self-efficacy	(6) Exclusion	(7) Violence type 1	(8) Violence type 2	(9) Freq. bullying	(10) Prevalence bullying
Panel A. Ratio of EA over total enrollment										
SEP dummy	-0.008	0.000	0.069***	-0.098***	0.002	-0.030***	-0.001	-0.010	-0.050***	-0.014**
	(0.014)	(0.003)	(0.013)	(0.013)	(0.016)	(0.007)	(0.017)	(0.015)	(0.013)	(0.006)
EA ratio	-0.088***	-0.006***	-0.041***	0.028***	0.030***	0.000	0.067***	0.115***	0.038***	-0.007***
	(0.004)	(0.001)	(0.007)	(0.005)	(0.003)	(0.002)	(0.005)	(0.006)	(0.004)	(0.002)
SEP dummy×	-0.137	0.330***	1.131***	-0.587***	0.320*	-0.532***	-0.830***	-1.142***	-1.002***	-0.341***
EA ratio	(0.163)	(0.055)	(0.167)	(0.169)	(0.180)	(0.084)	(0.212)	(0.215)	(0.187)	(0.080)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473
R-squared	0.297	0.068	0.100	0.046	0.044	0.023	0.132	0.123	0.037	0.025
Panel B. Ratio of technical-pedagogical EA over total enrollment										
SEP dummy	-0.008	0.007***	0.094***	-0.108***	0.011	-0.040***	-0.020	-0.027**	-0.066***	-0.022***
	(0.012)	(0.002)	(0.011)	(0.011)	(0.014)	(0.007)	(0.015)	(0.013)	(0.011)	(0.005)
EA ratio	-0.228***	-0.013***	-0.109***	0.093***	0.083***	0.006	0.197***	0.341***	0.133***	-0.013
	(0.010)	(0.004)	(0.014)	(0.009)	(0.009)	(0.011)	(0.024)	(0.015)	(0.038)	(0.009)
SEP dummy×	-0.470	0.553***	1.737***	-1.133***	0.308	-0.990***	-1.267***	-2.491***	-2.039***	-0.507***
EA ratio	(0.310)	(0.106)	(0.322)	(0.326)	(0.325)	(0.162)	(0.401)	(0.411)	(0.368)	(0.153)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473	583,473
R-squared	0.297	0.068	0.100	0.046	0.044	0.023	0.132	0.123	0.037	0.025

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

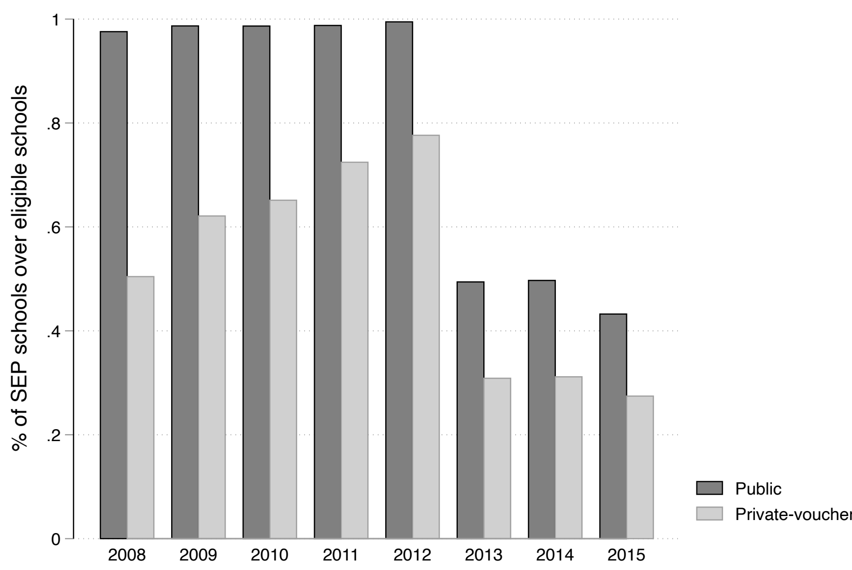
Notes: regressions use data for students one year before and three years after the introduction of the SEP reform. Additional controls include mother's years of education, father's years of education and log household income (in Chilean pesos). Standard errors are clustered at the school level.

Figure 3.1: Monthly per-student subsidy, 8th graders (US dollars)



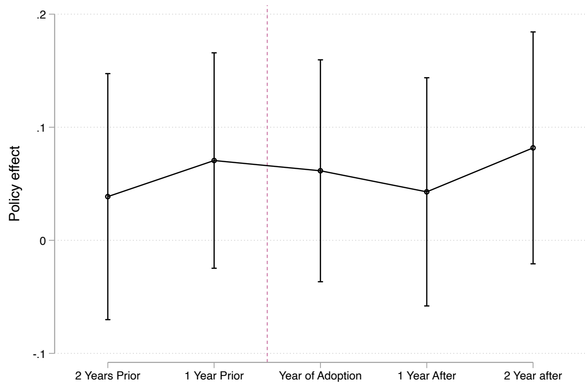
Notes: Amounts are in 2015 US dollars. The Concentration subsidy corresponds to a subsidy that depends on the percentage of disadvantaged students enrolled. These extra resources were available to public and private voucher schools that signed up for the policy.

Figure 3.2: Evolution of the participation of schools in SEP policy

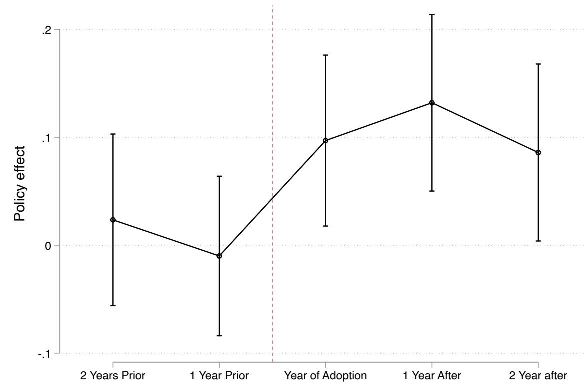


Notes: The figure displays the percentage of schools participating in the SEP policy over the total number of eligible schools. The set of eligible schools includes, until 2012, schools that offer primary education, and after 2013, schools that provide either primary or secondary education.

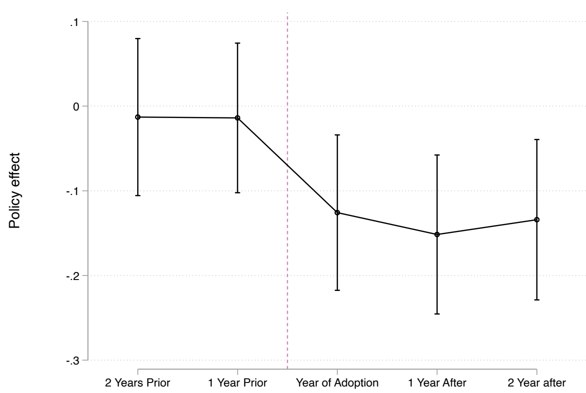
Figure 3.3: Time passage relative to year of adoption of SEP policy



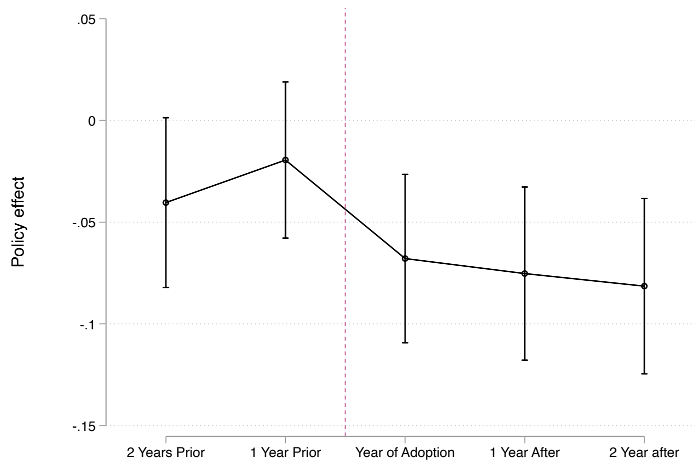
(a) Test Scores



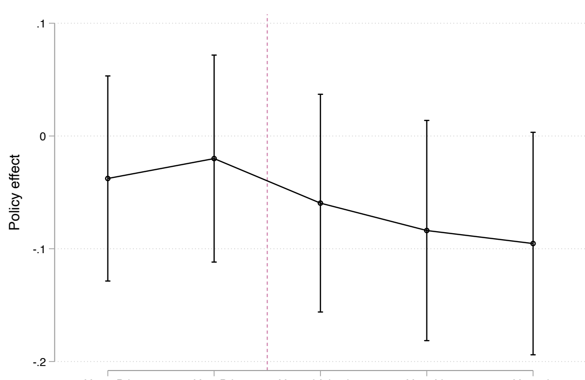
(b) Academic self-efficacy



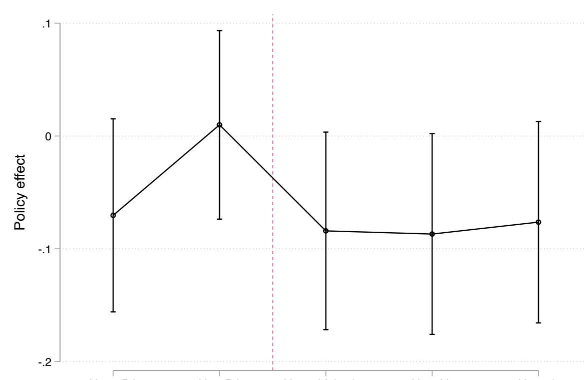
(c) Academic insecurity



(d) Exclusion



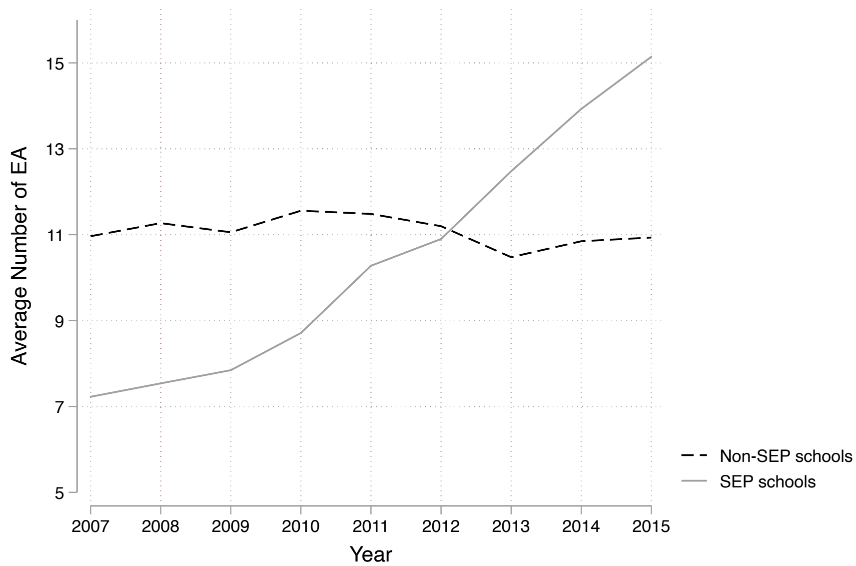
(e) Severe Violence



(f) Frequency of bullying

Notes: The figure tests differences in pre-trends using the leads and lags test. We can see that most of the pre-treatment coefficient are nearly zero and, with the exception of the test scores, they immediately shoots up (or down) after the treatment.

Figure 3.4: Evolution of Education Assistants in SEP and non-SEP schools, 2007-2015



Notes: The figure above displays the average number of education assistants, informed by each school, on June 30 of each year. For 2007, I classify as SEP schools those who joined the reform in 2008.

Figure 3.5: Effect of the policy reform on EA ratio

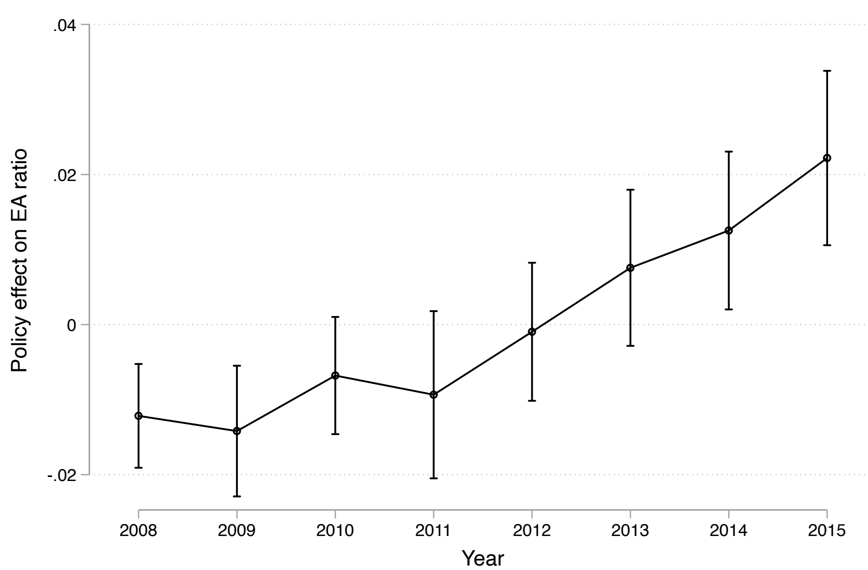
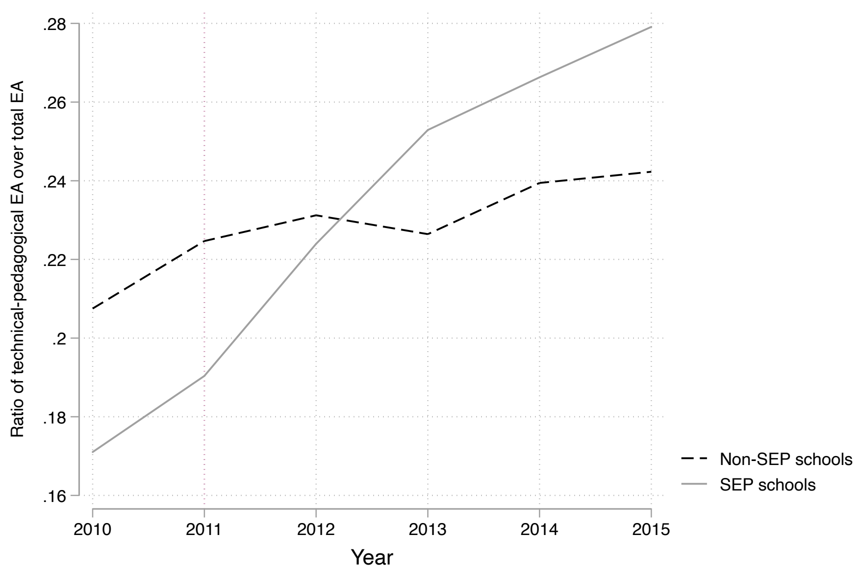


Figure 3.6: Evolution of ratio of technical-pedagogical assistants over total number of assistants in SEP and non-SEP schools, 2010-2015



Notes: The figure above displays the average number of education assistants, informed by each school, on June 30 of each year. For 2007, I classify as SEP schools those who joined politics in 2008.

Chapter 4: Centralized Admission Systems and School Segregation: Evidence from a National Reform

Note: This chapter of the dissertation is coauthored with Shanjukta Nath and Sergio Urzúa.

4.1 Introduction

All over the world, governments are increasingly adopting centralized school admission systems to coordinate students' assignment to schools. Different versions of these mechanisms have been implemented in Belgium, England, France, Finland, Ghana, Holland, Hungary, South Korea, Spain, and the United States [Abdulkadiroğlu et al., 2005a, 2011, 2017, Calsamiglia and Güell, 2018]. Also, pilot programs have been initiated recently in Brazil, Ecuador, and Peru. The arguments in favor of centralized over decentralized systems work mainly through three avenues. First, they eliminate supply-side selection, allowing equal access to higher quality schools for all [Abdulkadiroğlu and Sönmez, 2003, Pathak et al., 2011]. Second, centralized systems should reduce the cost and effort of collecting information and applying to every school separately [Whitehurst and Whitfield, 2013]. Third, and most relevant for this paper, they are also used as policy tools to promote diversity within educational systems, often giving priority to low SES students at the randomization stage under these algorithms. For instance, New York and Boston have modified their centralized admission systems in recent years to give minority groups higher chances to attend better public schools.[1]

However, a more efficient and welfare-enhancing centralized allocation might not necessarily lead to less segregated schools relative to the decentralized setting. For instance, by examining a mechanism design problem embedded in a city model of public school choice, Calsamiglia et al. [2020] show that the trade off between travel cost and school quality in parent's preferences can, in theory, increase segregation levels under centralized admission systems. Moreover, within their framework, heterogeneity in outside options (e.g., private schooling) can also lead to a similar phenomenon as access to private schools gives an edge to families who can afford them.[2] Despite these conceptual insights, no empirical assessment of these ideas is found in the literature.

This paper closes this gap. Formally, it estimates the impact on school segregation of a centralized school admission system relative to a decentralized setting and assesses potential channels. To this end, we take advantage of the largest school-admission reform implemented to date: Chile's adoption of a centralized system. In a nutshell, in 2016 the Chilean government introduced a Deferred Acceptance (DA) mechanism as the central component of a national education reform aimed at promoting social inclusion and reducing school segregation. The new school admission protocol (SAS) was undertaken between 2017 and 2019, and it came to replace

[1]There is evidence that school desegregation policies in the United States have resulted in better educational outcomes for minority groups as reflected by higher academic achievement, lower drop out rates, and higher graduation percentages [Guryan, 2004, Hanushek et al., 2009, Johnson, 2011]. Additionally, effective desegregation has improved health and labor market outcomes [Ashenfelter et al., 2006]. Other researchers have shown comparable results on segregation for OECD countries [Vandenberge, 2006, Baysu and de Valk, 2012]. This body of evidence is relevant even for developing countries as one of the impending challenges for them has been the increasing levels of stratification in school [Epple et al., 2017].

[2]Students from wealthier families can be strategic in their rankings by only applying to the best public schools, opting out to private providers if they are not assigned to those schools [Calsamiglia et al., 2020]. Akbarpour et al. [2018] discuss how this can increase segregation under alternative and manipulable centralized assignment mechanisms (e.g. the Boston mechanism). They, however, do not consider the decentralized case. The outside options have been shown to be relevant for segregation in other contexts as well. For example, Baum-Snow and Lutz [2011] find an increase in private school enrollment following the desegregation of large urban public school districts in the US.

the country's widely studied decentralized system [Epple et al., 2017, Hsieh and Urquiola, 2006, Mizala and Romaguera, 2000].

We exploit the sequential introduction of the reform across regions to quantify its impact on segregation using a difference-in-differences strategy. We analyze rich administrative data from the universe of Chilean students during the period 2015-2019. Following the literature, the empirical analysis uses the unevenness of schools' socioeconomic distribution relative to the community (school district) as the outcome of interest [Allen et al., 2015, Napierala and Denton, 2017]. In particular, we construct a dissimilarity indicator based on the share of students within a district that would have to be reassigned across schools to achieve an even distribution of students of different socioeconomic status [Duncan and Duncan, 1955]. The socioeconomic status is defined using detailed information on family background characteristics. Moreover, we use Chile's comprehensive road network and its rich geospatial data to construct a measure of residential segregation. For this, we calculate the travel times of student's place of residence to essential amenities (e.g., schools, hospitals and policy stations) in their neighborhoods [Massey et al., 1987]. The main analysis focuses on ninth-graders (approximately 250,000 individuals each year) as for this group we observe socio-economic background information and standardized test scores. Importantly, related research has shown that segregation at this level (high school) is more pronounced than in primary education [Torche, 2005].

The richness of the data allows us to go beyond the average impact of SAS on aggregate school segregation and explore the heterogeneous effects suggested by the theory. Specifically, we document considerable and systematic heterogeneity across local school districts (municipalities), which we empirically link to existing local outside options and residential segregation. In particular, we find that the DA mechanism increased (decreased) within-school segregation of students from low socio-economic (SES) backgrounds in areas with high (low) levels of existing residential segregation. When it comes to role of outside options, we show that school districts with higher pre-reform market shares of private schools report an increase in segregation relative to districts with a smaller percentage of private schools. We confirm this result using alternative proxies for the availability of outside options. Our other proxies include variation in travel distance to private schools. These results provide further evidence that a higher variation in access to private schools increases school segregation in the presence of policy. With respect to the mechanisms, our evidence suggests this result is mainly driven by the migration of high SES families from public and voucher to private schools after the policy is in place.

In perspective, our analysis unravels a complex association between centralized admission systems and changes in school segregation, particularly in areas characterized by sharp variation in neighborhood quality and schooling structure. Consistent with recent theoretical developments, we show that under Chile's SAS parental preferences and market forces can induce strategic behavior among families, which can increase segregation in the public school system under a DA mechanism (relative to the decentralized alternative). These settings and landscapes can be found in developed and developing countries. Overall, our findings alert about the potential unintended consequences of centralized systems and suggest venues to secure the efficacy of public school choice [Hastings et al., 2009].

The paper is organized as follows. Section 4.2 describes the key features of Chile's new centralized school admission system. Section 4.3 describes our data, while Section 4.4 presents our identification strategy. In Sections 4.5 we present our main findings and robustness checks. Section 4.6 concludes.

4.2 Background

In the early 1980s, Chile's military regime undertook a structural reform of the educational system. It involved a decentralization of the public school system by transferring the administration of local school districts from the central government to municipalities. A nationwide flat per-student voucher was established, paid directly to schools, and parents were allowed to freely choose their children's school. The school market was opened to new entrants and, as a result, three types of schools were established: (a) public schools, managed by local municipalities and financed by vouchers; (b) private-voucher schools, privately managed but financed by vouchers and; (c) private non-voucher schools, in which the private sector provides both funding and administration. We refer to private voucher and private non-voucher schools as voucher and private schools for the rest of the paper.

The reform has led to a massive entry of voucher and private schools into the market and, as a result, enrollment in public schools declined substantially. In 1993, voucher schools were allowed to charge out-of-pocket tuition to families, with a corresponding (and progressive) reduction of the state voucher according to the amount of tuition charged. Public schools could also charge add-on fees, but only at the secondary level and if all parents agreed.[3] Consequently, the Chilean education setting emerged into a system with multiple private providers, with for-profit and not for profit schools, with and without receiving public vouchers, and with and without parental co-payment. By 2015, out of the 3.5 million total students in the system, 36% attended public schools, 55% voucher schools, and 9% attended private schools.

One of the criticisms of the voucher system is that it led to higher socioeconomic stratification,

[3] By 2014, the average fee was US $30.23 in voucher schools and US $4 in public schools (in nominal terms).

with attendance to different school types depending greatly on family socioeconomic characteristics [Hsieh and Urquiola, 2006, Valenzuela et al., 2014]. The student admission process was highly unregulated until 2008, and most voucher and private schools selected students based on parents' interviews, entry exams, and required proof of income. [OECD, 2010, Contreras et al., 2010b]. In addition, not only parents were likely discouraged from applying to top schools because of the out-of-pocket tuition and the selection processes, but there is also evidence that parents from low-income parents reacted less strongly to school quality when choosing a school [Carnoy and McEwan, 2003, Gallego and Hernando, 2010]. This resulted in an overwhelming majority of low SES students in public schools, which is the principal reason for increased stratification.

Given the significance of school segregation for short- as well as long- term student outcomes, in 2008, the Chilean Congress passed the Law 20,248 of *Subvención Escolar Preferencial* (SEP), which established a targeted voucher that provided schools with an additional voucher for every priority student that they enroll. This policy aimed to address the existing segregation by socioeconomic status in the Chilean educational system and to improve educational outcomes for disadvantaged students [Sánchez, 2018b, Kutscher, 2020].

In a new attempt to promote inclusion, in May of 2015 the Congress passed the Inclusion Act (*Ley de Inclusión Escolar*, N. 20.845). The new legislation set the expectations high as its text demonstrates "... [this Act] will allow us to make decisive progress in ending the high school segregation that characterizes our current system".

The Inclusion Act included three broad provisions. First, the country's decentralized admission model would be replaced by a centralized school admission system. This is the policy change examined in this paper. Second, by annually increasing the value of the vouchers from 2016 and on, the reform sought to eliminate the student co-payment system allowed in public and voucher schools since 1993. Third, starting in 2018, only not-for-profit schools could receive vouchers. The last two components of the reform were implemented nationally and without regional considerations. On the contrary, the centralized system was implemented sequentially across regions in a process that would take several years. This distinctive features enables our identification strategy.

4.2.1 The Centralized School Admission System (SAS)

As stated above, one of the foundations of the Inclusion Act was the regulation of the school admission system for public as well as voucher schools receiving state support. This implies that a student must participate in SAS if he/she seeks admission into public and/or voucher schools. Admissions to private schools continues to be decentralized.

Deferred Acceptance algorithm with multiple tie breaking was used for student assignments under this new mandate. This algorithm was first proposed by Gale and Shapley [1962] and modified later by Abdulkadiroğlu and Sönmez [2003].[4] It eliminated supply-side choice for state sponsored schools. Therefore, since 2016, families are required to apply to public and voucher schools through a centralized web application platform.

The centralized web platform provides information about the schools' educational project, its facilities, extra-curricular activities, and other features. The algorithm allows for special priorities such as i) applicants that have a sibling enrolled in the school; ii) applicants classified as priority students, up to the minimum of 15% per level; iii) the children of school officials; and iv) former students of the school who had left for various reasons except for expulsion.

[4]For evidence from Boston, New York, Denver see Abdulkadiroğlu et al. [2005a, 2011, 2017, 2005b, 2009]. For Charlotte see Hastings et al. [2009], Barcelona see Calsamiglia and Güell [2018], and Beijing see He et al. [2012].

Families indicate these priorities in their web applications. Note that there are no restrictions on the school set the family can list, and the proximity to school does not play a role (there is no neighborhood priority). The new system intends to allocate every student to the highest plausible preference conditional on the priorities and vacancies at schools. If there are fewer applications than vacancies at a school, there is no requirement for the multiple tie-breaking rules. On the contrary, if the number of applications exceeds the number of vacancies, the tie-breaking rule kicks in.

In practice, the algorithm involves two stages. All the applicants are allowed to participate in the regular stage, which starts in August of each year. Once it ends (October), students are informed about the schools they will be attending during the next school year (next March). At that point, each student has the right to turn down the assigned school, in which case he/she can submit a new list of preferences during the complementary stage (November). Individuals who did not participate in the regular stage can submit preferences as well. The algorithm is launched again but only for the set of vacant seats. The complementary stage concludes in December. Students are assigned to the closest participating school if the process fails to assign a student to any of the preferred choices. Since private schools do not participate in DA, parents could switch to those providers if they are not satisfied with the DA assignment either in the regular or complementary stage.

The implementation of SAS has been gradual, which is critical for our identification strategy. The reform began in 2016 in Magallanes region, in 2017 it continued in the regions of Tarapacá, Coquimbo, O'Higgins, and Los Lagos, in 2018 included Arica and Parinacota, Antofagasta, Atacama, Valparaíso, Maule, Biobío, Araucanía, Los Ríos, and Aysén. Finally, in 2019 it was introduced in the Metropolitana Region (Santiago).[5]

4.3 Data and key variables

Our primary source of information comes from the student-level administrative files reported under Chile's SAS system. These documents contain detailed application data for all students who submitted their preferences to enroll in primary (pre-k to 8th grade) and secondary (9th to 12th grade) schools.

Figure 4.1 illustrates the differences in program participation by grade. The number of participants in the new policy has been largest for the first year of primary school (pre-K) and the first year of secondary school (ninth grade) relative to the other grades. The participation is high for pre-K since it is mandatory for parents seeking admission into public and voucher schools to apply through SAS. For ninth-graders, a notable percentage of students use SAS because some of them are forced to switch schools as their primary schools do not offer secondary education and others participate as they wanted to get themselves enrolled in a different institution. Table 4.1 provides details on the participation of students and schools for 2016, 2017, and 2018. On average, 46% of eighth-graders participated in SAS for ninth grade admissions in 2016 in the region where SAS was implemented. This increased to 51% in 2017 and 52% in 2018.

We focus our study on the ninth graders primarily because of data limitations. The dissimilarity index construction requires background information on a student's socioeconomic status, which we gather by matching students to their previous records from administrative sources. We use data from the Education Quality Measurement System (*Sistema de Medición de Calidad de la Educación* or SIMCE). It contains detailed individual-level information, including mother's

[5] Table **??** in the Web Appendix shows the number of schools that participated in each region.

education, father's education, and household income. Chile's SIMCE is generally administered twice during the students' educational history, either in second, fourth, sixth, eighth, or tenth grade. Therefore, despite its richness, no background information for students seeking admission in pre-k is available. In addition, there is evidence suggesting that the increase in school-segregation levels is more pronounced in secondary than in primary education [Torche, 2005]. Hence, studying ninth-grade students is more appealing as they face higher levels of segregation. Thus, we focus on the entire universe of ninth graders in 2015, 2016, 2017, 2018, and 2019 in Chile and examine the school preferences and decisions of approximately 250,000 individuals per year.

We complement the student-level data with multiple sources of longitudinal information. We gather pre- and post-reform student-level enrollment information from the administrative records, containing student's residential and school locations, and school characteristics. Critical for this paper, these files can be linked to official information collected under the new admission system.[6]

Since our main empirical analysis is carried out at the school district level, from these data sets we construct a panel of 327 municipalities (out of a total of 345) over five years for an overall sample size of 1646 observations. For only 18 municipalities (all small and rural) student-level information from SIMCE files was not available.

[6]To obtain the background information on students, we match every ninth-grade cohort to the previous SIMCE available, either sixth or eighth depending on the cohort. This process was carried out in collaboration with the Ministry of Education of Chile and produced a sucesful match for approximately 80% of the students. See Table **??**.

4.3.1 School segregation measures

Any measure of school segregation is constructed relative to a specific geographical area. In this paper, we use the school district, defined by the municipality, as the unit of analysis.[7] Multiple reasons explain this decision. First, as Table 4.2 suggests, a local district can be conceptualized as a proxy for a school market as 78% of students in Chile choose a school within this area (the corresponding figure for ninth graders is 66%). Importantly, we do not observe any notable difference in this fraction pre and post the inception of SAS, which suggests most of the parents were choosing among schools within their municipality both before and after the reform. Second, by examining school segregation across municipalities, we can capture significant within-region heterogeneity, hidden under broader definitions of local schooling markets. Third, this is the most commonly used definition of school markets in related work in Chile and other countries [Hsieh and Urquiola, 2006]. This helps us to compare the implications of the current policy change with segregation related research on earlier policies of the government and other geographies.

After setting our unit of analysis, we proceed to combine the information from the sources listed above. This process results in a panel of 346 municipalities covering the period 2015-19. For each municipality and year, we construct the Duncan index for school segregation, which measures the percentage of students belonging to low socioeconomic status who have to be reallocated across schools for equal representation of students from all socioeconomic

[7] Although the funding for public education comes from the central government, local authorities (majors) are responsible for the management of public schools. Contrary to the school districts in the United States, students in Chile can attend public schools outside their local school districts. The robustness of our findings to this complexity is discussed in Section 4.5.3.

backgrounds within the district. Formally, we follow Duncan and Duncan [1955] and compute:

$$y_{ct} = \frac{1}{2}\sum_{j=1}^{n_{ct}} \left| \frac{P_j}{P} - \frac{NP_j}{NP} \right|,$$

where c and t correspond to municipality and year, respectively, whereas P_j (NP_j) is the number of students reporting low (non-low) socioeconomic status in school $j \in \{1, 2, ..., n_{ct}\}$ with n_{ct} denoting the number of schools in municipality c in year t. N and NP represent the total number of poor and non-poor students for the municipality.

We use the Duncan index for our main analysis as it is the most commonly used segregation index. However, we acknowledge that it suffers from various limitations. First, it does not always satisfy the transfers principle. Second, we can compare the disparity of only two groups in this index. Third, Duncan index does not account for the segregation resulting from randomness [Allen et al., 2015, Winship, 1978, Carrington and Troske, 1997, Hellerstein and Neumark, 2008, Söderström and Uusitalo, 2010]. Fourth, it cannot be decomposed. Thus, to confirm the robustness of our findings, we complement our primary analysis using an alternative measure of segregation in Section 4.5.3.

As for the definition of socioeconomic status, we classify students who have mothers without a high school degree as low SES. This dimension has been identified as a crucial socioeconomic marker [McLanahan, 2004]. Figure 4.2 displays the resulting Duncan index for Chile's fifteen regions for 2017 and 2019, documenting significant variation.[8] To put this in context, Jenkins et al. [2008] argue that school segregation in OECD countries during the last decade varied

[8]Chile created the sixteenth region Nuble, formerly part of Biobio in 2018. We merge Nuble with Biobio for our analysis. The Metropolitana Region (Santiago) initiated the policy in 2019, and the new student enrollment through the policy will be reflected in 2020 enrollment files

between [0.2,0.5] with countries such as Belgium, Germany, and Hungary being at the upper end of the spectrum, while Nordic countries reported lower levels than others in this group. Similarly, Reardon and Yun [2001] demonstrate that dissimilarity index estimates on racial lines varied between [0.38,0.46] for 323 Metropolitan Statistical Areas in the United States. For Chile, Valenzuela et al. [2014] report segregation levels varying between 0.4 and 0.6 for the period 1999 and 2008. According to our estimations, in 2019, the Duncan index ranges between 0.3 and 0.5 for the fifteen regions in Chile.

In addition, we intend to capture heterogeneous effects of SAS. We focus on two dimensions highlighted in the literature. Specifically, we exploit heterogeneous effects based on a residential segregation and the outside option value.

4.3.2 Residential segregation

As in other developing countries, geographical segregation is a widespread phenomenon in Chile [Garreton et al., 2020]. This inherent constraint might limit the integration of students from different socioeconomic background within an educational community even under centralized admission systems. Intuitively, if low income families are more likely to list poor quality nearby schools in SAS, residential segregation could translate into higher levels of segregation in schools. In this paper we explore this hypothesis.

Several regions in the country, particularly the most populated ones, present concentration of individuals from different socio-economic strata in specific pockets. Figure 4.3 illustrates this fact for Chile's third largest region, Biobio, as it depicts its spatial segregation by income levels. Panels (A) and (B) show that spatial density contours are not overlapping: while high income families (A) are spatially more concentrated in the south west and the east, low income families (B) have higher densities in the north and central parts of the region.

To capture residential segregation within our framework, we build an index of commuting time to amenities such as hospitals and police stations. This strategy is consistent with the evidence from urban economics suggesting that neighborhood quality is a function of access to health care, police stations, and good quality schools [Cheshire and Sheppard, 2004, McKenzie, 2013]. As a result, residents of advantaged and disadvantaged areas are exposed to very different bundles of amenities, unfolding inequalities that may be more deleterious than those of income inequality [Diamond, 2016]. Figure 4.4 exemplifies the point that access to hospitals varies across municipalities in Chile.

We take advantage of the geospatial student-level data and the comprehensive road network of Chile for the construction of the index. We define an amenity as accessible if the driving time is within an hour for a student and compute the travel time for every student amenity pair within municipalities.[9] Given that the empirical analysis is done at the local level, we construct:

$$\text{Residential Segregation}_c = \sqrt{\frac{\sum_{i=1}^{N_c}(A_{ic} - \bar{A}_c)^2}{N_c - 1}},$$

where A_{ic} denotes the number of amenities within an hour of travel time for student i in municipality c, $\bar{A}_c$ is the average of this measure for the municipality, and N_c denotes the number of ninth graders in municipality c. The index then captures the variation (standard deviation) in access to amenities within a municipality. We generate it for the universe of ninth graders in 2018.[10]

[9]To avoid the endogeneity of travel time to schools with respect to the policy change, we exclude schools from the list of amenities.

[10]In collaboration with the Ministry of Education of Chile, we use geocoded information for the universe of ninth-grade cohort in Chile in 2018 (246,937 individuals) to generate precise measures of student's travel duration to basic

The existing residential segregation can translate into higher segregation in schools if parents have a preference for nearby schools. Figure 4.5 presents evidence suggesting the prevalence of such preferences in Chile. It displays the distribution of travel times for ninth-grade participants in 2018 and confirms the affinity of parents towards shorter travel times. Figure 4.6 explores this further as, after taking into account the road network of Chile and real time traffic, it shows that the density of enrolled students (red) is much higher closer to the school relative to those who live far away from this school. The density of non-enrolled students consistently increases as students move further away from this school. In addition, there is direct evidence in Chile that parents are sensitive to travel costs and that the burden of these costs decrease with student income. In this same setting, Nath [2020] shows that high income families are more likely to list high score schools than low income families, and are less sensitive to travel distance to good quality schools.

Under these conditions, differential socio-economic ranking behavior could translate into under-representation of low income families in good schools. As Calsamiglia et al. [2020] argues, the trade off between travel cost and school quality in parent's optimization problem can, in theory, increase segregation levels across schools even under deferred acceptance algorithm. Low SES students often value school academic quality less than high SES students, and usually, travel costs to school are more binding for the former than the latter group. In other words, disadvantaged students have their effective choice sets restricted, as they cannot easily afford travel costs [Laverde, 2020] Thus, parental preferences for shorter travel time to schools can affect reallocation under the new policy. This can be reinforced by the evidence from developing

amenities. The Open Source Routing Machine API was used for this purpose. As Web Appendix **??** discusses, the travel time measure (or travel distance) provides a different and more accurate picture relative to the geodetic measures of distance employed in related research. Figures **??** and **??** illustrate why it is pertinent to examine distance to schools using the actual travel time.

countries suggesting that high income families are better at understanding the centralized systems and make more informed choices [Luflade, 2017, Ajayi et al., 2020].

4.3.3 Outside options

The second dimension of heterogeneity motivating this paper is the variation in outside options. Since private schools do not participate in DA, parents can switch to private schools if they do not like the DA assignment. However, private schools are expensive and there might be income barriers to entry (entry exams, interviews to parents, etc.). Table 4.3 shows that only 8.8% of ninth grade students are enrolled in private institutions.

The local provision of education throughout Chile is highly heterogeneous, across and within regions and municipalities. To illustrate this point, Figure 4.7 displays the spatial distribution of school types in two large regions. Panel (A) shows that while private schools are more common in municipalities belonging to the north-eastern sector of Santiago, it is almost non-existent in the southern districts. Therefore, even though the participation of private schools in Chile's capital is 13.7% (close to the national average), its distribution among districts is far from being homogeneous. For Coquimbo, Panel (B) offers quite a different picture as the distributions of school types are appreciably more homogeneous. Figure 4.8 examines this further as it presents spatial density plots of the different types of schools for Concepcion, Chile's second-largest region. It documents very distinctive patterns: higher concentrations of public (panel B), voucher (C), and private (D) in certain areas. Thus, these differences shed light on an obvious point and well-known fact: schools are strategically located in the territory [Epple et al., 2017]. Our empirical analysis exploits this.

However, private school supply itself does not translate into higher segregation if the assignment mechanism is strategy proof. Nonetheless, it can increase segregation if parents are behaving strategically [Calsamiglia et al., 2020]. One of the cases in which DA fails strategy proofness is when there are positive application costs to schools [Fack et al., 2019, Kuersteiner et al., 2020]. Those costs can be direct (e.g., application fees) or indirect (e.g., gathering information on school attributes or adding schools to the list). The existence of such costs lead to partial rank order lists.

In the Chilean DA system, we observe parents revealing a partial rank order list over the schools available to them. Table 4.4 depicts reduced form evidence on the factors determining the length of the Rank Order List (ROL). It confirms that the supply of private schools has a negative impact on the length of ROL relative to the total number of schools participating in DA. On the other hand, Table 4.5 depicts the results obtained from a regression of the fraction of DA participants on the fraction of private and voucher schools. We can conclude that school districts with a higher fraction of private schools received fewer applications in DA. This shows that private schools are a feasible substitute for voucher and public schools at least for some families and the presence of such schools not just impacts strategic behavior in DA for participants but also the participation in DA.

To quantify the extend to which individuals within a school districts could potentially opt out of the centralized system and attend an outside option, we first construct the Herfindahl index (HHI) of local school market concentration. Since this is a measure of the size of schools in relation to the local supply, we would expect it to capture the amount of competition within the district, including private providers, and potentially shape how SAS affects within-school diversity.

And to take further advantage of the complexities of the Chilean schooling system, we analyze the distribution of school types (public, voucher and private) in each district. Importantly, since the contemporaneous proportions could be correlated with the new policy, we use those from 2015 (i.e., pre-SAS). In this way, we assess whether the impact of SAS may differ across municipalities as a function of the pre-reform distribution of school types. In theory, municipalities with a higher relative representation of private schools should experience an increase in school segregation after SAS.

We provide further evidence on the access to outside options and its implications on school segregation in the presence of the new policy using the travel time to private schools. We begin by extracting the ninth-grader residential addresses from the enrollment files. Next, we match each student's residential district to all the private schools within their district. Then we calculate the travel time for every student private school pair. These travel times are used to compute the standard deviation in access to private schools. This measure of variation to private schools and the local school structure are used to shed light on the role of outside options on school diversity.

4.4 Empirical Strategy

Table 4.6 presents the summary statistics. The mean for the municipality Duncan index is 0.25, with a standard deviation of 0.19, which is larger than the variation observed across regions. Additionally, we observe significant variation in residential segregation and the local schooling systems across school districts.

We take advantage of the longitudinal dimension of our data as well as the gradual implementation of SAS to estimate its impact on school segregation. In particular, we follow a standard difference-in-differences strategy where the outcome variable is the Duncan index in municipality c, located in the region r, constructed at time t, y_{crt}. Thus, we estimate the following DD regression:

$$y_{crt} = \delta_0 \times D_{rt} + Z_{1cr}\beta + \gamma_r + \lambda_t + \epsilon_{crt}, \tag{4.1}$$

where D_{rt} is the treatment variable, which takes a value of one if the program is implemented in the region r in year t, and zero otherwise. Since the Chilean government stepped up the new program gradually across regions, we add region fixed effects (γ_r) to account for time-invariant heterogeneity at that level. Moreover, to account for any aggregate variation in segregation over time, we control for year fixed effects (λ_t). The covariates Z_{1cr} comprise of the pre-SAS measures of local schooling structure such as the fraction of public, voucher and private schools, and the fraction of rural schools in the municipality. ϵ_{crt} is the error term. Throughout our analysis, we cluster the standard errors at the school district, as this accounts for serial correlation [Bertrand et al., 2004].The policy parameter of interest is δ_0.

The identification of δ_0 depends on the following assumptions [Athey and Imbens, 2018, Goodman-Bacon, 2018]. First, the adoption date of the policy for every region in Chile should be random to existing levels of school segregation before the policy.[11] Second, there should not be any responses in anticipation of the treatment. This suggests that if region r has not adopted the policy, then the exact date at which it intends to adopt the policy should not affect the current outcomes. Third, school choices for students entering high school in a region in year t depends on SAS in year t and not on SAS's availability in previous years. Finally, we rely on the common trends assumption. We assess each of these assumptions in Section 4.5.

[11] Table **??** in Web Appendix illustrates that the start date of the program was not correlated with the existing levels of school segregation in 2015.

Under these assumptions, equation (4.1) secures the identification of the average effect of SAS for all school districts. To explore the heterogeneity in the effect of DA on segregation, below we extend expression (4.1) allowing for heterogeneous responses by residential segregation and the outside option value.

Residential segregation. School segregation could emerge as a result of residential segregation [Massey and Denton, 1985, Bonal et al., 2019]. As a first step towards breaking down the source of school segregation, we document the association between school and our measure of residential segregation for Chile. We use access to hospitals and police stations as our primary measure of residential segregation (see Section 4.3.1 for details on the construction of this measure). The spatial location of essential amenities such as hospitals or police stations does not depend on the new policy[12] We examine the impact of SAS for regions with varying levels of residential segregation for ninth graders using the expression:

$$
\begin{aligned}
y_{crt} \quad &= \delta_0 \times D_{rt} + \delta_1 \times D_{rt} \times \text{Residential Segregation}_c \\
&+ \delta_2 \times \text{Residential Segregation}_c + Z_{2cr}\beta + \gamma_r + \lambda_t + \epsilon_{crt},
\end{aligned} \tag{4.2}
$$

where residential segregation corresponds to the municipality level variation in access to amenities. From this expression, we explore the interplay of SAS and residential segregation using the interaction between the two variables. The coefficient of interest is δ_1. A positive δ_1 would imply that municipalities with higher residential segregation experienced an increase in school segregation due to the policy's implementation. The additional covariate Z_{2cr} is the measure of

[12] See Table **??** in Web Appendix for evidence suggesting no association between the new policy and the measure of residential segregation used in this paper. We also show below that there is no significant internal migration of students in response to the policy for targeting the schools of their choice.

pre-SAS local schooling structure in municipality c in region r.

Although δ_1 informs about the heterogeneity in SAS impact by residential segregation, we are also interested in learning about the main effect of residential segregation on school segregation, which is captured by δ_2 [Graham, 2018]. This since, as Figure 4.4 shows for Chile's two largest cities (Santiago and Concepción), there might exist extensive variation in the spatial location of amenities within metropolitan areas affecting the levels of school segregation. Furthermore, since Metropolitana did not have the new policy in place until 2019, from equation (4.2) we recover the effect of residential segregation in the absence of the new policy. Region and year fixed effects account for variation across municipalities within a region, warranting the identification of δ_2.

The outside option The new centralized student assignment system expanded school choices for Chilean families. However, under a schooling system characterized by heterogeneous quality and with private schools operating outside SAS, this might lead to complex changes in incentives and strategic behaviors [Hsieh and Urquiola, 2006, Gilraine et al., 2019]. Therefore, we explore pre-reform variation in local school supply across districts as a source of identification of systematic differences in parental responses to SAS.

We approach the heterogeneity by local schooling structure in two ways. First, we take advantage of the heterogenous distribution of school types across Chile's school districts. In particular, we construct the local fraction of school types in 2015 and estimate:

$$\begin{aligned} y_{crt} &= \delta_0 \times D_{rt} + \delta_{1p} \times D_{rt} \times \text{Public}_c + \delta_{1v} \times D_{rt} \times \text{Voucher}_c \\ &+ \delta_{2p} \times \text{Public}_c + \delta_{2v} \times \text{Voucher}_c + X_{crt}\beta + \gamma_r + \lambda_t + \epsilon_{crt}, \end{aligned} \tag{4.3}$$

where "Public$_c$" and "Voucher$_c$" correspond to the pre-reform percentages of public and voucher schools in the district c, respectively; and the coefficients of interest are consequently δ_{1p} and δ_{1v}. The baseline category for this specification is the percentage of private schools pre-reform. Notice that $\delta_{1p} < 0$ and $\delta_{1v} < 0$ would hint the plausibility that SAS enabled easier school switches for higher SES compared to low SES students as it could be easier for the former to overcome the constraints of school fees and transport costs. This unbalanced relocation would result in a disproportionate flight of non-poor students from public and voucher schools to private schools. Hence, municipalities with a higher fraction of private schools could witness an increase in school segregation due to SAS.

Secondly, we use a measure of access to private schools within the school district. This measure provides additional information on access to private schools that goes beyond the fraction of private schools. For example, there could be two school districts with the same fraction of private schools, but in district A the private schools are clustered in specific pockets, and in district B, they are more evenly distributed. Consequently, the standard deviation in travel time will be higher for A than B. The interaction of travel time with the new policy variable will suggest to what extent this spatial unevenness in access to outside options (private schools) impacts school segregation.

Thirdly, we examine how market concentration affects school segregation. To this end, we include HHI and its interaction with SAS (D_{rt}) as additional controls in equation (4.1). Any influence of market concentration on school segregation would indicate that the market structure can explain some of the policy variations.

Equations (4.1), (4.2) and (4.3) use the binary treatment status of each region under SAS. However, the new policy's impact could vary depending on the number of schools that participated in the new system. In other words, a binary indicator of policy veils these participation differences across municipalities. To account for such variation, we construct a treatment intensity variable using the information on the number of schools that participated in SAS every year within each region. Specifically, we define $I_{rt} \in [0,1]$ as the fraction of schools that participated in SAS in region r in period t. Thus, while the treatment dummy D_{rt} for region r is 1 if SAS was in place in year t, I_{rt} would be a continuous variable between [0,1]. Notice that a school participates if it has non zero vacancies for at least one school grade in year t, and the school accepts student applications for these vacancies. We complement our analysis with this intensity variable along with binary SAS dummy.

4.5 Main Results

We begin by exploring the overall impact of the new policy on school segregation. Table 4.7 presents the results for equation (4.1). The coefficient δ_0, which captures the average impact of the new policy (SAS dummy, D_{rt}), is small in magnitude (around -0.001) and statistically insignificant. The finding remains if we use treatment intensity (Intensity of SAS, I_{rt}) instead of the treatment dummy as the independent variable. The result is also robust to controlling for a rich set of other pre-SAS covariates.

Apart from SAS dummy (D_{rt}), it is interesting to examine the effect of other covariates on segregation reported under columns (3) and (4) of Table 4.7. We have decomposed the school market structure within a municipality into the pre-SAS fraction of public, voucher, and private schools and segregation can vary by school type (Figure 4.9). A higher fraction of public schools relative to private schools in a municipality drives down segregation. Additionally, voucher schools also affect segregation in the same negative direction, but the magnitude of the coefficient on the fraction of public schools is considerably higher than voucher schools. The primary reason behind this difference in coefficients is that although all public schools are free, some voucher schools might charge a small school fee. This small add on fee might act as a cost barrier for low SES students in voucher schools.[13]

The difference-in-differences parameter δ_0 identifies the average treatment effect across regions [Abadie, 2005]. Consequently, it is plausible that the increased segregation in some regions is offsetting the decrease in other regions. Therefore, we examine the heterogeneous effects next.

4.5.1 Heterogeneous effects

The previous results document small estimates for δ_0 in equation (4.1). In this Section, we disentangle systematic differences in the impact of the policy by residential segregation due to travel costs and social segregation, which is related to the concentration of individuals from different socioeconomic strata in specific pockets within a district.

First, we report the estimated coefficients for equation (4.2). The coefficient of interest

[13] As described in Section 4.2, the Inclusion Act included two additional provisions. Despite being implemented nationwide, they could threaten our identification strategy. However, official statistics suggest that restricting access to public subsidies to only not-for-profit voucher schools had a small impact on the industrial organization of the sector as less than 2% of the voucher schools in 2015 turned into private schools by 2018. On the other hand, the evidence presented in Web Appendix **??** suggests the absence of an interaction between the provision aimed at eliminating the co-payment and the sequential application of SAS. First, Figure **??** confirms that there was a decline in the number of voucher schools with co-payment immediately after the Inclusion Act went into effect in 2016. But the decline emerged early on across all regions and it stabilized post-2016. Consequently, the reduction in the number of schools with co-payment does not correlate with the year at which SAS was introduced in each region. Figure **??** repeats the analysis but for the evolution of average fees (co-payment levels) within school districts. As expected, there was an overall decline in school fees among voucher schools during this period, but there is no clear association with SAS. Table **??** confirms this result. It presents the estimated parameters of equation (4.1) but using average fees as the outcome of interest. Finally, Table **??** shows that the findings displayed in Table 4.7 are robust to restricting the sample to school districts where the fraction of voucher schools with co-payment is 0, less than 10% or less than 60%. Thus, the overall impact of SAS does not seem to be connected to or mediated by policy changes affecting co-payment.

corresponds to the interaction of treatment dummy and variation in access to amenities (δ_1). Table 4.8 presents these results. The estimated value for δ_1 is positive (0.008) and statistically significant, suggesting that municipalities with higher variance in the access to these public goods experienced an increase in school segregation due to the policy. This finding of a positive association between residential segregation and school segregation has been observed in other geographies as well. Rivkin [1994] finds that schools remained segregated due to the persistent residential segregation in the United States and Boterman [2019] reveals that most of the variation in school segregation in Netherlands is explained by existing residential segregation levels.

We also observe that school segregation levels at baseline (without the policy) are consistently higher in more segregated municipalities. This is consistent with the evidence for the United States, showing that the disadvantaged groups face higher constraints in accessing better quality neighborhoods. For instance, the clustering of minorities in specific neighborhoods often results in them having access to public schools overpopulated with minority students. In other words, the higher concentration of minorities in certain neighborhoods results in higher levels of school segregation in those areas [Massey et al., 1987].[14] All in all, income-based residential clustering can translate into higher segregation in school if families prefer to send their children to nearby schools, which is exactly the case in our context, as we showed in section 4.3.2.

We now turn to the empirical analysis of outside options proxied by the presence of private school providers. If high-income families respond by switching to private schools, then the centralized admission system does not necessarily reduce school segregation [Calsamiglia et al., 2020]. Such switches will be more prevalent in municipalities with a higher fraction of private

[14] For the main analysis, we use the data on access to hospitals. Results using policy stations as amenities are reported in Table **??** in the Web Appendix. The findings are robust to different types of amenities.

schools. We first present results supporting this hypothesis.

Table 4.9 reports the results from equation (4.3). Column (1) explores potential underlying factors driving this result. We interact the fraction of students attending each type of school within a municipality in 2015 with the policy dummy which is our variable of interest. The reference category is the fraction attending private schools. We report negative and significant interaction coefficients γ_{1p} and γ_{1v}, indicating that municipalities that had a higher fraction of public/voucher schools than private schools experienced a decrease in segregation due to the policy. We replicate the analysis in column (2) but with the fraction of each school type rather than the fraction of students attending different school types within a municipality. In this case, we conclude that municipalities with a higher fraction of private schools in 2015 became more segregated due to the new admission system.

Column (3), on the other hand, examines the impact of private schools on segregation using the dispersion in travel time to private schools within the school district. The coefficient on the interaction of the standard deviation in travel time to private schools and SAS dummy is positive. This provides further support to our results that differential access to private schools drives up school segregation in the presence of the new policy. Lastly, the fourth column reports the results on market concentration (HHI index), suggesting that municipalities with higher existing levels of market concentration experienced an increase in Duncan index due to the policy.

Additionally, the findings in Table 4.9 show that the pre-existing schooling structure also impacts the levels of school segregation without the new policy (baseline). The main effects for a higher fraction of public and voucher schools are consistently positive and significant across all specifications. The factors driving these heterogeneous effects requires us to explore the response of high versus low SES families to the new policy.

Switches to private schools. We also investigate whether differential responses by household characteristics can explain why SAS pushed segregation upwards in regions with a high market concentration of private schools. School choice literature has shown that parents care about the SES make-up of the school when choosing schools.[15] Such parental affinity could have resulted in high-income families switching their children from public/voucher schools to private schools if they do not like the final allocation.

To this end, we first construct the fraction of students switching from public/voucher to private schools between eighth and ninth grade for consecutive cohorts from 2015 to 2019. We anticipate that municipalities which had a higher fraction of private will make this transition easier. Additionally, we intend to understand what kind of families are more likely to make this transition. The outcome variable for this regression is the proportion of families that switched from public/voucher to private schools. We capture if these switches were more accessible for high-income parents in districts with a higher representation of private schools using a triple interaction between SAS dummy, the fraction of high SES families, and the presence of private schools in 2015. A positive coefficient on this triple interaction indicates that high SES students responded to the policy by moving to private schools from public/voucher schools.

Table 4.10 presents this result. The coefficient on the triple interaction on SAS dummy, private school dummy in 2015, and fraction of high SES mothers within a municipality is positive and statistically significant. This finding indicates that higher SES families responded to the new

[15] For instance, Hastings et al. [2009] exhibit that parents value the school's racial composition when reporting their top three school choices. An analysis of parental preferences under centralized allocation in Paris also reveals parental inclination to send their children to schools that had a higher representation of students belonging to the same SES [Fack et al., 2019]. Baum-Snow and Lutz [2011] highlight the importance of investigating heterogeneous responses across racial groups for exposing some of the unintended consequences of the desegregation laws in the 1960s and 1970s in the United States (out-migration of minority students cannot invalidate the hypothesis that whites used private schools to avoid desegregation in the south).

policy by switching to private schools. Consequently, we observe an increase in segregation in municipalities with a higher fraction of private schools in 2015.

This examination helps us unmask a critical underlying mechanism that is driving the mixed effects of the new policy on school segregation in Chile. High SES parents seem to have anticipated an increase in the representation of low SES students due to the policy. Consequently, they responded by switching to expensive private schools. Hence, we depict that these pre-existing variations on the supply and price of different types of schools matter across municipalities.

4.5.2 Threats to identification

In what follows, we perform a series of tests examining the common pre-trends assumption. Additionally, we examine the plausibility of any agent responses in anticipation of the policy. Such responses could make difficult to disentangle the changes in segregation due to SAS.

A critical characteristic of DA implementation in Chile was that it was scaled up gradually across regions. We have a panel of school districts from 2015 to 2019. By 2016 students in only one small region had participated in DA, and by 2017 implementation happened in four additional regions. Consequently, for the majority of regions (10 out of 15), we observe multiple years of pre-treatment data. We perform diverse robustness and placebo tests to alleviate any concerns on the violation of common trends assumption in this generalized Differences-in-Differences set-up with variation in treatment timing.

The first identification assumption for the parameter of interest is that we expect to observe parallel trends in baseline outcomes denoted by $Y_{crt}^{baseline}$ across all groups. In other words, it implies that all groups should have followed a parallel trend in school segregation in the absence of treatment. The year and region-specific effects could explain these constant gaps in segregation across groups.

Since SAS's adoption was staggered over time, standard parallel trends visualization will not suffice as regions are switching their treatment status over time. We tackle this issue by providing formal tests to support the parallel trend assumption for our context with multiple regions and treatment time variation. First, we check for differences in pre-trends using a leads test. Any evidence of significant coefficients for the lead treatment indicators is a threat to the parallel trend assumption). To this end, we estimate:

$$y_{crt} = \gamma_r + \lambda_t + \sum_{j=0}^{q} \delta_j \times D_{rt+j} + e_{crt}.$$

Here we include the treatment indicator for q years after the implementation of policy for the leads test. Figure 4.10 summarizes the result of this analysis when q is set to one. The coefficient for the lead term is not statistically different from zero. In other words, any policy change in the future does not affect the prevailing school segregation. We also perform a similar test with a one year lag (D_{rt-1}) and find (again) no statistically significant effect.[16]

Second, we present an additional test for the pre-trends assumption using random assignment into treatment. We allocate the 15 regions into treatment using a random number generator for the implementation date of the treatment. In Table 4.11 shows that we do not find significant results for the overall effect as well as the heterogeneous effect.

Given the sequential implementation of the policy, we can visualize a modified version of the parallel trend assumption. We can split the regions into groups based on their year of treatment

[16]Results available upon request.

and then compare the early, late intervention and non-treated groups. The critical thing to note is that early and late treatment groups change their treatment status at different points between 2016 and 2019. This is the crucial point of divergence from a standard Differences-in-Difference set-up where all entities in the treatment group are treated at the same time. In our context, Metropolitana region is not treated till the end of 2018, and consequently, it serves as a control for all other treated regions in every period. Figure 4.11 depicts the parallel trend assumption using regions treated in 2018 and 2019 and the untreated group. This conclusion, combined with the other results, alleviates any concerns on the violation of equal pre-trends assumption.

Lastly, we follow the region-specific trend test from Angrist and Pischke [2008] to provide further evidence to aid our assumption on pre-trends. Table 4.12 presents these results. The comparison of its columns (1) and (2) in Panel (A) reveals that the inclusion of these additional variables does not alter the coefficient on the treatment dummy.

In addition to the average treatment effect, we perform the above pre-trends test for the specifications with heterogeneous effects. A change in the coefficients for the interaction terms in the heterogeneous effect specification once we incorporate the region specific trend variables would indicate a threat to identification. Table 4.13 shows that our key results on residential segregation and local school structure remain unchanged even when we add the region specific trend variables.

On the other hand, the identification of δ_0 requires the policy adoption date to be random, and rules out any strategic responses by the parents in anticipation of the policy (see Section 4.4 for further details). The algorithm takes into account residential proximity to school if it fails to allocate a student to any of the listed preferences. We want to provide evidence against any plausibility of parents relocating to different neighborhoods for a better school outcome before the policy implementation.

We take advantage of the student addresses for the complete universe of ninth graders for this analysis. We compare student addresses between 2017 and 2018 for nine regions. SAS was in place in these nine regions in 2018. Consequently, a high fraction of internal migration within these regions will be a threat to the identification strategy. We calculated the fraction of eighth-grade students who did not change their residence between 2017 and 2018. We compared these fractions with the average for the prior years to identify any threats to the identification strategy.

Figure 4.12 suggests that migration is very limited in these regions. Moreover, we do not observe any pattern of abruptly higher migration between 2017 and 2018. The fractions of families who do not change their residences in 2018 are comparable to the numbers computed for 2017. Most of these averages are well above 95% of the total population of students. Therefore, we conclude that parents are not responding to the policy announcement by changing their addresses.

A final concern is the potential correlation between the policy adoption date and the existing levels of school segregation in a region. Table **??** in Web Appendix addresses this issue. It displays the results from a linear regression of year of implementation on pre-SAS segregation. The evidence indicates no statistical association between the two variables.

4.5.3 Robustness Checks

Alternative definitions of socioeconomic status. We have used mother's education to define low and high SES students for the central part of our analysis. However, the SIMCE files also report father's education and a family income index. We replicate our main results using these alternative proxies for student SES.

Panel (A) of Table 4.14 mimics the analysis of Table 4.7 but using father's education to define the Duncan Index (as before a high school degree separates low and high SES groups). We reach the same conclusion. Overall, SAS does not affect segregation, and this result is not affected by the inclusion of covariates.

We repeat the analysis using the household income index. These results are displayed in panel (B) of Table 4.14. We categorize all students whose household income is less than 100,000 Chilean pesos as low SES and others with higher income as high SES. Again, we get no overall effect of SAS on segregation across all the specifications. These two exercises confirm the robustnesses of our findings.

Urban vs. rural areas. Urban and rural areas are likely to have different composition of school types. Public schools have a better reach in terms of geography and affordability than their private counterparts. Consequently, rural areas have a much higher fraction of public schools than urban areas.

To ensure that some of these rural-urban differences do not drive the principal conclusions, we replicate the central results solely for municipalities with urban schools. These results are reported in Table 4.15. We again get marginally positive estimates of the policy parameter δ_0, where the estimate is statistically insignificant across all specifications.

Provinces as school districts. From the analysis of Table 4.2, we conclude that around two-thirds of the students choose a high school in the same municipality as their residence. Since we compute the dissimilarity index at the municipality level, we implicitly assume that students are mostly choosing schools within the same municipality of residence. Alternatively, it implies that schools are catering to the students in the same municipality as their location. However, this assumption is not valid for one-third of the sample. Consequently, we proceed to confirm that our results are robust to the definition of the relevant market. To this end, we re-evaluate our main findings but this time using provinces, legally defined as a set of municipalities, as school districts. Panel (B) in Table 4.2 shows that 90% of ninth graders attend schools in the same province as their province of residence.

Table 4.16 displays the estimated impact of SAS on school segregation using this approach (there are a total of 54 provinces, so we get 270 observations for five years of data). This aggregate-level regression suggests qualitatively similar results to what we observed for the municipality level analysis. The policy parameter for the overall sample is marginally negative. However, the policy effect is not statistically significant.

Overall, we find that the differences in the definition of the schooling market, computation of Duncan index based on other socioeconomic indicators, and rural-urban differences do not make any substantial changes to the results. However, one concern that we did not address through the above checks is whether the results are robust to alternative socioeconomic disparity measures. We address this issue next.

Alternative segregation measure. Using the Duncan index as the primary outcome variable makes our analysis easy to interpret for policy recommendations and comparable to existing studies in the literature. However, as discussed above, Duncan index has some shortcomings. In this Section, we introduce and discuss an alternative measure of segregation (M index) as it overcomes some of the Duncan index's limitations and helps us ensure that our key results are robust to a different measure.

The Mutual Information index (M index) was first introduced by Theil [1971] and developed in Frankel and Volij [2011]. This index is based on the concept of diversity. In our school segregation setting, it compares the representation of students from different socioeconomic backgrounds in schools with the overall levels of diversity in a region.

We first introduce the required notation. Let r denote a specific region (there are fifteen regions in Chile). For sake of simplicity, we assume the existence of S schools in every region. Of course, the empirical analysis relaxes this assumption allowing the number of schools (and municipalities) to differ across regions. Every student belongs to either a low or high SES group. Let N be the number of students residing in region r. For the sake of notation clarity, we omit the subindex r. Let N_1 and N_2 be the number of low SES students and high SES students, respectively ($N = N_1 + N_2$). Thus, the M index for school segregation is defined as:

$$M = \frac{N_1}{N} \ln\left(\frac{N}{N_1}\right) + \frac{N_2}{N} \ln\left(\frac{N}{N_2}\right) - \sum_{j=1}^{S} \frac{n_j}{N} \left[\frac{n_j^1}{n_j} \ln\left(\frac{n_j}{n_j^1}\right) + \frac{n_j^2}{n_j} \ln\left(\frac{n_j}{n_j^2}\right)\right],$$

where n_j is the fraction of students in school $j \in \{1, ..., S\}$. n_j^1, n_j^2 denote the number of low and high SES students in school j, respectively.

One interesting feature of this index is that it can be decomposed into between and within components, i.e., $M = I_b + I_w$. The first component I_b measures the extent of segregation across municipalities in a region, while the second component, I_w, measures the within municipality school segregation.

Note that the within component measures the extent of school segregation in a municipality, which is comparable to the Duncan index analyzed so far in the paper. We first replicate our key findings using the within component of the M index. Columns (1) and (2) in Table 4.17 provide results for equation (4.2) and (4.3), respectively. We confirm that the increase in school segregation happened in municipalities with more residential segregation and with a higher fraction of private schools in 2015. This ensures that our results are robust to this other measure of segregation.

In addition, we can use the M index to understand the source of variation helping us identify the policy implications. Panel (A) in Figure 4.13 shows the decomposition of the M index of school segregation into within and between municipality components for 2019. The graph suggests that the within component is the primary source of school segregation across regions. The M index varied between [0.07,0.14] across regions in 2019, and the extent of variation in the within component was [0.06,0.11]. On the other hand, the magnitude of between component variation was much lower ([0.00,0.04]).

Finally, Figure 4.13 also illustrates that there is substantial heterogeneity across regions in Chile. For example, in Magallanes, which was the first region that adopted the new policy in 2016, the M index showed a slight drop between 2016 and 2017, but it has sharply risen since then. The rise in within component pushed up the M index and the between component decreased between 2017 and 2019. Another compelling case is Los Rios (panel (C)), which adopted the program in 2018. In this region, the M index declined marginally between 2018 and 2019, and most of this decline came through the within component. Some of this drop was undone by the slight uptick in the between component. Such variation is also seen for other regions such as Valparaiso (Panel (D)).

4.6 Conclusion

In 2016 Chile adopted a Deferred Acceptance algorithm (SAS) to optimize the complex assignment of students to public and voucher schools. We contribute to the literature by shedding light on the impact of this new centralized school admission system on school segregation.

Despite the fact the Chilean government aimed this policy at advancing the representation of low SES students across schools, our findings do not suggest an overall improvement in the evenness of the student background distribution. We confirm this using multiple indicators. Interestingly, our results do indicate heterogeneous effects by levels of pre-existing residential segregation and local school supply. In particular, we document that school districts that had high (low) residential segregation experienced an increase (decline) in school stratification post the implementation of the new system. Beyond residential segregation, we find regions with a pre-SAS higher fraction of private schools have also seen an uptick in segregation due to SAS. These findings highlight the relevance of spatial distribution of schools across local districts as they might strategically locate to target specific groups.

We also provide evidence on a potential mechanism explaining our findings. If high SES parents anticipated a more integrated school as a result of SAS, heterogeneous preferences for school socioeconomic composition could have led to strategic responses, with some families switching to private schools. We present evidence supporting these transitions. Indeed, higher SES families responded to the new policy by out-migrating from public and voucher schools. SAS's unintended consequence explains why school districts that had a lower pre-SAS proportion of public or voucher schools witnessed more of this transition to private schools.

We conclude that the ultimate impact of centralized school admission systems depends on how the parents choose schools, which in turn depends on their location, and pre-existing characteristics of their neighborhoods. Any policy prescription that aims to improve the extent of diversity within schools requires taking into account the school district's features for desired results.

Tables and Figures

Table 4.1: Student and school participation in SAS, national average

Year	All Students	All Schools	Ninth-grade students (%)
	(1)	(2)	(3)
2016	3436	63	46.2
2017	76821	2172	51.2
2018	274990	6421	52.4

Notes: Column (1) and (2) in panel A provide total number of students and schools that participated in SAS across all grades. Column (3) corresponds to the percentage of ninth-grade students participating in SAS. SAS implementation was sequential, therefore, every subsequent year, new regions were added.

Table 4.2: Percentage of students choosing schools in their municipality/province of residence, national level

Year	Students attending school in municipality/province of residence (N^*)	Total number of students (N)	% ($\frac{N^*}{N}$)
	(1)	(2)	(3)
	A. Municipality (School district)		
All students			
2015	2772746	3548845	78.1
2016	2758348	3550949	77.7
2017	2750364	3558394	77.3
2018	2759966	3582448	77.0
2019	2774174	3611057	76.8
Ninth graders			
2015	182643	265093	68.9
2016	176889	259037	68.3
2017	174451	255400	68.3
2018	167782	246937	67.9
2019	166001	246115	67.4
	B. Province		
All students			
2015	3289121	3548845	92.7
2016	3278951	3550949	92.3
2017	3273885	3558394	92.0
2018	3286719	3582448	91.8
2019	3301120	3611057	91.4
Ninth graders			
2015	242349	265093	91.4
2016	235396	259037	90.9
2017	230838	255400	90.4
2018	222144	246937	90.0
2019	219723	246115	89.3

Note: We match the resident municipality of each student with the municipality of their school using the enrollment files (panel A). Similarly, matching of student residential province and school province is done in panel B. Column (3) displays the percentage of students attending school in their municipality/province of residence.

Table 4.3: Composition and distribution of high school types at the national level

Variable	Number of schools	Percentage of schools/students
	A. **Supply of schools, by type**	
Public	915	30.9
Voucher	1614	54.5
Private	433	14.6
Total	2962	100
	B. **Student enrollment, by school type**	
Public	100793	40.8
Voucher	124295	50.3
Private	21849	8.8
Total	246937	100

Note: The sample for this table consists of all schools offering high school education and all ninth-grade students enrolled in high school in 2018.

Table 4.4: Rank Ordered List (ROL) and Private school supply

VARIABLES	Dependent Variable: Ratio of ROL and total school supply in SAS (1)
% enrollment in Private Schools	0.003
	[0.059]
Mother's Education	0.004***
	[0.001]
% enrollment in Private Schools×Mother's Education	-0.017***
	[0.005]
Constant	0.079***
	[0.006]
Observations	14,926
R-squared	0.008

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table uses the data at the student level seeking admission in ninth grade for five regions that participated in SAS in 2017. The table presents the results obtained from the following regression:

$$y_{ic} = \beta_0 + \beta_1 \text{Private supply}_c + \beta_2 \text{Mother's Education}_i + \beta_3 \text{Mother's Education}_i \times \text{Private Supply}_c + \eta_{ic},$$

where the outcome variable is the ratio of ROL of student i to total school supply in DA in municipality c (this includes public and voucher schools). This equation illustrates the relationship between ROL and private school supply. The coefficient on the interaction term suggests that once we condition on student SES (Mother's Education), higher private school supply is negatively associated with the ratio of ROL to total schools in DA.

Table 4.5: Participation in SAS and Private school supply

VARIABLES	Dependent Variable: % Participated in SAS (1)
Private	-0.529*
	[0.288]
Voucher	-0.077
	[0.067]
Constant	0.580***
	[0.029]
Observations	86
R-squared	0.066
Covariates	y

Note: *** p<0.01, ** p<0.05, * p<0.1. This table uses municipality-level data for five regions that participated in SAS in 2017. We regress the fraction of DA participants in each municipality on the fractions of private and voucher schools. The reference dummy is fraction of public schools. Thus, the table presents the estimates obtained from the following regression:

$$y_c = \beta_0 + \beta_1 \text{Private supply}_c + \beta_2 \text{Voucher supply}_c + \eta_{ic},$$

where the outcome variable of interest is the fraction of students participating in DA in municipality c. The sample was constructed using the information from ninth graders in 2018. $\beta_1 < 0$ (also statistically significant) provides reduced form evidence that the presence of private schools reduces the participation in DA. In other words, municipalities that had a higher supply of private schools relative to baseline category of public schools had fewer families participating in DA.

Table 4.6: Descriptive Statistics

Variables	N	Mean	Std. dev.	Min.	Max.
School segregation in municipality (Duncan index)	1,646	0.25	0.19	0.00	0.82
SAS dummy (D_{rt})	1,646	0.22	0.42	0.00	1.00
Intensity of SAS (I_{rt})	1,646	0.19	0.36	0.00	0.96
Residential segregation (access to amenities)	1,623	2.29	5.54	0.00	55.25
% of enrollment in public schools in municipality pre-SAS	1,646	0.60	0.26	0.05	1.00
% of enrollment in voucher schools in municipality pre-SAS	1,646	0.37	0.24	0.00	0.95
% of enrollment in private schools in municipality pre-SAS	1,646	0.03	0.10	0.00	0.81
% of public schools in municipality pre-SAS	1,646	0.61	0.26	0.06	1.00
% of voucher schools in municipality pre-SAS	1,646	0.37	0.24	0.00	0.94
% of private schools in municipality pre-SAS	1,646	0.03	0.09	0.00	0.79
Herfindahl Index pre-SAS	1,646	0.15	0.16	0.01	1.00

Notes: The sample size in our analysis corresponds to 327 municipalities that have Duncan index for all 5 years. Since Chile has 345 municipalities, for only 18 there is missing student-level information in SIMCE files preventing the construction of the Duncan index for one or more years. Hence, we have 1646 observations for the Duncan index panel. When it comes to residential segregation, we lose 23 observations due to missing travel duration data to the amenities. The missing travel data is due to the quality of geocoding of student addresses in some of the rural municipalities where the HERE geocoding API was not able to locate student addresses. The Duncan index measures school segregation at the level of the municipality. SAS dummy takes a value 1 if SAS was implemented in region r in year t and 0 otherwise. Intensity of SAS $\in [0, 1]$ depends on the fraction of schools in region r that participated in SAS in year t. Pre-SAS corresponds to the year 2015. Percentage (%) of enrollment in public (voucher) pre-SAS corresponds to the fraction of students attending a public (voucher) school in each municipality in 2015. Percentage (%) supply of public (voucher) pre-SAS corresponds to the proportion of public (voucher) schools in each municipality in 2015.

Table 4.7: The impact of SAS on School Segregation
Difference-in-differences estimates

	Dependent Variable: Duncan index			
VARIABLES	(1)	(2)	(3)	(4)
SAS dummy (D_{rt})	-0.001	-0.000		
	[0.006]	[0.006]		
% enrollment in public pre-SAS		-1.051***		-1.051***
		[0.073]		[0.073]
% enrollment in voucher pre-SAS		-0.564***		-0.564***
		[0.079]		[0.079]
Intensity of SAS (I_{rt})			-0.001	-0.000
			[0.007]	[0.007]
Constant	0.109*	1.033***	0.109*	1.033***
	[0.064]	[0.082]	[0.064]	[0.082]
Observations	1,646	1,646	1,646	1,646
R-squared	0.141	0.603	0.141	0.603
Region FE	y	y	y	y
Year FE	y	y	y	y
Additional covariates	n	y	n	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. Column (1) and (2) use SAS dummy as the treatment variable, while column (3) and (4) use intensity as treatment variable.

Table 4.8: The impact of SAS on School Segregation: The role of Residential Segregation
Difference-in-differences estimates

	Dependent Variable: Duncan index	
VARIABLES	(1)	(2)
SAS dummy (D_{rt})	-0.012	-0.013
	[0.008]	[0.008]
Residential Segregation	0.002*	0.002***
	[0.001]	[0.001]
SAS dummy (D_{rt}) $\times$ Residential Segregation	0.008*	0.008*
	[0.004]	[0.004]
Constant	1.039***	1.046***
	[0.083]	[0.080]
Observations	1,623	1,623
R-squared	0.598	0.612
Region FE	y	y
Year FE	y	y
Covariates	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In specification (1) we also control for the pre-SAS percentage of students enrolled in public and voucher schools. In specification (2) we add pre-SAS fraction of students in rural schools.

Table 4.9: The impact of SAS on School Segregation: The role of Outside Options
Difference-in-differences estimates

	Dependent Variable: Duncan index			
VARIABLES	(1)	(2)	(3)	(4)
SAS dummy (D_{rt})	0.372***	0.610**	-0.002	-0.015
	[0.137]	[0.255]	[0.006]	[0.010]
% of enrollment in public pre-SAS	-1.053***			
	[0.072]			
SAS dummy (D_{rt})× % of enrollment in public pre-SAS	-0.373***			
	[0.136]			
% of enrollment in voucher pre-SAS	-0.557***			
	[0.077]			
SAS dummy (D_{rt})× % of enrollment in voucher pre-SAS	-0.389***			
	[0.148]			
% of public schools pre-SAS		-1.029***		
		[0.087]		
SAS dummy (D_{rt})×% of public schools pre-SAS		-0.601**		
		[0.253]		
% of voucher schools pre-SAS		-0.569***		
		[0.092]		
SAS dummy (D_{rt}) × % of voucher schools pre-SAS		-0.656**		
		[0.274]		
Travel time to private (std dev.)			0.053	
			[0.045]	
SAS dummy (D_{rt})×Travel time to private (std dev.)			0.034*	
			[0.018]	
Herfindahl Index				-0.744***
				[0.066]
SAS dummy (D_{rt})× Herfindahl Index				0.080*
				[0.042]
Constant	1.082***	1.077***	0.992***	0.367***
	[0.071]	[0.086]	[0.094]	[0.012]
Observations	1,623	1,623	1,623	1,623
R-squared	0.599	0.501	0.534	0.403
Region FE	y	y	y	y
Year FE	y	y	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In all specifications residential segregation is also included as an additional covariate. Column (1): Percentage (%) of enrollment in public (voucher) pre-SAS corresponds to the fraction of students attending a public (voucher) school in each municipality in 2015. Column (2): Percentage (%) supply of public (voucher) pre-SAS corresponds to the proportion of public (voucher) schools in each municipality in 2015. Column (3) also includes the composition of local school structure in 2015 as additional covariates.

Table 4.10: Students switching from public/voucher to private schools as a response to SAS
Difference-in-differences estimates

VARIABLES	Dependent Variable: % of switchers
SAS dummy (D_{rt})	0.004**
	[0.002]
Educ mother $>=$ 12	0.010***
	[0.003]
SAS dummy (D_{rt})$\times$ [Educ mother $>=$ 12]	-0.004
	[0.004]
Private dummy (pre-SAS)	-0.009**
	[0.004]
Private dummy (pre-SAS) $\times$[Mother educ. $>=$ 12]	0.015**
	[0.006]
SAS dummy (D_{rt}) $\times$Private dummy (pre-SAS)	-0.041*
	[0.025]
SAS dummy (D_{rt}) $\times$Private dummy (pre-SAS)$\times$ [Educ mother $>=$ 12]	0.068*
	[0.038]
Constant	-0.004**
	[0.002]
Observations	**1,712**
R-squared	0.179
Region FE	y
Year FE	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. The outcome variable used here (transition) is the fraction of students that switched from public/voucher to private school in a municipality. Private dummy pre-SAS takes a value 1 for municipalities which had at least one private school previous to the reform, and 0 otherwise. Educ mother dummy takes a value 1 if student's mother has a high school degree, and 0 otherwise.

Table 4.11: Test for pre-trends: Random assignment into treatment

	Duncan index			
VARIABLES	(1)	(2)	(3)	(4)
SAS dummy (D_{rt})	-0.002		-0.008	0.087
	[0.006]		[0.007]	[0.081]
% of enrollment in public pre-SAS				-1.001***
				[0.087]
SAS dummy (D_{rt})$\times$ % of enrollment in public pre-SAS				-0.100
				[0.080]
% of enrollment in voucher pre-SAS				-0.571***
				[0.094]
SAS dummy (D_{rt})$\times$ % of enrollment in voucher pre-SAS				-0.070
				[0.090]
Residential Segregation			-0.005**	
			[0.002]	
SAS dummy (D_{rt})$\times$Residential Segregation			0.003	
			[0.002]	
Intensity of SAS I_{rt}		-0.001		
		[0.007]		
Constant	0.111*	0.109*	0.135*	0.991***
	[0.064]	[0.064]	[0.073]	[0.092]
Observations	1,646	1,646	1,623	1,646
R-squared	0.141	0.141	0.156	0.506
Region FE	y	y	y	y
Year FE	y	y	y	y
Additional covariates	n	y	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. For this test we randomly allocate regions into treatment using a random generator with replacement.

Table 4.12: Test for pre-trends with region specific trend variables

	Duncan index	
VARIABLES	(1)	(2)
SAS dummy (D_{rt})	-0.001	-0.001
	[0.006]	[0.007]
Constant	1.020***	1.011***
	[0.095]	[0.096]
Observations	1,646	1,646
R-squared	0.535	0.536
Region FE	y	y
Year FE	y	y
Set of covariates	y	y
State specific trend	n	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In panel (A) we perform the formal test for parallel trends using state specific trend variable.

Table 4.13: Test for pre-trends with region specific trend variables: Heterogeneous effects

	Duncan index	
VARIABLES	(1)	(2)
SAS dummy (D_{rt})	-0.013	0.391***
	[0.009]	[0.139]
% of enrollment in public schools pre-SAS		-1.052***
		[0.072]
SAS dummy (D_{rt}) $\times$% of enrollment in public schools pre-SAS		-0.391***
		[0.137]
% of enrollment in voucher schools pre-SAS		-0.556***
		[0.077]
SAS dummy (D_{rt}) $\times$ % of enrollment in voucher schools pre-SAS		-0.409***
		[0.150]
Residential Segregation	0.002*	
	[0.001]	
SAS dummy (D_{rt})$\times$ Residential Segregation	0.008*	
	[0.004]	
Constant	1.028***	1.015***
	[0.086]	[0.086]
Observations	1,623	1,623
R-squared	0.599	0.600
Region FE	y	y
Year FE	y	y
complete set of covariates	y	y
State specific trend	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In panel (A) and (B) we perform the formal test for parallel trends using state specific trend variable for the specifications with heterogeneous effects.

Table 4.14: Alternative socioeconomic backgrounds when defining the Duncan Index
Father's education and family income
Difference-in-differences estimates

	Duncan index			
VARIABLES	(1)	(2)	(3)	(4)
	A: **Father's education**			
SAS dummy (D_{rt})	0.000		0.001	
	[0.006]		[0.006]	
Intensity of SAS (I_{rt})		0.001		0.002
		[0.007]		[0.007]
Constant	0.096	0.096	1.072***	1.072***
	[0.060]	[0.060]	[0.085]	[0.085]
Observations	1,646	1,646	1,646	1,646
R-squared	0.133	0.133	0.609	0.609
Region FE	y	y	y	y
Year FE	y	y	y	y
Additional covariates	n	n	y	y
	B: **Household Income Index**			
SAS dummy (D_{rt})	-0.004		-0.004	
	[0.011]		[0.010]	
Intensity of SAS (I_{rt})		-0.004		-0.003
		[0.012]		[0.012]
Constant	0.115***	0.115***	0.930***	0.930***
	[0.005]	[0.005]	[0.064]	[0.064]
Observations	1,646	1,646	1,646	1,646
R-squared	0.182	0.182	0.561	0.561
Region FE	y	y	y	y
Year FE	y	y	y	y
Additional covariates	n	n	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In panel (A) we use father's education to construct the Duncan index, while in panel (B) it is computed using household income. In columns (1) and (3) we employ SAS dummy as the treatment variable, and in columns (2) and (4) we use intensity of SAS. Columns (3) and (4) also include additional covariates such as the pre-SAS fraction of public, voucher and private schools in a municipality.

Table 4.15: The impact of SAS on School Segregation
Sample of school districts with urban schools only
Difference-in-differences estimates

	Duncan index			
VARIABLES	(1)	(2)	(3)	(4)
SAS dummy (D_{rt})	0.003		0.004	
	[0.010]		[0.012]	
Intensity of SAS (I_{rt})		0.006		0.006
		[0.014]		[0.014]
Constant	0.407***	0.407***	1.167***	1.167***
	[0.005]	[0.046]	[0.087]	[0.087]
Observations	224	224	224	224
R-squared	0.304	0.304	0.778	0.778
Region FE	y	y	y	y
Year FE	y	y	y	y
Additional covariates	n	n	y	y

Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In columns (1) and (3) we employ SAS dummy as the treatment variable, and in columns (2) and (4) we use intensity of SAS. Columns (3) and (4) also include additional covariates such as the pre-SAS fraction of public, voucher and private schools in a municipality.

Table 4.16: The impact of SAS on School Segregation using Province as School District
Difference-in-differences estimates

VARIABLES	(1) SAS Dummy	(2) SAS Intensity
SAS dummy (D_{rt})	-0.006	
	[0.006]	
SAS treatment intensity (I_{rt})		-0.007
		[0.006]
Constant	0.288***	0.288***
	[0.108]	[0.108]
Observations	269	269
R-squared	0.348	0.348
Region FE	y	y
Year FE	y	y

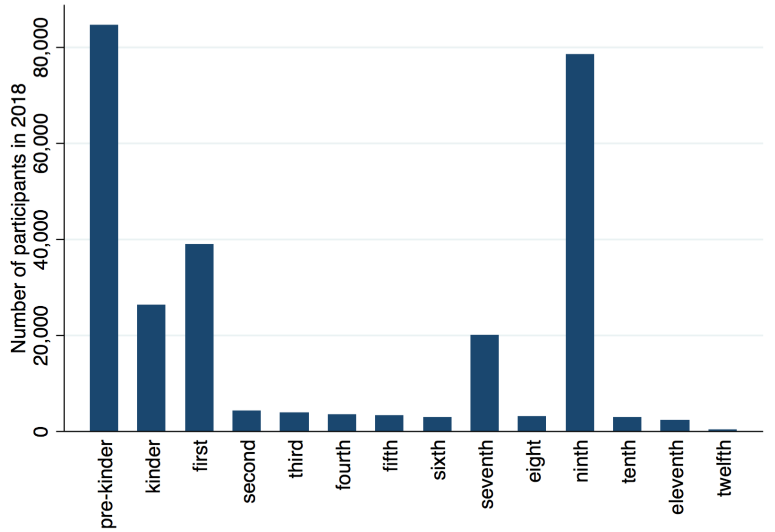
Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors in square brackets. As the Duncan index for this robustness check is constructed at the province level instead of municipality, standard errors are clustered at province. In column (1) we employ SAS dummy as the treatment variable, and in column (2) we use intensity of SAS.

Table 4.17: Alternative definition of School Segregation: The M index (within component)
Difference-in-differences estimates

VARIABLES	(1)	(2)
SAS dummy (D_{rt})	-0.007*	0.251***
	[0.004]	[0.068]
% of enrollment in public pre-SAS		-0.206***
		[0.054]
SAS dummy (D_{rt})×% of enrollment in public pre-SAS		-0.248***
		[0.068]
% of enrollment in voucher pre-SAS		-0.078
		[0.059]
SAS dummy (D_{rt})×% of enrollment in voucher pre-SAS		-0.261***
		[0.072]
Residential segregation	-0.001	
	[0.001]	
SAS dummy (D_{rt})×Residential segregation	0.002***	
	[0.001]	
Constant	0.030**	0.210***
	[0.015]	[0.055]
Observations	1,623	1,646
R-squared	0.094	0.433
Region FE	y	y
Year FE	y	y
Covariates	n	n

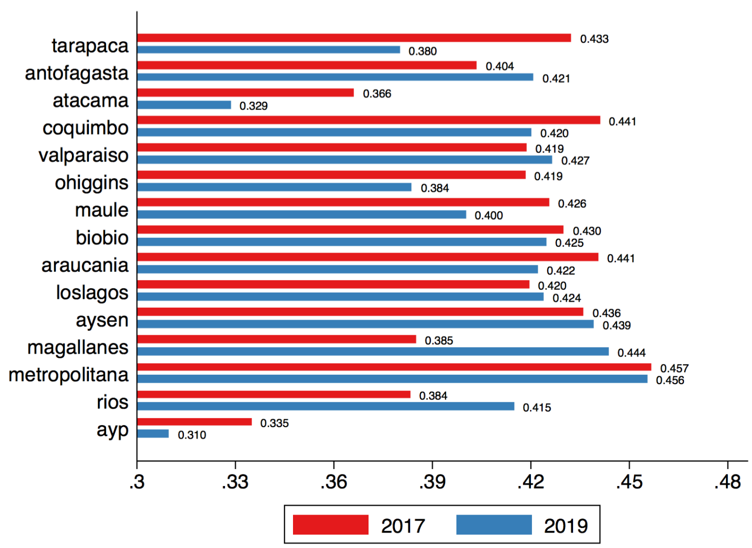
Notes: ***p<0.01, ** p<0.05,* p<0.1. Standard errors clustered at municipality in square brackets. In column (1) we perform the heterogeneous effect using residential segregation. In column (2) we do the same using the local pre-SAS school structure.

Figure 4.1: Number of Students participating in SAS across grades (2018)



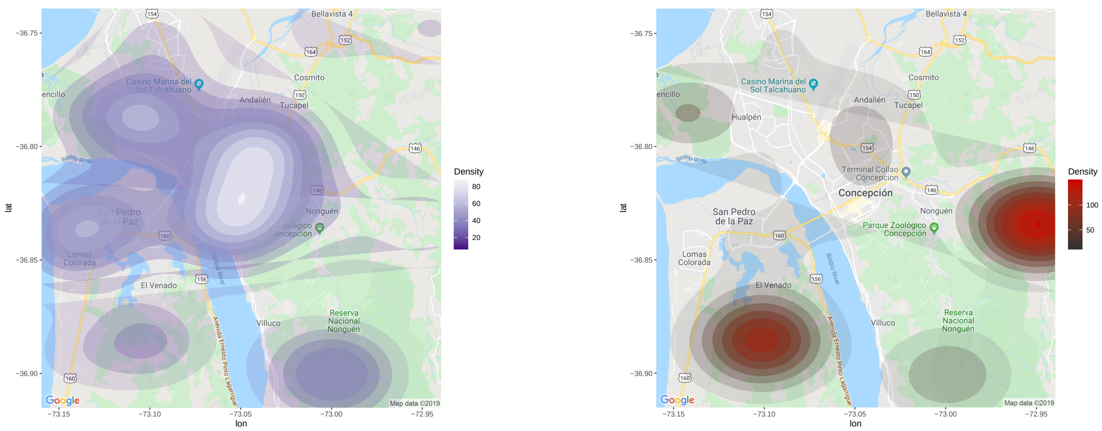
Notes: By 2018, all regions in Chile except Metropolitana had the new policy for school admission process. We observe that most participants apply to pre-k and ninth grade admissions.

Figure 4.2: Duncan index: 2017 versus 2019, by region



Notes: The only region that had the new policy implemented in 2017 was Magallanes. The new system was implemented in all other regions by 2018, except Metropolitana.

Figure 4.3: Spatial density plots of low and high SES students: The Biobio region

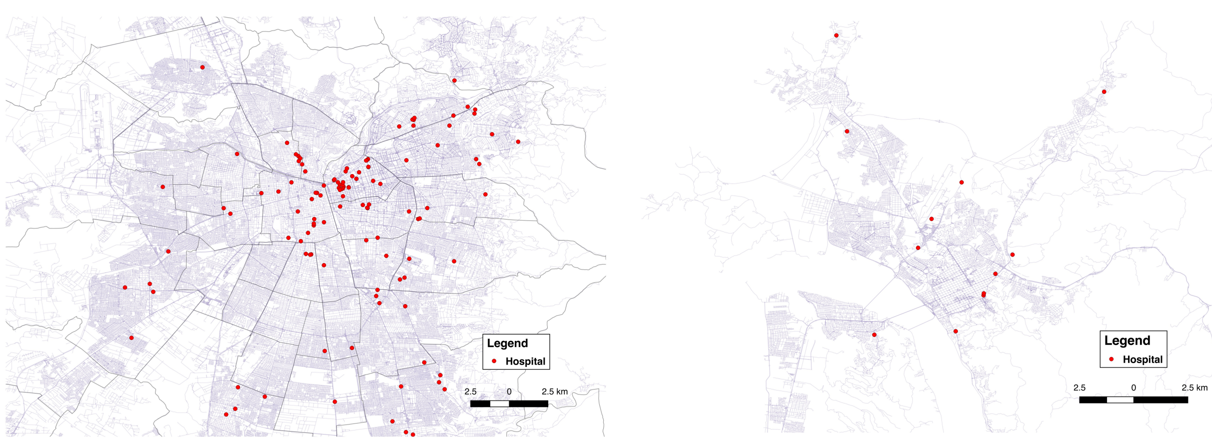


(a) Students from low income families

(b) Students from high income families

Notes: We plot the contour densities for low and high income students in Biobio region. The concentration can be seen using the color gradient for the densities shown with each graph. For instance, high income families have higher concentration in east and south west as the contour color gradient is red which corresponds to high density according to the color gradient (see density color gradient in panel (B)). According to the color gradient for low income families, they have high concentration in the north and central part of Biobio.

Figure 4.4: Spatial distribution of hospitals in Chile's two largest provinces

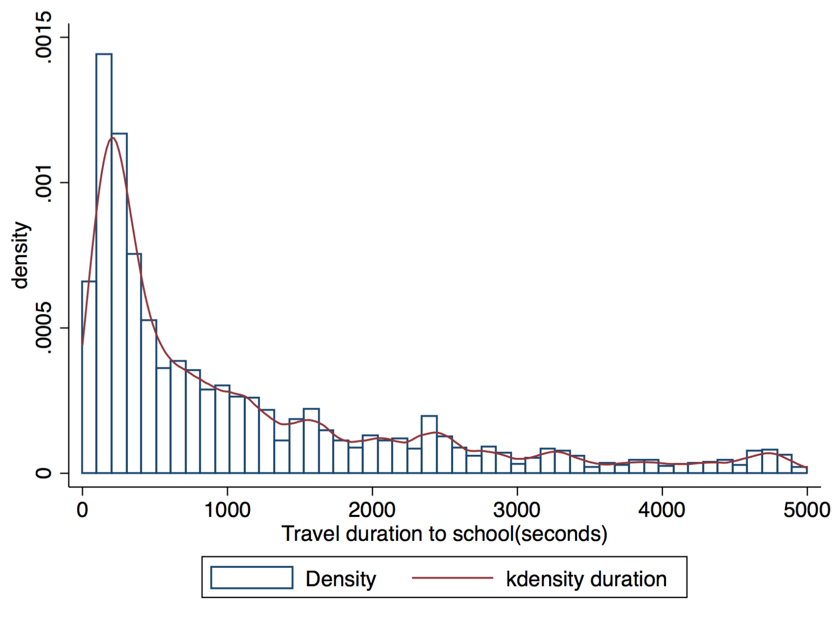


(a) Santiago

(b) Concepcion

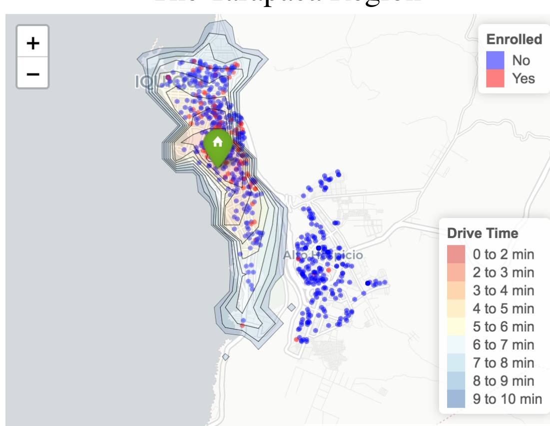
Notes: Panel (A) displays the spatial distribution of hospitals in Santiago province (in Metropolitana region). Panel (B) does it for Concepcion province (in Biobio region).

Figure 4.5: Travel time to school for ninth graders in 2018



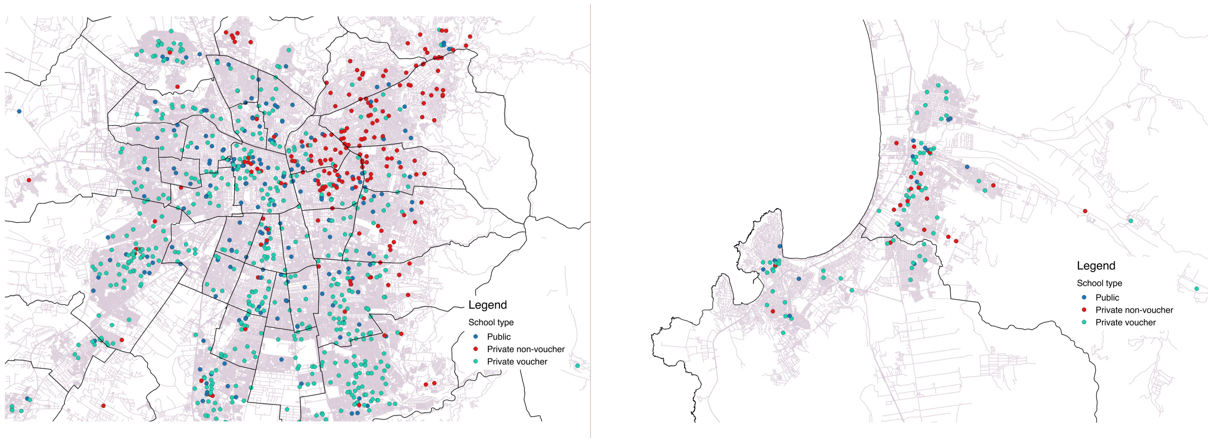
Notes: We display the travel time by car for ninth-grade students in 2018. This travel time computation was done using OSRM API.

Figure 4.6: Enrollment and travel time to school
The Tarapaca Region



Notes: We use the sample of students who participated in SAS for ninth grade admissions in 2017 and they were assigned new schools in 2018 in Tarapaca for illustration. The green marker indicates a large school but the average SIMCE score is below the mean SIMCE score for this school.

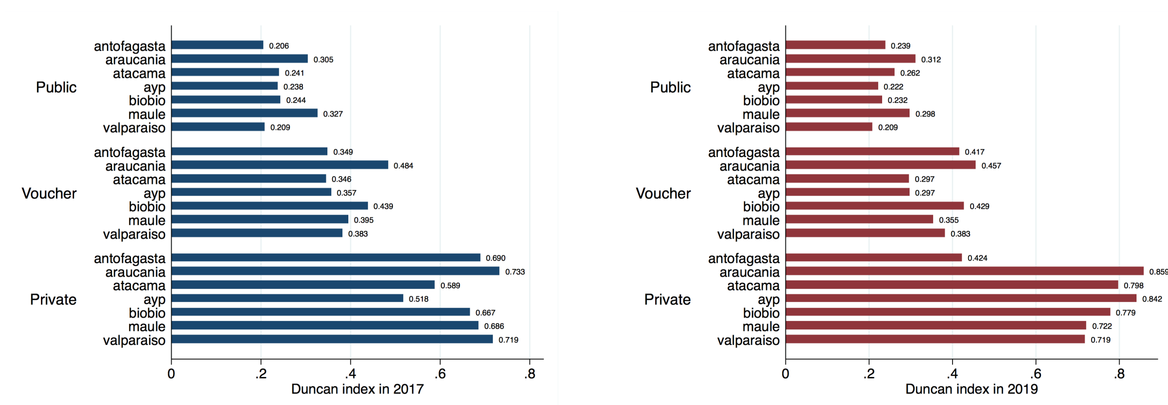
Figure 4.7: Spatial distribution of school types in the Metropolitana (Santiago) and Coquimbo regions



(a) Metropolitana (b) Coquimbo

Notes: The plots show the spatial distribution of public, voucher and private schools for different municipalities in Metropolitana and Coquimbo regions, respectively.

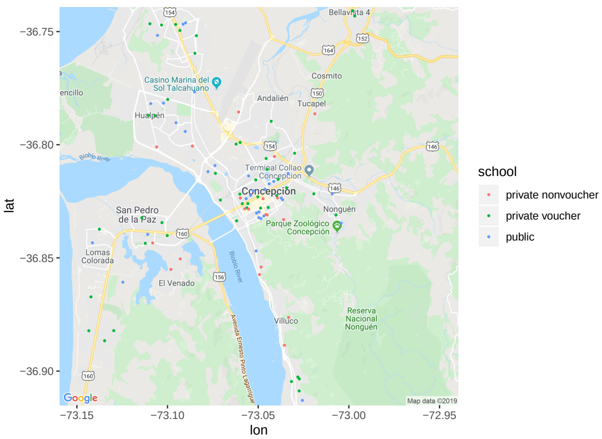
Figure 4.9: Differences in segregation by school type: 2017 vs. 2019 by region



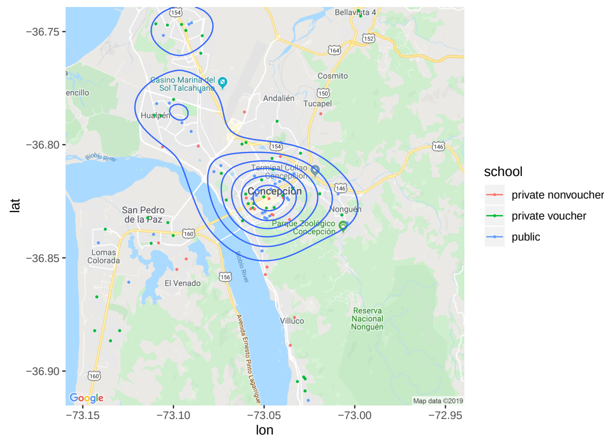
(a) 2017 (b) 2019

Notes: We consider differences in the Duncan index by three types of school-public, voucher, and private. Segregation is more pronounced in the private schools than in the public/voucher schools.

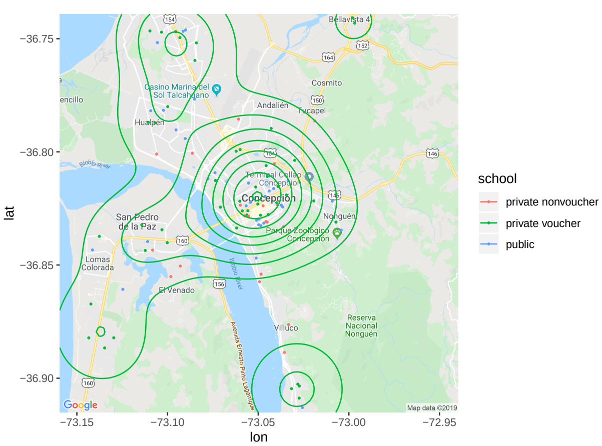
Figure 4.8: Spatial density plots of schools in the Biobio region by school types



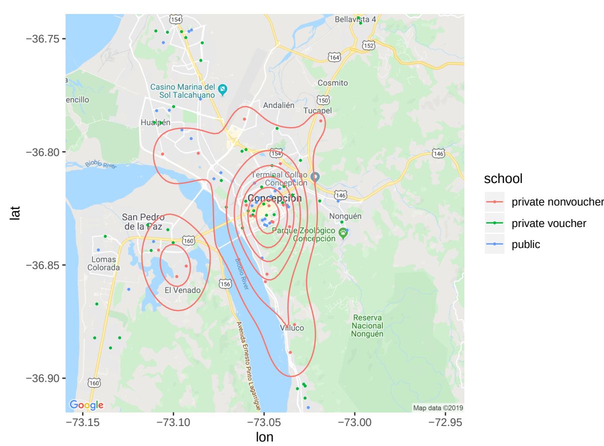
(a) All schools



(b) Public schools



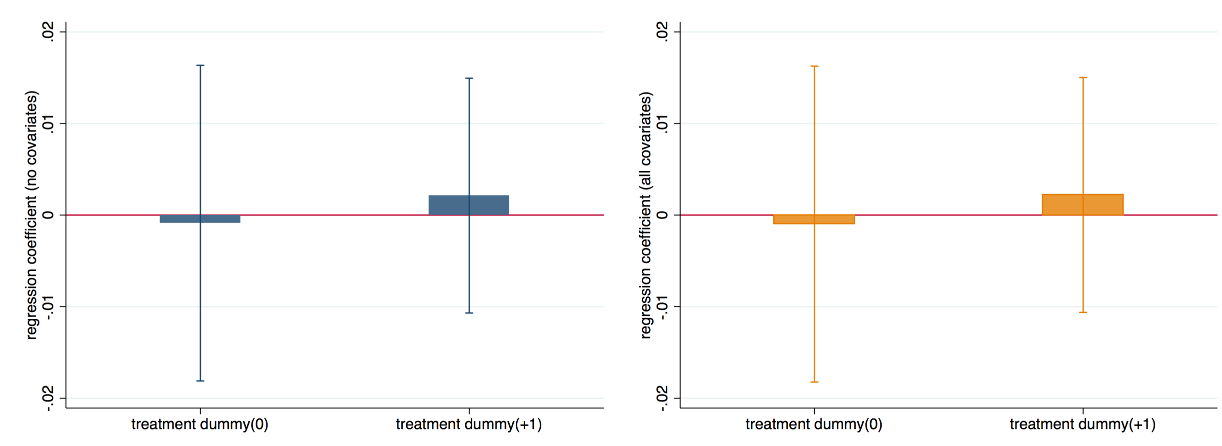
(c) Voucher schools



(d) Private schools

Notes: Figure displays contour plots for spatial density of different school types. We observe that voucher schools are more evenly spread as compared to public and private schools in Biobio region.

Figure 4.10: Leads analysis for parallel trends



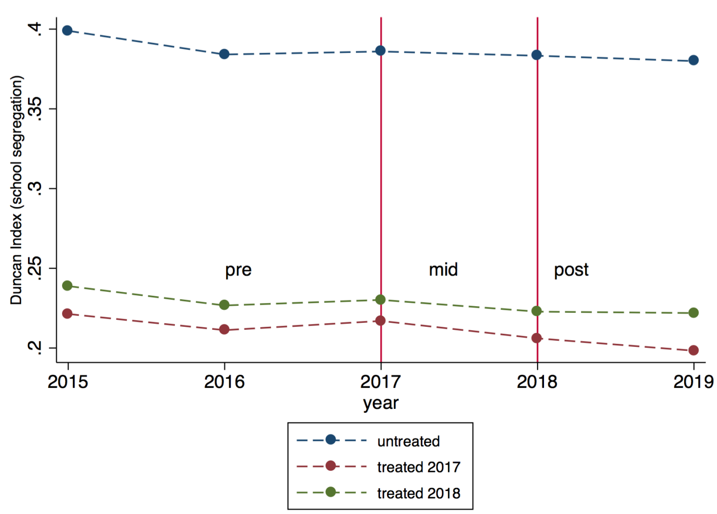
(a) Model 1: No covariates (b) Model 2: With covariates

Notes: Figure displays the leads test for parallel trends assumption. The list of control variables for the second specification (panel B) includes the pre-SAS fraction of public, voucher, private schools and rural schools. The figure presents the estimates (vertical bars) and associated confidence intervals (vertical lines) obtained from the following regression:

$$y_{crt} = \delta_{01} \times D_{rt} + \delta_{02} \times D_{rt+1} + Z_{1cr}\beta + \gamma_r + \lambda_t + \epsilon_{crt}. \tag{4.4}$$

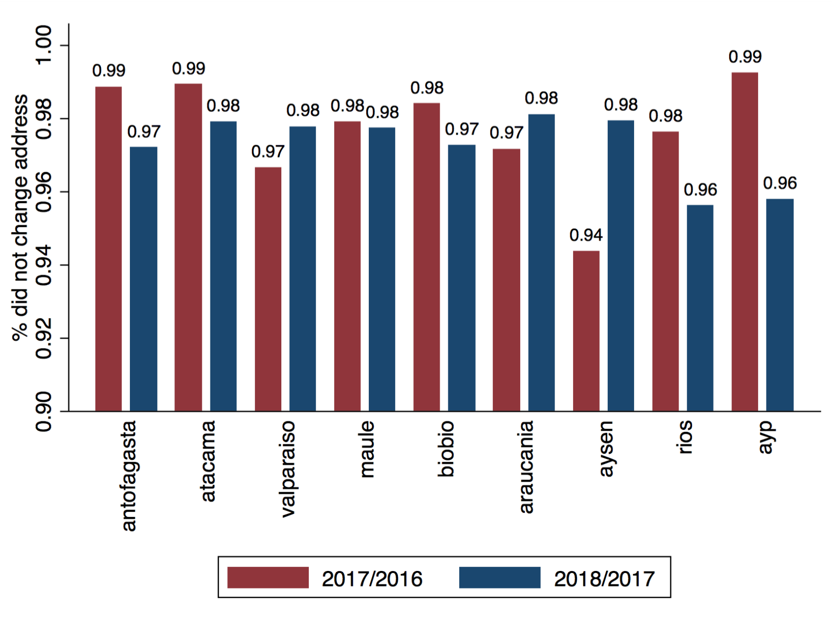
Here, y_{crt} corresponds to the Duncan index in municipality c (in region r) in year t. We include the lead of the treatment dummy D_{rt+1}. Future policy should not impact the Duncan index in year t. We find δ_{02} to be statistically insignificant in our test.

Figure 4.11: Parallel trends with treatment time variation



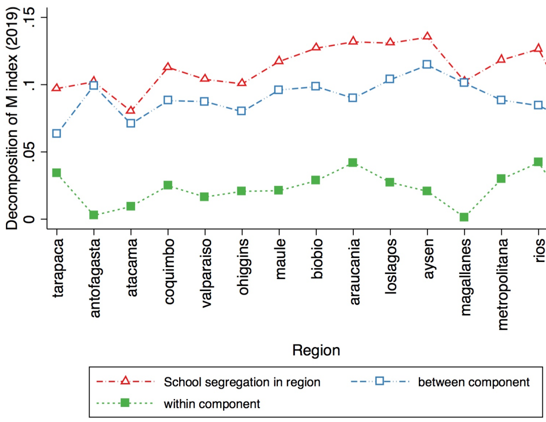
Notes: This is the visualization of the modified parallel trends assumption due to sequential implementation of SAS. For the purpose of this exercise the regions were divided into early (2017), late (2018) and control groups. We plot the average Duncan index for these groups and examine trends in the Duncan index before the policy was implemented. However, since the control group is varying overtime due to staggered implementation of SAS, we need to perform formal tests of pre-trends. We perform formal tests for pre-trends and provide details in section 4.5.2.

Figure 4.12: Change in residential addresses in response to introduction of SAS, by region

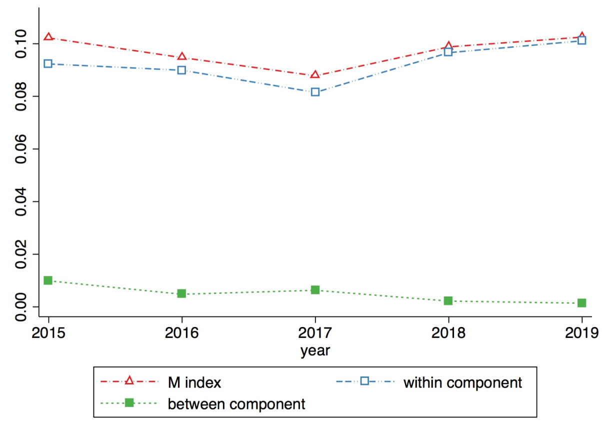


Notes: This figure illustrates that there were no systematic changes in residential addresses pre and post the introduction of SAS. We perform this analysis for regions in which SAS was implemented in 2017 and was effective in 2018. We display the fraction of students that did not change address between 2016 and 2017 (maroon vertical bars) and compute the same fraction for 2017 and 2018 (blue vertical bars). A comparison of these two fractions display that there are no substantial variations in the families changing addresses before and after the introduction of the policy.

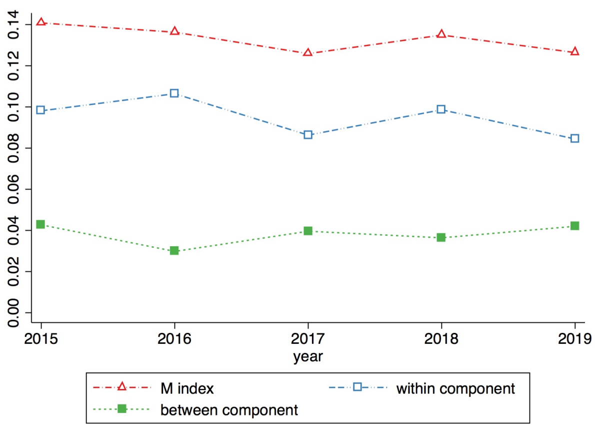
Figure 4.13: Decomposition of *M* information index into within and between components



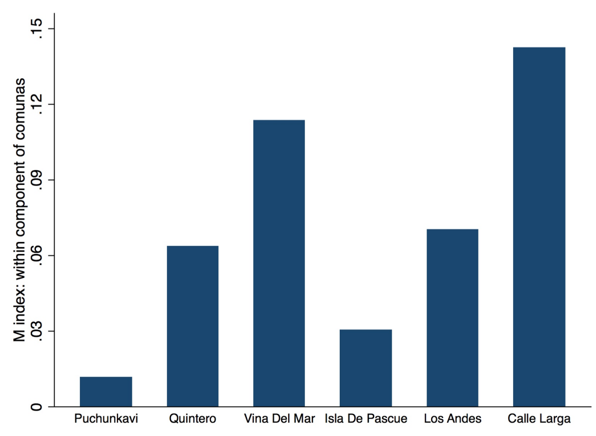
(a) 2019 by Region



(b) 2015-19: Magallanes



(c) 2015-19: Los Rios



(d) Within component: Valparaiso

Notes: Figure displays the decomposition of the M index into within and between components. For this exercise we use SIMCE and enrollment data for 2015, 2016, 2017, 2018 and 2019 for this analysis.

A.1 Proofs

A.1.1 Existence and uniqueness of the Nash Equilibrium

The matrix form equivalent of Equation 2.3 is

$$e_t^* = \alpha_0 + \lambda \sum_{j=1}^{n} g_{ij,t} e_{jt} + \alpha_1 X_{it} + \alpha_2 \sum_{j=1}^{n} g_{ij,t} X_{jt} + \alpha_3 S_t + \alpha_4 e_{i,t-1}$$

In their Theorem 1, Ballester et al (2006) prove that if the matrix $(I - \lambda G_t)^{-1}$ is well-defined and nonnegative, then the game has a unique Nash equilibrium $e_t^* = (e_{1t}, \cdots, e_{nt})$,which is interior and is given by

$$e_t^* = (I - \lambda G_t)^{-1} \big[\alpha_0 + \alpha_1 X_t + \alpha_2 G_t X_t + \alpha_3 S_t + \alpha_4 e_{t-1} \big]$$

Let $\mu_1(\mathbf{G}_t)$ denote the spectral radius of $\mathbf{G}_t$. Ballester et al. (2006) show that the condition $\lambda\mu_1(G_t) < 1$ ensures that the matrix $I - \lambda G_t$ is invertible by Theorem III of Debreu and Herstein (1953, p. 601).[17] Since G_t is a row-normalized matrix, then $\mu_1(G_t) = 1$. Hence, for $\lambda\mu_1(G_t) < 1$, it has to be true that $\lambda < 1$.

A.1.2 Mapping from effort to achievement equilibrium

Following Fruehwirth [2013] closely, I show how the game in effort maps into a game in achievement. Given that achievement is monotonically increasing in effort, achievement can

[17] This condition stipulates that local complementarities must be small enough compared to own concavity, which prevents excessive feedback which can lead to the absence of a finite equilibrium solution [Jackson and Zenou, 2015].

proxy for effort. Let achievement Y_{it} be the standardized test score of student i at time t. Denote the achievement production function, in matrix notation, as

$$Y_t = q\big(e_t, \mathbf{e}_{-i,t}, X_{it}, \mathbf{X}_{-i,t}, S_t\big)$$

where $\mathbf{e}_{-i,t} = (e_{1t}, ..., e_{i-1,t}, e_{i+1,t}...)$ and $\mathbf{X}_{-1,t}$ represents the effort and characteristics of i's peers. The system that describes the effort in the classroom as a function of achievement and peer effort is

$$\begin{aligned} e_{1t} &= q^{-1}(Y_{it}, e_{2t}, \ldots, e_{nt}, X_{it}, \mathbf{X}_{-i,t}, S_t) \\ &\vdots \\ e_{nt} &= q^{-1}(Y_{nt}, e_{1t}, \ldots, e_{n-1,t}, X_{it}, \mathbf{X}_{-i,t}, S_t) \end{aligned}$$

Assuming that the solution to this system is unique, we have

$$e_{it} = h(Y_{it,} Y_{-i,t}, X_t, \mathbf{X}_{-i,t}, S_t)$$

We can express the vector of peer effrot as a function of the vector of achievement as

$$\begin{aligned} \mathbf{e}_{-1,t} &= (\ldots, h(Y_{(i-1)t,} Y_{-(i-1),t}, \mathbf{X}_t, S_t), h(Y_{(i+1)t,} Y_{-(i+1),t}, \mathbf{X}_t, S_t), \ldots)v \\ &\equiv \mathbf{h}_{-i}(Y_{it}, \mathbf{Y}_{-i,t}, \mathbf{X}_t, S_t) \end{aligned}$$

Therefore, the effort best response can be written as a function of peer achievement, that is,

$$
\begin{aligned}
e_{it}^{*}(\mathbf{e}_{-i,t}, \mathbf{X}_t, S_t, e_{t-1}) &= e_{it}^{*}(\mathbf{h}_{-i}(Y_{it}, \mathbf{Y}_{-i,t}, \mathbf{X}_t, S_t), \mathbf{X}_t, S_t, e_{t-1}) \\
&= e_{it}^{*}(Y_{it}, \mathbf{Y}_{-i,t}, \mathbf{X}_t, S_t, e_{t-1})
\end{aligned}
$$

Plugging utility-maximizing effort back into the achievement production function, we have the achievement best response of a student to any level of peer achievement :

$$
Y_{it}^{*} = q\big(e_{it}^{*}(Y_{it}, \mathbf{Y}_{-i,t}, \mathbf{X}_t, S_t, Y_{i,t-1}), \mathbf{X}_t, S_t\big)
$$

Note that, under this same logic, past achievement serves as a proxy for past effort. This is a value-added specification, where knowledge in the previous grade is taken to be a sufficient statistic for student effort in all previous grades, and for parental and school inputs [Todd and Wolpin, 2003].

Let $\tilde{q}(\cdot)$ represent an explicit solution for Y_t as

$$
Y_{it}^{*} = \tilde{q}\big(Y_{it}, \mathbf{Y}_{-1,t}, \mathbf{X}_t, S_t, Y_{i,t-1}\big)
$$

A.1.3 A closed form expression for $(\mathbf{I} - \delta \mathbf{G}_t)^{-1}$

Following Lema 3 of Kuersteiner, Prucha and Zeng (2022), it is possible to show that

$$
(\mathbf{I}_n - \delta \mathbf{G}_t)^{-1} = \frac{(m-1)(1-\delta)+\delta}{(m-1+\delta)(1-\delta)} \mathbf{I}_n + \frac{\delta(m-1)}{(m-1+\delta)(m-\delta)} \mathbf{G}_t
$$

Proof

We check that

$$(\mathbf{I}_n - \delta\mathbf{G}_t)\left(\frac{(m-1)(1-\delta)+\delta}{(m-1+\delta)(1-\delta)}\mathbf{I}_n + \frac{\delta(m-1)}{(m-1+\delta)(m-\delta)}\mathbf{G}_t\right) = \mathbf{I}_n \tag{5}$$

Replacing the fact that $\mathbf{G}_t$ can be written as

$$\mathbf{G}_t = (\mathbf{1}_n\mathbf{1'}_n - \mathbf{I}_n)/(m-1)$$

We can express Equation 5 as

$$\left(\mathbf{I}_n\left(\frac{m-1+\delta}{m-1}\right) - \frac{\delta}{m-1}\mathbf{1}_n\mathbf{1'}_n\right)\left(\frac{(m-1)(1-\delta)+\delta}{(m-1+\delta)(1-\delta)}\mathbf{I}_n + \frac{\delta}{(n-1+\delta)(1-\delta)}(\mathbf{1}_n\mathbf{1'}_n - \mathbf{I}_n)\right) = \mathbf{I}_n$$

After a few manipulations,

$$\left(\mathbf{I}_n\left(\frac{m-1+\delta}{m-1}\right) - \frac{\delta}{m-1}\mathbf{1}_n\mathbf{1'}_n\right)\left(\frac{m-1}{(m-1+\delta)}\mathbf{I}_n + \frac{\delta}{(n-1+\delta)(1-\delta)}\mathbf{1}_n\mathbf{1'}_n\right) = \mathbf{I}_n$$

$$\mathbf{I}_n + \frac{(m-1+\delta)}{m-1}\frac{\delta}{(m-1+\delta)(1-\delta)}\mathbf{1}_n\mathbf{1'}_n - \frac{\delta}{(m-1)}\frac{(m-1)}{(m-1+\delta)}\mathbf{1}_n\mathbf{1'}_n - \frac{m\delta^2}{(m-1)(m-1+\delta)(1-\delta)\mathbf{1}_n\mathbf{1'}}$$

$$\mathbf{I}_n + \frac{m\delta - \delta + \delta^2 - (m\delta - \delta)(1-\delta) - m\delta^2}{(m-1)(m-1+\delta)(1-\delta)}\mathbf{1}_n\mathbf{1'}_n = \mathbf{I}_n$$

Since

$$m\delta - \delta + \delta^2 - (m\delta - \delta)(1 - \delta) - m\delta^2$$
$$= m\delta - \delta + \delta^2 - m\delta + m\delta^2 + \delta - \delta^2 - m\delta^2 = 0$$

The result follows.

A.2 Predicted G_t

A.2.1 Educational markets

In order to estimate the predicted networks, I assume interactions are formed within educational markets. This assumption help me reduce the dimensionality problem of the estimation and obtain more informative predictions.

There are 345 municipalities in Chile, grouped into 15 regions. About 96 percent of the students in my sample go to a school in their region of residence. However, there is great diversity, both socially and geographically, within regions. This motivates the idea of defining *educational markets* at a more narrow level. Municipalities are a good start, since about 80% of the students in 8th grade in my sample go to a school in their municipality of residence. That fraction decreases to 68% in 10th grade, as older students are willing to travel more.

As stated in Section **??**, I constructed educational markets in a data-driven way, based on two criteria: I join two municipalities i and j if

i) 10% or more of the students who live in municipality i attend a school in municipality j, or vice versa, and

ii) the travel distance by car between the centroids of the municipalities i and j is less than 2 hours.

This definition gives us a total of 236 markets in 8th grade and 111 markets in 10th grade. As expected, there are fewer markets for 10th graders because they are willing to travel longer distances. This measure of educational markets allow me to capture the variety of landscapes across and within regions. To illustrate this point, in the municipalities of Recoleta, in the Metropolitana region, there are 40 schools are located at an approximate travel distance of less than 5 minutes, whereas in the municipality of Río Ibañez, in the Aysén region, there are 5 schools located within an average travel distance of 137 minutes.

A.2.2 Geocoding

I use the HERE geocoder API for geocoding of the residential addresses of the my sample of students.[18] Table A.1 provides details on the quality of geo-referencing. I was able to determine the precise latitude longitude of the residential address for about 60% of the sample. For students who had a missing first line of address in the enrollment file or the geocoder API failed to locate the coordinates, I use the school location as a proxy for their residential address if they go to a school in their municipality of residence. If not, then I use municipality centroids as the location proxy.

[18]For complete details on the HERE geocode API refer to the documentation provided in `https://developer.here.com/documentation`.

Table A.1: Quality of forward geocoding

	Cohort 1				Cohort 2			
	8th grade		10th grade		8th grade		10th grade	
	N	%	N	%	N	%	N	%
Residential geo-coordinates located by API	106,329	0.60	92,033	0.60	110,515	0.62	93,794	0.61
School coordinates as location proxy	56,081	0.32	41,938	0.27	54,238	0.30	40,416	0.26
Municipality centroids as location proxy	15,221	0.08	18,826	0.12	14,463	0.08	18,883	0.12
Total observations	177,631	100	153,597	100	179,195	100	153,093	100

B.1 Definition of outcomes

Table B.1: Definition of non-academic outcomes

Variable	Description
General academic self-efficacy and motivation	This measure includes both the students' perceptions of their aptitudes and abilities to learn, as well as their attitudes towards learning and academic achievement. An exploratory factor analysis (EFA) delivers two factors. The first factor is important in linearly reconstructing all the questions regarding high levels of self-efficacy and motivation, such as "I am as capable of learning as the rest of my classmates". In contrast, the second factor is relevant only for the questions associated with low levels of self-efficacy and motivation, such as"In general, I understand little of what goes on in class". Given these results, I use this information to construct to measures: the first one captures students' self-efficacy and motivation, and the second one captures academic insecurity and lack of motivation.
Math academic self-efficacy and motivation	This measure captures the students' perceptions of their aptitudes and abilities to learn math, and their attitudes towards learning and academic achievement in that subject. It is constructed in a similar way to the general academic self-efficacy and motivation index, but taking into account questions directly related to math.
Student's expectation of Higher Education	This variable captures the student's perceived probability of attending higher education. Its construction is based on the question "Thinking ahead, what is the highest level of education you think you will be able to complete?". Takes the value 1 if the student thinks he/she will be able to complete a higher education degree, and 0 otherwise.
Exclusion	This measure indicates if the student has reported feeling excluded for some reason during that year. It is based on the question: "During this year, have you felt discriminated against or excluded for any of the following reasons? gender, disability, sexual orientation, being an immigrant, among others." It takes the value 1 if the student reports feeling excluded for any of those reasons, and 0 otherwise.
Violence Type 1	Variable that captures the frequency of robberies, malicious rumors , fights and threats and/or harassment among students in the school. It is based on the question: "During this year, how often have the following situations occurred in your establishment? robberies, malicious rumors among students, etc." It is constructed as an average of the responses, which range from 1:never to 5:every day.
Violence Type 2	Variable that captures the frequency of assaults with bladed weapons, assaults or threats with firearms, and vandalism by students. It is based on the question: "During this year, how often have the following situations occurred in your establishment? assaults with bladed weapons, assaults or threats with firearms, students break or damage the establishment, etc." It is constructed as an average of the responses, which range from 1:never to 5:every day.
Frequency of bullying (physical, verbal, social and cyberbullying)	Variable that captures the frequency of bullying experienced by students. It it based on the question: "During this year, how often have other students in your establishment intimidated or abused you as follows? Physically (hitting or breaking your stuff), Verbally (insulting you, mocking or threatening you), Socially (isolating yourself, speaking badly of you or humiliating yourself in front of others), Electronically (threatening you, humiliating you or making fun of you for text or Internet messages, such as networks social, emails, etc.)". It is constructed as an average of the responses, which range from 1:never to 5:every day.
Prevalence of bullying	Variable that captures the prevalence of bullying experienced by students. It takes the value 1 if the student reports having suffered bullying, either physical, verbal social or cyber bullying, and 0 otherwise.

Source: Own elaboration based on 8th-grade SIMCE questionnaires, years 2011, 2013, 2014 and 2015.

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