The Equilibrium Effects of Informed School Choice

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Abstract

This paper studies the small and large scale effects of a policy designed to produce a more informed consumer demand in the context of the market for primary education. We develop and test a specific personalized information intervention that targets families of public Pre-K students entering the elementary school system in Chile. Using a randomized control trial, we find that the intervention shifts parents school choice decisions towards schools with higher average test scores, test score value added, higher prices and that tend to be further distances from their home. Tracking students using administrative data, we find that student academic achievement was higher among treated families four years later, providing suggestive evidence that a policy intervention could be successful. To quantitatively gauge how average treatment effects might vary in the context of a scaled up version of this policy, we embed the randomized control trial within a structural model of school choice and competition where price and quality are chosen endogenously and schools face capacity constraints. We use the estimated model of demand and supply to simulate policy effects under different assumptions about equilibrium constraints. We find that capacity constraints play an important role mitigating the policy effect on impact but that in counterfactual simulations, the supply-side responses increasing quality contributes to a overall positive average treatment effect similar to that of the randomized control trial.

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1 Introduction

The lack of information about product quality can affect consumer behavior and have important effects on equilibrium market outcomes (Akerlof, 1970). In markets for educational services, information and its effects on consumer behavior can potentially have important effects on equilibrium levels of school quality, given it can be difficult to observe. In addition, lack of information can have distributional effects, given families from less educated socioeconomic context may be particularly misinformed and have more difficulty acquiring it (Hastings and Weinstein, 2008; Andrabi et al., 2017).[1] Additionally, poorer families may not have accurate information about the returns to many profitable investments, including underestimating the return to human capital investments (Jensen, 2010; Banerjee and Duflo, 2011). This combination can lead poor families in developing countries to under invest in human capital including spending less time and energy searching and acquiring information about what school to send their children. In the aggregate, a generalized lower interest in school quality could also lead to an equilibrium with lower provision of quality than would be expected in a market with full information. These concepts are supported by empirical evidence and an emerging consensus that information and marketing interventions in education settings seem to be able to shift individual choice behavior, although specific effects depend on context, implementation and design details.[2] However, with the notable exception of Andrabi et al. (2017), evidence regarding the equilibrium policy effects of such interventions implemented at scale is much less common, and evidence of what mechanisms are at play is even more scarse.

In this paper we explore the quantitative equilibrium implications of a policy based on information provision in the context of the market for elementary schools in Chile. We develop a scalable intervention that is policy relevant and evaluate the effects on potential equilibrium effects of the policy to generate quantitative recommendations in a setting where randomized control trials at scale are not feasible. To do this, we integrate evidence of policy effectiveness from a small scale randomized control trial within a model of school choice and competition to evaluate how the policy effects vary when equilibrium effects from both demand and supply are taken into account.

Specifically, we use a small scale randomized control trial to evaluate the impact of a potential policy intervention on individual household school choice decisions and later academic outcomes in the context of Chile. The intervention adapts ideas from previous work in other contexts and

[1] Less educated families will have less experience in judging the quality of educational services and are less likely to get information through social networks. There is ample empirical evidence of an information-socioeconomic gradient. See Hastings and Weinstein (2008) for evidence of parents in the USA lacking information about schools and their characteristics and Elacqua (2016); Hastings et al. (2016) for additional evidence from Chile.

[2] Lavecchia et al. (2016) review evidence on information interventions in education as well as other interventions based on insights from behavioral economics.

the design is chosen so that it is adapted to local policy constraints and scalable.[3] The results from the randomized control trial show that household school choice decisions shift towards schools with higher test scores and prices on average. In addition, after four years, the treated group had higher test scores on average, suggesting the intervention lead to increased academic achievement, at least partly, due to changes in school choice induced by the intervention.

We quantify the way the policy changes how families trade off school characteristics such as quality, distance and price through the lens of an empirical model of school choice modifying the framework in Neilson (2017) to explicitly incorporate families with incomplete information and to accomodate the results of the randomized control trial as well as administrative data of the school choices of the population of students. Using the estimated empirical model of school choice, we explore how average policy effects change when implementation is carried out at the national scale taking into account capacity constraints, and heterogeneity in market structure. We find that when capacity constraints are taken into account, the average effect of the policy is still positive but reduced by fifty percent as increased demand for schools with higher quality in disadvantaged neighborhoods crowds itself out.

To explicitly evaluate the potential for equilibrium supply side effects in the medium run, we use recent variation in voucher policy together with panel data on the population of schools to estimate a static model of school competition among current providers in the spirit of work in empirical industrial organization such as Berry et al. (1995); Wollmann (2018). We use the estimated parameters of the cost structure to evaluate the effects that changing aggregate demand has on school incentives and the resulting equilibrium distribution of school characteristics such as price and quality induced by the new policy. We find that the new equilibrium is characterized by higher quality schools and this increase in quality more than compensates for the original lack of capacity at higher quality schools. Effects of the policy are found to be higher than the positive average treatment effects found in the small scale randomized control trial under different assumptions regarding schools objective function and estimates of cost structure.

The paper makes two main contributions. The first is that it provides evidence on the role of a policy relevant information and marketing intervention in the context of education markets at both the micro and macro level. This distinction is relevant because the difference between partial and equilibrium effects can be very important in education contexts as has been famously noted in Heckman et al. (1998a,b). At the same time, aggregate level ex-ante policy evaluation is difficult, if not impossible, to implement in many cases and thus is unable to provide relevant quantitative policy advice. This is unsurprising given the context where shifting behavior of many individuals is likely to have nontrivial general equilibrium effects that are difficult to take into account in typical settings where randomized controlled trials are feasible. One important

[3]For example, relevant prior work by Hastings and Weinstein (2008), and Andrabi et al. (2017) in the context of school choice.

exception is recent work by Andrabi et al. (2017), that has shown that in the context of Pakistan, providing school report cards in small village markets lead to increased academic achievement, suggesting that information on school quality and price can lead to changes in educational outcomes in the aggregate and that the supply side seems to be an important margin to consider. While this experimental evidence is a rigourous proof of concept, the relevance of context and policy design can make it difficult to take these experiences and extrapolate specific lessons to make policy recommendations in other settings. In fact some policies that are similar in spirit do not seem to have provided the same results. One example is Mizala and Urquiola (2013), where an implemented policy provided a signal of school quality that seems to have not had any effect on school choice. The evidence presented in this paper shows that the relevant policy intervention has effects on individual school choices of families and works out the quantitative implications of imposing equilibrium conditions to sorting and supply side adjustments. The results point toward a positive partial equilibrium policy effect that is considerably dampened by capacity constraints. However the equilibrium effects including supply side reactions suggest large positive effects across a spectrum of assumptions thus providing quantitative evidence for making policy recommendations.

The second main contribution is to present an empirical framework that builds on a small scale experiment to approximate the effects of a large scale implementation of the policy when aggregate at scale experiments are impossible. By explicitly modeling the consequences of changing individual choices on both the demand and the supply side, this empirical framework allows the researcher to provide some notion of quantitative implications of the policy. This approach using counterfactual analysis based on models of economic behavior that are estimated using credible sources of exogenous variation including the randomized control trial is shown to be one way to move forward with quantitative policy recommendations in situations when implementing randomized control trials at scale is not feasible.

The empirical methods used add on to a growing body of research that takes advantage of the variation created by RCTs and other credible exogenous sources of variation. Some papers have used RCTs to help estimate key parameters or to validate the predictions of a structural model. This approach was famously applied to the evaluation of PROGRESA in seminal work by Todd and Wolpin (2006), for example. This paper differs from that approach in that the RCT is not used to validate the model nor is the structural model used specifically to better understand the mechanisms of the policy tested or even the effects of changing different features of the policy. Similar to Attanasio et al. (2011), the experimental data provide a way to identify new parameters associated to the effect of the policy. Not having experimental variation in the aggregate, this paper differs from Attanasio et al. (2011) in that the purpose of adding structure to the behavior of families and schools is to be able to estimate how the policy affects choices and extrapolate the effects of the policy to individuals beyond the experiment. This allows the researcher to provide insight on the

behavior of families who face different choice sets beyond the experimental sample and to explore the consequences of aggregate effects on demand with capacity constraints and then on supply side considerations such as the choice of quality. Work by Lise et al. (2015) evaluate employment programs taking advantage of experimental variation are more similar in spirit, although applied to the context of job training and employment.

The paper also contributes to this line of work joining structural modeling and RCTs by explicitly adding the experimental variation to the modeling of supply and demand of school quality. While empirical models of demand and supply are commonly used to evaluate policy in empirical industrial organization research, these types of models have rarely been applied to education markets and have traditionally have not explicitly incorporated experimental variation.[4] These new insights and analysis come with additional assumptions and require additional instruments. In this paper we argue that using a coherent economic framework to follow the logical implications of changing individual behavior expands the set of questions that can be asked and allows for a range of policy relevant predictions that can be useful for translating evidence and research into policy recommendations.

2 Data and Institutional Setting

2.1 School Choice in Chile

In Chile, most students enter primary school in Kindergarten at the age of 5[5]. Prior to entering primary schools, the vast majority of students attend pre-K institutions where enrollment rates at age 3 and 4 are 55% and 87% respectively. The two main providers of free public pre-K are Junji and Fundación Integra, which administered approximatly 3,000 and 1,000 centers respectively and are explicitly tasked with providing access to pre-K educational services for students all over the country. There are also paid private sector providers. For students enrolled in preschools from 3 to 4 years old in 2016, 42% were in Junji, and 18% in Integra.[6] The majority of these students live in urban markets (88%of the ones enrolled in Junji and Integra) and families tend to send their children to a local pre-Ks very close to there homes.[7]

[4]Some exceptions on work on school choice in education markets include work by Walters (2014); Dinerstein and Smith (2015); Neilson (2017). The model of school choice and competition in this paper builds on prior work in this space by Neilson (2017) and earlier work on school choice in Chile by Gallego and Hernando (2008) and Chumacero et al. (2011).

[5] Students should be 5 years old on March 31st of the school year they enter to Kindergarten. Some private schools offer preK-4, allowing students to enter primary school when they are 4 years old.

[6]There are few official sources of information on private pre-K. We calculate this numbers by calculating the number of enrolled 3 to 4-year-old students (55% of the cohort according to the OECD) and then calculated the share of students that are in Junji and Integra based on their administrative records.

[7]In our sample of 1800 students described below, the average distance from their home to the pre-K is 1.12 km.

Transiting from a Pre-K institution to a primary school requires parents to apply and sign up for school at some point before the start of the academic year. Until 2016 this process was decentralized and the timing of the application and matriculation process was heterogeneous. In 2016 they implemented a pilot version of a centralized application system in the southernmost region of the country. In 2017 the system was extended to 5 regions, and it will be implemented in the whole country in 2019.

Primary schools in Chile are either free public schools, private voucher schools or private non-voucher schools. The system is characterized by a high degree of choice and large participation of the private sector. In 2016, the market share for first-grade students was 36% for public schools, 55% for voucher schools, and 8% for private schools. Private voucher schools can charge an additional fee beyond the voucher, but there are some caps and discounts that limit the fee amounts for schools that receive the voucher. In 2016, 63% of voucher schools in urban areas are free and 86% have a fee lower that 70 USD (15% of the minimum wage). In addition, policy changes in 2008 introduced a larger voucher for approximately the poorest 40% of the students in schools that signed up for the policy and required schools to not charge eligible students any tuition fees. In practice, this resulted in zero top off fees for poor students at all the public schools and at 80% of the voucher schools that have signed up for the policy in 2016. Additional reforms implemented in 2016 froze the prices charged by vouchers schools and implemented a gradual 10-year plan to completely eliminate fees in voucher schools.

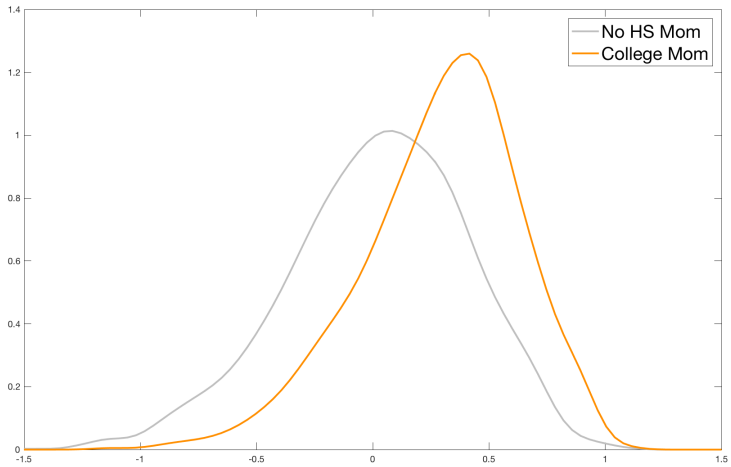
Both public and voucher schools operate under the same subsidy per student, and a large portion of private voucher schools have traditionally operated as for-profit.[8] However, it is reasonable to assume that while many private schools may maximize profit, public schools face different incentives and different constraints. On the one side, public schools may behave like firms in a competitive market trying to increase revenues, which are proportional to the number of students the school attracts. On the other hand, public schools are administrated at the municipality level where the same administration controls a set of schools potentially pooling funds. As a result, an individual public school can receive additional transfers and cross transfers from the municipality to cover their expenses, independent of their level of enrollment. Public schools also have less flexibility in how they can spend their money and hire staff given that public teacher contracts are highly regulated.

In spite of the variety of schools and choices available, students from poorer families tend to go to schools with lower outcomes and lower inputs. A series of policy changes over the years have tried to reduce this stratification in the system. Recent policy changes include implementing larger targeted vouchers for the poor which is studied in Neilson (2013); Mizala and Torche

[8]Voucher schools are operated by both for-profit firms and non-profit organizations. (Aedo, 1998) argues that non-for-profit schools behave similarly to for-profit schools as they raise additional funds for operating the school in a relatively competitive market for donations.

(2013); Elacqua and Santos (2013) among others. The recent introduction of centralized school applications, further expansions of voucher amounts, price caps and gradual elimination of fees in voucher schools all follow a tradition of public policies that seek to increase access to high-quality education by disadvantaged students. While these reforms seem to have helped,the distribution of school inputs and outputs conditional on family socioeconomic status continue to be very different. Figure 1 shows the distribution of school quality by mother education for students entering first grade.[9]

Figure 1: Inequality of School Quality Across SES



Note: This figure shows the distribution of school value added in 2012 conditional on student mothers education. Similar graphs showing school inputs such as teacher quality, school infrastructure measures and outcomes such as student test scores are presented in the online appendix.

One reason that can explain the lack of convergence across groups even after important investments in access, is that poor families may not be fully informed about the importance of choosing a high-quality school. In addition, it is possible that families that come from a socioeconomic context with less experience with educational institutions may find it more difficult to accurately assess school quality. These hypotheses would lead poor families to put more weight on the school's proximity or other characteristics when deciding what school to choose. Note that if this is true, even in the case of total equality of access, we can expect differences across SES groups.

Policy makers in Chile have been interested in promoting information provision for many years. Standardized testing has been implemented in a continuous way since 1987, and government websites have posted school test scores for many years. In 2010 the government of Chile

[9]The online appendix presents similar graphs for school test scores as well as school inputs such as teacher quality and measures of infrastructure.

pushed an agenda of *"Mas Informacion, Mejor Educacion"* (More information, Better Education) showing interest in the idea of providing information.[10] Evidence from other countries and contexts such as the US and Pakistan suggest that there may be some scope for an information provision policy that could improve outcomes.

This paper builds on a J-PAL LAC project which design began in 2009. The randomized control trial was implemented in 2010. At the time, the government was considering increasing information provision to promote more informed school choice and had some specific ideas in mind, such as providing school report cards. Government policy and feasibility concerns shaped the intervention and the design of the implementation. This context influenced the development and testing of the information intervention evaluated in this paper. The goal of the design was to accommodate scalability and policy feasibility, while rigorously evaluating effectiveness at a small scale, eventually arriving at a quantitative recommendation relevant for policymakers.

3 Conceptual Framework

3.1 Framework for Policy Analysis

In this section we provide a framework to analyze the effects of an information provision policy. We are interested in studying how this policy might change individual behavior of families and specifically how it might change the quality of the schools chosen. We are also interested in studying how the policy applied at scale might result in different effects in the short and long run when firms can adjust their quality. The challenge is to incorporate enough institutional background into our empirical model so that we can make sense of the data while keeping it simple enough to be tractable. We need to incorporate institutional details about schools and families context but at the same time restrict the set of possible responses to the policy changes to have any hope of approximating what reactions are likely to occur in equilibrium.

The model specifies the behavior of families and schools together with a notion of equilibrium. Each make choices to maximize their objective function subject to financial and other regulatory constraints.[11] A notion of short and long run determine what variables are under the control of schools.

[10] See online appendix for a description of this government program and how it relates to the current policy evaluation.

[11] Several papers have studied school demand systems in the context of Chile, notably Gallego and Hernando (2012) and Neilson (2013, 2018). Very few studies include supply side considerations in education context. Sanchez (2017) is one recent exception.

3.2 Families

When a student $i \in \{1, ..., N\}$ is entering school, the family must choose a school $j \in J_i^m$ where $J_i^m \subset J^m$ is the set of schools that are available to student i and $\#J^m = N_J$. Families might differ in the set of schools that are available to them so that J_i^m is not the same for all i. Families can also differ by their socioeconomic status type$_i \in \{1, ..., T\}$ and location node n_i across an urban market m. Families can have heterogeneous preferences for school characteristics such as out of pocket price p_j, quality q_j and distance to their location d_{ij}. Government voucher policy v_{ij} determines the out-of-pocket expenses for different families i at potentially different schools j.

The value the family gets by choosing j is given by $U_{ij}(\omega)$ where $\omega \in \Omega$ is a state of the world indicating the price, quality and distance of all schools as well as government policy and how important school quality is for future outcomes of the children. Families have an information set $\mathcal{I}_i \in \mathcal{I}$ so that, at the time of choosing a school, the perceived value of a school, given the information set the family has, is given by Equation 1.

$$U_{ij}^{\mathcal{E}}(\mathcal{I}_i) = \mathcal{E}\left(U_{ij}(\omega)|\mathcal{I}_i\right) \tag{1}$$

We further assume that families choose the school that provides the highest perceived value conditional on their information set so that $j_i^* = \text{argmax}_{j \in J_i^m} U_{ij}(\mathcal{I}_i)$. Having defined the latter, we can sum over all such choices to write the share of families of each SES type that choose school j as in Equation 2. The average school quality for each type can be written as the weighted average in Equation 3.

$$s_j^{\text{type}}(U, \mathcal{I}, J^m) = \frac{1}{N_{\text{type}}} \sum_{i=1}^{N_{\text{type}}} 1(j = j_i^* | U_{ij}^{\mathcal{E}}(\mathcal{I}_i), J_i^m) \qquad \forall j, \text{type} \tag{2}$$

$$\mu_q^{\text{type}} = \sum_{j=1}^{N_J} q_j \cdot s_j^{\text{type}}(U, \mathcal{I}, J^m) \qquad \forall \text{type} \tag{3}$$

With this very basic framework we can conceptually decompose the differences in school quality attended by students of low and high socioeconomic status as a composite of several potential forces. Part of the difference can be due to differences in choice sets available, possibly due to heterogeneity across markets or due to supply side selection. Differences in location within markets also change the value of different options. Additionally, differences can be attributed to heterogeneity in preferences for school characteristics. Finally differences could arise due to differences in the information sets available to different types of families, which lead them to make different choices due to different beliefs about school characteristics and also how important school quality can be.

3.3 Schools

An elementary school $j \in J^m$ can be public or private and is located at a node n_j in an urban market m. The school can potentially choose to make investments and exert effort to adjust their quality[12], q_j, and over time possibly their capacity k_j as well. Private schools can also potentially choose a price p_j under some restrictions given by policy. Schools can also differ in their ability to mix inputs to generate quality so that their cost structure is heterogeneous $C_j(q)$. This could reflect that some schools may be run more or less efficiently than others, have access to cheaper inputs or both, allowing them to have lower costs for a given level of quality and capacity. Public schools are potentially run jointly across municipalities, thus sharing resources. Schools receive a student-level transfer v_{ij} that is potentially different for different students at different schools.

Given the choice of individual families described above, it follows that the demand a school can expect to get given the government policy, quality, price and location of other schools also depends on the information structure that partially determines decisions of families and thus can potentially influence quality and prices.

$$s_j(U, \mathcal{I}, J^m) = \frac{1}{N} \sum_{\text{type}=1}^{T} N_{\text{type}} \cdot s_j^{\text{type}}(U, \mathcal{I}, J^m) \tag{4}$$

Schools maximize some combination of profit and quality weighted average, subject to a set of financial and technological constraints. Thus, conditional on capacity, quality and price are chosen endogenously as a function of government policy, own costs/productivity, objectives and local market conditions, which are partially determined by the distribution of information sets over which families base their decisions on.

$$(q_j^*, p_j^*) = \text{argmax}_{(p,q)} \Pi(C_j(q), v_j, s_j(U, \mathcal{I}, J^m)) \tag{5}$$

This setup highlights that schools quality, price and other valued attributes are endogenous to a series of environmental factors. The heterogeneity in school quality in a particular market can be due to government policy, differences in costs, differences in objectives and differences in market structure and competitive pressure. Importantly for this paper, the quality and price chosen by schools can also depend on the information structure of local families given that this can affect the demand (Ns_j) faced by schools.

[12]For simplicity, note that quality is assumed to be the same for all students at the school and, while potentially chosen with some uncertainty, it is not a function of what students attend. This rules out peer effects and other more complicated school-student match effects.

3.4 Equilibrium and Potential Policy Effects

In equilibrium schools will have chosen quality and prices, and families have chosen what school to attend, such that there is no excess demand for any particular school given school capacities. Due to fixed costs and the zero lower bound on prices, there may exist excess capacity at some schools. Schools can expand capacity, and raise or reduce quality over the medium term.

The gap in school quality chosen by different types of family, μ_q^{type}, is due to both demand and supply side considerations, as well as policy. The model can be used to decompose the factors that define the gap. The policy of providing information takes an aim at shifting the information set families use when making school choices. At the individual family level, such a policy directly affects the optimal school choice, assuming the choice set J_i^m and the characteristics of all schools in that set are unchanged. A small scale randomized control trial is an approximation to this situation and helps identify the effect of the policy on families' optimal school choice.

$$\frac{\partial \mu_{q,\text{small}}^{\text{type}}}{\partial \text{Treatment}} = \sum_{j=1}^{N_J} q_j^* \cdot \frac{\partial s_j^{\text{type}}}{\partial \mathcal{I}} \frac{\partial \mathcal{I}}{\partial \text{Treatment}} \tag{6}$$

A larger, scaled version of the policy could induce additional reactions that could affect the average quality chosen. To simplify, assume the policy is implemented to all families in the short run, but schools are unable to adjust prices, quality or capacity. We would have that average quality that a particular type of family chooses is now affected by the changing information set, but also due to a change in the schools available. Unexpected shifts in demand could lead to excess demand at some schools and crowd out some of the families' demand.

$$\frac{\partial \mu_{q,\text{scale sr}}^{\text{type}}}{\partial \text{Treatment}} = \sum_{j=1}^{N_J} q_j^* \cdot \left(\frac{\partial s_j^{\text{type}}}{\partial \mathcal{I}} \frac{\partial \mathcal{I}}{\partial \text{Treatment}} + \frac{\partial s_j^{\text{type}}}{\partial J^m} \frac{\partial J^m}{\partial \text{Treatment}} \right) \tag{7}$$

In the medium term, a large scale policy that shifts demand for schools by shifting information sets could have additional effects through the supply side, as schools may adjust their quality and price as a function of changing demand.

$$\frac{\partial \mu_{q,,\text{scale mr}}^{\text{type}}}{\partial \text{Treatment}} = \sum_{j=1}^{N_J} \left[\frac{\partial q_j^*}{\partial s_j} \frac{\partial s_j}{\partial \text{Treatment}} \cdot s_j^{\text{type}} + q_j^* \cdot \left(\frac{\partial s_j^{\text{type}}}{\partial \mathcal{I}} \frac{\partial \mathcal{I}}{\partial \text{Treatment}} + \frac{\partial s_j^{\text{type}}}{\partial J^m} \frac{\partial J^m}{\partial \text{Treatment}} \right) \right] \tag{8}$$

In the long run, schools are expected to adjust capacity and re-optimize price and quality to maximize their objective function, given local market conditions. Entry and exit margins are likely to be relevant and a series of dynamics can be of interest as well. Without going into further

details, this framework suggests that there could be meaningful adjustments in both demand and supply side considerations once equilibrium constraints are imposed on the policy effects. The relevance of these adjustments depends on the quantitative importance of particular mechanisms. The first is that the policy affects individual decisions in a meaningful way. Secondly, the changing demand could make capacity constraints binding and limit the adjustments in demand in the short run. The third important aspect that links supply side reactions is whether schools change their behavior as a function of changing demand and local market conditions. These three aspects and their implications are explored quantitatively in the following sections.

We first quantify the effects of the policy on individual choices using a randomized control trial. We estimate average treatment effects on the characteristics of the schools chosen and students' later outcomes. Once we verify potential meaningful effects on individual choices, we lay out an estimation strategy to recover how the treatment changes the way families choose, even if the econometrician does not observe information sets. We then propose a strategy to recover estimates of the schools' cost structures and how to use these to recover new equilibrium behavior of all schools.

4 School Choice Experiment

4.1 Design of the Randomized Control Trial

The main objective of the intervention was to encourage a carefully thought school choice process by increasing the awareness of neighborhood schools characteristics and the perceived returns to school quality. The intervention was restricted to be low cost and easily scalable by government agencies that provide Pre-k services. To this end, we collaborate with the network of Integra preschools that provide Pre-K education to 25,229 students in the cohort of 3 to 4 years (30% or public Pre-K) to test the intervention. The information provision treatment consisted of a session within the context of regular parent-teacher meetings. Parents were shown a video that emphasized the returns to investing in school quality and choosing a school carefully. Through relatable testimonials of students from known poor neighborhoods, the video makes an effort to show that there are good schools in poor neighborhoods and that going there can make a difference in terms of future opportunities, showing real-life examples. Parents also received an informative report card which highlights test scores and prices of schools in the neighborhood. A discussion with parents provided space for making open questions about the school choice process. We refer the reader to the Online Appendix for more details on the design of the treatment.

The study is conducted in the three larger regions of Chile: Valparaíso, Biobío, and Santiago. The main criteria used to choose the sample of preschools was for them to be located in urban areas (according to Integra's classification) and in areas with at least 10 schools within 1.2 miles

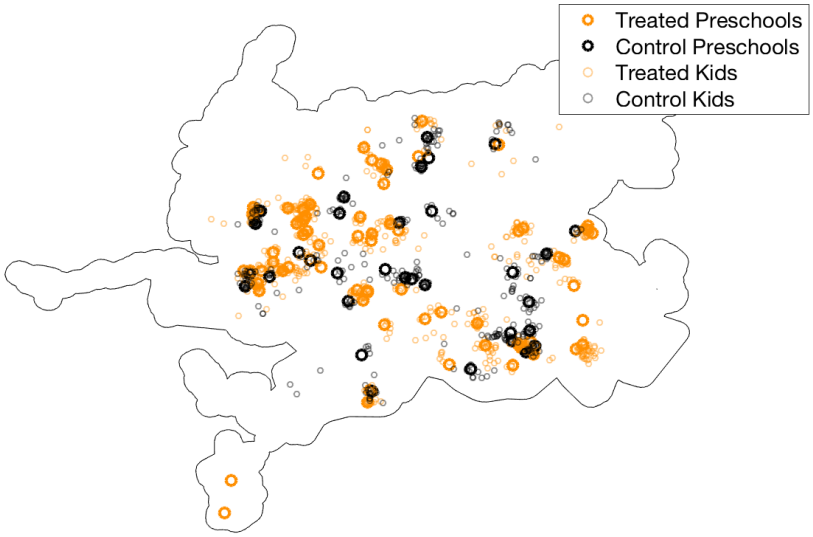
and with the ratio $(\text{primary schools}_m / \text{preschools}_m) \geq 2$. We restricted our calculations to schools in the three lower SES groups defined by the Ministry of Education (levels A, B, and C), as families that attend Integra tend to come from the three first income quintiles.

We randomly assigned preschools to control (C) and treatment (T) groups, stratifying by region, grade and school competition, measured as the number of schools within 1.2 miles.[13][14] The experiment was implemented between August and December 2010 by trained staff who participated in the parents' meeting scheduled by the preschools. In the 133 selected preschools that agreed to participate, a total of 1,832 parents signed the informed consent and answered a baseline survey. This survey was taken before handing out any information and it included contact information and questions regarding the application process. We asked parents about whether they had decided to send their child to primary school in 2011, if they had already chosen a specific school, and whether they had already enrolled them. Parents were also asked if they had any other children already enrolled in primary school. After the survey, parents in treatment schools received the school choice intervention (see the treatment description below). Parents did not know about the intervention before the meeting, to prevent self-selection due to a special interest in the enrollment process or preferences toward quality and demand for information. The staff was mainly composed by people hired by the surveying firm, which came from a relatively similar background of the preschool parents. Between May and July 2011, we conducted a follow-up survey asking parents about their enrollment decisions. Of the 1,832 who received information, we were able to survey 1,611 (87%). In addition, we were able to match 1,795 out of 1,832 (98%) in our original sample to administrative records using student individual identifiers. Figure 2 shows the distributions of treatment and control schools and students in the map of the city of Santiago. It is important to mention that the Pre-K schools were selected so that the report card provided would not overlap with any other Pre-K schools in the study.

[13] We worked with the two higher levels offered by Integra: *Medio Mayor*, for children up to 3 years and 11 months old and *Transición 1*, equivalent to Pre-K. We only worked with the highest grade in each preschool, in order to maximize the exogeneity of the enrollment decision. Depending on the school, the highest level was *Medio Mayor* or *Transición 1*

[14] Initial design of the experiment included separate arms with subgroups showing only the video or only the report card. Implementation difficulties lead us to not have perfect tracking of the exact treatment received at each school leading us to pool the potential treatments. The online appendix describes the design in detail.

Figure 2: Report Card - Treatment and Control Schools and Students in the Map



The characteristics at the pre-school level come from Integra's administrative data and include total enrollment, mean attendance, and measures of SES proxied by mothers' education, income quintile and poverty status of the children in each pre-school. We see no difference in the characteristics between the treatment and control pre-schools.[15] Family characteristics are self-collected in the baseline and follow-up surveys and also show no systematic differences across treatment and control groups when observing a host of characteristics, which include SES characteristics (household size, possession of durable goods, whether the family owns the dwelling, whether the mother is the head of the household and measures of the level of education of the mother), information about school in the baseline (whether the kid is already enrolled in a school or the parents have an older kid that is already going to school), and an indicator for whether the kid will start school in the following year (2011) or later.

An important aspect of the evaluation was that during the course of the implementation, more and more families had already matriculated their students at a school. Understandably the treatment is expected to have a much smaller impact on school choice decisions for families who have already made their decisions. It is also reasonable to expect that the timing of matriculation could

[15] Table A1 presents the coefficients and standard errors for regressions of each school characteristic listed on treatment status. Table A2 shows the coefficients and standard errors for regressions of each characteristic of the families in our sample on treatment status.

be correlated with the characteristics of the family.[16] Matriculating early does seem to be correlated with some observable characteristics associated with slightly higher SES (possession of durable goods), but we see no difference between groups in other SES family characteristics (except for a marginally higher probability of being born at a hospital) that can affect school choice and with other background characteristics of the children that have been shown to affect academic achievement (Almond and Currie, 2011; Bharadwaj et al., 2017). Going back to Table A2, we see in columns (3) to (6) that there are no systematic differences across treatment and control groups when we restrict our sample to the families that were and were not enrolled in the baseline. This is because treatment and control groups are balanced across time by design. We present all results for the pooled sample as well as for the sample that has not yet matriculated.

The experiment is designed to compare the school choices of families in treated groups to the choices of control groups. The outcomes in the short run include the administrative data about the characteristics of the schools chosen such as price, distance, and measures of school inputs and outputs. In the medium term, we look at student own outcomes on test scores when students are in 4th grade and take their own standardized tests.

4.2 Results of the Randomized Control Trial

Table 1 shows a summary for the main the results of the effect of the treatment on the characteristics of the schools chosen by families (columns 1 to 6) and on the academic results of the students some years after the experiment took place (columns 7 and 8). These are our most preferred specifications for each variable and include controls for randomization units[17], and correspond to the coefficients in the odd columns for other tables. We present subsample analyses by matriculation status at the time of treatment.[18] The online appendix explores a series of alternative specifications with expanded controls, including a list of variables measuring family socioeconomic status and student health.

Column 1 in Table 1 shows the impact of the treatment on the distance from the family location to the school chosen. If we look at the full sample in panel A, we see that treated families travel 0.13 additional km. to attend school, a significant treatment effect of approximately 0.1 standard deviations. However, as we see in panel C, most of this effect comes from a significant and positive treatment effect for families that were not enrolled in the baseline, with an order of magnitude of

[16] Table A3 shows the coefficients and standard errors for regressions of each characteristic of the families in our sample on enrollment status at baseline.

[17] These include market characteristics such as the number of schools nearby, the average, the standard deviation and percentiles 25, 50 and 75 of test scores of schools nearby, as well as municipality fixed effects.

[18] Please note that from the original 1,832 students in our sample, we only have information for 1,612 observations on whether they were enrolled or not at the time of the intervention. That is the reason why the number of observations in the pooled regression and the sum of the ones that separate by enrollment do not coincide.

Table 1: Summary - Effects of the Treatment

	Characteristics of Chosen Schools						Student Own Test Scores	
	Distance	Positive Price	Lang 2nd	Lang 4th	Math 4th	V. Added	Lang 4th	Math 4th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Treatment	0.1371**	0.0438	0.0108	0.0107	0.0147	0.0274	0.0617	0.1298**
	(0.0595)	(0.0354)	(0.0224)	(0.0275)	(0.0293)	(0.0273)	(0.0612)	(0.0556)
N obs.	1,378	1,775	1,758	1,752	1,752	1,752	1,443	1,442
Panel B: Already enrolled								
Treatment	-0.0843	0.0091	-0.0123	-0.0097	-0.0348	-0.0320	-0.1247	-0.0635
	(0.1234)	(0.0522)	(0.0430)	(0.0489)	(0.0570)	(0.0496)	(0.1211)	(0.1036)
N obs.	487	596	589	590	590	590	506	495
Panel C: Not enrolled								
Treatment	0.2390***	0.1198***	0.0591**	0.0377	0.0658*	0.0718**	0.2163**	0.2210***
	(0.0658)	(0.0399)	(0.0268)	(0.0323)	(0.0386)	(0.0345)	(0.0898)	(0.0723)
N obs.	780	975	967	961	961	962	772	779

Note: Randomization controls are used, which include market characteristics of schools (number and test scores mean, standard deviation and percentiles 25, 50 and 75.). Column (1) restrcits observations to students travelling less than 4 km. Value Added in column (6) corresponds to version 4 in Appendix Table 3.

0.2 standard deviations. It is important to note that in this analysis we impose two restrictions on our sample.[19] We restrict the sample to families for which the municipality in which their geocoded location coincides with the municipality on which the family reported living in the administrative data from the Ministry of Education. Additionally, we only consider families that are located less than 4 km away from the school to which the kid attends.[20] As a robustness check, we look at the treatment effect on distance for several maximum distance restrictions in Figure A1.

Column 2 in Table 1 shows the impact of the treatment on whether the family went to a school that would charge them a positive price beyond the voucher. We see here students are slightly more likely to attend schools that charged additional top off fees than students in the untreated group.

Columns 3-6 show the impact of the treatment on the quality of the school chosen by families, measured using the mean math and language test scores for the school available at 2nd and 4th grade. If we look at the full sample, we see that there is a significant effect on the math test scores of the schools chosen. If we look at panel C, which shows the results for the subsample of students that were not already enrolled at the moment of the intervention, we find larger but only marginally significant effects, probably because of a smaller sample. This provides some evidence that our intervention pushed parents to choose schools with higher test scores. It is interesting to

[19] We do this because some parents may have moved from their location at the moment of the baseline survey, or there can be measurement error in the exact location because of limitations in the geocoding process of the address that are beyond our control.

[20] (Neilson, 2017) geocodes the addresses of a large sample of students in Chile and finds that 4 km is in the 99th percentile of the distribution of distance traveled.

note that the test scores are correlated with value-added measures and other proxies for quality such as teacher quality and parents satisfaction.[21]

The last two columns present the effects on individual tests scores in 4th grades, four to five years after the treatment took place. For the sample of students that were not enrolled at the moment of the treatment, we see positive and significant impacts on virtually all specifications. This is an important result because it provides evidence that the policy changes not only behavior, but also changes outcomes. Table A5 in the Appendix presents results for additional specifications.

The results presented in this section suggest that the intervention does indeed shift families' school choice towards schools of better quality, in spite of the fact that they can be farther away and are more likely to charge positive prices. The results on student own test scores indicate the policy shifts students to schools that will help them learn more. The intervention is of low cost and easy to scale-up by design, suggesting a policy expanding this intervention could lead the education system to be more efficient by moving students to more productive schools and learning more.

5 Empirical Model of for Policy Analysis

5.1 Empirical Model of School Choice with Incomplete Information

The utility that family i derives from sending their children to school j in time t is a linear function of the school observable and unobservable characteristics. To simplify the notation, we drop the time subscript t from the demand model. The observable characteristics include quality, q_j, the out-of-pocket price for them, op_{ij}, the proximity of the school to the location of the family, d_{ij}. Other observable characteristics at the school level, x_j^r, are the school administration type (public, voucher or private), religious orientation, co-education and type of corporation (for-profit or not-for-profit). Families share a common preference for unobservable characteristics of the school, ξ_j. Finally, each family i has a random iid preference shock for school j, ϵ_{ij}. Preferences over quality, price and location are heterogeneous across family observable discrete type k.

Family i's utility from sending their children to school j will be:

$$U_{ij} = \beta_k q_j - \alpha_k p_{ij} + \lambda_k d_{ij} + \sum_r \eta_k^r x_j^r + \xi_j + \epsilon_{ij} \tag{9}$$

Families have incomplete information about school quality, price and distance and must choose a school based on their beliefs formed given potentially heterogeneous information sets $\mathcal{I}_i$. We assume that the true distribution for quality is known $q_j \sim N(0, \sigma_q^2)$, but families only observe a

[21] See the Online Appendix for more descriptive evidence regarding the correlation between value added measures, test score outcomes, school inputs and parent satisfaction.

noisy signal which correponds to the true quality plus an error distributed $v_{ij} \sim N(0, \sigma^2_{k,\epsilon})$. The expected quality would be: $q^e_{ij} = \rho_k(q_{ij} + v_{ij})$, where $\rho^q_k = \frac{\sigma^2_q}{\sigma^2_q + \sigma^2_{k,\epsilon}}$. Beliefs about prices and distance have a similar form with varying ρ^p_k and ρ^d_k given these maybe more or less observable.

$$U^{\mathcal{E}}_{ij} = \phi^q_k q_j - \phi^p{}_k p_{ij} + \phi^d{}_k d_{ij} + \sum_r \eta^r_k x^r_j + \xi_j + \widetilde{\epsilon}_{ij} \tag{10}$$

The reduced form parameters ϕ represent the weight placed on the true quality, price and distance that are weighted by the precision of the signal. For example $\phi^p_k = \alpha_k \rho^p_k$ and $\phi^q_k = \beta_k \rho^q_k$. Residual terms derived from signals is accumulated in the idiosyncratic term $\widetilde{\epsilon}_{ij}$ which for simplicity is assumed to have an extreme value distribution.

It is important to pause to note that the role of incomplete information in this setting is to modify the weight families place on school characteristics. The more noise associated with the signals about a school characteristic, the lower the weight placed on that characteristic $\frac{\partial \phi}{\partial \sigma^2_\epsilon} < 0$. This allows for the model to accommodate differences in choice produced by systematic differences in the precision of the signals across socioeconomic groups. This, in turn, opens a role for the information treatment to play a part in shifting choices.

The families will choose school j to maximize their expected utility $U^{\mathcal{E}}_{ij}$ based on their information and their choice set J_m which we assume includes all schools in the market m. The following expression describes the share of families of type k who live at node n who will select school j:

$$s^{nk}_j(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi}, \mathcal{I}) = \sum_{i=1}^{N_{v_i}} w_{v_i} \left(\frac{\exp(\phi^q_k q_j - \phi^p{}_k p_{ij} + \phi^d{}_k d_{ij} + \sum_r \eta^r_k x^r_j + \xi_j)}{\sum_{\ell \in J_m} \exp(\phi^q_k q_\ell - \phi^p{}_k p_{i\ell} + \phi^d{}_k d_{i\ell} + \sum_r \eta^r_k x^r_\ell + \xi_\ell)} \right) \tag{11}$$

In practice, we will define discrete family types based on: (i) their poverty status -poor or non poor-, (ii) the level of education of the mother -incomplete high school, complete high school or college-. The market definition joins all urban areas that are five kilometers apart or less at their closest point, and this union of areas will define one market. The assumption is that these areas are close enough like for these students to feasibly travel within them. Each market is comprised of a total of N students who live on the discrete set of N^m nodes. In order to get the market level shares, we need to aggregate over the distribution of students of each type across the nodes in the city and across the distribution of students across nodes. The distribution of students of type k across nodes is given by the vector w^m_k with $\sum_n^{N^m} w^m_{nk} = 1 \quad \forall k$. The proportion of the students in the market who are of type k is given by π^m_k where $\sum_{k=1}^K \pi^m_k = 1$ so that average school quality for students k is given by Equation 12 and market shares for each school are given by Equation 13.

$$\mu_q^{\text{type}} = \sum_{j \in J^m} \sum_{n \in N^m} q_j \cdot s_j^{nk}(q, p, \xi, \mathcal{I}) \cdot w_{nk}^m \tag{12}$$

$$s_j(q, p, \xi, \mathcal{I}) = \sum_{k}^{K} \sum_{n}^{N_m} s_j^{nk}(q, p, \xi, \mathcal{I}) \cdot w_{nk}^m \cdot \pi_k^m \tag{13}$$

We will assume that all the effect of the treatment is through changes in the information set available to treated families. This implies treatment affects ϕ^q, ϕ^p, and ϕ^d differentially across types k that had different priors before treatment. We can expand the types described above to incorporate the families in the RCT (types 1, 2, 3 and 4) and generate new *treated types* who received the treatment. Thus we add 6 parameters to the model that modify the weight given to each school characteristic (φ_k^q, φ_k^p, φ_k^d) for $k = 1, 2, 3, 4$.

5.2 Supply side

Let the marginal cost of providing education be linear in a vector of cost characteristics. We decompose these characteristics into a subset of variables that are observed by the econometrician, which includes the quality chosen q_{jt} as well as other characteristics of the school that can potentially affect costs[22] which are included in the vector w_j^l, and an unobserved component that affects the marginal cost of rising quality, ω_{jt}. We include the t time subscript to highlight the fact that some components of the cost function vary over time and others are fixed. We allow the unobservable to have a school-specific fixed component $\overline{\omega_j}$ and a time-school-specific component $\Delta\omega_{jt}$ which we interpret as a shock. Then, $\omega_{jt} = \overline{\omega_j} + \Delta\omega_{jt}$ and can be potentially correlated with ξ_{jt}, as schools with higher unobserved quality may also face different marginal costs for increasing observable quality. We define the marginal cost of school j as:

$$MC(q_{jt}) = \sum_{l} \gamma_l w_j^l + (\gamma_q + \omega_{jt})\, q_{jt} \tag{14}$$

We model schools' behavior by assuming that their objective function is to maximize profits[23]. The profit function for a school j in a market with N students is given by:

$$\pi_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi}) = N s_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi}) \left(v + p_{jt} - MC(q_{jt})\right) - F_{jt} \tag{15}$$

[22] For example, characteristics like the type of administration, religious orientation, whether the school is for profit, among others.

[23] In future versions we plan to allow public schools to have a different objective function in which they care about profits but also overall quality, with the objetive of estimating the relative weight that they put to this two objectives.

Schools maximize profits by choosing price and quality. However, we will focus on the first order conditions for quality, as the incentives generated by the voucher and changes in regulation of school prices complicates the way in which we think about school pricing decisions. As we only have one shock per market-year-school observation, we only need one equation per market-year-school observation to identify the paraments.

Schools choose quality by comparing the marginal benefit of attracting more students relative to the marginal increase in the costs. The first order condition is:

$$\frac{\partial \pi_{jt}(\mathbf{q},\mathbf{p},\boldsymbol{\xi})}{\partial q_{jt}} = N\frac{\partial s_{jt}(\mathbf{q},\mathbf{p},\boldsymbol{\xi})}{\partial q_{jt}}\left(v + p_{jt} - MC(q_{jt})\right) - Ns_{jt}(\mathbf{q},\mathbf{p},\boldsymbol{\xi}) \cdot \frac{\partial MC(q_{jt})}{\partial q_{jt}} = 0 \tag{16}$$

And the expression for quality is:

$$q^*_{jt} = \underbrace{\left[\frac{v + p_{jt} - \sum_l \gamma_l w^l_{jt}}{\gamma_q + \omega_{jt}}\right]}_{\text{Competitive Quality}} \underbrace{-s_{jt}(\mathbf{q},\mathbf{p},\boldsymbol{\xi})\left[\frac{\partial s_{jt}(\mathbf{q},\mathbf{p},\boldsymbol{\xi})}{\partial q_{jt}}\right]^{-1}}_{\text{Quality Mark Down}} \tag{17}$$

5.3 Estimation

We have to estimate three sets of parameters: the linear parameters in the utility function ($\theta_1 = \eta$), the non-linear parameters in the utility function ($\theta_2 = (\phi, \varphi)$) and the marginal cost function parameters ($\theta_3 = \gamma$), which also include the vector of school fixed effects for the marginal cost of quality ($\overline{\omega_j}$). Our estimation is done in three steps which are detailed below.[24]

5.3.1 First Step: Demand Parameters Estimation

In the first step, we estimate the parameters (θ_1, θ_2) following Berry (1994), Berry et al. (1995), Petrin (2002), Berry et al. (2004) and Neilson (2017). We combine aggregate moments to get the unobservable quality for each school, micro moments to approximate the heterogeneity in preferences across different types of families, and IV (demand) moments to deal with endogeneity. We describe each set of moments in detail below. To implement the estimation we use the MPEC approach as described by (Dubé et al., 2012). See the Appendix A2 for a detailed representation of the optimization problem.

First, we use *aggregate moments for the shares*. These moments make us choose the parameters such that for each year and school the model matches the predicted school market shares to ob-

[24]For som estimation specifics and a discussion on how we calculate standard errorse see the Appendix A2

served shares, what will help us identifying the unobservable school quality (ξ) parameter. We can summarize them as:

$$G^1(\theta_2) = s_{jt} - s_{jt}(\theta_2) \tag{18}$$

Where $s_{jt}(\theta_2)$ is the expression in Equation 13. In these aggregate share calculations will not consider the treated types -we are assuming there are no general equilibrium effects-. Then, the vector $\boldsymbol{\pi}^m$ will be such that:

$$\sum_{k=1}^{6} \pi_k^m = 1 \text{ and } \quad \pi_j = 0, j = 7, 8, 9, 10$$

Second, we use *micro moments* as in Petrin (2002) and Berry et al. (2004). These moments will help us choose parameters such that the expected characteristics of the chosen schools (in terms of quality, price and distance) match the true chosen characteristics. In each market, period, type $k = 1, .., 6$, we define these expectations as:

$$E(d|k,t,m); \quad E(p|k,t,m); \quad E(q|k,t,m) \qquad \forall\, t, m \quad \text{and } k$$

These moments are particularly useful for identifying the heterogeneity of preferences for observed school characteristics by observed family types. From the microdata we have N_m observations in market m of students identified as type k at time t and their choices. Then, we can use the empirical averages of the quality, price and distance chosen by these families to approximate the expectations in the expressions above. The expectation for each characteristic given the parameters of the model can be obtained from the distributions of student of each type in each node across schools in the market. The comparison between these two values define moments for price, quality and distance:

$$G_q^2(\theta_2) = \sum_{i \in N_k^m} q_{ik} - \sum_{n}^{N_m} \sum_{j}^{N_m^f} s_j^{nk}(\theta_2) \cdot w_{nk}^m \cdot q_{jn} \tag{19}$$

$$G_p^2(\theta_2) = \sum_{i \in N_k^m} p_{ik} - \sum_{n}^{N_m} \sum_{j}^{N_m^f} s_j^{nk}(\theta_2) \cdot w_{nk}^m \cdot p_{jn} \tag{20}$$

$$G_d^2(\theta_2) = \sum_{i \in N_k^m} d_{ik} - \sum_{n}^{N_m} \sum_{j}^{N_m^f} s_j^{nk}(\theta_2) \cdot w_{nk}^m \cdot d_{jn} \tag{21}$$

where N_m^f are schools in each market m.

In third place, we include *IV-demand moments*. Noting that ξ_j is correlated with both q_j and p_j, we deal with the endogeneity problem using an IV strategy that follows Berry et al. (1995). We define instruments taking advantage of the variation of costs across markets and changes to the voucher policy over time. These moments express an orthogonality condition between the demand side unobservable ξ_j and the chosen instruments.

$$G^3(\theta_2) = \quad \xi_j \quad \cdot IV' \tag{22}$$

We need instruments that are related to price and quality but not related to the unobserved quality of the school. The instruments include cross-market cost shifters such as teacher wages in each market, which we obtained using IRS data. We use the baseline voucher which varies across time. Finally, we also use the variation in prices that is induced by the SEP policy. This policy effectively eliminated prices at a significant number of schools for almost half of all students. The change in prices induced by this policy affect equilibrium prices and quality for all students through schools first order conditions. This equilibrium effect occurs differentially across neighborhoods that have more or less concentration of eligible students, so the timing of the policy is interacted with the concentration of eligible students around the school.

5.3.2 Second Step: Estimation of Parameters for Treated Types

We estimate the parameters in (φ) in a second step using a new set of moments which we call the *RCT Moments*, conditional on the demand estimates obtained in the first step.

With this set of moments we exploit the random assignment of the treatments. The idea is that the additional parameters for the treated types should replicate the treatment effects that we find in the reduced form, in terms of the quality, price and distance of the schools chosen by treated and non treated families, conditional on their family type. In particular, the moments will match the difference of the characteristics chosen between the control and treated families. These moments are useful at identifying the effect of the treatments in preferences for specific attributes.

We have two moments for each characteristic (one for each family type given by mother's education, incomplete high school complete high school):

$$G^4_{q,k}(\theta_2, \theta_2^T) = \quad \hat{\beta}_k - \hat{\beta}_k{}^{sim} = (X'M_DX)^{-1}X'M_DQ_k - (X'M_DX)^{-1}X'M_DQ_k{}^{sim} \tag{23}$$

$$G^4_{p,k}(\theta_2, \theta_2^T) = \quad \hat{\alpha_k} - \hat{\alpha_k}{}^{sim} = (X'M_DX)^{-1}X'M_DP_k - (X'M_DX)^{-1}X'M_DP_k{}^{sim} \tag{24}$$

$$G^4_{D,k}(\theta_2, \theta_2^T) = \quad \hat{\lambda_k} - \hat{\lambda_k}{}^{sim} = (X'M_DX)^{-1}X'M_DD_k - (X'M_DX)^{-1}X'M_DD_k{}^{sim} \tag{25}$$

Where X is a vector with the treatment indicator, Q, Q^{sim} are the vector of true and simulated qualities for each experiment observation (analogous for price and distance). The matrix M_D transforms the data to include pre-school municipality fixed effects (the level of stratification of the original randomization).

5.3.3 Third Step: Supply Parameters Estimation

Finally, we estimate supply side parameters (θ_3) using *IV-Supply Moments* that exploit the orthogonality between unobserved costs and the instruments, together with the panel nature of the data.

We obtain an expression for the unobserved costs from the school first order conditions. As mentioned in the previous section, we will focus in the FOC for quality, as the pricing decision is more complicated in this context because of the voucher. Voucher schools face some restrictions on how much they can charge and many choose not to charge a fee on top of the voucher. The kink solutions generated by these restrictions and by the fact that some schools would even be willing to charge a negative price (given the voucher), which we cannot observe, complicates the way in which we think about pricing decisions. Rearranging Equation 17 we get an expression for the unobservable shock that affects the marginal cost of rising quality:

$$\Delta\omega_{jt} = \frac{v + p_{jt} - \sum_l \gamma^l w^l_{jt}}{\left[q^*_{jt} + s_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi}) \left[\frac{\partial s_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi})}{\partial q_{jt}} \right]^{-1} \right]} - \gamma^q - \overline{\omega_j} \tag{26}$$

For a description of how we estimate the school-specific fixed component of the cost, $\overline{\omega_j}$ see the Appendix A2. We use this expression to create the IV moment for supply:

$$G^5(\theta_2) = \quad \Delta\omega_{jt} \quad \cdot IV' \tag{27}$$

5.4 Parameter Estimates

5.4.1 Demand

Table 2 presents results for the estimated parameters for school choice ϕ and the policy parameters φ. The three first rows show preferences for quality by family characteristics. The estimates for the policy parameters show positive effect for all types and reducing the differences in the weight given to school characteristics across types.

The policy parameter is larger for poor and less educated families, suggesting that although many of those families don't have many options to move to (thus average treatment effects are small), when they do have those options they take them. This is likely due to the fact that less educated families tend to live in areas where high quality schools are more rare.

Table 2: Demand Model Estimates

Panel A: Demand Parameters	
	Estimate
Quality	
NHS Mom	1.3663***
HS Mom	0.1979***
College Mom	0.5239***
Poor	-0.5815***
Treatment (Mother No HS)	0.5626***
Treatment (Mother HS)	0.3430***
Price	
NHS Mom	-9.899***
HS Mom	-2.8413***
College Mom	-0.0001
Poor	-3.3190***
Treatment (Mother No HS)	9.8990***
Treatment (Mother HS)	2.8413***
Distance	
NHS Mom	-0.9928***
HS Mom	-0.7027***
College Mom	-0.3844***
Poor	-0.2178***
Treatment (Mother No HS)	0.3872***
Treatment (Mother HS)	0.1203***
Sigma Preference - Quality	0.1361***

Note: Estimation integrates over v_i^q using Gauss Hermite quadrature.

5.4.2 Supply

Estimated marginal cost presented in Table 3. It can be seen in Figure 3 that firm specific marginal costs of quality are larger for public schools. There are also systematic differences in costs faced by schools in different markets as can be seen in Figure 4.

Table 3: Supply Model Estimates

	Coef.	Std. Error
γ_l		
Voucher	0.1244	0.0137
Public	0.6482	0.0742
For Profit	0.2466	0.0882
Religious	-0.1035	0.0651
Constant (Mean Market FE)	0.4394	-
γ_q		
Constant (Mean Firm FE)	1.5767	-

Figure 3: Firm Level MgC(q)

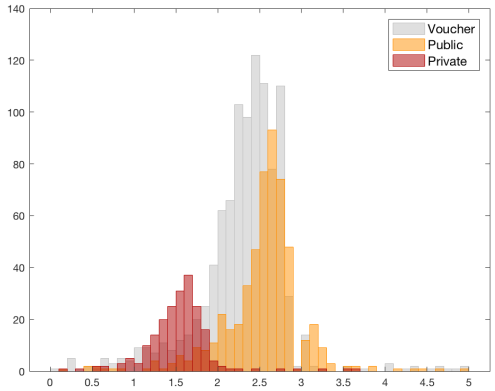
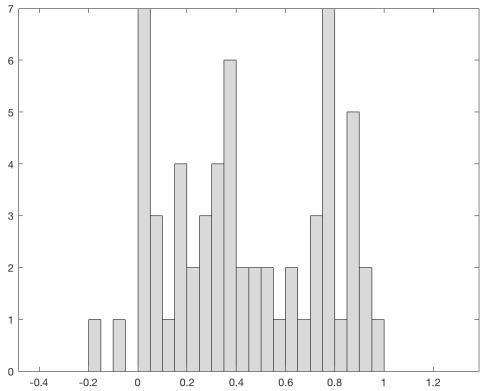


Figure 4: Market Level MgC(q)



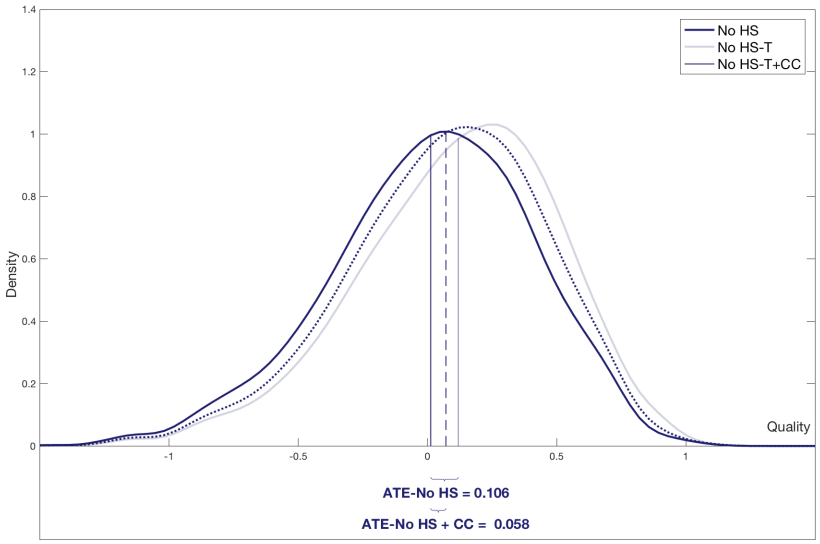
5.5 Counterfactuals

Using the estimated parameters we can use the empirical model to evaluate the potential effects of the policy doing counterfactual simulations. The different counterfactuals will allow us to disentangle between short and long run effects of a policy implemented at scale taking into account both demand and supply side adjustments.

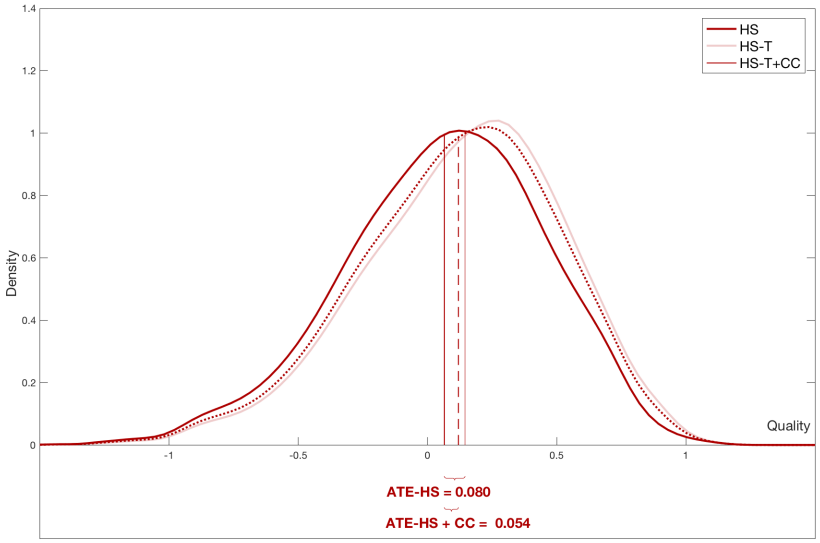
The first counterfactual to explore is the impact of the policy implemented at scale in the very short run. The idea is to extrapolate from the RCT to the population using the estimated demand model parameters. In the very short run we can imagine schools will not react and capacity constraints can bind. To implement this simulation, we will hold the set of schools and their quality, price and capacity fixed and apply the treatment to all students in the market. We simulate preferences for schools for each student in each market. Using these preferences we run the current centralized admissions system algorithm to assign students to schools based on their preferences. Oversubscribed school wait lists are resolved using lotteries as is current policy in Chile. This is useful for simulations of counterfactuals as it limits the ability for schools to change their admissions policy given potential changes in demand.

In this simulation, capacity constraints are expected to bind but we are unsure how quantitatively important this constraint will be. Figure 5 shows the impact on the distribution of school quality with and without the policy. The two panels show the distribution for students with mothers with less than high school (top) and mothers with at most high school (bottom). The mean effects are indicated in the figures but the notable result is that average treatment effects found in the randomized control trial are almost halved when the policy is scaled up and capacity constraints are active.

Figure 5: Distribution of Quality - At Scale Policy - Short Run



(a) No High School Moms' Type, Short Run

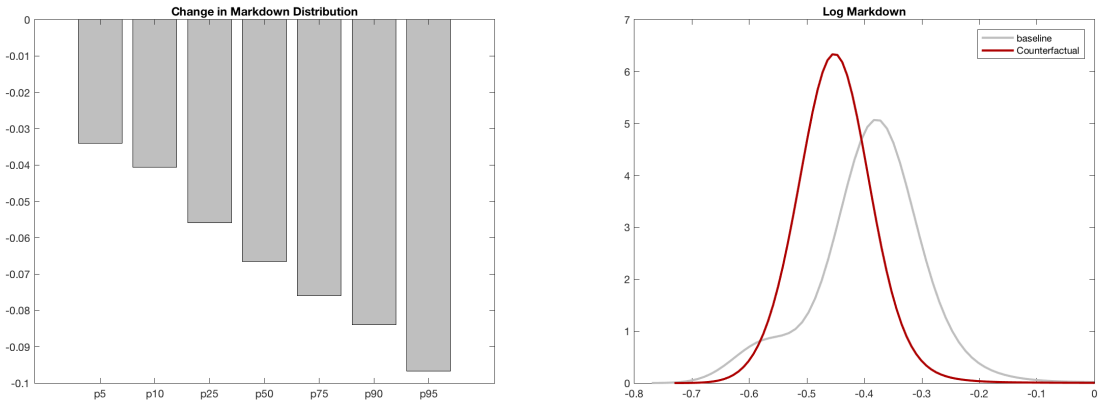


(b) High School Moms' Type

The simulations show lower effects due to congestion when the policy is scaled up. This suggests there could be meaningful effects to school incentives. To explore the extend to which school quality markdowns change when the policy is implemented we plot the distribution of the markdowns with and without the policy on impact in Figure 6. The distribution of change in mark-

downs show significant degree of heterogeneity suggesting that some schools would be faced with larger changes in incentives than others.

Figure 6: Markdown Change - At Scale Policy - Short Run



(a) Percentiles

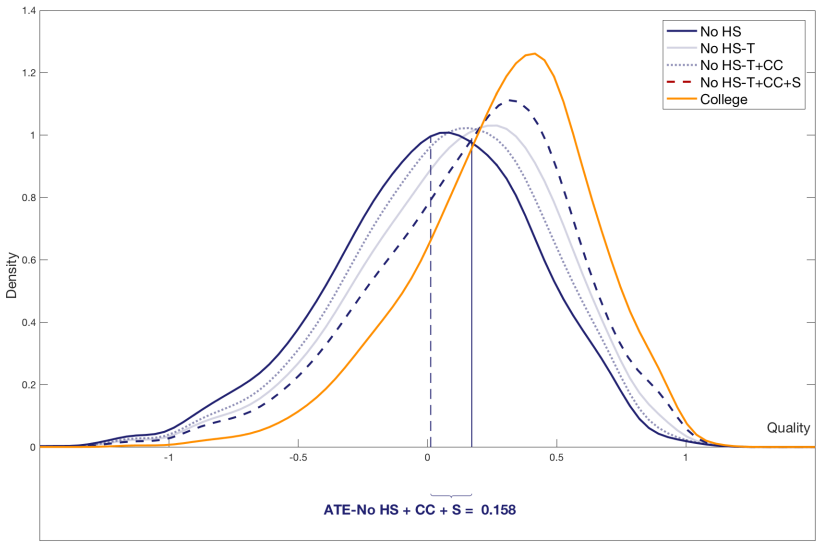
(b) Distribution

This evidence is consistent with recent research emphasizing supply side reactions in education markets Andrabi et al. (2017); Neilson (2017) and suggests exploring to what extent aggregate supply side effects can change effects of the policy. In this context the supply side has many margins it can adjust over a long horizon. We focus on how the changing environment would lead to readjustment of the characteristics of the current schools and ignoring other margins of entry, exit or investments in capacity. To explore the extent to which schools might readjust their characteristics once the policy is in place we conduct a simulation with a scaled up policy and allow school to adjust quality. Prices have now been frozen due to recent policy changes. We calculate the new equilibrium vector of quality when demand shifts in response to the policy implemented at scale. Capacity is held constant so this simulation could be interpreted as a medium run outcome. Figure 5 shows the distribution of quality once the policy is expanded at scale and schools can adjust levels of quality but assignment to schools is still subject to the original capacity constraints.

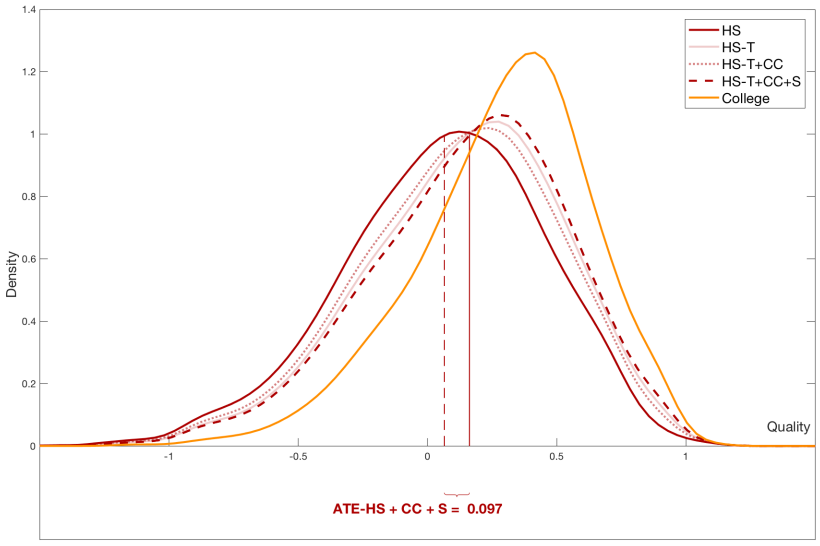
The effects are quite significant and the average treatment effects are similar if not bigger to the effects found in the randomized control trial. These simulations suggest the equilibrium effects will tend to be raised by increasing supply of school quality once families in poor neighborhoods are exposed to the policy and put more weight on school quality when choosing schools.

It is expected that over time, investment in capacity or entry may play a bigger role. However in prior work in the context of Chile, entry/exit margins were not found to be large drivers of change given current market structure and policy that seemingly has excess capacity (Neilson, 2017). Similar to the analysis in Wollmann (2018), this counterfactual focuses on the adjustment of product characteristic and is what we consider a lower bound on supply side effects over time. Capacity constraints and limiting entry both presumably dampen competition that is driving the

Figure 7: Distribution of Quality - At Scale Policy - Supply Side



(a) No High School Moms' Type



(b) High School Moms' Type

change in incentives to invest in quality. This could also represent a medium run approximation to what could be expected to happen given the policy implementation.

The results from the counterfactual analysis are summarized in Table 4. We find that capacity constraints play an important role in limiting the policy effects. We also find evidence that schools will have incentives to improve quality, especially in poor neighborhoods. In practice these aggregate effects imply students not affected directly by the policy (parents did not attend meetings for example) would find the set of schools available to them having higher quality. Table 4

Additional simulations presented in the appendix study repeat the exercise when public schools do not react at all assuming they maximize quality given budget and ignore market conditions otherwise. Another simulation explores what might happen if increased demand for school quality inputs increased marginal costs across the board by 5%,10% and 20%. In all cases we find positive effects although increasing costs and limiting supply side reactions reduce the average policy effect relative to the benchmark in Figure 7.

An important aspect to note is that in these counterfactuals simulations we are leaning on several current institutional aspects that could play a crucial role in the quantitative exercises. One is that applications to schools are processed in a centralized application system and the other is that prices have been fixed. These allow us to ignore potential changes to admissions policies when demand suddenly shifts due to the policy. Estimation is implemented in a stable environment where we assume excess demand is less of an issue as school have had time to adjust price and quality but this assumption seems less reasonable if a large policy change happened suddenly. The second policy fixing prices is also likely to play a role because this shuts down the ability for a high quality school with excess demand to raise prices, potentially dissuading poorer families that value quality more but are still more price sensitive that richer families. While this reduces incentives for high quality firms to increase quality it still leaves low quality schools to have incentives

Table 4: Counterfactuals

	Experiment	Model		
	ATE	ATE	ATE+CC	ATE+CC+S
All (No HS, HS and College)	-	0.063	0.036	0.085
No HS Mon	0.1176	0.106	0.059	0.159
HS Mom	0.0483	0.080	0.054	0.097
College Mom	-	0.000	-0.016	0.012

to increase their quality.

6 Discussion

It is generally understood that an informed consumer demand is an important aspect of a well functioning market. A lack of information can lead individuals to make inefficient choices and this could potentially have aggregate effects on efficiency. In the case of education services, a growing body of evidence from difference contexts suggests that providing information to individual families may indeed shift their school choice decisions and in some cases information provision can have aggragate effects (Hastings and Weinstein, 2008; Andrabi et al., 2017). The prospect of a government policy based on this idea is very attractive given it would have the potential to improve equity and efficiency of the market for educational services at a very low cost. This is especially true in a developing countries where private provision of services like education is common but government supervision and regulation tend to be hard to implement effectively. However, when it comes to designing and implementing government policy, it is not straightforward to extrapolate from the existing evidence from other contexts and there are pitfalls at both the level of individual choices, as well as in the aggregate that could render these policies ineffective.

In this paper we draw upon insights from prior research to develop an intervention that is low cost, scalable and compatible with local political, institutional and logistical restrictions. The specific information intervention developed targets families of public Pre-K students that must transition into elementry school the following academic year. The intervention provides both personalized information about characteristics of schools nearby as well as a broad message emphasizing the feasibility and importance of searching for a school carefully. Using a small scale randomized control trial, we evaluate whether this type of intervention could have any effects on choices and later outcomes. The results provide evidence that an intervention of this type does indeed shift parents school choice decisions towards schools with higher test scores, higher prices and tend to be further distances from their home. Four years later, we find that student achievement and was higher among treated families, suggesting that a policy intervention based around this intervention could be successful in reducing inequality in access to quality schooling in the context of the Chilean system.

Given the evidence from the randomized control trial, we move to quantitatively evaluate the consequences of a scaled up version of this policy. This step is important for policy recomendations given that capacity constraints or supply side reactions could potentially mitigate or eliminate the positive effects found in the small scale trial. To evaluate these posibilities, we embed the randomized control trial within a structural model of school choice and competition where price and quality are chosen endogenously, families have imperfect information about school features and schools face capacity constraints. We estimate the parameters of the model taking advantage

of rich administrative data and variation from recent policy changes, together with the variation generated from the randomized control trial. We estimate parameters of the demand for school choice with and without being treated with information. We also estimate school marginal costs taking advantage of the panel structure of the data to estimate school specific marginal costs to produce quality.

Using the estimated empirical model of school choice and competition, we evaluate the policy effects of an at-scale evaluation when schools do not react, students sort and capacity constraints bind. We then evaluate the effects of supply side reactions in equilibrium under different assumptions regarding how public and private schools react and how costs may vary with the scale of the policy. We find positive effects of the policy that range from 50%-120% of the average treatment effect in the randomized trial, suggesting positive effects overall, but that these are lower than the average treatment effects found in the randomized control trial. In practice the effects on average school quality attended by low socioeconomic families is between $0.06\sigma - 0.22\sigma$. These results suggest that this information intervention can be a cost effective way to improve the efficiency and equity in education markets with high participation of the private sector as in many developing countries.

These conclusions were reached after employing a series of different empirical tools and data sets. Starting with a randomized control trial and then building an empirical model of economic behavior around the intervention to quantify the equilibrium considerations of a large scale policy implementation. Using these tools in combination can be useful to policy makers for several reasons. The first is that evaluating policy through a randomized control trial at scale is often politically or technically not feasible. Small scale RCTs can be more cost effective for discovering what design details seem to work best and then embeded into an empirical model that incorporates the main features of interest (demand and supply in this case), allows for extrapolation to gauge the effects of a large scale implementation. This step can be seen to be a complementary addition to help provide policy recomendations. We would argue that in many cases where a small scale randomized control trial is already planned, adding additional analysis as in this paper is a cost effective way of gaining further insight into potential policy effects and aspects of design in practice.

Studying the quantitative relevance of potential mechanisms when scaling up the policy also highlights several important insights for policy makers beyond determining the average effectiveness of the policy. This is useful for thinking about what issues to look out for, or what modifications might be useful when designing the policy at scale. For example in this application capacity constraints seem to play an important role, especially in the short run. While not explicitly modeled, it is straightforward to imagine that policies to promote investment to expand capacity or allow entry of high quality schools would be complementary to policies that increase demand for quality schooling. In the current policy context in Chile, price controls together with a central-

ized admissions system both seem to help the adjustment in the short run by getting rid of excess demand without increasing prices. However, price controls might also reduce incentives for investment if not accompanied with increases in the voucher subsidy. In essence the intervention is increasing families willingness to pay for school quality at the same time the system is limiting their ability to pay out of pocket to increase investment in quality. Future work should further develop the dynamic effects of regulation on investments in quality to better understand these policy effects.

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Appendix

A1 Appendix Tables and Figures

Table A1: Treatment Balance at the School Level

	Treatment		Constant	
	(1)		(2)	
Enrollment	-1.898	(3.248)	41.467***	(2.530)
Mean attendance	-1.053	(2.430)	28.689***	(1.903)
Mother HE	-0.643	(1.552)	9.495***	(1.320)
Mother HS	-0.915	(2.195)	48.347***	(1.652)
Mother NHS	0.760	(1.010)	7.309***	(0.697)
Q1 Income	0.577	(2.996)	57.970***	(2.348)
Q2 Income	0.288	(2.142)	31.365***	(1.587)
Q3 Income	-1.136	(1.216)	8.752***	(0.930)
Very Poor	0.637	(1.865)	14.947***	(1.406)
Poor	0.083	(2.233)	40.619***	(1.816)

Notes: Column 1 presents the coefficient and standard error for the regression of each variable on treatment. Column 2 presents the coefficient and standard error for the constant term. p-value<10% ** p-value<5% *** p-value<10%.

Table A2: Treatment Balance at the Family Level

	Full Sample (N=1,832)				Enrolled (N=606)				Non-Enrolled (N=1,006)			
	Treatment		Constant		Treatment		Constant		Treatment		Constant	
	(1)		(2)		(3)		(4)		(5)		(6)	
Panel A: SES characteristics												
Household size	0.114	(0.126)	4.837***	(0.101)	0.060	(0.172)	4.852***	(0.144)	0.146	(0.158)	4.829***	(0.118)
Durable goods	0.069	(0.179)	4.558***	(0.160)	0.221	(0.259)	4.694***	(0.231)	-0.023	(0.182)	4.477***	(0.159)
Owns Dwelling	0.022	(0.039)	0.345***	(0.032)	0.116**	(0.058)	0.311***	(0.046)	-0.035	(0.042)	0.366***	(0.036)
Mother head of hh	0.010	(0.027)	0.826***	(0.021)	-0.035	(0.047)	0.857***	(0.029)	0.037	(0.031)	0.807***	(0.025)
Mother NHS	-0.022	(0.031)	0.203***	(0.026)	-0.017	(0.046)	0.194***	(0.038)	-0.025	(0.034)	0.209***	(0.029)
Mother HS	-0.051	(0.039)	0.411***	(0.032)	-0.038	(0.057)	0.378***	(0.046)	-0.059	(0.040)	0.431***	(0.033)
Mother HE	0.029	(0.028)	0.820***	(0.022)	0.041	(0.039)	0.816***	(0.032)	0.022	(0.032)	0.822***	(0.025)
Panel B: Baseline school choice												
Already enrolled	0.002	(0.048)	0.375***	(0.038)	-	-	-	-	-	-	-	-
Another child	-0.012	(0.032)	0.416***	(0.024)	-0.008	(0.051)	0.420***	(0.039)	-0.015	(0.037)	0.414***	(0.029)
Panel C: School choice 2011												
Will apply on 2011	-0.015	(0.042)	0.759***	(0.033)	-0.00778	(0.0512)	0.420***	(0.0389)	-0.0146	(0.0367)	0.414***	(0.0294)

Notes: Columns 1, 3 and 5 present the coefficient and standard error for the regression of each variable on treatment status and Columns 2, 4 and 6 present the coefficient and standard errors for the constant. Combining the baseline and follow up surveys, there are 1,612 observations for which we have information on whether they were enrolled or not at the time of the intervention. p-value<10% ** p-value<5% *** p-value<10%.

Table A3: Balance for Being Enrolled at Baseline

	Enrolled		Constant	
	(1)		(2)	
Panel A: SES characteristics				
Household size	-0.035	(0.108)	4.927***	(0.081)
Durable goods	0.382***	(0.116)	4.461***	(0.080)
Owns Dwelling	0.047	(0.031)	0.343***	(0.019)
Mother head of hh	0.001	(0.030)	0.832***	(0.014)
Mother NHS	-0.010	(0.022)	0.192***	(0.016)
Mother HS	-0.039	(0.025)	0.391***	(0.019)
Mother HE	0.007	(0.019)	0.836***	(0.016)
Poor	-0.013	(0.016)	0.895***	(0.011)
Another child in primary	0.010	(0.029)	0.405***	(0.018)
Panel B: Birth characteristics				
Gestation Weeks	-0.019	(0.094)	38.751***	(0.056)
Birth Weight	-3.982	(25.338)	3,342.137***	(15.176)
Mother's Age	0.329	(0.364)	25.332***	(0.232)
Father's Age	-1.646	(1.217)	36.472***	(0.933)
Marital Status	-0.021	(0.023)	1.735***	(0.014)
Doctor	-0.011	(0.024)	0.333***	(0.015)
Hospital	0.014*	(0.008)	0.959***	(0.006)
Number of Children	0.102	(0.087)	1.871***	(0.035)

Notes: Column 1 presents the coefficient of the regression each variable on an indicator for being enrolled at baseline. Regressions include the observations for which there is data on baseline enrollment (N=1,612). p-value<10% ** p-value<5% *** p-value<10%.

Figure A1: Treatment Effects on Distance by Bandwidth

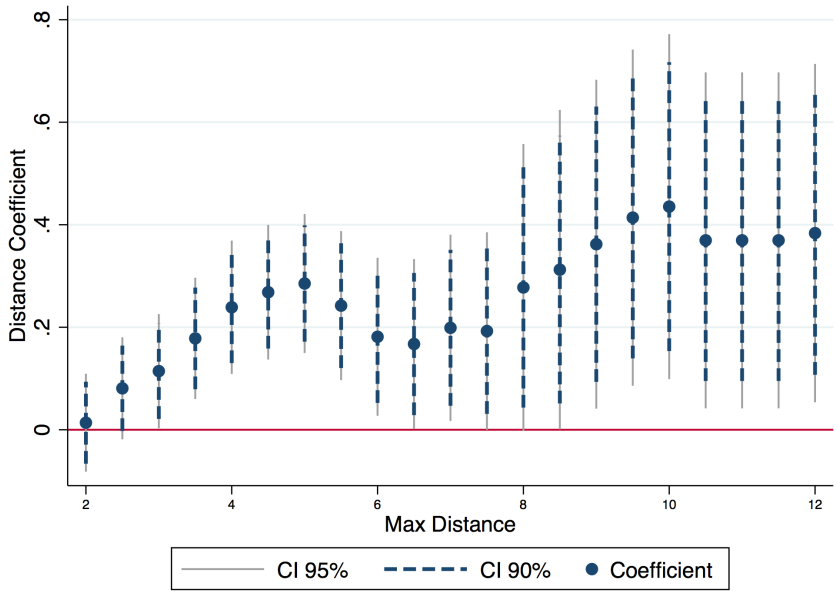


Figure A2: Markdown distribution in the Map - Santiago

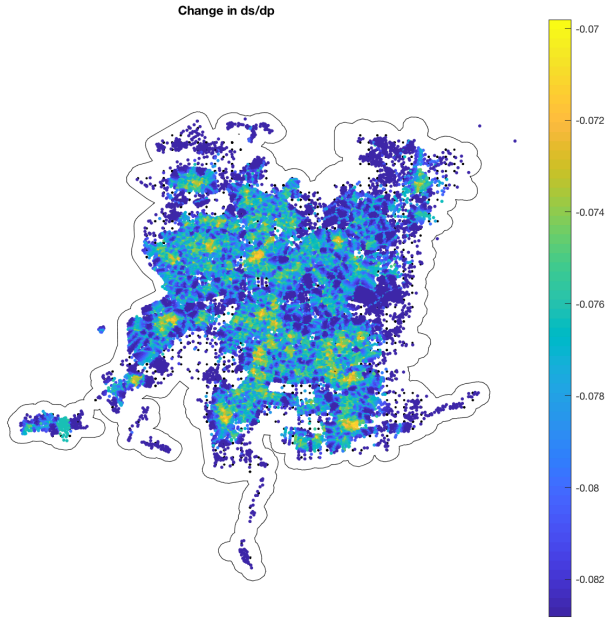


Table A4: Effect of Treatment on Value Added Chosen

	VA 1 (1P)		VA 2 (2P)		VA 3 (1E)		VA 4 (2E)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Treatment	0.027	0.023	0.025	0.022	0.027	0.023	0.027	0.024
	(0.029)	(0.027)	(0.028)	(0.026)	(0.029)	(0.027)	(0.027)	(0.026)
N obs.	1744	1744	1744	1744	1752	1752	1752	1752
Panel B: Enrolled sample								
Treatment	-0.034	-0.050	-0.035	-0.052	-0.033	-0.049	-0.032	-0.048
	(0.051)	(0.051)	(0.050)	(0.050)	(0.051)	(0.051)	(0.050)	(0.050)
N obs.	590	590	590	590	590	590	590	590
Panel C: Not enrolled sample								
Treatment	0.067*	0.064*	0.068*	0.066*	0.071**	0.068*	0.072**	0.069**
	(0.036)	(0.036)	(0.036)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
N obs.	956	956	956	956	962	962	962	962
Randomization controls	×		×		×		×	
Expanded controls		×		×		×		×

Note: Randomization controls include market characteristics of schools (number and test scores mean, standard deviation and percentiles 25, 50 and 75.). Expanded controls include Mother's education, household information (size, durable goods, owned house), baseline school choice information.

Table A5: Effect of Treatment on Student Test Scores

	Lang - 2nd		Average - 4th		Lang - 4th		Math - 4th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Treatment	-0.013	-0.033	0.104*	0.075	0.056	0.014	0.128**	0.112**
	(0.049)	(0.049)	(0.054)	(0.048)	(0.061)	(0.054)	(0.056)	(0.052)
N obs.	1581	1581	1478	1478	1449	1449	1447	1447
Panel B: Enrolled sample								
Treatment	-0.128	-0.157	-0.093	-0.138	-0.125	-0.189	-0.064	-0.079
	(0.102)	(0.100)	(0.101)	(0.101)	(0.121)	(0.121)	(0.104)	(0.104)
N obs.	537	537	509	509	506	506	495	495
Panel C: Not enrolled sample								
Treatment	0.107	0.092	0.226***	0.190***	0.204**	0.156**	0.218***	0.196***
	(0.069)	(0.070)	(0.074)	(0.067)	(0.089)	(0.079)	(0.073)	(0.069)
N obs.	855	855	798	798	778	778	784	784
Randomization controls	×		×		×		×	
Expanded controls		×		×		×		×

Note: Randomization controls include market characteristics of schools (number and test scores mean, standard deviation and percentiles 25, 50 and 75.). Expanded controls include Mother's education, household information (size, durable goods, owned house), baseline school choice information.

A2 Estimation Specifics

In the first step, we implement the estimation of the parameters (θ_1, θ_2) by using the MPEC approach. This method exploits the sparsity structure of the Jacobian of the market share equations,as the unobserved qualities affect the demand of other products in the market but not the demand for products in other markets. The method includes the unobserved qualities as addi-

tional parameters to be estimated. The optimization problem that we solve is:

$$(\theta_1^*, \theta_2^*, \xi) = \text{argmin}_{\theta_1,\theta_2} \begin{bmatrix} g_2 \\ g_3 \end{bmatrix}' \begin{bmatrix} W_{MM} & 0 \\ 0 & W_{IV-D} \end{bmatrix} \begin{bmatrix} g_2 \\ g_3 \end{bmatrix} \tag{28}$$

Subject to the following constraints:

$$\begin{aligned} (M(\delta, \theta_2) - \bar{M}) - g_2 = 0 \quad & \text{Micro moments from school choice decision} \quad (i) \\ \begin{bmatrix} \omega(\theta_2) \end{bmatrix}' \cdot IV - g_3 = 0 \quad & \text{IV moments} \quad (ii) \\ \delta - s^{-1}(\bar{S}, \theta_2) = 0 \quad & \text{Inner loop} \quad (iii) \\ \xi(\theta_2) - \delta(\theta_2) - f(\theta_1) = 0 \quad & \text{Demand disturbance} \quad (iv) \\ \xi^{norm} = 0 \quad & \text{Normalization restrictions} \quad (vi) \end{aligned}$$

Where $f(\theta_1) = \sum_r \eta_k^r x_{jt}^r$.

In the second step, we estimate φ under the following optimization problem:

$$\varphi^* = \text{argmin}_{\varphi} \; g_4(\theta)' \, W_{RCT} \, g_4(\theta) \tag{29}$$

Subject to the following constraints:

$$\hat{\beta}^{\text{RCT}} - \hat{\beta}^{sim} - g_3 = 0 \quad \text{RCT moments} \quad (i)$$

In the third step, we estimate supply side parameters (θ_3). To do so, we need to ger an expression for $\Delta\omega_{jt}$

When we rearrange Equation 17 we get an expression for the nobserved component that affects the marginal cost of rising quality :

$$\omega_{jt} = \underbrace{\frac{v + p_{jt} - \sum_l \gamma^l w_{jt}^l}{\left[q_{jt}^* + s_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi}) \left[\frac{\partial s_{jt}(\mathbf{q}, \mathbf{p}, \boldsymbol{\xi})}{\partial q_{jt}} \right]^{-1} \right]}}_{A_{jt}} - \gamma^q \tag{30}$$

Where $\omega_{jt} = \overline{\omega_j} + \Delta\omega_{jt}$. We use two strategies to identify the supply parameters. First, we exploit the panel nature of our data to estimate the fixed unobservable that impacts the marginal cost of quality $\overline{\omega_j}$.

To do so, lets name the first term at the right side of Equation 30 A_{jt}. For a given set of parameters γ_l, we can calculate the expression A_{jt} for every school-year combination and take the mean.

we also redifine A_{jt} as $A_{jt} = \overline{\omega_j} + \Delta\omega_{jt} + \gamma^q$.

$$\overline{A_j} = \frac{\sum_{t=1}^{T} \overline{\omega_j} + \Delta\omega_{jt} + \gamma^q}{N_T} = \overline{\omega}_j + \gamma^q$$

We can rearrange the expression in Equation 30 and substract $\overline{A_j}$ at both sides:

$$\omega_{jt} + \gamma^q - \overline{\omega}_j + \gamma^q = A_{jt} - \overline{A_j}$$

$$\Delta\omega_{jt} = A_{jt} - \overline{A_j}$$

our optimization problem for the third step will be:

$$\theta_3^* = \text{argmin}_{\theta_3}\ g_5(\theta)'\ W_{IV-S}\ g_5(\theta) \tag{31}$$

Subject to the following constraints:

$$w(\hat{\theta_2}) - h(\theta_3, s(\hat{\theta_2}), \nabla s(\theta_2)^{-1}) = 0 \quad \text{Cost disturbance} \quad \text{(v)}$$

A3 Calculating Standard Errors

The standard errors of the estimated parameters in each step of the estimation procedure are obtained from the variance-covariance matrix for the GMM estimator proposed by **?**. We will discuss how we calculate the standard error for a generic case (the parameters θ and moments $\mathbf{M}$), and then we discuss the case of each set of parameters more specifically. Each one of our GMM estimators is the result of an optimization problem in which the objective function has a quadratic form:

$$\min_{\theta} Q_{obj} = \mathbf{M}' W_m \mathbf{M}$$

For which the gradient is:

$$\frac{\partial Q_{obj}}{\partial \theta} = 2 \cdot \mathbf{J}_M' W_m \mathbf{M}$$

Where $\mathbf{J}_M$ is the Jacobian.

The variance-covariance matrix for a GMM estimator is calculated using the estimator pro-

posed by **?**:

$$\text{cov}(\theta) = (\mathbf{J}_M' W_m \mathbf{J}_M)^{-1} \mathbf{J}_M' W_m \mathbf{V} W_m' \mathbf{J}_M (\mathbf{J}_M' W_m \mathbf{J}_M)^{-1}$$

Where V is the vector for the variance of the moments. For estimating the demand a supply parameters, this variance corresponds to simulation error in our calculations of the models predictions. This element is estimated by simulating the sample moment at the estimate of θ for many independent sets of N_v simulation draws and calculating the variance across the calculated moment vectors. In the case of the parameters that we estimate from the experiment moments, we need to take into account the fact that the variance in our moments is not only affected by simulation error but also by sampling error in the OLS estimator for the treatment effects. As discussed by Berry et al. (2004) the simulation and sampling errors are independent of each other. The RCT moments in Equation 23 take the difference between the estimated treatment effects and our models predictions for it. Then, the variance of the moment conditions can be expressed as the sum of the variances due to sampling and simulation errors. The second one can be estimated as we already mentioned. The variance due to sampling error can be consistently estimated by calculating the variance of the moment conditions at the estimate of the parameter values holding the simulation draws constant.

Online Appendix

OA1 Preschool Provision in Chile

OA1.1 Fundación Integra

Fundación Integra (Integra, here on) is the second largest public supplier of preschools in Chile. It serves more than 72,000 children throughout the country in its 1,000 tuition-free centers. Integra focuses on low-income neighborhoods in order to "[constitute] *a real support for families living in poverty, offering a safe space and an excellence educative program to their children from three months up to four years old*".

Working with Integra provides us with a unique setting to study school choice decisions by providing us with (i) an environment through which we can have access to families that are about to choose primary schools, being relatively confident that the results are not driven by self-selection into primary schools, and (ii) a cost-effective way to implement the intervention. Integra does not offer primary education, so students in the upper level of this program will necessarily have to choose a primary school to continue their education. In addition, working with them provides us with an exceptional opportunity to collect data and deliver interventions using the existing infrastructure of the program. Otherwise, finding families that are about to choose would be very costly to implement.

OA1.2 Selection on enrollment at baseline

The timing in the delivery of information interventions is a key aspect for their effectiveness. Ideally, our treatment should have been delivered before parents decided which school they wanted to send their children to. Unfortunately, we find that an important number of parents in our sample had already enrolled their children in a school at baseline. In most of our reduced form analysis, we distinguish between parents who have already enrolled their children and the ones that have not made their enrollment decision, with the idea that the second sample will give us the causal effects of the intervention when the treatment is delivered on time.

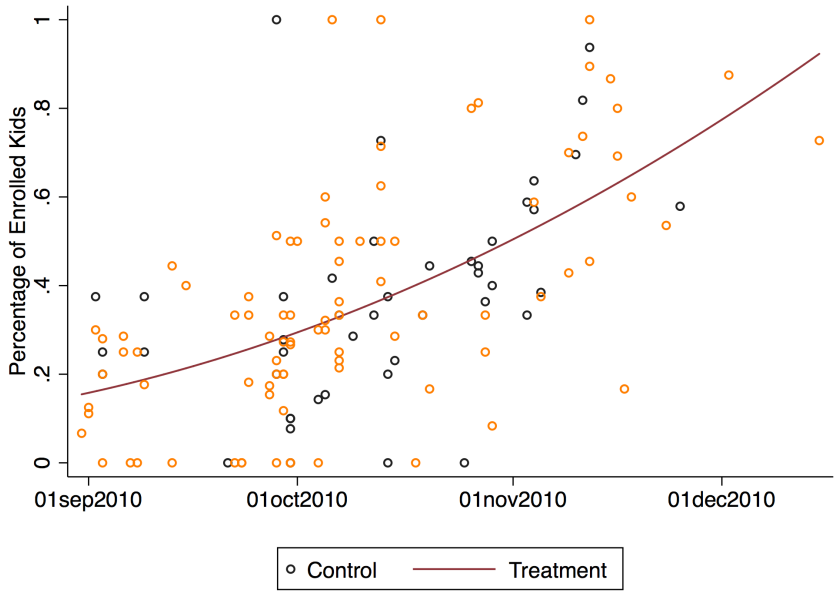
However, our results for the sub-sample of non-enrolled kids may not be generalizable if their parents are different to the ones of the kids that are already enrolled. In this section, we provide evidence that the likelihood of being enrolled in the baseline is mainly driven by the timing o the intervention and that both groups are not different in observable characteristics.

The meetings were conducted between August and December 2010, in a 16 weeks period. Figure OA1 shows the percentage of parents that reported to have chosen a school and already enrolled by the date of the meeting. Schools in which meetings were closer to the end of school

year (December) had a higher share of enrolled parents. While in September a 20% of parents reported having already enrolled their kids, in November this number grew up to 65%.

Table A3 shows balance on observable for being enrolled at baseline. Both SES and birth characteristics are included. Enrolled parents seem to have more durable goods, but are not different in terms of mothers education, poverty status, house hold size or birth outcomes, except for being born at a hospital, for which they are marginally more likely.

Figure OA1: Timing and Probability of Matriculation



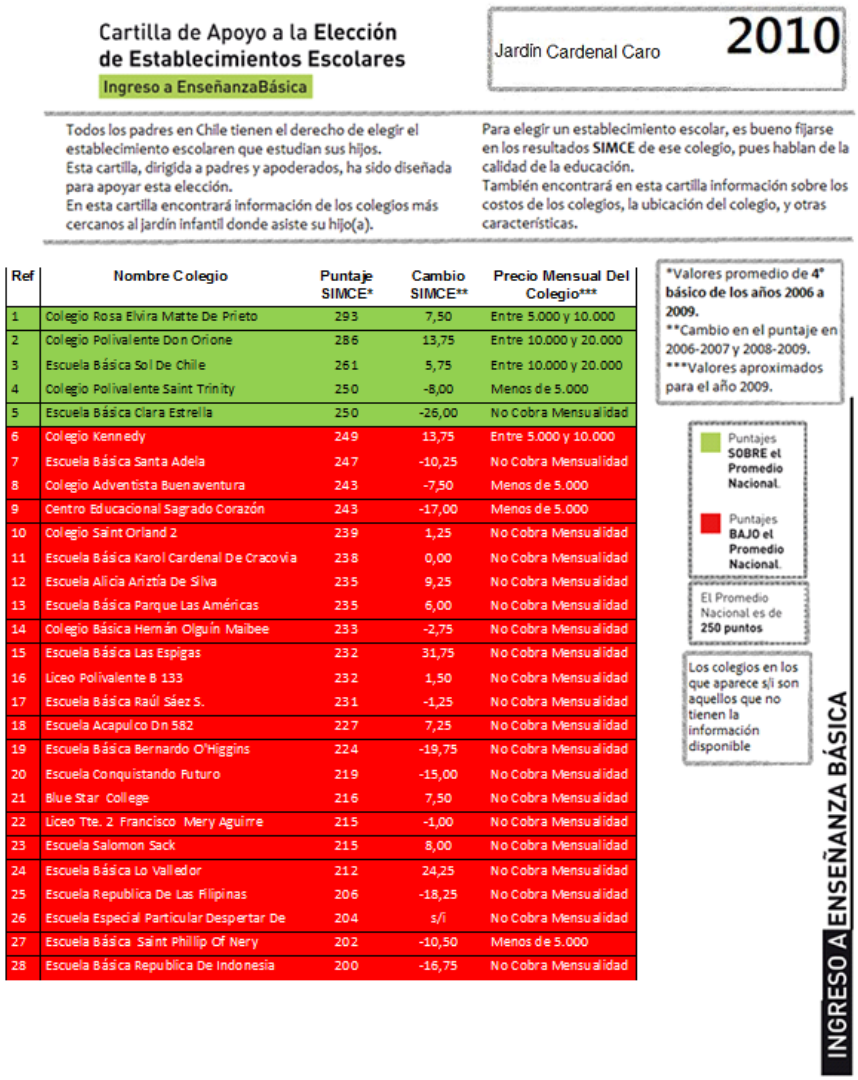
OA1.3 Treatment Design and Implementation

The intervention included two main components. The first was the provision of a Report Card designed for each preschool that included information about a subset of characteristics of the schools located in the same neighborhood[25]. The information provided in the Report Card included: (i) test scores, where to reduce the noise produce by a single observation, we averaged the results on Math and Reading (Spanish) over four years, between 2006 and 2009;(ii) a measure of the change in test scores between years, since a school in the median, but that has largely increased its test scores may be a better (o worst) match for some parents, than a school with the same median score, but that has largely worsened its results; (iii) the official tuition cost for parents, using data

[25] As argued above, we excluded primary schools of higher SES, which generally charge higher fees and have more restrictive selection process, thus are not included in the effective choice set of parents in this context. We were also limited to include up to 30 schools due to space constraints. When a preschool had more than 30 schools within 2 Km. we randomly deleted some schools that were not in the extremes of the Report Card, in order to reduce the bias from presenting a selected part of information.

from the Ministry of Education[26]; (iv)the type of the school (whether it is public or private) and (v) its location (i.e. address). We also provided parents a map with all schools included in the Report Card.

Figure OA2: Report Card - Front



Cartilla de Apoyo a la **Elección de Establecimientos Escolares**

Ingreso a EnseñanzaBásica

Jardín Cardenal Caro **2010**

Todos los padres en Chile tienen el derecho de elegir el establecimiento escolaren que estudian sus hijos.
Esta cartilla, dirigida a padres y apoderados, ha sido diseñada para apoyar esta elección.
En esta cartilla encontrará información de los colegios más cercanos al jardín infantil donde asiste su hijo(a).

Para elegir un establecimiento escolar, es bueno fijarse en los resultados **SIMCE** de ese colegio, pues hablan de la calidad de la educación.
También encontrará en esta cartilla información sobre los costos de los colegios, la ubicación del colegio, y otras características.

Ref	Nombre Colegio	Puntaje SIMCE*	Cambio SIMCE**	Precio Mensual Del Colegio***
1	Colegio Rosa Elvira Matte De Prieto	293	7,50	Entre 5.000 y 10.000
2	Colegio Polivalente Don Orione	286	13,75	Entre 10.000 y 20.000
3	Escuela Básica Sol De Chile	261	5,75	Entre 10.000 y 20.000
4	Colegio Polivalente Saint Trinity	250	-8,00	Menos de 5.000
5	Escuela Básica Clara Estrella	250	-26,00	No Cobra Mensualidad
6	Colegio Kennedy	249	13,75	Entre 5.000 y 10.000
7	Escuela Básica Santa Adela	247	-10,25	No Cobra Mensualidad
8	Colegio Adventista Buenaventura	243	-7,50	Menos de 5.000
9	Centro Educacional Sagrado Corazón	243	-17,00	Menos de 5.000
10	Colegio Saint Orland 2	239	1,25	No Cobra Mensualidad
11	Escuela Básica Karol Cardenal De Cracovia	238	0,00	No Cobra Mensualidad
12	Escuela Alicia Ariztía De Silva	235	9,25	No Cobra Mensualidad
13	Escuela Básica Parque Las Américas	235	6,00	No Cobra Mensualidad
14	Colegio Básica Hernán Olguín Maibee	233	-2,75	No Cobra Mensualidad
15	Escuela Básica Las Espigas	232	31,75	No Cobra Mensualidad
16	Liceo Polivalente B 133	232	1,50	No Cobra Mensualidad
17	Escuela Básica Raúl Sáez S.	231	-1,25	No Cobra Mensualidad
18	Escuela Acapulco Dn 582	227	7,25	No Cobra Mensualidad
19	Escuela Básica Bernardo O'Higgins	224	-19,75	No Cobra Mensualidad
20	Escuela Conquistando Futuro	219	-15,00	No Cobra Mensualidad
21	Blue Star College	216	7,50	No Cobra Mensualidad
22	Liceo Tte. 2 Francisco Mery Aguirre	215	-1,00	No Cobra Mensualidad
23	Escuela Salomon Sack	215	8,00	No Cobra Mensualidad
24	Escuela Básica Lo Valledor	212	24,25	No Cobra Mensualidad
25	Escuela Republica De Las Filipinas	206	-18,25	No Cobra Mensualidad
26	Escuela Especial Particular Despertar De	204	s/i	No Cobra Mensualidad
27	Escuela Básica Saint Phillip Of Nery	202	-10,50	Menos de 5.000
28	Escuela Básica Republica De Indonesia	200	-16,75	No Cobra Mensualidad

*Valores promedio de 4° básico de los años 2006 a 2009.
**Cambio en el puntaje en 2006-2007 y 2008-2009.
***Valores aproximados para el año 2009.

Puntajes **SOBRE el Promedio Nacional.**

Puntajes **BAJO el Promedio Nacional.**

El Promedio Nacional es de **250 puntos**

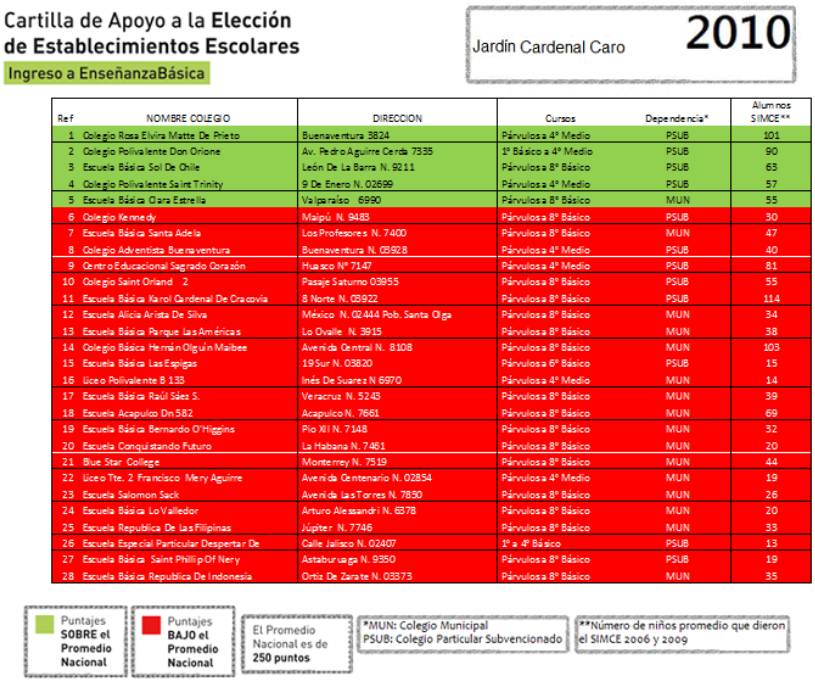
Los colegios en los que aparece s/i son aquellos que no tienen la información disponible

INGRESO A ENSEÑANZA BÁSICA

In order to to send a signal about relatively "good" and "bad" schools, establishments that lied above the nationwide mean test score (roughly 250 points) where signaled in green and schools that lied below the nationwide mean test scores where signaled in red. Figures OA2, OA3 and **??** present an example of the Report Card and a map. The hope is this aspect of the policy will ad-

[26]Note, however, that this is not a perfect measure of what parents actually pay, since there may be other costs, including materials or fees for parents' association. Schools could also offer discounts and scholarships to some students. Since we do not have data on those payments, we included the official co-payment since it is an objective measure and it is comparable across schools.

Figure OA3: Report Card - Back



Cartilla de Apoyo a la **Elección de Establecimientos Escolares**

Ingreso a EnseñanzaBásica

Jardín Cardenal Caro **2010**

Ref	NOMBRE COLEGIO	DIRECCION	Cursos	Dependencia*	Alumnos SIMCE**
1	Colegio Rosa Elvira Matte De Prieto	Buenaventura 3824	Párvulos a 4° Medio	PSUB	101
2	Colegio Polivalente Don Orione	Av. Pedro Aguirre Cerda 7335	1° Básico a 4° Medio	PSUB	90
3	Escuela Básica Sol De Chile	León De La Barra N. 9211	Párvulos a 8° Básico	PSUB	63
4	Colegio Polivalente Saint Trinity	9 De Enero N. 02699	Párvulos a 4° Medio	PSUB	57
5	Escuela Básica Clara Estrella	Valparaíso 6990	Párvulos a 8° Básico	MUN	55
6	Colegio Kennedy	Maipú N. 9483	Párvulos a 8° Básico	PSUB	30
7	Escuela Básica Santa Adela	Los Profesores N. 7400	Párvulos a 8° Básico	MUN	47
8	Colegio Adventista Buenaventura	Buenaventura N. 03928	Párvulos a 4° Medio	PSUB	40
9	Centro Educacional Sagrado Corazón	Huasco N° 7147	Párvulos a 4° Medio	PSUB	81
10	Colegio Saint Orland 2	Pasaje Saturno 03955	Párvulos a 8° Básico	PSUB	55
11	Escuela Básica Karol Cardenal De Cracovia	8 Norte N. 03922	Párvulos a 8° Básico	PSUB	114
12	Escuela Alicia Arista De Silva	México N. 02444 Pob. Santa Olga	Párvulos a 8° Básico	MUN	34
13	Escuela Básica Parque Las Américas	Lo Ovalle N. 3915	Párvulos a 8° Básico	MUN	38
14	Colegio Básica Hernán Olguín Maibee	Avenida Central N. 8108	Párvulos a 8° Básico	MUN	103
15	Escuela Básica Las Espigas	19 Sur N. 03820	Párvulos a 6° Básico	PSUB	15
16	Liceo Polivalente B 133	Inés De Suarez N 6970	Párvulos a 4° Medio	MUN	14
17	Escuela Básica Raúl Sáez S.	Veracruz N. 5243	Párvulos a 8° Básico	MUN	39
18	Escuela Acapulco Dn 582	Acapulco N. 7661	Párvulos a 8° Básico	MUN	69
19	Escuela Básica Bernardo O'Higgins	Rio XII N. 7148	Párvulos a 8° Básico	MUN	32
20	Escuela Conquistando Futuro	La Habana N. 7461	Párvulos a 8° Básico	MUN	20
21	Blue Star College	Monterrey N. 7519	Párvulos a 8° Básico	MUN	44
22	Liceo Tte. 2 Francisco Mery Aguirre	Avenida Centenario N. 02854	Párvulos a 4° Medio	MUN	19
23	Escuela Salomon Sack	Avenida Las Torres N. 7850	Párvulos a 8° Básico	MUN	26
24	Escuela Básica Lo Valledor	Arturo Alessandri N. 6378	Párvulos a 8° Básico	MUN	20
25	Escuela Republica De Las Filipinas	Júpiter N. 7746	Párvulos a 8° Básico	MUN	33
26	Escuela Especial Particular Despertar De	Calle Jalisco N. 02407	1° a 4° Básico	PSUB	13
27	Escuela Básica Saint Philip Of Nery	Astaburuaga N. 9350	Párvulos a 8° Básico	PSUB	19
28	Escuela Básica Republica De Indonesia	Ortiz De Zarate N. 03373	Párvulos a 8° Básico	MUN	35

Puntajes **SOBRE el Promedio Nacional**

Puntajes **BAJO el Promedio Nacional**

El Promedio Nacional es de **250 puntos**

*MUN: Colegio Municipal
PSUB: Colegio Particular Subvencionado

**Número de niños promedio que dieron el SIMCE 2006 y 2009

dresses the potential asymmetry of information parents may have regarding the quality of schools. The underlying hypothesis is that parents do care about the quality of the education their children receive, though are not aware of which schools are those that provide such high-quality education.

A second component of the policy is a video where we prepared with testimonies of: (i) a mother that had decided to change her son that attended second grade to a better school, with higher test scores, in order to give him a better education, (ii) a current college student ending his degree, who went to a relatively good high school in a poor neighborhood, and (iii) a young girl who also came from a poor background but, in part due to her relatively good high school, was able to study a vocational career and now holds a job in a bank.

What these three testimonies share in common is that their characters belong to a low socioeconomic status. The objective was to show people that they can access good schools and higher educational levels and that this is not restricted to high-income families. The choice of these role models is in line with Nguyen (2008) results on the provision of information by people from similar background as the intervened group.

The video also provided some information about rates of return of tertiary education in Chile[27], and argued that there is a relation between the primary school results and the chances of enrollment in college or vocational tertiary school, although it didn't argue any causal effect, only the

[27]Specifically the video showed that on average, a person with college degree earns around three times what the average person only with high school does.

observed correlation (in a similar way than Jensen, 2010).

This aspect aimed to complement the potential lack of information regarding good schools with information on the benefits of providing the child with high-quality education. The hypothesis that Treatment 2 seeks to test is that even if parents were aware of which are the high-quality schools in their neighborhood, they might not be conscious of the potential benefits of a good education, thus their schooling decisions reflect other determinants rather than quality, such as distance, or parents simply enrolling their children in the same school they once attended.

We argue that one of the main channels through which the combination of the report card and the video could change parents school choices are the perceived returns to education. To measure this, we asked parents about their beliefs on the monthly earnings of an average adult in Chile with different educational levels, primary school, high school, vocational higher education and college. Using this information we were able to compute the perceived return to high school, vocational and college education. With this information we are able to construct simple measures of the implicit returns to different educational levels, and test whether presenting simple information about good schools, along with information on the benefits of a good education can change parents perceptions.