Approximating the Equilibrium Effects of Informed School Choice

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Abstract

This paper studies the potential 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 personalized information provision 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’ choices toward schools with higher average test scores, higher test score value added, higher prices, and schools 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 five years later. 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 have capacity constraints. We use the estimated model of demand and supply to simulate policy effects under different assumptions about equilibrium constraints. In counterfactual simulations, we find that capacity constraints play an important role mitigating the policy effect on impact but that in several scenarios, the supply-side responses leads to increased quality which contributes to a overall positive average treatment effect. Finally, we show how model estimates can be used to inform the design of a large scale experiment such that reduced form estimates can capture equilibrium effects and spillovers.

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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 (Andrabi et al., 2017). In addition, lack of information can have distributional effects, given that families from less educated socioeconomic contexts may be particularly misinformed and have more difficulty acquiring information (Hastings and Weinstein, 2008; Elacqua, 2016).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 by spending less time and energy searching for 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 a 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 scarce.

In this paper we explore the quantitative equilibrium implications of a government policy based on information provision in the context of the market for elementary schools in Chile. We develop a scalable, policy relevant intervention that consists of a video and a report card which provide 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. The intervention adapts ideas from previous work in other contexts and the design is chosen so that it is adapted to local policy constraints.3 We use a small-scale randomized control trial to evaluate the impact of this potential policy intervention on individual household school choice decisions

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1Less 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.

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

3For example, relevant prior interventions with successful impacts include work by Hastings and Weinstein (2008), and Andrabi et al. (2017) which both provide a type of report card with school test scores in the context of school choice. Jensen (2010) provides middle school children in the Dominican Republic information on earnings by levels of education and Dinkelman and Martinez (2014) information about financial aid through videos in the context of Chile.
and later academic outcomes. The results from the randomized control trial show that household school choice decisions shift toward schools with higher test scores and prices on average. We use administrative data to track students over a five year period and find that 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 (2013) to explicitly incorporate families with incomplete information, as well as accommodate the results of the randomized control trial, and 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, specifically taking into account capacity constraints and the heterogeneity in market structure across different neighborhoods. 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 detailed 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) and 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 induced by the policy 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 functions and estimates of cost structure.

The results from the randomized control trial and the modeling of demand and supply both complement each other to provide insight for policy recommendations. The small-scale randomized control trial shows there is scope for the simple information intervention to change behavior. The different simulations of an at scale implementation highlight the importance of equilibrium considerations such as capacity constraints and the supply side reaction to invest in quality, raise prices, or expand capacity. Taken together, the simulations imply a range of results indicating that the low SES test scores achievement would increase, and the SES achievement gap decreases, suggesting a policy implementation at scale would be recommended. However, the effects could be significantly reduced if changes to regulation reduce investment and dampen needed adjustments in quality and capacity.
The paper makes two main contributions. The first is that it provides evidence on the role of 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 noted in Heckman et al. (1998). At the same time, aggregate level ex-ante policy evaluation is difficult, if not impossible, and takes much more time and resources to implement in many cases and thus is unable to provide relevant quantitative policy advice in a timely way. 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 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 leads 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 rigorous 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 generate signals of quality and are similar in spirit do not seem to provide the same results. One important 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. Taken together, the empirical evidence to date seems to suggest that the interventions do have the potential to change behavior but that policy details can matter quite a lot. The evidence presented in this paper shows that the specific policy intervention tested 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 then approximate the effects of a large scale implementation of the policy. The use of both theory and data as an integral part of the process of providing evidence for policymakers without yet doing an at-scale experiment. 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 and counter-factual analysis. This approach is one way to provide quantitative policy recommendations in situations when implementing randomized control trials at scale is not feasible, expensive or just not timely. It offers the opportunity to better understand the competitive mechanisms at play when equilibrium considerations are taken into account.
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 randomized control trials to help estimate key parameters or to validate the predictions of a structural model. For example, this approach was famously applied to the evaluation of PROGRESA in seminal work by Todd and Wolpin (2006). This paper differs from that approach in that the randomized control trial is not used to validate the model, nor is the structural model used specifically to quantify the effects of changing different features of the policy. In Attanasio et al. (2011), the experimental data provide variation at scale and a way to identify new parameters associated to the effect of the policy. However, in this paper our main objective is to be able to extrapolate the effects of the policy to individuals beyond the experiment and including the equilibrium supply side reaction when we do not observe aggregate policy effects.

In this way, the equilibrium analysis is closer to policy evaluations of potential mergers or recent work studying the bail out of a car manufacturer, where estimating equilibrium responses to demand and supply play a key role in evaluating the impacts of counterfactual policies that could be implemented. 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. Lise et al. (2015) evaluate employment programs taking advantage of experimental variation but considering equilibrium considerations is similar in spirit, although applied to the context of job training and employment where firms.

The paper also contributes to the line of work developing structural models of education markets by explicitly adding the experimental variation to the modeling of supply and demand. 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 traditionally have not explicitly incorporated experimental variation. 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. The framework additionally allows for a range of policy relevant predictions that can be useful for translating evidence and research into policy recommendations in education settings.

The rest of the paper is organized as follows. Section 2 describes the institutional background that motivates and describes the informational intervention and the data used in the analysis. Section 3 describes a conceptual framework that outlines the potential mechanisms that could rationalize why an information intervention could shift behavior and have equilibrium effects on schools. Section 4 provides a description of the randomized control trial and the results. Section 5

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4Some exceptions on work on school choice in education markets include work by Neilson (2013); Walters (2014); Dinerstein and Smith (2015). The model of school choice and competition in this paper builds on prior work in this space by Neilson (2013), and earlier work on school choice in Chile by Gallego and Hernando (2008) and Chumacero et al. (2011).
describes the empirical model of school choice and competition as well as the estimation procedure used. This section also describes the results from the estimation and develops a series of counterfactual simulations. Finally, Section 6 presents the conclusions.

2 Data and Institutional Setting

In Chile, most students enter primary school in Kindergarten at the age of 5. Prior to entering primary schools, the vast majority of children attend Pre-K institutions where net enrollment rates at age 3 and 4 are 55% and 87%, respectively (OECD, 2016). Children can attend public and private centers. The two main providers of free public Pre-K are Junji and Fundación Integra, which administer approximately 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. For students enrolled in preschools from 3 to 4 years old in 2016, 42% were in Junji, and 18% in Integra. 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 their homes. We implement our information experiment among Integra students.

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 (ie., during the period of our experiment), this process was decentralized and the timing of the application and matriculation process was heterogeneous. In 2016, a pilot version of a centralized application system started working 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 by 2020.

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 than 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 80% of the

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5These numbers are calculated by taking the number of enrolled 3 to 4-year-old students (according to the OECD) and then calculating the share of students that are in Junji and Integra centers based on their administrative records. There are few official sources of information on private Pre-K centers. Some of these centers receive public funding and others charge tuition, which is out-of-reach for most lower income families as the ones included in our sample.

6In our sample of 1800 students described below, the average distance from their home to the Pre-K is 0.70 miles.
voucher schools that have signed up for the policy in 2016. Additional reforms implemented in 2016 froze the prices charged by voucher schools and implemented a gradual 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. However, it is reasonable to assume that while many private schools may maximize profit, public schools face different incentives and different constraints. On one hand, public schools may behave like firms in a competitive market trying to increase revenue, 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 in terms of test scores, and lower inputs in terms of teach quality and resources. 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 (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 for 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 mothers’ education for students entering first grade.

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7Voucher schools are operated by both for-profit firms and not-for-profit organizations. Aedo (1998) argues that not-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.

8Gallego (2013) argues that the fact that public schools receive other transfers different from the voucher implies that they operate under soft budget constraints, where they react partially to the incentives created by the voucher system.

9The online appendix presents similar graphs for school test scores as well as school inputs, such as teacher quality and measures of infrastructure.
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 web sites have posted school test scores for many years. For instance, in 2010, the government of Chile pushed an agenda called "Mas Informacion, Mejor Educacion" (More information, Better Education) showing interest in the idea of providing information.\textsuperscript{10} Evidence from other

\textsuperscript{10}See online appendix for a description of this government program and how it relates to the current policy evaluation.
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 (see eg., Hastings and Weinstein (2008), and Andrabi et al. (2017) for discussion on this issue in two different contexts).

This paper builds on a project that began in 2009, and the randomized control trial was implemented in the second half of 2010. The project objective was to study the effects of increasing information provision through government policy so that policy feasibility concerns shaped the intervention and the design of the implementation. The policy context influenced the development and testing of the information intervention evaluated in this paper as well. 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.

We use several data sources for this project. First, we use administrative data on preschools from Integra, which includes information on the preschool location, enrollment, attendance, socioeconomic level (measured as mother’s schooling), income, and poverty. Second, we use self-collected data through the baseline and follow up surveys in the preschools included in our experiment, which we discuss in detail in Section 4. This includes contact information, individual identifiers, location of the family, and questions regarding the application process. Then, we match this information with administrative data from the Ministry of Education using several datasets, which we discuss below.

The first source of administrative data are records from the Ministry of Education of Chile (MINEDUC) that record the school attended by each student for every year as well as information on grades and some basic demographic information. This also includes individual-level eligibility for the Subvencion Escolar Preferencial (SEP) targeted voucher. The second source of administrative data from MINEDUC is related to students test scores from the SIMCE test and an accompanying survey of the population of 4th and 8th grade students. The survey contains detailed information about the household composition, demographics, and income.

A final source of administrative is composed by the administrative records on all schools in the country, which is available from the Ministry of Education. This lists the type of school, the aggregate matriculation by grade level and address of the school among other school characteristics, such as religious orientation and tuition. We associate each school with the markets defined by Neilson (2013) using census track information. We also add data on the all the transfers made by the Ministry of Education to private and voucher schools.

\[11\]Most of these datasets are described in detail in the online appendix of Neilson (2013), so we refer the reader there for more details.
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. A notion of short and long run determine what variables are under the control of schools.

3.2 Families

When a student $i \in \{1, \ldots, N\}$ 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 $\#J^m = N_f$. 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 $i \in \{1, \ldots, T\}$ 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.

$$U_{ij}^*(\mathcal{I}_i) = \mathcal{E}_i(U_{ij}(\omega)|\mathcal{I}_i)$$

12Several 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.
We further assume that families choose the school that provides the highest perceived value conditional on their information set so that \( j_i^* = \arg\max_{j \in J^m} U_{ij}(I_i) \). 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.

\[
s_{j}^{\text{type}}(U, I, J^m) = \frac{1}{N_{\text{type}}} \sum_{i=1}^{N_{\text{type}}} 1(j = j_i^*|U_{ij}(I_i), J^m) \quad \forall j, \text{ type}
\]

\[
\mu_{q}^{\text{type}} = \sum_{j=1}^{N_{I}} q_{j} \cdot s_{j}^{\text{type}}(U, I, J^m) \quad \forall \text{ type}
\]

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 \( q_j \), 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

\(^{13}\text{For simplicity, note that quality is assumed to be the same for all students at the school and, while potentially chosen with some uncertainty, it is not a function of what students attend. This rules out peer effects and other more complicated school-student match effects.}\)
depends on the information structure that partially determines decisions of families and thus can potentially influence quality and prices.

\[
s_j(U, I, J^m) = \frac{1}{N} \sum_{\text{type} = 1}^{T} N_{\text{type}} \cdot s_{j, \text{type}}^*(U, I, J^m)
\]  

(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.

\[
(q^*_j, p^*_j) = \arg\max_{(p,q)} \Pi(C_j(q), v_j, s_j(U, I, J^m))
\]  

(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 \(N_{s_j}\) faced by schools.

### 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, \(\mu_{q, \text{type}}\), 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 \(J^m\) 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. Indeed, considering that the treatment changes the information set to \(I'\), and defining \(\Delta\) as a conditional post-treatment difference operator, such that \(\Delta A x := x|_{A'} - x|_{A}\) for any \(x\) and \(A\), then
\[ \Delta_{\text{Treat}}\mu_{\text{type}, \text{q, small}} \approx \sum_{j=1}^{N_j} q_j^* \cdot \Delta T_j \]

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.

\[ \Delta_{\text{Treat}}\mu_{\text{type}, \text{scale, sr}} \approx \sum_{j=1}^{N_j} q_j^* \cdot \left( \Delta T_j + \Delta q_j^\text{type} \right) \]

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.

\[ \Delta_{\text{Treat}}\mu_{\text{type}, \text{scale, mr}} \approx \sum_{j=1}^{N_j} \left[ \frac{\partial q_j^*}{\partial s_j} \cdot \Delta T_j \cdot s_j^\text{type} + q_j^* \cdot \left( \Delta T_j + \Delta q_j^\text{type} \right) \right] \]

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 an empirical 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 parents to invest in doing a careful and informed process of choosing a school for their child. To do this, the intervention looks to increase the awareness of neighborhood schools characteristics and the perceived returns to school quality and search. 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% of public Pre-K enrollment) 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. The video urged parents to think about how their choice of school today could affect their child’s future. One segment of the video asks parents to think about what kind of job their child might have and what opportunities going in to higher education can provide them. The video then explains that higher education is associated with more job opportunities and generally higher earnings. The video puts emphasis that going to a good school can be very important in helping their child be prepared for a good job and for higher education options.

**Figure 2: Choosing a School Carefully is Important for Your Child’s Future**

(a) Think about your child’s future.
(b) Think about your child’s future education.
(c) Think about your child’s future job.
(d) High average return to attending college.

The idea that the school choice decision parents are making now is important for their child’s future is combined with testimonials to provide credible context. Through engaging 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 of two students and one parent.
The video explains that to get into higher education, students need to do well on standardized tests. So that one thing they should make sure to check when comparing schools is to see how well the students are doing on standardized tests. Parents 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 overall message is reiterated with a message to invest in getting information and comparing options to be able to choose well.

Figure 4: To choose well, get information and compare options

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 (primary schools$_m$/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 re-
region, grade and school competition, measured as the number of schools within 1.2 miles.\textsuperscript{14} We chose to work only with the highest grade in each preschool, in order to maximize the exogeneity of the enrollment decision. This implies that, depending on the school, the highest level was: \textit{Medio Mayor}, for children up to 3 years and 11 months old and \textit{Transición 1}, equivalent to Pre-K because . We only worked \textit{Medio Mayor} or \textit{Transición 1}.

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 above). 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 5 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.

\textsuperscript{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.
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.\footnote{Table A1 presents the coefficients and standard errors for regressions of each school characteristic listed on treatment status. In turn, Table A2 shows the coefficients and standard errors for regressions of each characteristic of the families in our sample on treatment status.} 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
be correlated with the characteristics of the family. 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, and correspond to the coefficients in the odd-numbered columns for tables included in the online appendix. We present subsample analyses by matriculation status at the time of treatment. The online appendix explores a series of alternative specifications with expanded controls, including a list of variables measuring family socioeconomic status and student health (these specifications correspond to even-numbered columns).

---

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 Notice 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

<table>
<thead>
<tr>
<th>Panel A: Full Sample</th>
<th>Characteristics of Chosen Schools</th>
<th>Student Own Test Scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Distance (1)</td>
<td>Positive Price (2)</td>
</tr>
<tr>
<td>Treatment</td>
<td>0.1371**</td>
<td>0.0438</td>
</tr>
<tr>
<td></td>
<td>(0.0595)</td>
<td>(0.0354)</td>
</tr>
<tr>
<td>N obs.</td>
<td>1,378</td>
<td>1,775</td>
</tr>
</tbody>
</table>

| 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) restricts observations to students travelling less than 4 km. Value Added in column (6) corresponds to version 4 in Appendix Table 3.

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 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 test scores 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

\(^{19}\)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, 2013) 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.
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 note that the test scores are correlated with value-added measures and other proxies for quality such as teacher quality and parents satisfaction.\textsuperscript{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.\textsuperscript{22} 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

In this section, we present our empirical model in more detail, guided by the ideas discussed in the framework section 3. Our starting point for the empirical analysis is a model of demand for schools based on the individual decisions of families making a discrete choice about what school to send their child. We follow recent applications of empirical industrial organization methods to demand estimation in education markets and draw on the framework developed in Neilson (2013). We model families as heterogeneous based on their geographic location within a market as well as having potentially heterogeneous preferences based on their observable and unobservable characteristics. We model schools as spatially differentiated firms that can choose price and quality. We abstract from explicitly modeling the firms production function and input choices and instead choose a more parsimonious model where schools choose quality and price given capacity constraints. We explicitly allow families to have imperfect information about school attributes and avoid interpreting the weight they put on different school characteristics as a deep parameter associated with preferences.

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

\textsuperscript{22}Notice that students already enrolled chose students with much better outcomes that non-enrolled students (with a difference of NN standard deviations). They also traveled about 0.14 kms more and were more 25% more likely to attend schools with positive prices. This suggests that the non-enrolled children in baseline faced more information intervention frictions, which the intervention at least partially corrected.
This specification allows for rich heterogeneity among firm and consumer preferences and provides a realistic characterization of the choice set that each family faces. Viewing the experimental sample through the lens of this model, we can describe in detail how the set of schools and characteristics that each family faced at the moment of choosing a school. This allows us to rationalize the experiment results taking into account all the relevant dimensions of heterogeneity in the choice set and across subjects in the experiment. We see this approach as a key contribution to uncovering the role of information in school choices given that a key input to characterize the school choice problem is the set of options that are available for each family and their characteristics. In the case of spatially differentiated products -in which distance to the school is a key attribute- characteristics of the products in the choice sets are unique for each family’s location, rather than just differing across markets. This makes the reduced form approach of comparing average decisions of treated and control families less informative in terms of its ability to change parents behavior. In what follows we describe the model in detail and then go through the estimation strategy used.

5.1 Empirical Model of School Choice with Incomplete Information

We model the utility for family $i$ from sending their children to school $j$ in time $t$ as 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 $p_{ij}$, that is specific to family $i$ due to targeted vouchers. The proximity of the school to the location of the family is approximated with the linear distance $d_{ij}$. Other observable characteristics at the school level, $x^r_j$, 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 that do not translate into higher test scores, $\xi_j$. Finally, each family $i$ has a random iid preference shock for school $j$, $\epsilon_{ij}$. Preferences over quality, price and location are heterogeneous across family observable discrete type $k$ that is given by the mother’s education and income.

With these definitions, we can describe family $i$’s utility from sending their children to school $j$ to be:

$$U_{ij} = \beta_k q_j - \alpha_k p_{ij} + \lambda_k d_{ij} + \sum_r \eta^r_k x^r_j + \xi_j + \epsilon_{ij}$$ \hspace{1cm} (9)

We now assume families have incomplete information about school quality, price and distance. This implied that families must choose a school based on their beliefs, which are given by potentially heterogeneous information sets $I_i$. To operationalize this assumption, we assume that families know the true distribution of quality, $q_j \sim N(0, \sigma_q^2)$, but only observe a noisy signal for school. This signal corresponds to the true quality plus an error distributed $v(q)_{ij}^k \sim N(0, \sigma(q, e)^2_k)$. The
expected quality would be: \( q_{ij}^\alpha = \rho_k^\alpha (q_{ij} + v(q)_{ij}^\alpha) \), where \( \rho_k^\alpha = \frac{\sigma^2}{\sigma^2 + \sigma(q)\epsilon^2_k} \). Beliefs about prices and distance have a similar form with varying \( \rho_p^\alpha \) and \( \rho_d^\alpha \) given these attributes may be more or less easy to observable. For simplicity in what follows, we assume these signals are independant and unbiased but these assumptions are not crucial. With these additional assumptions we have that expected utility from the families perspective is given by the following expression:

\[
U_{ij}^E = \phi_q^k q_j - \phi_p^k p_{ij} + \phi_d^k d_{ij} + \sum_r \eta_r x_r^j + \bar{\epsilon}_j + \bar{\epsilon}_{ij}
\]

where \( \bar{\epsilon}_{ij} = \sum_{\ell \in \{q, p, d\}} \rho_{\ell}^k v(\ell)_{ij}^k + \epsilon_{ij} \) collects additional idiosyncratic terms.

The reduced form parameters \( \phi \) 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 \( \bar{\epsilon}_{ij} \) which for simplicity is assumed to have an extreme value distribution.

The families will choose school \( j \) to maximize their expected utility \( U_{ij}^E \) 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 \( j \) as a function of observables and parameters, \((q, p, \xi, \theta)\) where \( \theta = \{\delta, \bar{\xi}, \phi, \eta, \sigma^2\} \) parameter vector \( \theta \):

\[
s_n^k(q, p, \xi, \theta) = \sum_{i=1}^{N_{ni}} w_{ni} \left( \frac{\exp(\phi_q^k q_j - \phi_p^k p_{ij} + \phi_d^k d_{ij} + \sum_r \eta_r x_r^j + \bar{\epsilon}_j)}{\sum_{\ell \in J_m} \exp(\phi_q^k q_{i\ell} - \phi_p^k p_{i\ell} + \phi_d^k d_{i\ell} + \sum_r \eta_r x_r^{i\ell} + \bar{\epsilon}_{i\ell})} \right)
\]

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 when choosing what option maximizes their expected utility. The more noise associated with the signals about a school characteristic, the lower the weight placed on that characteristic \( \frac{\partial \phi}{\partial \sigma^2} < 0 \). 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.

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 $\sum^N_m w^m_k = 1 \quad \forall k$. The proportion of the students in the market who are of type $k$ is given by $\pi^m_k$ where $\sum^K_k \pi^m_k = 1$ so that average school quality for students $k$ is given by Equation 12 and market shares for each school are given by Equation 13.

$$\mu^\text{type}_q = \sum_{j \in I^m} \sum_{n \in N^m} q_j \cdot s^j_n(q, p, \xi, \mathcal{I}) \cdot w^m_{nk}$$  \hspace{1cm} (12)$$

$$s_j(q, p, \xi, \mathcal{I}) = \sum^K_k \sum^N_m s^j_n(q, p, \xi, \mathcal{I}, \theta) \cdot w^m_{nk} \cdot \pi^m_k$$  \hspace{1cm} (13)$$

5.1.1 Imbedding the RCT Effects into the Model

We allow the information treatment to modify school choice by shifting the weight families put on price, distance and quality. Given assumptions made above, this can occur either because treatment reduced the noise associated with the signal ($\rho \to 1$) or because the structural preference parameter changed due to the exposure to treatment. From the perspective of the empirical work implemented in this paper, this distinction is not material but it is important to note we will recover only a reduced form parameter quantifying how families change their choices but not their welfare. We allow for treatment to affect $\phi^q, \phi^p,$ and $\phi^d$ differentially and potentially in heterogeneous ways for each type $k$. To operationalize this idea, we expand the types described above to incorporate the families in the RCT and generate new treated types. Thus we add six parameters to the model that modify the weight given to each school characteristic ($\phi^q_k, \phi^p_k, \phi^d_k$) for $k = 1, 2, 3, 4$. With this modified empirical framework, we can now describe household school choice behavior for all families, both with and without treatment, which is the goal to evaluate the particular policy of expanding treatment to more families.

$$\bar{\phi}^p_i = \sum_k (\phi^p_k + \psi^p_k \cdot \text{Treat}_i) \cdot \text{Type}_{ik}$$  \hspace{1cm} (14)$$

$$\bar{\phi}^d_i = \sum_k (\phi^d_k + \psi^d_k \cdot \text{Treat}_i) \cdot \text{Type}_{ik}$$  \hspace{1cm} (15)$$

$$\bar{\phi}^q_i = \sum_k (\phi^q_k + \psi^q_k \cdot \text{Treat}_i) \cdot \text{Type}_{ik} + \beta^{U} u^q_i$$  \hspace{1cm} (16)$$

5.2 Supply side

We now present an empirical framework to model the supply. We model the marginal cost of providing a particular level of quality to be linear in a vector of school characteristics. We decompose these characteristics into a subset of variables that are observed by the econometrician, such as the
quality $q_{jt}$ which summarizes all input choices made by the school, as well as other characteristics of the school that can potentially affect costs such as the type of administration, religious orientation, whether the school is for profit and are summarized in the vector $w_j^t$. In addition, we allow for an unobservable cost shifter that adds on to the marginal cost of quality, $\omega_{jt}$. 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 costs to have a school-specific fixed component $\omega_j$ and a time-school-specific component $\Delta \omega_{jt}$ which we interpret as a shock. Then, $\omega_{jt} = \omega_j + \Delta \omega_{jt}$ and can be potentially correlated with $\zeta_{jt}$, 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:

$$MC(q_{jt}) = \sum_l \gamma_l w_j^l + (\gamma_q + \omega_{jt}) q_{jt}$$ (17)

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

$$\pi_{jt}(q_t, p_t, \xi_t) = N \left[ s_{jt}(q_t, p_t, \xi_t) \cdot (v_{jt}^B + p_{jt} - MC(q_{jt})) + \sum_{k=1,3,5} \pi_m \cdot \tau_{jt} \cdot s_k(q_t, p_t, \xi_t) \cdot \left( v_{jt}^T - p_{jt} \right) \right] - F_j$$ (18)

Where $v_{jt}^B$ is the base voucher at time $t$, $\tau_{jt}$ is an indicator for whether the school gets an additional voucher 1 [School participates in targeted voucher program], and $v_{jt}^T$ is the additional targeted voucher.

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:

$$\frac{\partial \pi_j}{\partial q_{jt}}(q_t, p_t, \xi_t) = \frac{\partial s_{jt}(q_t, p_t, \xi_t)}{\partial q_{jt}} \cdot \left( v_{jt}^B + p_{jt} - MC(q_{jt}) \right)$$

$$+ \sum_{k=1,3,5} \pi_m \cdot \frac{\partial s_k(q_t, p_t, \xi_t)}{\partial q_{jt}} \cdot \tau_{jt} \cdot \left( v_{jt}^T - p_{jt} \right) - s_{jt}(q_t, p_t, \xi_t) \cdot \frac{\partial MC(q_{jt})}{\partial q_{jt}} = 0$$

And the expression for quality is:
where

$$p^*_j = \left( \sum_{k=1,3,5} \pi^m_k \cdot \tau_{jt} \cdot \frac{\partial s^k_{jt}(q, p, \xi)}{\partial q_{jt}} \right) \bigg/ \left( \frac{\partial s^k_{jt}(q, p, \xi)}{\partial q_{jt}} \bigg) \right) \cdot \left( \frac{\partial s^k_{jt}(q, p, \xi)}{\partial q_{jt}} \bigg) \right)$$

In choosing price, they compare the marginal gain from raising the price to the marginal cost of attracting fewer students. Then, the first order condition with regard to price and the implied pricing equation are the following:

$$\frac{\partial \pi_j(q, p, \xi)}{\partial p_j} = \frac{\partial s_{jt}(q, p, \xi)}{\partial p_j} \cdot (v^B_{jt} + p_{jt} - MC(q_{jt})) + \sum_{k=1,3,5} \pi^m_k \cdot \tau_{jt} \cdot \left( v^T_{jt} - p_{jt} \right) + s_j(q, p, \xi) - \sum_{k=1,3,5} \pi^m_k \cdot \tau_{jt} \cdot s^k_{jt}(q, p, \xi) = 0$$

$$p^*_j = \left[ \sum_i \gamma_i w^i_j + (\gamma_q + \omega^q_j) q_j - v^B_{jt} \right] \frac{1}{1 - \rho^p_j} - \left[ \frac{v^T_{jt}}{1/\rho^p_j - 1} \right]$$

where

$$\rho^p_j = \left( \sum_{k=1,3,5} \pi^m_k \cdot \tau_{jt} \cdot \frac{\partial s^k_{jt}(q, p, \xi)}{\partial p_{jt}} \right) \bigg/ \left( \frac{\partial s^k_{jt}(q, p, \xi)}{\partial p_{jt}} \bigg) \right) \cdot \left( \frac{\partial s^k_{jt}(q, p, \xi)}{\partial p_{jt}} \bigg) \right)$$
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 \( \omega_j \). Our estimation is done in three steps which are detailed below.23

5.3.1 First Step: Demand Parameters Estimation

In the first step, we estimate the parameters \( \theta_1, \theta_2 \) following Berry (1994), Berry et al. (1995), Petrin (2002), Berry et al. (2004) and Neilson (2013). 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.

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

\[
G^1(\theta_2) = s_{jt} - s_{jt}(\theta_2)
\]

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 \( \pi^m \) will be such that:

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

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 \( k = 1, \ldots, 6 \), we define these expectations as:

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

---

23See Appendix A2 for additional estimation details and a discussion on how we calculate standard errors.
These moments are particularly useful for identifying the heterogeneity of preferences for observed school characteristics by observed family types. From the micro-data 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:

$$G^2_q(\theta_2) = \sum_{i \in N^q_m} q_{ik} - \frac{N_m}{n} \sum_j s_{nj}^{nk}(\theta_2) \cdot w_{nk}^m \cdot q_{jn}$$  \hspace{1cm} (22)$$

$$G^2_p(\theta_2) = \sum_{i \in N^p_m} p_{ik} - \frac{N_m}{n} \sum_j s_{nj}^{nk}(\theta_2) \cdot w_{nk}^m \cdot p_{jn}$$  \hspace{1cm} (23)$$

$$G^2_d(\theta_2) = \sum_{i \in N^d_m} d_{ik} - \frac{N_m}{n} \sum_j s_{nj}^{nk}(\theta_2) \cdot w_{nk}^m \cdot d_{jn}$$  \hspace{1cm} (24)$$

where $N^f_m$ are schools in each market $m$.

In third place, we include IV-demand moments. Noting that $\xi_j$ 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 $\xi_j$ and the chosen instruments.

$$G^3(\theta_2) = \xi_j \cdot IV'$$  \hspace{1cm} (25)$$

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 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 $\varphi$ in a second step using a 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):

\begin{align*}
G_{q,k}(\theta_2, \theta_{T2}^2) &= \hat{\beta}_k - \hat{\beta}_k^{sim} = (X'M_DX)^{-1}X'M_DQ_k - (X'M_DX)^{-1}X'M_DQ_k^{sim} \quad (26) \\
G_{p,k}(\theta_2, \theta_{T2}^2) &= \hat{\alpha}_k - \hat{\alpha}_k^{sim} = (X'M_DX)^{-1}X'M_DP_k - (X'M_DX)^{-1}X'M_DP_k^{sim} \quad (27) \\
G_{D,k}(\theta_2, \theta_{T2}^2) &= \hat{\lambda}_k - \hat{\lambda}_k^{sim} = (X'M_DX)^{-1}X'M/DD_k - (X'M_DX)^{-1}X'M/DD_k^{sim} \quad (28)
\end{align*}

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 ($\theta_3$) 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 19 we get an expression for
the unobservable shock that affects the marginal cost of rising quality:

\[
\Delta \omega_{jt} = \frac{v + p_{jt} - \sum_l \gamma_l w_{jl}}{q_{jt} + \bar{s}_{jt}(q, p, \xi)} \left[ \frac{\partial s_{jt}(q, p, \xi)}{\partial q_{jt}} \right]^{-1} - \gamma - \bar{\omega}_j
\]  

(29)

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

\[
G_5(\theta_2) = \Delta \omega_{jt} \cdot IV'
\]  

(30)

5.4 Parameter Estimates

5.4.1 Demand

Table 2 presents results for the estimated parameters for school choice \( \phi \) and the policy parameters \( \varphi \). The three first rows show preferences for quality by family characteristics\(^{24} \). 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.

5.4.2 Supply

The estimated marginal cost fixed effect at the school level is presented in Table 3. It can be seen in Figure 6 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 we show in the appendix. Religious schools have lower costs and for profit schools face higher marginal costs.

At this point, it is interesting to explore whether the estimated marginal costs by firm make sense. To check how these results compare with out of sample data, we merged the public records of the college entrance exam for each school’s principal. shows a binned scatter plot for the cor-

\(^{24}\)Using the estimated model parameters, we can show how well the model fits the empirical features we are interested in replicating. The distribution of school quality in aggregate fits perfectly given that the model must replicate the aggregate share of each school perfectly. The Online Appendix shows the fit of the model by the mothers’ educational group, showing a relatively adequate fit given moments include only means across markets.
Table 2: Demand Model Estimates

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Estimate</th>
<th>Significance</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \varphi^q_k ) - Weight on Quality</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mother No HS</td>
<td>1.37</td>
<td>†</td>
</tr>
<tr>
<td>Mother HS</td>
<td>1.57</td>
<td>†</td>
</tr>
<tr>
<td>Mother College</td>
<td>1.89</td>
<td>†</td>
</tr>
<tr>
<td>Poor Household</td>
<td>-0.58</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother No HS</td>
<td>0.55</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother HS</td>
<td>0.34</td>
<td>†</td>
</tr>
<tr>
<td>( \varphi^p_k ) - Weight on Price</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mother No HS</td>
<td>-9.89</td>
<td>†</td>
</tr>
<tr>
<td>Mother HS</td>
<td>-2.84</td>
<td>†</td>
</tr>
<tr>
<td>Mother College</td>
<td>-0.01</td>
<td>†</td>
</tr>
<tr>
<td>Poor Household</td>
<td>-3.31</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother No HS</td>
<td>9.26</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother HS</td>
<td>2.80</td>
<td>†</td>
</tr>
<tr>
<td>( \varphi^d_k ) - Weight on Distance</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mother No HS</td>
<td>-0.99</td>
<td>†</td>
</tr>
<tr>
<td>Mother HS</td>
<td>-0.70</td>
<td>†</td>
</tr>
<tr>
<td>Mother College</td>
<td>-0.38</td>
<td>†</td>
</tr>
<tr>
<td>Poor Household</td>
<td>-0.21</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother No HS</td>
<td>0.38</td>
<td>†</td>
</tr>
<tr>
<td>Treated Mother HS</td>
<td>0.12</td>
<td>†</td>
</tr>
<tr>
<td>( \sigma ) - Quality</td>
<td>0.13</td>
<td>†</td>
</tr>
</tbody>
</table>

Note: † indicates significance at 0.01 confidence level.

relation between the estimated marginal costs of increasing quality by school and a measure of the school’s principal ability given by their college entrance exam test average score. We find a negative correlation between these two variables, what is reasonable as we may expect that more skilled principals can increase the quality of the schools at a lower cost.
Table 3: Supply Model Estimates

<table>
<thead>
<tr>
<th>( \gamma_l ) - Marginal Cost</th>
<th>( \gamma_q ) - Quality Marginal Cost FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Voucher</td>
<td>0.12†</td>
</tr>
<tr>
<td>Public</td>
<td>0.65†</td>
</tr>
<tr>
<td>For Profit</td>
<td>0.25†</td>
</tr>
<tr>
<td>Religious</td>
<td>-0.10†</td>
</tr>
<tr>
<td>Constant (Mean Market FE)</td>
<td>0.44</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>( \gamma_q ) - Quality Marginal Cost FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant (Mean Firm FE)</td>
</tr>
</tbody>
</table>

Note: † indicates significance at 0.01 confidence level. Mean Market FE and Mean Firm Effects are show to give a sense of the magnitude.
Figure 6: Firm Level MgC(q)

Figure 7: Correlation between MgC(q) and Principal’s PSU score
5.5 Counterfactuals

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

We begin with a counterfactual that assumes no supply side reaction and no capacity constraint. This counterfactual thus simulates sorting and does not take into account any adjustments or institutional restriction into account. We then add more layers of complexity and simulate sorting under less restrictive assumptions allowing for capacity constraints and supply side reactions. We recover the distribution of school value added across different SES groups under each counterfactual and use the difference in the average school quality as our main metric to compare outcomes under different scenarios.

5.5.1 At Scale Implementation

These set of counterfactuals aim to explore the very short run impact of implementing the information policy at scale. 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 be able to respond to an unpredicted demand shock. To implement this simulation, we will hold the set of schools and their quality and price fixed and apply the treatment to all students in the market. We start with this assumption and simulate probabilities of going to each school for all the students in each market. This will be our \( T \) counterfactual. We compute the implied distribution of value added by SES group.

In our second counterfactual, we take into account the fact that there capacity constraints can bind so our base assignment is not feasible. Using the estimated preferences and the same assumptions as in the first counterfactual, we simulate rank order lists for each student. We use these rank order lists to assign students to schools. Oversubscribed school wait lists are resolved using lotteries as is current policy in Chile where the centralized assignment system implements a deferred acceptance algorithm. 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 8 shows the impact on the distribution of school quality with and without the policy (the first and the \( T \) lines) and when capacity constraints are taken into account (\( T+CC \)). The plot shows 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 8:** Distribution of Quality - At Scale Policy - Supply Side

(a) No High School Moms’ Type

(b) High School Moms’ Type
5.5.2 Supply Responses

The simulations in the previous section show lower effects due to congestion when the policy is scaled up. This suggests that there could be meaningful effects on school incentives. To explore the extent 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 9. The distribution of change in markdowns shows significant degree of heterogeneity, suggesting that some schools would face larger changes in incentives than others.

This evidence is consistent with recent research emphasizing supply side reactions in education markets Andrabi et al. (2017); Neilson (2013) 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.

For the simulation, we need to find a new equilibrium for the quality vector. Based on Doraszelski et al. (2018), we assume that schools have uncertainty about their rivals’ costs but know the demand parameters. Then, after a policy change, schools will not immediately play the Nash equilibrium but will rather choose quality computing their first order conditions based on the demand they expect and their beliefs about their rivals’ quality. We will iterate over a process in which the schools learn about the rival’s quality choices. In each iteration, the schools update

Figure 9: Markdown Change - At Scale Policy - Short Run

(a) Percentiles

(b) Distribution
the expected quality for the rivals by using the last observation for it. The results in Doraszelski et al. (2018) support the idea that in stable environments play will generally converge to a Nash equilibrium.

Figure 8 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 (T+CC+S).

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, 2013). 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 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.

Additional simulations repeat the exercise with more realistic assumptions of supply responses. These results are presented in Figure 10. The first one is presented in the second column. For this exercise, we assume that public schools do not react at all, as they maximize quality given budget and ignore market conditions otherwise. We see that under this assumption low SES students are particularly affected. Then, we allow schools to also rise price together with quality, following the first order conditions presented in the previous section. This exercise shows that, compared to column 2, low SES benefit the most from price raises, as most of them are eligible for the targeted voucher so they pay no fee and benefit from the additional resources. On the other hand, students whose mother finished high school do not benefit that much as both higher prices and congested schools undermine their chances of going to better schools. In the final counterfactual, we ask the question of how much the voucher should go up if we freeze price and want to get a similar average treatment effect for the kids whose mother did not finish high school. We find that a rise in base voucher close to 19% can achieve similar treatment effects for low SES kids and higher effects for mid SES kids.
5.5.3 Potential Spillovers to Inputs Markets

Policy changes that induce an increase in the provision of quality can put pressure on inputs markets. For example, schools may have to hire better teachers to increase their quality, increasing the demand and thus wages in teachers’ labor markets. Schools’ decisions could be different as they face higher marginal costs.

In this simulation, we explore 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 11.
5.5.4 Summary

An important aspect to note is that in these counterfactual 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 to increase their quality.

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: Summary of Policy Effect Simulations

<table>
<thead>
<tr>
<th>Experiment</th>
<th>ATE</th>
<th>T</th>
<th>T+CC</th>
<th>T+CC+S (All)</th>
<th>T+CC+S (noPub)</th>
<th>∆+5%</th>
<th>∆+10%</th>
<th>∆+15%</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>-</td>
<td>0.0756</td>
<td>0.0464</td>
<td>0.1013</td>
<td>0.0449</td>
<td>0.0770</td>
<td>0.0569</td>
<td>0.0193</td>
</tr>
<tr>
<td>No HS Mon</td>
<td>0.1210</td>
<td>0.1662</td>
<td>0.1072</td>
<td>0.1964</td>
<td>0.0817</td>
<td>0.1477</td>
<td>0.1061</td>
<td>0.0299</td>
</tr>
<tr>
<td>HS Mom</td>
<td>0.0560</td>
<td>0.0709</td>
<td>0.0463</td>
<td>0.0985</td>
<td>0.0600</td>
<td>0.0721</td>
<td>0.0518</td>
<td>0.0150</td>
</tr>
<tr>
<td>College Mom</td>
<td>-</td>
<td>0.0000</td>
<td>-0.0168</td>
<td>0.0127</td>
<td>0.0126</td>
<td>0.0110</td>
<td>0.0080</td>
<td>0.0060</td>
</tr>
</tbody>
</table>

6 Discussion

It is generally understood that 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 different contexts suggests that providing information to individual families may indeed shift their school choice decisions, and in some cases, information provision can have aggregate effects. 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 developing countries where private provision of services like education is common, but government supervision and regulation tend to be hard to implement effectively. In spite of this, when it comes to designing and implementing government policy, it is not straightforward how to extrapolate the existing evidence from different contexts. There are pitfalls that could render these policies ineffective at both the level of individual choices, as well as in the aggregate.

In this paper, we employ a series of different empirical tools and data to study the small- and large-scale effects of a particular policy that promotes information provision. 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. Using a small-scale randomized control trial, we evaluate whether this type of intervention could affect choice and later outcomes. The results provide evidence that an intervention of this type does indeed shift parents school choice decisions and raises student achievement several years later. To extrapolate to aggregate policy implications and evaluate equilibrium considerations, we embed the randomized control trial within a structural model of school choice and competition, estimating the parameters that describe how demand and supply would react to the policy intervention. We estimate the parameters of the demand and supply side model taking advantage of rich administrative data, variation
from recent policy changes, and the variation generated from the randomized control trial.

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. 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 is the case in many developing countries. Future research should innovate on the design details of the information provision and should explore beyond the specific intervention that we implemented in 2010 using video and printed report cards. For example, virtual assistants or chatbots leverage artificial intelligence to provide rich and dynamic personalization of information and are rapidly being deployed in many markets, such as retail and health care. Given the promising results found in this paper, future research should study whether these new tools will have an impact on individual decisions and market efficiency in education contexts.

It should be noted that measuring school quality and causal estimates of value added is difficult and testing students regularly is expensive and not practical in many contexts. The evidence presented in this paper suggests that governments in developing countries could have a high return on investing in systems to collect and disseminate even basic information about schooling options given that this very simple implementation was seemingly successful. It is also important to note that the intervention studied did not focus exclusively on information about test scores but also made an important emphasis on persuading families that the return on investing in effort to search and choose a school carefully is high. This second aspect is complementary to the informational structure that is available and potentially less dependent on the quality of information available.

Beyond the specific policy implications of the information intervention, we also argue that the combination of randomized control trials and structural models of demand and supply can be useful for policy evaluation for several reasons. The first is that a randomized control trial implemented at scale is often politically or technically not feasible. Small-scale randomized control trials can be more cost effective for discovering what design details seem to work best. The effects of the best version of the intervention can then be embedded into an empirical model that incorporates the main features governing demand and supply and used to evaluate impact before implementing a final and costly large-scale evaluation. In this sense, we would argue that in many cases, a small-scale randomized control trial combined with an additional analysis to evaluate equilibrium considerations is a cost effective way of gaining further insight into the po-
potential policy effects and aspects of design in practice. This could make small-scale experiments more appealing to governments and be a relevant intermediate step that can help guide funding institutions like USAID when deciding funding for expensive large at-scale evaluations.

References


Banerjee, Abhijit V and Esther Duflo, Poor economics: A radical rethinking of the way to fight global poverty, Public Affairs, 2011.


## Appendix

### A1 Appendix Tables and Figures

**Table A1: Treatment Balance at the School Level**

<table>
<thead>
<tr>
<th></th>
<th>Difference T-C</th>
<th>Control Group Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Enrollment</td>
<td>-1.898 (3.248)</td>
<td>41.467 (2.530)</td>
</tr>
<tr>
<td>Mean attendance</td>
<td>-1.053 (2.430)</td>
<td>28.689 (1.903)</td>
</tr>
<tr>
<td>Mother HE</td>
<td>-0.643 (1.552)</td>
<td>9.495 (1.320)</td>
</tr>
<tr>
<td>Mother HS</td>
<td>-0.915 (2.195)</td>
<td>48.347 (1.652)</td>
</tr>
<tr>
<td>Mother NHS</td>
<td>0.760 (1.010)</td>
<td>7.309 (0.697)</td>
</tr>
<tr>
<td>Q1 Income</td>
<td>0.577 (2.996)</td>
<td>57.970 (2.348)</td>
</tr>
<tr>
<td>Q2 Income</td>
<td>0.288 (2.142)</td>
<td>31.365 (1.587)</td>
</tr>
<tr>
<td>Q3 Income</td>
<td>-1.136 (1.216)</td>
<td>8.752 (0.930)</td>
</tr>
<tr>
<td>Very Poor</td>
<td>0.637 (1.865)</td>
<td>14.947 (1.406)</td>
</tr>
<tr>
<td>Poor</td>
<td>0.083 (2.233)</td>
<td>40.619 (1.816)</td>
</tr>
</tbody>
</table>

Notes: Column 1 presents the coefficient and standard error for the difference between the treatment and control groups in a regression of each variable on treatment status. Column 2 presents the coefficient and standard error for the control group mean. p-value < 10% ** p-value < 5% *** p-value < 10%.

**Table A2: Treatment Balance at the Family Level**

<table>
<thead>
<tr>
<th></th>
<th>Full Sample (N=1,832)</th>
<th>Enrolled (N=606)</th>
<th>Non-Enrolled (N=1,006)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Difference T-C</td>
<td>Control Mean</td>
<td>Difference T-C</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Panel A: SES characteristics</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Household size</td>
<td>0.114 (0.126)</td>
<td>4.837 (0.101)</td>
<td>0.060 (0.172)</td>
</tr>
<tr>
<td>Durable goods</td>
<td>0.069 (0.179)</td>
<td>4.558 (0.160)</td>
<td>0.221 (0.259)</td>
</tr>
<tr>
<td>Owns Dwelling</td>
<td>0.022 (0.039)</td>
<td>0.345 (0.032)</td>
<td>0.116** (0.058)</td>
</tr>
<tr>
<td>Mother head of hh</td>
<td>0.010 (0.027)</td>
<td>0.826 (0.021)</td>
<td>-0.035 (0.047)</td>
</tr>
<tr>
<td>Mother NHS</td>
<td>-0.024 (0.010)</td>
<td>0.203 (0.026)</td>
<td>-0.017 (0.046)</td>
</tr>
<tr>
<td>Mother HE</td>
<td>-0.051 (0.039)</td>
<td>0.411 (0.032)</td>
<td>-0.038 (0.057)</td>
</tr>
<tr>
<td>Mother HE</td>
<td>0.029 (0.028)</td>
<td>0.820 (0.022)</td>
<td>0.041 (0.039)</td>
</tr>
</tbody>
</table>

Panel B: Baseline school choice

|                      | Difference T-C | Control Mean | Difference T-C | Control Mean | Difference T-C | Control Mean |
| 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

|                      | Difference T-C | Control Mean | Difference T-C | Control Mean | Difference T-C | Control Mean |
| 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 difference between the treatment and control groups in a regression of each variable on treatment status. Columns 2, 4 and 6 present the coefficient and standard errors for the control group mean. 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

<table>
<thead>
<tr>
<th></th>
<th>Difference Enrolled - Non-Enrolled</th>
<th>Non-Enrolled Group Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
</tbody>
</table>

#### Panel A: SES characteristics

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Household size</td>
<td>-0.035</td>
<td>4.927</td>
</tr>
<tr>
<td>Durable goods</td>
<td>0.382***</td>
<td>4.461</td>
</tr>
<tr>
<td>Owns Dwelling</td>
<td>0.047</td>
<td>0.343</td>
</tr>
<tr>
<td>Mother head of hh</td>
<td>0.001</td>
<td>0.832</td>
</tr>
<tr>
<td>Mother NHS</td>
<td>-0.010</td>
<td>0.192</td>
</tr>
<tr>
<td>Mother HS</td>
<td>-0.039</td>
<td>0.391</td>
</tr>
<tr>
<td>Mother HE</td>
<td>0.007</td>
<td>0.836</td>
</tr>
<tr>
<td>Poor</td>
<td>-0.013</td>
<td>0.895</td>
</tr>
<tr>
<td>Another child in primary</td>
<td>0.010</td>
<td>0.405</td>
</tr>
</tbody>
</table>

#### Panel B: Birth characteristics

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gestation Weeks</td>
<td>-0.019</td>
<td>38.751</td>
</tr>
<tr>
<td>Birth Weight</td>
<td>-3.982</td>
<td>3,342.137</td>
</tr>
<tr>
<td>Mother’s Age</td>
<td>0.329</td>
<td>25.332</td>
</tr>
<tr>
<td>Father’s Age</td>
<td>-1.646</td>
<td>36.472</td>
</tr>
<tr>
<td>Marital Status</td>
<td>-0.021</td>
<td>1.735</td>
</tr>
<tr>
<td>Doctor</td>
<td>-0.011</td>
<td>0.333</td>
</tr>
<tr>
<td>Hospital</td>
<td>0.014*</td>
<td>0.959</td>
</tr>
<tr>
<td>Number of Children</td>
<td>0.102</td>
<td>1.871</td>
</tr>
</tbody>
</table>

Notes: Column 1 presents the coefficient for the difference between households enrolled and non-enrolled at baseline in a regression of each variable on an indicator for being enrolled at baseline. Column 2 presents the coefficient and standard errors for the non-enrolled group mean. 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 A1: Treatment Effects on Distance by Bandwidth](image)
### Table A4: Effect of Treatment on Value Added Chosen

<table>
<thead>
<tr>
<th></th>
<th>VA 1 (1P)</th>
<th>VA 2 (2P)</th>
<th>VA 3 (1E)</th>
<th>VA 4 (2E)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td><strong>Panel A: Full Sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>0.027</td>
<td>0.023</td>
<td>0.025</td>
<td>0.022</td>
</tr>
<tr>
<td>N obs.</td>
<td>1744</td>
<td>1744</td>
<td>1744</td>
<td>1744</td>
</tr>
<tr>
<td><strong>Panel B: Enrolled sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>-0.034</td>
<td>-0.050</td>
<td>-0.035</td>
<td>-0.052</td>
</tr>
<tr>
<td>N obs.</td>
<td>590</td>
<td>590</td>
<td>590</td>
<td>590</td>
</tr>
<tr>
<td><strong>Panel C: Not enrolled sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>0.067*</td>
<td>0.064*</td>
<td>0.068*</td>
<td>0.066*</td>
</tr>
<tr>
<td>N obs.</td>
<td>956</td>
<td>956</td>
<td>956</td>
<td>956</td>
</tr>
</tbody>
</table>

**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

<table>
<thead>
<tr>
<th></th>
<th>Lang - 2nd</th>
<th>Average - 4th</th>
<th>Lang - 4th</th>
<th>Math - 4th</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td><strong>Panel A: Full Sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>-0.008</td>
<td>-0.028</td>
<td>0.103*</td>
<td>0.085</td>
</tr>
<tr>
<td>N obs.</td>
<td>1392</td>
<td>1218</td>
<td>1267</td>
<td>1112</td>
</tr>
<tr>
<td><strong>Panel B: Enrolled sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>-0.076</td>
<td>-0.110</td>
<td>-0.080</td>
<td>-0.115</td>
</tr>
<tr>
<td>N obs.</td>
<td>492</td>
<td>484</td>
<td>450</td>
<td>443</td>
</tr>
<tr>
<td><strong>Panel C: Not enrolled sample</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment</td>
<td>0.068</td>
<td>0.050</td>
<td>0.232***</td>
<td>0.214***</td>
</tr>
<tr>
<td>N obs.</td>
<td>761</td>
<td>734</td>
<td>695</td>
<td>669</td>
</tr>
</tbody>
</table>

**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.
Figure A2: Markdown distribution in the Map - Santiago

Figure A3: Markdown distribution in the Map - Santiago
Figure A4: Markdown distribution in the Map - Santiago
A2  Estimation Specifics

In the first step, we implement the estimation of the parameters ($\theta_1, \theta_2$) 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 additional parameters to be estimated. The optimization problem that we solve is:

\[
(\theta_1^*, \theta_2^*, \xi) = \arg\min_{\theta_1, \theta_2} \begin{bmatrix} g_2 \\ g_3 \end{bmatrix}^\prime \begin{bmatrix} W_{MM} & 0 \\ 0 & W_{IV-D} \end{bmatrix} \begin{bmatrix} g_2 \\ g_3 \end{bmatrix}
\]

Subject to the following constraints:

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

Where $f(\theta_1) = \sum_r \eta^r_k x^r_j$.

In the second step, we estimate $\varphi$ under the following optimization problem:

\[
\varphi^* = \arg\min_{\varphi} g_4(\vartheta)^\prime W_{RCT} g_4(\vartheta)
\]

Subject to the following constraints:

\[
\hat{\beta}_{RCT} - \hat{\beta}_{sim} - g_3 = 0 \quad \text{(i) RCT moments)}
\]

In the third step, we estimate supply side parameters ($\theta_3$). To do so, we need to get an expression for $\Delta \omega_{jt}$.

When we rearrange Equation 19 we get an expression for the observed component that affects the marginal cost of rising quality:

\[
\omega_{jt} = \frac{v + p_{jt} - \sum_l \gamma^l w^l_{jt}}{q^*_j + s(j, p, \xi)} - \gamma^q
\]

\[
\lambda_{jt}
\]
Where \( \omega_{jt} = \omega_j + \Delta \omega_{jt} \). 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 \( \omega_j \).

To do so, lets name the first term at the right side of Equation 33 \( A_{jt} \). For a given set of parameters \( \gamma_l \), we can calculate the expression \( A_{jt} \) for every school-year combination and take the mean. we also redefine \( A_{jt} \) as \( A_{jt} = \omega_j + \Delta \omega_{jt} + \gamma^q \).

\[
A_j = \frac{\sum_{t=1}^{T} \omega_j + \Delta \omega_{jt} + \gamma^q}{N_T} = \omega_j + \gamma^q
\]

We can rearrange the expression in Equation 33 and substract \( A_j \) at both sides:

\[
\omega_{jt} + \gamma^q - \omega_j + \gamma^q = A_{jt} - A_j
\]

\[
\Delta \omega_{jt} = A_{jt} - A_j
\]

our optimization problem for the third step will be:

\[
\theta^*_3 = \arg\min_{\theta_3} g_5(\theta)' W_{IV-S} g_5(\theta) \tag{34}
\]

Subject to the following constraints:

\[
w(\hat{\theta}_2) - h(\theta_3, s(\hat{\theta}_2), \nabla s(\theta_2)^{-1}) = 0 \quad \text{Cost disturbance} \quad (v)
\]

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 Hansen (1982). We will discuss how we calculate the standard error for a generic case (the parameters \( \theta \) and moments \( M \)), 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:

\[
\min_{\theta} Q_{obj} = M' W_m M
\]

For which the gradient is:
\[
\frac{\partial Q_{obj}}{\partial \theta} = 2 \cdot J'_M W_m M
\]

Where \( J_M \) is the Jacobian.

The variance-covariance matrix for a GMM estimator is calculated using the estimator proposed by Hansen (1982):

\[
\text{cov}(\theta) = (J'_M W_m J_M)^{-1} J'_M W_m \mathbf{V} \mathbf{W}_m^\prime J_M (J'_M W_m J_M)^{-1}
\]

Where \( \mathbf{V} \) 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 model’s predictions. This element is estimated by simulating the sample moment at the estimate of \( \theta \) 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 26 take the difference between the estimated treatment effects and our model’s 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

O-1 Provision of Preschool Services in Chile

Access to preschool education has increased in the last years but it is still below levels of developed countries. Accordingly to for Economic Co-operation and Development (2016), enrollment of 0-2 year-olds in formal care is 18% (below the average of 33% in OECD countries). This increases to 54% for 3 year-olds in 2015 (which is almost twice enrollment in 2005 but below the average of 71% across the OECD). In terms of provision, there are public and private providers. Free public providers are organized in two networks: Junji and Fundación Integra, which administer approximately 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. 42% of the 3-4 year olds enrolled in preschools in 2016 attended Junji centers and 18% attended Integra centers. The reminder children attended private providers, some of them receiving public funding and others charging relatively high tuition fees.

The application process to public and private centers is decentralized and public providers give priorities to poor families Aguirre (2011). Several targeted public programs also support families in sending children to preschools. In addition, Junji was also in charge of (light) monitoring of private providers until 2016 (mainly related to inputs). Currently, there is an independent agency in charge of the monitoring of preschool providers. Expenditure per student was $ 6,408 in 2015 and 15% of that comes from private funding (OECD, 2016).

O-1.1 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

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25This subsection is partially based on for Economic Co-operation and Development (2016).
existing infrastructure of the program. Otherwise, finding families that are about to choose would be very costly to implement.

O-2 Sample Selection

With the aid of the Integra management team we decided to work in the three larger regions in the country, Valparaíso, Biobío and Santiago, and we identified which municipalities and preschools within each one were useful to our research questions. We chose preschools located in urban areas (according to Integra’s criteria) and that met our criteria for having an adequate level of school competition. To do so, we defined that all preschools in municipality \( i \) would participate if there were at least 10 schools within a 2 kilometer radius (around 1.2 miles) and the ratio 
\[
(\text{primary schools}_i / \text{preschools}_i) \geq 2 \quad \text{for municipality } i.
\]
In addition, we only considered schools in the three lower SES levels (according to the classification implemented by the Ministry of Education), which represent almost all the schools in the effective choice set among families of the first three income quintiles.

This left us with 143 preschools in the three regions mentioned above. Then we randomly assigned preschools to the treatment and control groups stratifying by region, grade and the number of schools within a 1.2 mile radius. We contacted each preschool to check whether they had any parents’ meeting scheduled between August and December, 2010. If they had a scheduled meeting with parents, we asked if a person of our staff could go and apply the baseline survey during the meeting and apply the treatment (if the preschool was in that group).

Out of the 143 original preschools we selected, there were 10 for which we could not schedule a meeting. Among the main reasons were, a refusal by the principal, unavailability of dates and no parents attending the meeting. Table O-1 presents some differences in observable characteristics between schools included and not included in the experiment. The 10 preschools without meetings have a larger share of mothers with complete tertiary education, larger share of families in the second income quintile, and a lower proportion of indigent families. None of the parents in these schools were surveyed in the follow up, since no one was able to sign the informed consent. Thus for all practical reason our sample consist of the 133 preschools where we were able to attend the meeting.

In the 133 intervened preschools, a total of 1,832 parents signed the informed consent and answered the survey asking for contact information. The surveyed was applied before handing out any information and it included 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 the child. Parents were also asked if they had any other children already enrolled in primary school, considering this could be an important determinant of school choice. Since our presence in the meeting was not announced,
Table O-1: Differences between preschools with and without meetings

<table>
<thead>
<tr>
<th></th>
<th>With Meeting</th>
<th>Without Meeting</th>
<th>Difference</th>
<th>t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enrollment</td>
<td>40.21</td>
<td>46.50</td>
<td>-6.29</td>
<td>-1.02</td>
</tr>
<tr>
<td>Mean Attendance</td>
<td>27.99</td>
<td>32.40</td>
<td>-4.41</td>
<td>-0.77</td>
</tr>
<tr>
<td>Mother w. Complete Tertiary (%)</td>
<td>9.07</td>
<td>12.81</td>
<td>-3.74</td>
<td>-1.76*</td>
</tr>
<tr>
<td>Mother w. Complete Secondary (%)</td>
<td>47.74</td>
<td>45.36</td>
<td>2.38</td>
<td>0.70</td>
</tr>
<tr>
<td>Mother w. Incomplete Secondary (%)</td>
<td>34.72</td>
<td>34.89</td>
<td>-0.17</td>
<td>-0.05</td>
</tr>
<tr>
<td>Mother w. Complete Primary (%)</td>
<td>7.81</td>
<td>6.94</td>
<td>0.87</td>
<td>0.65</td>
</tr>
<tr>
<td>Q1 of Income (%)</td>
<td>58.35</td>
<td>53.89</td>
<td>4.47</td>
<td>1.22</td>
</tr>
<tr>
<td>Q2 of Income (%)</td>
<td>31.56</td>
<td>38.42</td>
<td>-6.86</td>
<td>-2.02**</td>
</tr>
<tr>
<td>Q3 of Income (%)</td>
<td>8.00</td>
<td>5.95</td>
<td>2.05</td>
<td>1.27</td>
</tr>
<tr>
<td>Indigent (%)</td>
<td>15.37</td>
<td>9.68</td>
<td>5.69</td>
<td>2.73***</td>
</tr>
<tr>
<td>Poor non-indigent (%)</td>
<td>40.67</td>
<td>42.10</td>
<td>-1.42</td>
<td>-0.41</td>
</tr>
<tr>
<td>Non-poor (%)</td>
<td>43.96</td>
<td>48.23</td>
<td>-4.27</td>
<td>-1.33</td>
</tr>
<tr>
<td>Number of Preschools</td>
<td>133</td>
<td>10</td>
<td>143</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Columns 1 and 2 present the average value for the respective variable for preschools treated (with meeting) and untreated (without meeting). Column 3 shows the difference between groups and column 4 the t-test of the null hypothesis of equality between averages, using standard errors clustered at the preschool level in parenthesis.

* p-value<10% ** p-value<5% *** p-value<10%.

we would expect attendance to be similar across preschools, as we report in Table A3.

O-3 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 of 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 O-1 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, household size or birth outcomes, except for being born at a hospital, for which they are marginally more likely.

Figure O-1: Timing and Probability of Matriculation

O-3.1 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. 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 (or 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 from the Ministry of Education; (iv) the type of the school (whether it is public or private) and

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26 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.

27 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.
(v) its location (i.e. address). We also provided parents a map with all schools included in the Report Card.

**Figure O-2: Report Card - Front**

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.
<table>
<thead>
<tr>
<th>Nivel</th>
<th>NOVAMOR</th>
<th>Dirección</th>
<th>Correo</th>
<th>Departamento</th>
<th>Puntaje total**</th>
<th>Puntaje total***</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Básico 1</td>
<td>República 7054</td>
<td><a href="mailto:info@hola.com">info@hola.com</a></td>
<td>Pichilemu</td>
<td>81</td>
<td>51</td>
</tr>
<tr>
<td>2</td>
<td>Básico 1</td>
<td>República 7054</td>
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**Puntos de la Interacción Social**

***Puntos de la Interacción Social***
In order to send a signal about relatively “good” and “bad” schools, establishments that are 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 O-2, O-3 and O-4 present an example of the Report Card and a map. This aspect mirrors policy maker preferences for the type of intervention that was planned and the hope is this design feature will addresses 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 socio-economic 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, 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 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 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.

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28Specifically the video showed that on average, a person with college degree earns around three times what the average person only with high school does.