Distorted Information:
Evidence from Quality Signals in School Markets*

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Abstract

When information in a market is distorted, choices will potentially be inefficient. We study educational markets in Chile, where school quality is signaled using test scores. We show that quality signals are distorted due to differences in student attendance the day of the test, which translates into misleading information used by parents when choosing schools. We then estimate a school choice model and show that a low-cost information policy that eliminates distortions has positive and non-trivial effects on welfare.

Keywords: schools, quality, disclosure, competition, choice

JEL Codes: I20, L15

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1 Introduction

In markets where quality is imperfectly observed, firms utilize prices and advertising to inform consumers (Milgrom and Roberts, 1986). In the interest of consumers, regulators often implement disclosure policies, by which firms are required to provide information to the public. However, due to the multiple effects that disclosure policies have on firms, this type of policy has the potential to distort information.\(^1\) Distorted information directly affects consumer choices and could have non-trivial welfare consequences due to differences in market opportunities and heterogeneity in preferences.

In this paper, we study distortions in quality signals in a school choice environment. There are two reasons why school markets are particularly interesting. First, governments have developed school accountability systems that utilize quality signals as measures of success. The implementation of programs that rely on distorted quality signals has the potential to cause misallocation of resources. Second, school choices that rely on distorted information could lead to inefficient choices and welfare loss. We are not aware of any empirical attempt to quantify information distortions nor the welfare consequences of providing undistorted information. This is important because if quality signals are distorted in a known way, then correcting these inefficiencies is extremely low-cost.

We study the case of Chile, a market-oriented school system in which quality is signaled through media outlets using a standardized test called SIMCE. This test is taken every year by all students in 4th grade and is the focus of public debates about the quality of education. Using administrative data sets, we document that quality signals (test scores at the school level) are distorted due to non-random heterogeneous changes in attendance of students the day of the test. We provide a comprehensive empirical analysis to understand the variation in distortions and estimate a school choice model to calculate the welfare consequences of providing undistorted information.

Our analysis proceeds in three steps. First, we use administrative data on test-day attendance at the student level to construct measures of distortions in quality signals. We document the existence of distortions using a comparison of test takers and non-takers within schools. This comparison allows us to calculate how the distribution of attending students

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\(^1\)Disclosure policies might, for example, reduce seller effort in detecting quality or create trade-offs across disclosed and undisclosed dimensions of quality. Dranove et al. (2003) shows how providers in the U.S. health system responded to a quality disclosure program by avoiding to serve sicker patients. See Dranove and Jin (2010) for a review of quality disclosure.
changes on test days. Then, drawing from the multiple imputation literature in statistics, we propose a method to recover undistorted quality signals and, as a result, distortions. We utilize these estimated distortions in quality signals for the rest of our analysis.

In the second part of our analysis, we present a discussion of the empirical determinants of distortions in quality signals. We construct a panel dataset of distortions at the school level for the entire educational system between 2005 and 2013. Robust patterns emerge in our analysis. Distortions are not driven by within-school variation in observable characteristics. On the contrary, the variation in distortions is mostly explained by observable and unobservable time-invariant school characteristics. Low quality schools display more distorted quality signals. In addition, because of the salient role of competitive environments in the literature, we perform a comprehensive study of school competition using a spatial analysis.\(^2\) Our results provide no supporting evidence for the hypothesis of larger distortions in more competitive environments. Finally, we use quasi-experimental variation from two government programs and find a limited role for teacher performance prizes and informational channels. We discuss the existing limitations to understand the observed variation in distortions.

In the third part of our analysis, we use a school choice model to estimate the effect of distorted quality signals on school choices and households’ welfare. This exercise allows us to calculate the market value of providing undistorted information about quality. For estimation, we use the exact geographic location of approximately 100,000 students and 1,500 schools for the period 2011–2014. Exploiting the spatial content in our data, we define school markets as the connected components of the spatial network of all schools in the country. Identification of the parameters of the model is achieved using a combination of quasi-experimental variation in government programs, climate-induced variation in test scores, and fixed characteristics of competitors in a given year, i.e., BLP instruments. Our demand estimates are consistent with previous findings and display significant heterogeneity across poor and non-poor households.

Using the estimated preferences, we compute school choices in a counterfactual world with no distortions. Our results suggest that providing households with undistorted quality information would cause five percent of students to switch to higher-quality schools. The welfare gains from providing undistorted information are small on average (less than 1 USD

\(^2\)Recent studies have suggested a link between competitive environments and cheating behavior (e.g., Shleifer 2004, Schwieren and Weichselbaumer 2010, Gilpatric 2011, Cartwright and Menezes 2014).
per student). This result is driven by the fact that (1) most students would not switch schools, and (2) choice and experience utility are equal for households who would not switch. Welfare gains are heterogeneous and larger for households that would switch schools in a counterfactual world with no distortions (10 USD per student). In particular, non-poor households benefit more (14 USD) than poor households (5 USD). These differential benefits are explained by the observed heterogeneity in preferences and not by differences in market opportunities.

Although welfare gains from a policy that provides undistorted information are arguably small, we emphasize the following. First, welfare gains aggregate to approximately 0.5 million USD annually. Second, an intervention that corrects information about quality is free, as it only entails calculating a corrected average instead of an average. Third, distortions could be larger in other markets and countries. And fourth, this is the first estimate of welfare losses due to distorted quality information in school markets.

This paper has two main contributions. In the first place, we quantify the magnitude of distortions in quality signals in school markets. We propose a method to recover distortions that might be applicable to other sectors of the economy. Certain shocks or events might affect the distribution of quality signals in a known way, which makes our method to calculate distortions potentially appealing. In the second place, we quantify the choice and welfare implications of distorted information using an empirical school choice model and distinguishing between choice utility and experience utility.

Our work is related to three strands of the literature. First, it is related to a literature in industrial organization that study the roles of disclosure and advertising in different markets.\(^3\) A relevant question in this literature is whether quality disclosure effectively improves consumer choice. Most of the impacts seem to operate in the margin of vertical sorting, i.e., consumers switch to higher quality products following increased disclosure. Work on analyzing the effects of quality disclosure in educational markets is somehow scarce and has yielded mixed results.\(^4\) Bagwell (2007) provides a thorough review of the advertising literature. We focus on a case in which advertising is informative. Moreover, following the distinction pro-

\(^3\)Dranove and Jin (2010) provide a survey and describe theoretical arguments by which mandatory disclosure may cause undesirable effects.

\(^4\)Hastings and Weinstein (2008) report substantial impacts of providing households in Charlotte with report cards on the quality of chosen schools. Cooper et al. (2013) find evidence in the same direction for households in different cities of Chile, although of a smaller magnitude. On the contrary, Mizala and Urquiola (2013) analyze household responses to a program that disclosed school quality in Chile and find no impacts.
posed by Nelson (1970), the fact that schooling is an experience good implies that quality is hardly verifiable ex-ante, which implies that information acquired from advertising may be particularly important for consumers. Our paper adds to this literature in two lines. First, by focusing on educational markets, where there is limited work from an advertising perspective. Second, by measuring the implications of deceptive advertising in this market.

This paper is also related to a second literature that focuses on studying the perverse incentives of accountability systems in schooling markets, which have become increasingly frequent in the last decade (e.g., Jacob and Levitt 2003, Figlio and Winicki 2005, Neal and Schanzenbach 2010, Rouse et al. 2013). Moreover, as discussed by Figlio and Loeb (2011), incentive programs designed on the basis of standardized tests are often subject to perverse incentives, which is related to the discussion proposed in Neal (2013) regarding the usage of test scores for multiple objectives. Our paper shows how heterogeneous attendance on test day affects the provision of information and endows households with inaccurate information for their school choices.

Finally, this paper is also related to the literature studying school choice in competitive markets. This literature has focused on estimating households’ preferences over schools’ characteristics (e.g., Gallego and Hernando 2009 and Neilson 2014 for Chile, and Bayer et al. 2007, Hastings et al. 2009 and Walters 2014 for the U.S.). These papers generally find that school fees, distance between home and school, and school quality are the most relevant attributes of schools. The literature that focuses on understanding the role of information in school choice is still at an early stage (e.g., Hastings and Weinstein 2008, Cooper et al. 2013, Andrabi et al. 2014). To our knowledge, there is no paper measuring the value of information for school choice.

The remainder of the paper is organized as follows. In section 2, we describe the educational market in Chile, we present data sources, and study attendance on test day. In section 3, we construct measures of distortions in quality signals and provide an empirical discussion of explanations behind observed distortions. In section 4, we estimate a school choice model and study the impacts of a policy that eliminates distortions.

2 School Markets and Attendance on Test Day

Our analysis focuses on the Chilean market for primary schooling. After a market-oriented reform was implemented in 1980, education has been served by a mixture of public and pri-
Private (voucher and non-voucher) schools. Public schools are fully funded by the government. Private voucher schools are privately managed, although eligible for receiving public funding through vouchers. Private voucher schools are allowed to charge fees to parents in the form of copayments, although vouchers are phased out on the basis of those. Private non-voucher schools are privately managed and not eligible for receiving public funding. Over the last three decades, the private sector has steadily increased its market share.\(^5\)

In our analysis, we use multiple administrative data sets provided by the Ministry of Education. First, we use the administrative record of schools operating every year between 2005 and 2013. We observe the following characteristics of schools: type (public, private), enrollment, fees, participation in government programs and, importantly, their addresses, which we use to construct markets. Second, we use the administrative record of students between 2005 and 2013 (approximately 3.5 million every year). In this data set we observe demographic characteristics of students and their annual academic performance. Third, we use the performance of students at the national standardized test (SIMCE) to estimate distortions in quality signals. This test is implemented every year, during two days, and at the national level for a subset of grades. We focus on 4th grade, as it is tested every year in the period 2005–2013. Fourth, we use daily attendance to school, available for all students in 2013, to study heterogeneity in attendance on test day.

### 2.1 Descriptive statistics

We construct two datasets: (1) a panel dataset of schools observed every year between 2005–2013, and (2) a panel data set of students observed daily in 2013. Although remarkably rich, the student level data set is not available for all years, and is not available for non-voucher schools. Therefore, we only consider public and voucher schools in our analysis, which represent 93% of enrollment in 2013. The school level data set contains yearly information on schools offering 4th grade. The entry and exit of schools makes this panel unbalanced. There are 8,254 different schools and, on average, 6,200 schools operating in a given year. Panel A in Table 1 presents summary statistics for these schools: 51% are public, 42% are voucher schools, 6% are private, and 31% are located in rural areas. The average school serves approximately 39 students in 4th grade. More than half of schools charge no fees, and the average monthly fee is approximately 22 USD. The average SIMCE test score is 245 and

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\(^5\)In 2013 public schools concentrated 38\% of enrollment, while private voucher and non-voucher schools concentrated 54\% and 8\% of enrollment respectively (MINEDUC, 2013).
the standard deviation is 31.

Panel B in Table 1 presents descriptive statistics for the student level data set. For simplicity, we use math test scores throughout the paper. The academic performance of a student is captured by her GPA, which goes from 1 to 7, with a threshold of 4 to pass a class. The mean of this variable is 5.9. The last two variables are attendance rates in test day and non-test day. The former is simply the average of two indicator variables that take the value of one if a student went to school in test day—recall that there are two test days, so this variable takes the value of 0, 0.5, or 1 at the student level. The latter is the average attendance in the five non-test days previous to the test day.

### 2.2 Attendance on test day

School quality is signaled through media outlets using the average test score of students: if average test score is relatively high, then parents infer relatively high quality. As the average test score of a school is a function of which students take the test, quality signals are distorted if there is non-random variation in attendance on test day. We now show how attendance to school changes the day of the test. Because the central government attempts to increase attendance on test day through advertising, and schools have incentives to prevent low-performing students taking the test, it is not a priori clear if attendance should increase or decrease on test day. Moreover, our main concern is not with the average change, but with the heterogeneity behind this change.

We compare the attendance rate of 4th graders (who take the test) to attendance of 3rd graders (who do not take the test) daily around test day in 2013 (October 8th and 9th) to estimate changes in attendance:

\[
\overline{A}_t = \overline{A}_{4t} - \overline{A}_{3t}
\]  

(1)

where \( t = \{ -5, \ldots, 5 \} \) indexes days, with \( t = \{ 0, 1 \} \) denoting the two test days. We calculate \( \overline{A}_t \) in four subsamples of students: high-performing, above the 90th and 75th percentile of the GPA distribution, and low-performing, below the 10th and 25th percentile of the GPA distribution. In addition, to study the heterogeneity behind \( \overline{A}_t \), we calculate the following school-specific changes in attendance on test day:

\[
\overline{A}_{jt} = \overline{A}_{j4t} - \overline{A}_{j3t}
\]  

(2)
where $A_{jkt}$ is average attendance of students in $k$th grade at school $j$ in day $t$. In the next section, we show that a larger variance of $A_{jt}$ translates into more distorted signals.

Figure 1 presents $A_t$ and $A_{jt}$. In the upper panel, we plot the differential attendance rate around test day. On average, attendance increases by 2 percentage points, with the increase being larger among high-performing students than among low-performing students. Against the hypothesis of schools asking low-ability students to stay home on test day, we do not observe a decrease in attendance of students with low GPA. These averages, however, mask significant heterogeneity. In the lower panel, we plot the distribution of $A_{jt}$. The vertical red line denotes the average increase increase of 2 percentage points. It is this variance in attendance that causes quality signals to be distorted.

## 3 Distortions in Quality Signals

We argue that the quality signal of a school is “undistorted” if all students take the test. Empirically, however, the existence of non-random heterogeneous absenteeism the day of the test distorts quality signals. In this section, we describe our methodology to estimate the magnitude of distortions at the school-year level. In addition, we perform a comprehensive analysis to understand why these distortions exist.

### 3.1 Estimating undistorted quality signals

We consider a counterfactual scenario with no absenteeism to calculate undistorted quality signals. Although daily attendance is not available for all years, it is possible to identify absenteeism on test day at the student level using the administrative records of annual academic performance and test scores: students with non-missing academic performance but without test scores were absent on test day. The empirical challenge consists in estimating test scores for absent students. We use multiple imputation methods developed by Rubin (1987) to estimate test scores of absent students.

There are three steps in the multiple imputation method. In the first step, we define the variable with missing data and the predictors. Let $s_{ijt}$ be the test score of student $i$ in school

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6 As described by Rubin (1976), random absenteeism within a school would not lead to bias in quality signals. Absenteeism within a school is, however, non-random.
$j$ and year $t$, and $x_{ijt}$ be a matrix of variables that predict test scores at the student level:

$$s_{ijt} = f(x_{ijt}; \gamma_{jt}) \tag{3}$$

where $\gamma_{jt}$ is a matrix of parameters, which are to be estimated using the statistical model $f(\cdot)$ in the sample of observed test scores. The second step corresponds to the choice of $f(\cdot)$. For simplicity, we use a linear regression as statistical model, although results are robust to using other models. Importantly, the parameters $\gamma_{jt}$ are estimated with uncertainty. This uncertainty can be taken into account in the imputations using draws from the asymptotic variance of the estimated parameters $\hat{\gamma}_{jt}$.\(^7\)

In the third step, we specify predictors of student test scores within schools, i.e., $x_{ijt}$. These variables need to be strong predictors of test scores, both statistically and theoretically, and need to be available for all students in our dataset. We choose GPA at the end of the academic year and the following indicator variables: students who were in 4th grade the previous year, and students who studied at a different school the previous year. The latter variables capture academic history and context respectively. This model can be used to predict test scores at the student level during the period 2005–2013. As displayed by Figure A.1, GPA is a strong predictor of test scores.\(^8\)

After estimating test scores of absent students, we can estimate the “undistorted” quality signal $Y_{jt}$. This can be done by taking the average of test scores across students within schools every year. As we have multiple estimates of missing test scores, we estimate multiple average test scores for each school-year. Our estimate of an undistorted quality signal is the average of multiple averages. Let $\hat{Y}^{(n)}_{jt}$ be the average test score at school $j$ in year $t$ calculated using draw $n = 1, \ldots, N$ from the asymptotic variance of the estimated parameters. Then, our estimate for the undistorted quality signal is:

$$\hat{Y}_{jt} = \frac{1}{N} \sum_{n=1}^{N} \hat{Y}^{(n)}_{jt} \tag{4}$$

The uncertainty of our estimates is reflected in the variance of $\hat{Y}^{(n)}_{jt}$. We order $\hat{Y}^{(n)}_{jt}$ from

\(^7\)Note that parameters are specific to a school-year. One might worry that the asymptotic variance is poorly calculated when using a small number students. In order to address this concern, we have repeated our analysis using a bootstrap procedure and results are similar.

\(^8\)In additional exercises we replace average GPA by average GPA in Math and Language separately. Unfortunately, these variables are only available for the period 2011–2013.
lowest to highest within a school-year and take the percentiles 2.5 and 97.5 to generate bounds for our estimate $\hat{Y}_{jt}$.

### 3.2 Descriptive statistics of distortions

Let $\psi_{jt}$ be the distortion in quality signal at school $j$ in year $t$, defined as the difference between observed ($y_{jt}$) and undistorted ($Y_{jt}$) quality signals. As $Y_{jt}$ is unobservable, we use $\hat{Y}_{jt}$ from equation (4). Then, our estimates for distortions in quality signals are defined as:

$$\hat{\psi}_{jt} \equiv y_{jt} - \hat{Y}_{jt}$$

(5)

We generate bounds for these quantities using the bounds for $\hat{Y}_{jt}$. Every school-year in our dataset has an estimated distortion, together with a lower and upper bound, thus acknowledging the uncertainty that involves estimating an unobserved parameter such as the arithmetic mean of test scores across all students within a school.

To increase precision, our analysis restricts attention to schools in which the statistical model in equation (3) uses more than 10 observations. More precisely, we study schools in which the difference between the number of students enrolled and the number of students taking the test is greater than 10. Schools dropped from our analysis have few students, are usually located in rural areas and (not surprisingly) their estimated distortions have extremely wide bounds. We are confident that this restriction does not diminish the external validity of our results, as our analysis includes 96 percent of enrolled students in 4th grade.9

Figure 2-A presents estimated distortions for all schools and years in our data set. The $y$-axis represents distortions (in test score points), while the $x$-axis orders schools from lowest to highest distortion. In addition, distortions in green (gray) are (not) statistically different from zero. Approximately 24 percent of distortions are statistically larger than zero, and 65 percent of schools have a positive distortion. Some schools experience no absenteeism on test day and their distortions are zero with no confidence interval.

Figure 2-B presents the distribution of distortions. The average school in our dataset has a positive distortion of 2.7 test score points, approximately 0.1 standard deviations of observed test scores. The distribution of distortions has a standard deviation of 3.9 test score points and it is skewed towards the right. The facts that (1) the average distortion is

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9The total number of students in the full dataset is 2,203,707, a number that decreases to 2,114,347 when restricting attention to schools with more than 10 students taking the test.
different from zero, and (2) the distribution is far from normal, make clear that distortions in quality signals are not random variation in test scores.\textsuperscript{10}

3.3 Understanding distortions in quality signals

What explains the variation in distortions in quality signals? We now present a discussion of the empirical determinants of distortions. For this purpose, we employ a panel data set of distortions at the school level for the entire educational system between 2005 and 2013.

A significant share of the variation in distortions is explained by school time-invariant characteristics. Consider a regression of distortions on school fixed effects and three estimation samples. In the first sample, we regress all estimated distortions on school indicators and find that we can explain 35 percent of the variance in distortions. In a second sample, we restrict attention to schools in which at least one student did not take the test and find that school fixed characteristics explain 42 percent of the variance in distortions. In a third sample, we restrict attention to schools with distortions that are statistically positive and find that we can explain 69 percent of the variance in distortions. This is a significant share of the variation, specially considering that the maximum variation that can be explained is probably lower than one due to measurement error in the dependent variable. Consistent with these patterns, in the appendix we show that within school variation in observable characteristics of schools have little predictive power over distortions.\textsuperscript{11}

What fixed characteristics of schools predict distortions? In Table 2-A we provide correlations that are consistent with distortions being larger in small public schools with low historical attendance rates. These regressions correspond to pooled OLS estimates weighted by the inverse of the uncertainty associated with the estimation of distortions, where uncertainty is defined as the size of the 95 percent confidence interval. These correlations are particularly pronounced in schools with distortions that are statistically different from zero. Additionally, Table 2-B presents the autocorrelation of distortions, which is always positive.

\textsuperscript{10}In Figure A.2 of the appendix we present an empirical analysis of the rank correlation between undistorted and distorted quality signals at the market-year level. See section 4.1.2 for market definition. Approximately 40 percent of rank correlations are different from one, which suggests distortions in quality signals cause changes in the rankings of schools.

\textsuperscript{11}Figure A.3 displays the results of non-parametric regressions of distortions in quality signals on school characteristics, including school and year fixed effects. The only covariate that shows a strong relationship with distortions is the number of students absent on test day. This was expected as it is related to how distortions are computed.
and significantly different from zero. This positive autocorrelation is additional evidence that distortions are non-random.

A particularly salient explanation behind distortions could be the role competitive environments play in causing perverse incentives (e.g., Shleifer 2004). To study the role of competition, we perform a comprehensive spatial analysis of distortions using the presence and observable characteristics of schools within 3 kilometers (1.9 miles). In particular, we estimate the following regression equation:

\[ \hat{\psi}_{jt} = f(X_{jt}) + \eta_j + \nu_t + \varepsilon_{jt} \]  

(6)

where \( X_{jt} \) is an observable characteristic of schools within 3 kilometers from the reference school \( j \) in year \( t \), \( \eta_j \) is a school fixed effect, \( \nu_t \) is a year fixed effect, and \( \varepsilon_{jt} \) is an error term clustered at the school level. Due to the extensive number of variables that we cover, we present these results in the appendix.\(^\text{12}\) A leading \( X_{jt} \) variable corresponds to the number of schools operating close to the reference school, variation that essentially exploits the plausibly exogenous entry and exit of schools. The empirical evidence provides no support for the hypothesis of larger distortions in more competitive environments.

Finally, we exploit quasi-experimental variation from two government programs to understand the variation in distortions. More detail on this exercise is presented in Appendix A and results are displayed in Figure A.5. The first is a biannual performance pay system called SNED.\(^\text{13}\) This program effectively increases the wages of teachers in a school if students obtain high test scores and it provides variation in incentives depending on the probability that a school will earn the prize. Distortions are not higher in schools that are more likely

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\(^{12}\)Figure A.4 displays the results of non-parametric regressions of distortions in quality signals on a number of covariates related to the competitive environment faced by the school and its position relative to it, including school and year fixed effects. The only covariate that shows a notable relationship with distortions in this analysis is the percentile of the school in the distribution of quality in the market, for which the relationship is positive although small in magnitude.

\(^{13}\)SNED is a school performance evaluation system that takes the form of a tournament and provides awards to improved schools. SNED operates as follows: (i) groups of homogeneous schools are constructed, within which the contest is implemented; (ii) every two years, a multidimensional index is computed at the school level, which considers academic performance and improvement and socioeconomic integration among other outcomes; (iii) schools are ranked within their groups according to the value of such index; and (iv) schools covering the 25% – 35% of the total enrollment of each group get a monetary prize in an amount equivalent to around 40% of a monthly wage of a teacher, for each teacher in the school –the coverage of the prize was increased to 35% of the enrollment of the group since 2006–. Importantly, across dimensions of the index, SIMCE test scores account for as much as 70% of the weight of the components used for its calculation (Contreras and Rau, 2012).
to win the prize, providing evidence against the hypothesis that teachers will manipulate attendance to increase test scores.

A second government program we exploit, called “Educational Traffic Lights,” classified schools in 2010 according to their quality (test scores) in three categories: red (bad), yellow (regular), and good (green).\textsuperscript{14} More detail on this exercise is presented in Appendix A and results are displayed in Figure A.6. The cutoffs of these categories provide quasi-experimental variation in the incentives to manipulate test scores. We find some evidence that low quality schools have higher distortions around the low cutoff (red to yellow), but no differential distortions in the higher cutoff (yellow to green).

Overall, the empirical patterns presented in this section are consistent with distortions being a non-random phenomenon that is a function of school fixed characteristics. The fact that distortions are not explained by within-school variation in observable characteristics shed some light in the mechanisms generating distorted signals. However, it is certainly possible that a heterogeneous response of schools to idiosyncratic events generate a non-random distribution of attendance on test day. For example, schools might heterogeneously react to the government program that attempts to increase attendance on test day. The possibility of heterogeneous responses to events limits our empirical ability to understand distortions, but it does not prevent us from estimating the inefficiencies they create.

4 Welfare Consequences of Distorted Information

In the final part of our analysis, we estimate a school choice model to evaluate the impacts of distorted quality signals. Using the estimated preferences of parents, we implement the counterfactual exercise of providing undistorted quality signals. This exercise allows us to compare observed with counterfactual outcomes, as well as to compute the welfare loss caused by distortions.

\textsuperscript{14}ETL was announced in April, 2010 and consisted of sending information to all households in the market on the performance of schools in the SIMCE test implemented in 2009. That information included both test scores and a classification of schools as Red, Yellow or Green according to their test scores, with clear cutoffs determining such outcome. An evaluation of this policy by Allende (2012) that uses the discontinuities in such classification for identification, finds that it effectively impacted school enrollment: households in the margin responded by enrolling more in yellow than red schools and more in green than yellow schools.
4.1 School choice model

We estimate a simple model of school choice in the lines of Bayer et al. (2007). When constructing the model, we impose certain assumptions. First, households are assumed to have full information both regarding the set of available schools and their observed characteristics. Second, we assume schools do not select students based on attributes and do not face capacity constraints, i.e., households can enroll their children in any school in their choice set. As discussed by Neilson (2014), these assumption is likely to hold in the Chilean school system. Finally, we assume the household’s location choice is independent of the school choice problem. Although strong, this assumption is supported by the fact that there are no institutional constraints on the choice set based on residential location.

Let households be indexed by $i$ and schools by $j$. Households derive utility from schools’ fees, quality, and distance from their household, denoted respectively $p_j$, $q_j$ and $d_{ij}$. They also derive utility from other school characteristics $W_j$. For notational simplicity, we denote $X_j = [p_j, q_j, W_j]$, which includes $K$ attributes. Preferences are heterogeneous depending on households’ type, indexed by $r$. In our model, only observed heterogeneity in preferences is considered, as further explained below. Moreover, we allow for households to derive utility from schools’ unobserved characteristics $\xi_j$. Finally, each household has an idiosyncratic preference shock, $\varepsilon_{ij}$, which we assume is distributed T1EV.

Under these assumptions, household’s $i$ of type $r$ indirect utility from enrolling their children in school $j$ is:

$$u_{ij}^r = \sum_k x_{k,j} \beta_k^r + \xi_j^r + \beta_d^r d_{ij} + \varepsilon_{ij}$$  \hspace{1cm} (7)

where the first two terms measure utility from characteristics that depend only on the school and are therefore constant across households of type $r$ for a given school $j$, while the third term measures disutility from distance between household $i$ and school $j$ for households of type $r$, which varies across households. We can therefore rewrite equation (7) as follows:

$$u_{ij}^r = \delta_j^r + \beta_d^r d_{ij} + \varepsilon_{ij}$$  \hspace{1cm} (8)

such that the parameters of the model are contained in the vector $\beta^r$, but can be alternatively estimated.

\textsuperscript{15}If this assumption does not hold, our estimation would potentially yield attenuated elasticities. However, it is not in the scope of this paper to explore further in this direction.
represented by the vector $\delta^r$ and by $\beta_d^r$. Note that $\delta^r_j$ is the component of utility derived from choosing school $j$ that is constant across individuals, the mean value of school $j$ for households of type $r$.

The probability of household $i$ choosing school $j$ can be derived analytically using households’ utility. The choice probability of school $j$ by household $i$ of type $r$ predicted by the model is a function of schools’ and households characteristics:

$$
p_{ij}^r(\delta^r, d^r, \beta_d^r) = \frac{\exp(\delta^r_j + \beta_d^r d_{ij})}{\sum_{l \in J_i} \exp(\delta^r_l + \beta_d^r d_{il})}
$$

where $J_i$ is the set of schools in the market where household $i$ is located. We use this result in the next subsections for both estimation of the model and for computing the counterfactual exercise of interest.

### 4.1.1 Estimation

We estimate the parameters of the model using a two step procedure. First, we estimate standard conditional logit models for each group $r$ in each market and year in the data. Second, we exploit the assumed linear functional form of the utility function of households in order to estimate the relationship between the preference parameters and school-level characteristics.

The first stage of the estimation procedure consists of estimating equation (9), which can be done by maximum likelihood. In order to allow for heterogeneity in preferences, this procedure is implemented within each of multiple cells defined in on the basis of $R$ socio-economic levels, $T$ time periods and $M$ markets. The former is determined by the eligibility of a student for a national program called Subvención Escolar Preferencial (SEP), which is determined fundamentally by participation in social programs that aim at supporting poor households. Therefore, we estimate $R \times T \times M$ conditional logit models in the first stage, which yields the same number of estimates for $\delta^r$ and $\beta_d^r$.

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16In school choice models there is no obvious outside option. Therefore, we follow Neilson (2014) and instead normalize $\delta_1 = 0$ within each market.

17The SEP program was introduced in 2008 as a targeted voucher for poor students, with the objective of providing additional funding for schools serving them. Students’ eligibility to the program is essentially determined by household income and social programs’ participation. All public schools are eligible for SEP vouchers, while only vouchers schools that sign up for the policy are so. Schools that choose to sign up with the policy must comply with additional programs related to the usage of funds provided by SEP.
The second stage exploits the assumed linear functional form of the utility function in order to estimate the following linear regression:

\[
\delta_{jmt} = \delta_{0,mt} + \sum_k x_{k,jmt} \beta_k + \epsilon_{jmt}
\] (10)

where \(\delta_{0,mt}\) is a constant term specific to each market, year and household type; \(\beta_k\) measures the effect of \(x_k\) on school mean value for households of type \(r\) and maps to the preference parameters of our model; and \(\epsilon_{jmt}\) is a mean-zero error term. Note that \(\delta_{0,mt} + \epsilon_{jmt}\) maps to the unobserved school characteristic \(\xi_{jmt}\) in our model.

A concern with this regression is the potential endogeneity of school characteristics, in particular of prices and quality. Therefore, we estimate this regression using an instrumental variables approach. We use various instruments. First, for each school, we include as instruments the non-price and non-quality characteristics of other schools in the market, in lines with instruments suggested in Berry et al. (1995). Second, we follow Neilson (2014) and use teachers’ wages of other schools in the market, which arguably operates as a cost shifter for schools, such that it may affect their choices of prices, but not their unobserved attributes. Third, we use the amounts of funding provided by different components of voucher programs to schools, which display within market variation due to school characteristics that are fixed in the short run. Moreover, we instrument for quality offered by schools using a dummy variable for whether a school was awarded a SNED prize in its most recent version before each school choice year. This instrument is motivated by Contreras and Rau (2012), who show how these prizes impact quality in subsequent years.\(^{18}\) Finally, we utilize temperature data at the county level on the day in which the quality signal was generated (i.e., the days in which the SIMCE test was taken) as an instrument for quality, which is motivated by a literature that studies the relationship between climate and academic achievement, as discussed in Graff Zivin et al. (2015).\(^{19}\)

We estimate the model using data for years 2011 through 2014, the only years in which students’ location data is available. In addition, we only utilize data for students in 1st

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\(^{18}\)In practice, we utilize the residual of a regression of the SNED award dummy on quality in the year of the award, in order to further control for differences in quality between SNED awardees and non-awardees that may be driven by other factors that could be persistent in time.

\(^{19}\)We construct this variable using data from the Berkeley Earth dataset, which provides population-weighted estimates of daily temperature at the county level. In implementing the regression, we include both temperature and temperature squared in order to account for non-linear effects of temperature on academic achievement documented in Graff Zivin et al. (2015)
grade in order to focus on the margin in which most school choices are made. In terms of covariates to be included in the vector $X_j$, we include school fees, quality as measured by average SIMCE test score of the school, whether the school has a religious orientation or not, whether a school is public, and whether a school is part of the SEP program. 20 Finally, we are able to compute the distance between households and schools using georeferenced data on their locations. 21

4.1.2 Market definition and estimating dataset

Defining markets in contexts of spatial competition is difficult because determining which suppliers belong to the choice set of consumers is not straightforward. In contrast to the case of many school systems, in Chile there are no institutional constraints that limit the extent to which students can travel. Therefore, we need to define markets.

We adopt an approach based on the spatial distance between schools. Distance has been shown to be the most relevant determinant of school choice by previous studies in the literature. In our data, students average distance to chosen schools is of 2 kilometers (1.3 miles) and the 90th percentile of such distribution is 4.8 kilometers (3 miles). Therefore, it is sensible to argue that schools located far enough from each other may belong to different educational markets. We define an educational market as a cluster of schools in a closed polygon with no other school closer than 5 kilometers (3.1 miles) from its boundaries. Operationally, a market is uniquely identified from the adjacency matrix of schools, where links are defined as two schools being closer than 5 kilometers from each other. In implementing this procedure, and therefore in estimation as well, we only consider schools located in urban areas. For estimation, we only include markets with at least 20 schools and for which we have data for at least 300 students. The map presented in Figure A.7 provides an example for the resulting market definitions and Table A.1 displays summary statistics for it. 22

A description of the resulting sample is displayed by Table 3. The number of households

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20 We use data on monthly copayment faced by households as a measure of school fees. Moreover, we use data on whether students’ eligibility for SEP in order to adjust school fees accordingly: eligible students do not pay any school fees in schools that operate under the SEP regime.

21 We compute the Euclidean distance between every household and school in each market. We then proceed to clean these results by (i) removing mass points, which arise from imperfect georeference; and (ii) removing students located further than 55 kilometers from the median location in the market.

22 As a robustness exercise, we estimated the model using counties as markets. For estimation, we included counties for which a large share of students resided in the market (at least 90%) and were we had available data for more than 300 students. Our estimates were quantitatively similar.
types is \( R = 2 \), the number of markets included in the sample is \( M = 25 \), and the number of periods covered is \( T = 4 \). Therefore, the estimating dataset is comprised by 200 cells. The estimating dataset includes 1,556 schools and 97,471 students. On average, 32.6% of the students attending schools in markets in our sample are included in it, and 91.7% of the schools operating in each market are so respectively. Moreover, an average of 49.4% of students included in the sample across markets are eligible for the SEP program, which is the subsample of poor students.\(^{23}\)

4.1.3 Results

Given that the most relevant dimension of households’ types is socioeconomic levels, we present all the results by poor and non-poor households separately. Figure A.8 displays the resulting coefficients in each market for distance between households and schools, for both poor and non-poor households. In all these cases, the coefficient is negative, which reflects a decreasing utility for households from choosing a school further away from home. Interestingly, poor households show to be in average 14.2% more distance-sensitive than non-poor households.

Table 4 presents results for different specifications of instrumental variables linear regressions of the estimates of \( \delta_{jmt} \) on different sets of school characteristics and fixed effects. Columns 1 through 3 display results for all households in the sample, columns 4 through 6 display results for poor households and columns 6 through 9 do so for non-poor households. Overall, results point in the expected direction: households’ utility is decreasing in fees charged by schools and increasing in their reported quality. Adding market-year fixed effects to the regression increases the point estimates with respect to the baseline case, while adding other school attributes do not change them substantially.

There are interesting patterns of heterogeneity across poor and non-poor households. For instance, our preferred specifications in columns 6 and 9 imply that poor households are 92.3% more price-sensitive than non-poor households. Inversely, poor households are estimated to be 38% less quality-sensitive than non-poor households. These results imply in turn that the willingness to pay for quality of non-poor households is almost 3 times higher than that of poor households. This heterogeneity suggests that policies regarding

\(^{23}\)We tested for differences in observables across students included and excluded of the sample, within each market. While some of the differences across groups are statistically significant, these are not economically significant and do not show a clear pattern.
quality disclosure will have heterogeneous effects across these demographic groups. These patterns of heterogeneity coincide with previous findings within the school choice literature (e.g., Gallego and Hernando 2009, Hastings et al. 2009, and Neilson 2014).

4.2 Welfare analysis

In our setting, schools’ quality signals are distorted and therefore households choose schools on the basis of a misperceived vector of attributes. A key aspect of the situation however, is that while perceived school quality may be different than true quality, the school quality households ultimately obtain from a school is the true quality of their school choice. Throughout this section, we emphasize this aspect and account for it when measuring implications of distorted quality signals.

In order to compute the effects of distorted quality signals on choices and welfare, we define two scenarios: baseline and counterfactual. The former corresponds to the environment in which households actually choose schools. The latter corresponds to a counterfactual world in which households are provided with undistorted information about school quality. This exercise rules out changes in other variables (e.g., schools fees and school investments) as well as the existence of capacity constraints.

Throughout this section, we utilize our estimates for $\delta^r$ and $\beta^r_j$, and the observed vector of schools’ characteristics $X_j$ to compute choice probabilities and consumer welfare for the baseline scenario. For the case of the counterfactual scenario, calculations additionally use estimates of $\beta^r_j$ from the second stage of the demand model, and a counterfactual vector of school characteristics $\tilde{X}_{ij} = [p_j, \tilde{q}_j, W_j]$, where $\tilde{q}_j$ stands for undistorted quality of school $j$.\footnote{More precisely, we utilize the results for the second stage from our preferred specifications: columns 6 and 9 of Table 4.}

4.2.1 Choices

We begin the analysis by examining schools’ choice probabilities by households across both scenarios. We do so by adjusting the choice probabilities predicted by equation (9) of our demand model and using the estimates from such model and the data on school attributes for both scenarios. Following equation (9), choice probabilities are therefore computed as

$$p_{ijmt}(d^r, \delta^r_j, \beta^r_j) \text{ and } p_{ijmt}^c(d^r, \tilde{\delta}^r_j, \beta^r_j),$$

where $\tilde{\delta}^r_{jmt} = \sum_k \tilde{x}_{k,jmt} \beta^r_k + \tilde{\xi}^r_{jmt}$ is the mean utility of school $j$ in market $m$ in period $t$, computed using demand estimates and data on...
counterfactual school quality.

Figure A.9 displays the computed changes in choice probabilities between both scenarios. It is interesting to note that, despite the fact that the magnitude of the distortions is moderate, there is significant heterogeneity. This pattern holds when restricting the analysis to the set of schools actually chosen by parents, as displayed by Figures A.9-C and A.9-D. This shows that changes in the quality disclosure system would induce changes in choices by households. However, given that households have a limited number of schools in their choice sets, these changes in choice probabilities may only induce actual changes in choices for a small fraction of households. This, as marginal changes in the observed vector of schools’ quality may not be strong enough as to induce households to change their schools choices. Note that non-poor households display more variation in the computed changes, which is driven by their higher quality-sensitivity. Relatedly, a simple simulation based on the proposed model and our estimates shows that 2.2% of poor households and 2.7% of non-poor households would be induced to changing their school choice when provided undistorted quality information. We denote this subpopulation as switchers.\textsuperscript{25}

We compute the predicted attributes of schools chosen by households under both scenarios. Table 5 displays results from these calculations for both poor and non-poor households. We report both the average across all households as well as the average within the subpopulation of switchers. Columns 1 and 3 in Table 5 display results for the average across all households. It is easy to note that changes in predicted distance to chosen schools and fees charged by those are small. This is as expected, since non-switching households have their choices unaffected by the information policy we evaluate. The average changes in attributes of chosen schools by poor and non-poor households are not larger than 0.03 s.d. for any of the attributes we consider.

When considering the subpopulation of switchers, however, changes in attributes of chosen schools between both scenarios are substantive. Columns 2 and 4 in Table 5 display results for these groups of households. First, note that in the baseline scenario, switchers where receiving substantially less quality than the average household, which suggests that switchers come out mostly from schools that had highly distorted quality signals. Conditional on switching, we observe that households choose to travel substantially longer distances to chosen schools, to pay higher fees and, importantly, that they choose schools with remarkably

\textsuperscript{25}We calculate switching rates by simulating choices of consumers in our sample in both the baseline and counterfactual scenarios. Reported results corresponds to average switching rates for poor and non-poor households over 1,000 simulations across all households in the sample.
higher levels of true quality. In particular, our results show that poor (non-poor) switchers would choose schools with 1.02 s.d. (0.8 s.d.) higher true quality in the counterfactual than the baseline scenario. This would be coupled by an increase in fees charged by chosen schools of 0.79 s.d. (0.4 s.d.) for poor (non-poor) switchers and, similarly, an increase in distance travelled to chosen schools of 0.1 s.d. (0.07 s.d.). These results imply that the subpopulation of switchers would change its choices in a substantial way. Switchers move towards higher-quality schools, for which they would be willing to both travel more and pay higher fees.

4.2.2 Welfare

We now calculate the welfare changes of providing undistorted quality signals. In the baseline scenario households choose schools using the observed measure of school quality which, as discussed, is distorted. However, the effective utility of consumers is determined by undistorted school quality. Thus, our baseline scenario is a case in which choice utility and experience utility differ (Train, 2015). This is not the case in the counterfactual scenario, in which households choice and experience utility coincide.

Let \( u_{ij} \) be the utility of household \( i \) from school \( j \) under distorted school quality, choice utility. Similarly, let \( \tilde{u}_{ij} \) be the utility of household \( i \) from school \( j \) under undistorted school quality, experience utility. In our setting, these two utilities are related. Given that the only difference between choice and experience utility is the misperception of quality under the former, we know that \( \tilde{u}_{ij} = u_{ij} + \tau_j \), where \( \tau_j \) measures the wedge between choice and experience utility from school \( j \). Under the utility function assumed in section 4.1, we know that \( \tau_j = \beta_q(q_j - q) \).\(^\text{26}\)

The choices household \( i \) would make in each scenario would be:

\[
\begin{align*}
\hat{j}^*_i &= \arg \max_j \{ u_{ij} \} \quad j \in \mathcal{J}_i \\
\tilde{j}^*_i &= \arg \max_j \{ \tilde{u}_{ij} \} \quad j \in \mathcal{J}_i
\end{align*}
\]

which may or may not differ. Importantly, if the choice is the same in both scenarios then there is no welfare loss from distorted quality signals for that households, as experience utility is the same in both cases. This makes clear that welfare losses will be driven by households

\(^{26}\)From this expression, it becomes clear that at baseline all schools with positive distortions have \( \tau_j < 0 \), such that experience utility from those schools is lower than choice utility from them.
that were changing their behavior in the baseline scenario due to distorted quality signals, the switchers.

The change in household welfare from providing undistorted information would therefore be the difference in experience utility between the counterfactual and baseline scenarios, \( \tilde{u}_{it} - \tilde{u}_{it^*} \). Using the fact that \( \tilde{u}_{it^*} = u_{it^*} + \tau_{it^*} \), we can compute the expected welfare change as:

\[
E[\Delta W_i] = \frac{1}{\beta_p} \left[ \log \sum_j \exp(\tilde{v}_{ij}) - \log \sum_j \exp(v_{ij}) - \sum_j P_{ij} \tau_j \right]
\]

(11)

where we define \( \tilde{v}_{ij} = \delta_j + \beta_d d_{ij} \) and \( v_{ij} = \delta_j + \beta_d d_{ij} \) for notational simplicity. The first and second terms measure consumer surplus under undistorted and distorted school quality information respectively, and the results follow from the inclusive value formula in Small and Rosen (1981) given the assumed utility function. The third term measures the expected difference between choice and experience utility at baseline. Dividing by \( \beta_p \) simply transforms the welfare loss to monetary units. This formula calculates the average welfare gain over the whole sample of households. We can then compute average welfare gains for switchers or aggregate these gains across different dimensions. This welfare gains can alternatively be interpreted as the average willingness to pay for undistorted quality information by households.

The results from show that expected welfare would increase in the counterfactual scenario for all households. This is expected, non-switchers will obtain the same welfare in both scenarios, while switchers will be strictly better off in the counterfactual. For poor households, the average welfare gain we estimate is less than 1 USD. The average welfare gain for non-poor households is also less than 1 USD. Scaling up these results for the educational system, welfare gains would add up to 472,824 USD annually.

These results suggest small gains from providing undistorted information across all households. However, the relevant population for an intervention like the one proposed in our counterfactual is not the average household, but rather the subpopulation of switchers, households that would change their school choice in the counterfactual with respect to the baseline scenario. Welfare gains are larger for this subpopulation. The average welfare gain for switchers is of 5 USD among poor households and of 14 USD among non-poor households.
4.2.3 Heterogeneity in welfare gains

The fact that non-poor households benefit more than poor households from the information policy is evident, and the magnitudes of the differences, notable. There are two explanations for these differences. First, the former are more quality-sensitive, less price-sensitive and less distance-sensitive than the latter. Therefore, they will be more willing to take advantage of relative changes in perceived quality of schools in the market. A second channel that may explain part of the differences is related to the spatial distribution of households and schools in the market, which differs systematically across poor and non-poor households.

We can use our model and estimates to explore how heterogeneity in preferences and market opportunities determine the observed gap in welfare gains from disclosure of true quality. Results from this additional counterfactual calculations are displayed in Table 6. We start by studying how differences in preferences determine lower welfare gains for poor households. First, we let poor households be as quality-sensitive as non-poor households. The share of switchers among poor households would increase by 1 percentage point to 3.2%, and the average welfare gains for switchers would increase to 9 USD.

Second, we let poor households have the same preferences as non-poor households over all schools’ attributes. The share of switchers increases by 0.8 percentage point to 3%. Average gains for poor switchers in this counterfactual would climb up to 16 USD, more than three times those in the first counterfactual and higher than those for non-poor switchers.27 These results imply that differences in preferences are enough to explain the gap in welfare gains from the proposed information policy across groups. Moreover, they highlight the key role that households’ quality-elasticity plays in determining the impacts of information policies for school choice.

Finally, we explore the role that the spatial distribution of schools and households play in explaining the gap welfare gains across groups. We measure welfare gains from the evaluated policy for poor households if they were located were non-poor households are. Our results show that average welfare gains in that setting would be essentially the same we found in our baseline results above. The share of switchers in this case would be lower than in the first counterfactual, at 1.9%, while welfare gains for poor switchers would be only slightly larger.

27The fact that welfare gains for the poor when endowed with preferences of non-poor households over all attributes are larger than those when endowed with such preference only over school quality comes partly from the fact that we estimate non-poor households to be less price-sensitive. This implies that the willingness to pay of them for a given increase in quality is higher than under poor preferences, as can be noted in equation 11.
than in such counterfactual, at 5.4 USD. This result implies that, in our setting, differences in market opportunities faced by poor and non-poor households play basically no role in explaining the gap in welfare gains from undistorted quality information.

4.2.4 Discussion

We have estimated a school choice model and studied a counterfactual exercise by which information on undistorted quality signals is provided to households. Results point in three directions. First, distortions in quality signals have effects in choices, as choice probabilities would change significantly in the counterfactual scenario. Second, households would react to the change in the quality disclosure system mostly by increasing the probability of choosing higher quality schools. This is, there would be a shift of students towards relatively high quality schools available in the market. Third, our welfare calculation points towards small average gains across households but larger gains for switchers. In both cases, gains are larger for non-poor households, which is driven by them being more quality-sensitive and less price-sensitive. We show however that complementary policies that could increase poor households’ quality-sensitivity may increase welfare gains from this policy for them. The magnitude of welfare gains for switchers suggests that it may be socially beneficial to invest in quality disclosure systems that reduce the extent of distortions in quality signals in the context of educations markets.

5 Concluding Remarks

In this paper, we have calculated the magnitude of distortions in quality signals in school markets, and studied the welfare consequences associated to choices made with distorted information. We have focused on the Chilean educational system, for which we have all the necessary data to recover distortions. Our findings indicate that distorted quality signals have the potential to influence school choice. The relevant population affected by a policy that delivers undistorted information are the switchers, households with different school choices in a world with undistorted information. Households whose choices are unaffected by distorted information are not affected in terms of welfare. The subpopulation of switchers in our context is small, which is explained by the fact that the magnitude of distortions in quality signals is relatively small. Nevertheless, we document that switchers would choose schools of remarkably higher quality in an environment without distortions, and would derive
welfare gains from adjusting their choices.

The results of this paper shed light on different policy-relevant aspects. On the one hand, we have shown that informational policies aimed at improving precision of information may affect choices. Although this has already been discussed in previous literature, our work studies a different margin to improve information. Previous work emphasizes the importance of having some information. Our work extends previous analysis and emphasizes the importance of providing undistorted information. On there other hand, we have showed that heterogeneity in preferences determines the extent to which households can benefit from information policies. In school markets, we argue that complementary policies that increase quality-sensitivity of the poor may enable them to obtain further benefits from information.

Our work has a simple and potentially powerful policy implication for school markets. The findings presented in this paper suggest that quality signals need to be calculated as a corrected average of test scores instead of an arithmetic average. One way to accomplish this correction is using the imputation method we have proposed. Importantly, government institutions that offer incentives to achieve full attendance on test day might provoke more distortions in quality signals. This is more likely to happen if incentives have heterogeneous impacts on schools. We believe that the imputation method we proposed is the most cost-effective alternative to correct distorted information. Other policy alternatives, such as choosing random test days, are inherently more expensive to coordinate and subject to cheating behavior in the form of collusion agreements among subset of schools.

Promising areas for future research are to study the magnitude and consequences of distorted information in other sectors of the economy, and to calculate the misallocation of resources associated to distortions. Distortions in quality signals might cause misallocation of public resources. Consider, for example, a government program that offers increases in wages to teachers in schools with higher average test scores. It is possible that teachers will incorrectly obtain bonuses in some schools due changes in students' attendance the day of the test. Another fruitful area for future research corresponds to the measurement of distortions in environments that are more susceptible to corruption. Interestingly, municipalities with higher levels of corruption in Chile have larger distortions in the quality signals we study (see Tables A.2 and A.3). This suggests that our findings might be exacerbated in other countries with different levels of corruption. Correcting distorted information in such environments has the potential to provide substantial welfare gains.
References


Figure 1: Attendance on test day

(a) Difference in average attendance rate (%) between 4th graders (who take the test) and 3rd graders (who do not take the test) around test days in 2013.

(b) Distribution of changes in attendance the day of the test by school in 2013. The vertical red line denotes the average change in attendance at the school level (2.4 percentage points).
Distortion in quality signals (y-axis) are defined as (minus) the difference between observed test scores in a school and a counterfactual scenario in which all students in the school take the test. Schools are ordered from lower to higher distortions in the x-axis. Vertical lines represent the 95% confidence interval. Green (gray) lines represents distortions that are (not) statistically different from zero. Distortions without confidence interval represent schools in which all students took the test. Approximately 5,000 schools every year.
Table 1: Descriptive statistics

<table>
<thead>
<tr>
<th>Panel A: Schools in 2005-2013</th>
<th>Obs.</th>
<th>Mean</th>
<th>St. Dev.</th>
<th>p10</th>
<th>p50</th>
<th>p90</th>
</tr>
</thead>
<tbody>
<tr>
<td>Test score (SIMCE)</td>
<td>54,585</td>
<td>245</td>
<td>31</td>
<td>205</td>
<td>244</td>
<td>287</td>
</tr>
<tr>
<td>Students in 4th grade</td>
<td>55,928</td>
<td>39</td>
<td>35</td>
<td>6</td>
<td>29</td>
<td>84</td>
</tr>
<tr>
<td>Students absent on test day</td>
<td>55,928</td>
<td>3</td>
<td>4</td>
<td>0</td>
<td>2</td>
<td>7</td>
</tr>
<tr>
<td>Average class size</td>
<td>55,810</td>
<td>29</td>
<td>10</td>
<td>15</td>
<td>29</td>
<td>41</td>
</tr>
<tr>
<td>Average annual attendance</td>
<td>55,928</td>
<td>94</td>
<td>3</td>
<td>90</td>
<td>94</td>
<td>98</td>
</tr>
<tr>
<td>Public</td>
<td>55,928</td>
<td>0.51</td>
<td>0.50</td>
<td>0</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Voucher</td>
<td>55,928</td>
<td>0.42</td>
<td>0.49</td>
<td>0</td>
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<tr>
<td>Private</td>
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<td>0.06</td>
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<td>Religious</td>
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<td>0.50</td>
<td>0</td>
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<tr>
<td>Monthly fee (Ch§)</td>
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<td>15,989</td>
<td>37,568</td>
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<td>57,013</td>
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<table>
<thead>
<tr>
<th>Panel B: Students in 2013</th>
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<tr>
<td>Test score (SIMCE)</td>
<td>140,982</td>
<td>263</td>
<td>46</td>
<td>200</td>
<td>267</td>
<td>321</td>
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<tr>
<td>GPA</td>
<td>159,356</td>
<td>5.9</td>
<td>0.6</td>
<td>5.1</td>
<td>5.9</td>
<td>6.5</td>
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<tr>
<td>Attendance in test-day</td>
<td>137,604</td>
<td>0.95</td>
<td>0.20</td>
<td>1.0</td>
<td>1.0</td>
<td>1.0</td>
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<tr>
<td>Attendance in non-test day</td>
<td>137,127</td>
<td>0.92</td>
<td>0.17</td>
<td>0.8</td>
<td>1.0</td>
<td>1.0</td>
</tr>
</tbody>
</table>

Notes: Own construction based on administrative data provided by the Ministry of Education. Attention is restricted to schools in which distortions in quality signals are zero or there is sufficient data to calculate it. See section 3 for details. There are 8,254 schools in the period 2005–2013.
Table 2: Understanding distortions in quality signals

Dependent variable is distortions in quality signals (in test score points)

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Some absenteeism on test day</th>
<th>Distortions&gt; 0</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Panel A: Schools’ attributes</td>
<td></td>
<td></td>
<td>(4)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(5)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(6)</td>
</tr>
<tr>
<td>Public schools</td>
<td>1.01***</td>
<td>0.90***</td>
<td>1.96***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Voucher schools</td>
<td>0.53***</td>
<td>0.40***</td>
<td>0.63***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Religious schools</td>
<td>-0.21***</td>
<td>-0.12***</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Average annual attendance</td>
<td>-0.24***</td>
<td>-0.25***</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Students in 4th grade</td>
<td>0.01</td>
<td>-0.03</td>
<td>-0.26***</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.05)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Enrollment in grades 1st-8th</td>
<td>0.07</td>
<td>0.09*</td>
<td>-0.22***</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.05)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Constant (private schools)</td>
<td>0.37***</td>
<td>0.42***</td>
<td>1.18***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Panel B: Autocorrelation</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lagged distortion</td>
<td>0.19***</td>
<td>0.12***</td>
<td>0.28***</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.70***</td>
<td>0.84***</td>
<td>1.24***</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Mean of dep. variable</td>
<td>2.75</td>
<td>2.75</td>
<td>3.56</td>
</tr>
<tr>
<td>Municipality F.E.</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Explained by schools F.E.</td>
<td>0.35</td>
<td>0.35</td>
<td>0.42</td>
</tr>
<tr>
<td>Schools</td>
<td>7,612</td>
<td>7,612</td>
<td>5,945</td>
</tr>
<tr>
<td>Observations</td>
<td>54,043</td>
<td>54,043</td>
<td>39,020</td>
</tr>
</tbody>
</table>

Notes: All non-indicator variables have been normalized (except for lagged distortion). All regressions are weighted by the inverse of the uncertainty associated to the calculation of distortions, where uncertainty is the size of the confidence interval. Columns 3-4 restrict attention to schools in which at least 1 student did not take the test. Columns 5-6 restrict attention to school-year observations with distortions statistically different from zero. Robust standard errors in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1.
Table 3: Summary statistics of demand estimation dataset

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>SD</th>
<th>p10</th>
<th>p50</th>
<th>p90</th>
</tr>
</thead>
<tbody>
<tr>
<td>Students</td>
<td>1,009</td>
<td>844</td>
<td>324</td>
<td>665</td>
<td>2,446</td>
</tr>
<tr>
<td></td>
<td>33%</td>
<td>12%</td>
<td>17%</td>
<td>31%</td>
<td>48%</td>
</tr>
<tr>
<td>Schools</td>
<td>63</td>
<td>62</td>
<td>19</td>
<td>45</td>
<td>134</td>
</tr>
<tr>
<td></td>
<td>92%</td>
<td>13%</td>
<td>72%</td>
<td>97%</td>
<td>100%</td>
</tr>
<tr>
<td>Poor students</td>
<td>479</td>
<td>391</td>
<td>166</td>
<td>323</td>
<td>1,184</td>
</tr>
<tr>
<td></td>
<td>49%</td>
<td>11%</td>
<td>35%</td>
<td>50%</td>
<td>60%</td>
</tr>
</tbody>
</table>

Notes: This table displays market-level summary statistics for the sample utilized for demand estimation. The sample covers 25 markets for the period 2011–2014. For the number of students and the number of school per market, summary statistics for levels and coverage rate of full school markets are provided. For the number of poor students, summary statistics for levels and for their share over the sample size for each market are provided.
Table 4: IV results from the second stage of demand estimation

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All sample</td>
<td>Poor students</td>
<td>Non-poor students</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fee</td>
<td>-0.006***</td>
<td>-0.008***</td>
<td>-0.011***</td>
<td>-0.012***</td>
<td>-0.019***</td>
<td>-0.004***</td>
<td>-0.006***</td>
<td>-0.010***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td></td>
</tr>
<tr>
<td>Quality</td>
<td>0.012***</td>
<td>0.019***</td>
<td>0.019***</td>
<td>0.004**</td>
<td>0.011***</td>
<td>0.014***</td>
<td>0.021***</td>
<td>0.028***</td>
<td>0.022***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>Religious</td>
<td>-0.054**</td>
<td>-0.086***</td>
<td>-0.086***</td>
<td>-0.030</td>
<td>-0.18</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.029)</td>
<td>(0.029)</td>
<td>(0.046)</td>
<td>(0.029)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender constraint</td>
<td>0.154***</td>
<td>0.128**</td>
<td>0.169***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.054)</td>
<td>(0.054)</td>
<td>(0.031)</td>
<td>(0.031)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Public</td>
<td>0.083***</td>
<td>0.221***</td>
<td>-0.066*</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.039)</td>
<td>(0.039)</td>
<td>(0.064)</td>
<td>(0.064)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SEP school</td>
<td>-0.323***</td>
<td>-0.525***</td>
<td>-0.580***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.064)</td>
<td>(0.092)</td>
<td>(0.065)</td>
<td>(0.064)</td>
<td>(0.064)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Market-Year F.E.</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>10,774</td>
<td>10,774</td>
<td>10,774</td>
<td>5,335</td>
<td>5,335</td>
<td>5,335</td>
<td>5,439</td>
<td>5,439</td>
<td>5,439</td>
</tr>
</tbody>
</table>

First stage F-test

|                |           |           |           |           |           |           |           |           |           |
|----------------|-----------|-----------|-----------|-----------|-----------|-----------|-----------|-----------|
| F-test Fee     | 1,166     | 1,814     | 362       | 393       | 515       | 68        | 907       | 1,429     | 301       |
| F-test Quality | 77.6      | 18.3      | 15.4      | 36.7      | 10.1      | 8.4       | 40.3      | 8.3       | 7.0       |

Notes: Instrumental variable estimates utilize two sets of instruments: (i) the amount awarded by school vouchers, mean fixed characteristics of rivals in the market (i.e. BLP instruments) and rivals' market wages are used as instruments for schools' fees; and (ii) a linear and quadratic term on county-specific temperature and the residual of a regression of being awarded a SNED prize in the previous period on lagged school quality are use as instruments for school quality. All regressions are weighted by school enrollment. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Table 5: Means of predicted school attributes of households’ choices

<table>
<thead>
<tr>
<th>Attribute</th>
<th>Scenario</th>
<th>Poor students</th>
<th>Non-poor students</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>Average</td>
<td>Switchers</td>
<td>Average</td>
</tr>
<tr>
<td>Distance</td>
<td>Baseline</td>
<td>2.3</td>
<td>2.4</td>
</tr>
<tr>
<td></td>
<td>Counterfactual</td>
<td>2.3</td>
<td>2.6</td>
</tr>
<tr>
<td></td>
<td>Change</td>
<td>0.0</td>
<td>0.2</td>
</tr>
<tr>
<td>Fee</td>
<td>Baseline</td>
<td>8.7</td>
<td>12.8</td>
</tr>
<tr>
<td></td>
<td>Counterfactual</td>
<td>8.8</td>
<td>46.3</td>
</tr>
<tr>
<td></td>
<td>Change</td>
<td>0.2</td>
<td>33.5</td>
</tr>
<tr>
<td>Quality</td>
<td>Baseline</td>
<td>256</td>
<td>248</td>
</tr>
<tr>
<td></td>
<td>Counterfactual</td>
<td>256</td>
<td>273</td>
</tr>
<tr>
<td></td>
<td>Change</td>
<td>0</td>
<td>25</td>
</tr>
</tbody>
</table>

Notes: Columns 1 and 3 display average attributes of chosen schools across all poor and non-poor households. Columns 2 and 4 display average attributes of chosen schools for poor and non-poor switchers. Results for distance are measured in kilometers, results for school fees are measured in thousands of Ch$ and results for quality are measured in SIMCE test scores, net of distortions.
### Table 6: Welfare gains from true quality disclosure

<table>
<thead>
<tr>
<th>Comparison</th>
<th>Poor students</th>
<th>Non-poor students</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Counterfactual scenario</td>
<td>E[$\Delta W_i$]</td>
<td>Switchers’ E[$\Delta W_i$]</td>
</tr>
<tr>
<td>Poor households with non-poor preference for quality</td>
<td>$0.11$</td>
<td>$5.15$</td>
</tr>
<tr>
<td>Poor households with non-poor preferences</td>
<td>$0.27$</td>
<td>$8.59$</td>
</tr>
<tr>
<td>Poor households with non-poor market opportunities</td>
<td>$0.47$</td>
<td>$15.73$</td>
</tr>
</tbody>
</table>

**Notes**: All welfare results correspond to the average welfare gain between the counterfactual and baseline scenarios under each setting for each subpopulation. Columns 1 and 3 display average welfare gains across all poor and non-poor households. Columns 2 and 4 display average welfare gains for poor and non-poor switchers. Columns 3 and 6 display the share of switchers among the population of poor and non-poor households respectively. All welfare results are measured in US dollars.
Distorted Information: Evidence from Quality Signals in School Markets

José Ignacio Cuesta, Felipe González, and Cristián Larroulet

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A Understanding Distortions in Quality Signals

A.1 Competition

A competitive environment could change incentives to distort quality signals. We generate a school-year specific measure of competition calculating the number of schools operating within 3 kilometers. If competitive pressure pushes schools to signal higher quality in order to attract demand, then we would expect competition to have a positive effect on distortions. Conversely, if competition disciplines schools, we could expect competition to have a negative effect on distortions.

To test for the effect of competition, we estimate the following equation:

$$\hat{\psi}_{jt} = f(X_{jt}) + \eta_j + \nu_t + \varepsilon_{jt}$$

where $\hat{\psi}_{jt}$ are the estimated distortions in quality signals, and $X_{jt}$ is the number of competitors within 3 kilometers as independent variable. Given the inclusion of school and year fixed effects, we are using within school variation over time. This means that we are using $\Delta N_{jt} = \text{Entry}_{jt} - \text{Exit}_{jt}$ as variation. Grau et al. (2015) documents significant variation in this variable. Panels A-C in Figure ?? presents results. There is no economically significant variation of distortions with the competitiveness of the environment.

A.2 Monetary incentives for teachers

An additional mechanism that could explain distortions are incentives placed by performance pay systems that reward teachers based on test scores. In the market we study, there is biannual performance pay contest called SNED which we can use to test for the role of teachers. SNED operates as follows: (i) groups of homogeneous schools are constructed; (ii) every two years, a multidimensional index is computed at the school level, which considers academic performance, improvement, and socioeconomic integration among other outcomes; (iii) schools are ranked within their groups according to the value of such index; and (iv) all teachers in schools covering the 25% – 35% of the total enrollment of each group get a monetary prize equivalent to around 40% of a teacher’s wage.\(^1\) Importantly, SIMCE test

\(^1\)Since 2006, the coverage of the prize was increased to 35% of the enrollment of the group. More details about this program can be found in Contreras and Rau (2012).
scores account for 70% of the weight in the index calculation.

Given that (i) prizes are provided according to an index, and (ii) after each contest schools are informed of their outcomes, we can use a school’s index as a measure of incentives. We compute the distance of each school to the threshold for obtaining the prize. Schools closer to the threshold have more incentives to increase their test scores through distortions than those further away from the threshold either upwards (sure winners) or downwards (sure losers). Using this rationale, we estimate:

\[ \psi_{jt} = 1_{IN} f_{IN}^{SNED_{jt-1}} + 1_{OUT} f_{OUT}^{SNED_{jt-1}} + \eta_j + \nu_t + \varepsilon_{jt} \]  

(1)

where SNED_{jt-1}^{IN} measures distance to the threshold for winners, and SNED_{jt-1}^{OUT} measures distance to threshold for losers, both in terms of index points. We use information from the previous contest to construct these variables. Our objects of interest are the functions \( f^{IN} \) and \( f^{OUT} \). If schools closer to the threshold have larger distortions, there is evidence of teachers manipulating attendance on test day.

Figure A.5 presents six different plots for the relationship between distortions and schools’ distance to the threshold. We present results for the two years after the prize is awarded and both for raw distortions in quality signals and residualized distortions (net of school and year fixed effects, as well as school characteristics). Estimates of \( f^{IN} \) and \( f^{OUT} \) show, if anything, the opposite pattern: schools closer to the cutoff have lower or similar distortions to quality signals. These results suggest teachers are unlikely to be the source of distortions.

### A.3 Educational traffic lights

Other quality disclosure policies could incentivize schools to introduce distortions in quality signals. In 2010, the Ministry of Education implemented a policy called “Educational Traffic Lights” (ETL) that we can use to test for this mechanism. The ETL policy consisted in sending information about 2009 SIMCE test scores to all households. This information included test scores and a classification of schools in “Red”, “Yellow” or “Green” categories, with clear cutoffs in test scores determining the category. \textcite{Allende} uses these discontinuities and finds that the policy affected school enrollment: households in red schools responded by enrolling in yellow schools, and households in yellow schools enrolled more in green schools. The ETL policy generated discontinuities in perceived school quality. Therefore, we expect schools closer to the cutoffs to have larger distortions.
Figure A.6 presents the linear relationships between test scores and distortions around the ETL policy cutoffs. Again, we present results for distortions and residualized distortions.\(^2\) These plots show that distortions increase around the cutoff between red and yellow schools. This means that schools introduce larger distortions in order to move towards the yellow category or avoid moving to the red category. This pattern, however, is not the same around the second cutoff. These results provide evidence that (some) low-quality schools generate distortions to signal higher quality.

\(^2\)Note that this is a cross-sectional exercise, so we cannot include school and year fixed effects in this case, just school characteristics.
References


Figure A.1: Predictability of test scores

Coefficient estimates and 95% confidence interval from a linear regression of test score on (1) a full set of indicators for a student’s GPA, and (2) school fixed effects. Standard errors clustered at the school level. Gray lines indicate the mean.
**Figure A.2:** Distribution of rank correlations over time

(a) Percentiles in rank correlation distribution $f(\rho_{mt})$

(b) Percentage of markets with changes in ranking (i.e., $\rho_{mt} < 1$)

*Notes:* Let $\rho_{mt}$ be the rank correlation of distorted and undistorted quality in market $m$ and year $t$. Approximately 210 markets every year.
Figure A.3: Distortions on own variables

(a) Average attendance rate
(b) Class size
(c) Enrollment
(d) Missing students on test day
(e) Monthly fee
(f) Share SEP students
(g) Distorted test score
(h) Undistorted test score
(i) Students in 4th grade

Notes: All variables have been residualized with school and year fixed effects. The mean of distortion is 2.7 test score points.
**Figure A.4:** Distortions on variables of schools within 3km

Notes: All variables have been residualized with school and year fixed effects. The mean of distortion is 2.7 test score points.
Figure A.5: Monetary incentives for teachers

(a) Distortion in year $t + 1$

(b) Distortion in year $t + 1$

(c) Distortion in year $t + 2$

(d) Distortion in year $t + 2$

Figure A.6: “Educational Traffic lights” policy

(a) Distortion in year 2010

(b) Distortion in year 2010
Figure A.7: Market definition

Notes: See description of Table A.1 for details about market definition.
Figure A.8: Estimated coefficients on distance from the first stage

(a) Poor students, distance

(b) Non-poor students, distance

Figure A.9: Changes in choice probabilities

(a) Poor students, all schools

(b) Non-poor students, all schools

(c) Poor students, chosen school

(d) Non-poor students, chosen school
Table A.1: School markets as connected components

<table>
<thead>
<tr>
<th></th>
<th>3km</th>
<th>4km</th>
<th>5km</th>
<th>6km</th>
<th>7km</th>
<th>8km</th>
<th>9km</th>
<th>10km</th>
</tr>
</thead>
<tbody>
<tr>
<td>Markets</td>
<td>451</td>
<td>413</td>
<td>380</td>
<td>348</td>
<td>322</td>
<td>295</td>
<td>273</td>
<td>251</td>
</tr>
<tr>
<td>Markets with more than 1 schools</td>
<td>262</td>
<td>248</td>
<td>233</td>
<td>219</td>
<td>208</td>
<td>196</td>
<td>191</td>
<td>181</td>
</tr>
<tr>
<td>Markets with more than 5 schools</td>
<td>106</td>
<td>104</td>
<td>99</td>
<td>93</td>
<td>90</td>
<td>88</td>
<td>86</td>
<td>86</td>
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<td>Markets with more than 10 schools</td>
<td>63</td>
<td>63</td>
<td>60</td>
<td>55</td>
<td>52</td>
<td>49</td>
<td>48</td>
<td>50</td>
</tr>
<tr>
<td>Markets with more than 20 schools</td>
<td>36</td>
<td>36</td>
<td>33</td>
<td>31</td>
<td>30</td>
<td>29</td>
<td>28</td>
<td>29</td>
</tr>
</tbody>
</table>

Notes: Let $A$ be a $N \times N$ matrix representing the network of $N = 5,416$ urban schools in Chile in the period 2005–2013. In network theory, $A$ is referred to as adjacency matrix. This adjacency matrix represents an undirected network, i.e., $A$ is a symmetric matrix. The element $A(i, j)$ in this adjacency matrix takes the value of one if school $i$ and $j$ are closer than $\kappa$ kilometers from each other and zero otherwise. A “component” or “connected component” of $A$ is a sub-network in which any two schools are connected to each other through some other school, i.e., we can always find a “path” that connects any two pair of schools in the sub-network. A market is defined as a connected component of $A$. In the paper, we use $\kappa = 5$ (highlighted in gray), but results are robust to different definitions.
Table A.2: Schools in corrupt municipalities have larger distortions

*Dependent variable is distortions (in test score points)*

<table>
<thead>
<tr>
<th></th>
<th>Years with transfers</th>
<th>Before audits revealed</th>
<th>After audits revealed</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Irregular payments</td>
<td>0.33***</td>
<td>0.38***</td>
<td>0.22**</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.09)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>Government transfers</td>
<td>0.06</td>
<td>0.05</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Public schools</td>
<td>1.04***</td>
<td>0.99***</td>
<td>1.12***</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.10)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Voucher schools</td>
<td>0.73***</td>
<td>0.71***</td>
<td>0.74***</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.09)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Religious schools</td>
<td>-0.03</td>
<td>-0.07</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(0.10)</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Average annual attendance</td>
<td>-0.19***</td>
<td>-0.15*</td>
<td>-0.30***</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.08)</td>
<td>(0.11)</td>
</tr>
<tr>
<td>Students in 4th grade</td>
<td>0.25</td>
<td>0.35*</td>
<td>0.09</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.20)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>Enrollment in grades 1st-8th</td>
<td>-0.24</td>
<td>-0.30</td>
<td>-0.17</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.20)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>Constant (private schools)</td>
<td>0.42***</td>
<td>0.45***</td>
<td>0.36***</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.11)</td>
<td>(0.11)</td>
</tr>
<tr>
<td>Schools</td>
<td>2,426</td>
<td>2,332</td>
<td>2,197</td>
</tr>
<tr>
<td>Municipalities</td>
<td>76</td>
<td>76</td>
<td>76</td>
</tr>
<tr>
<td>Observations</td>
<td>11,426</td>
<td>7,349</td>
<td>4,077</td>
</tr>
</tbody>
</table>

Notes: All variables have been normalized. All regressions are weighted by the inverse of the size of the confidence interval of distortions to account for estimation of the dependent variable. Audits in 76 randomly chosen municipalities were implemented by the Comptroller General of Chile to disclose irregular payments from government transfers. The time of disclosure of irregular payments was May of 2012. “All years with transfers” correspond to the period 2008–2013. Column 2 restricts attention to years 2008–2012, and column 3 restricts attention to years 2012–2013. Robust standard errors in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1.
Table A.3: Differences-in-differences of audits

*Dependent variable is distortions (in test score points)*

<table>
<thead>
<tr>
<th></th>
<th>All schools</th>
<th>Schools in audited municipalities</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Audit × Post</td>
<td>0.04</td>
<td>0.07</td>
</tr>
<tr>
<td>Corrupt × Post</td>
<td>-0.29*</td>
<td>-0.32**</td>
</tr>
<tr>
<td>Post</td>
<td>-0.05</td>
<td>-0.06</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Mean of dep. variable</td>
<td>2.8</td>
<td>2.8</td>
</tr>
<tr>
<td>School-level controls</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Municipality F.E.</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Municipalities</td>
<td>344</td>
<td>344</td>
</tr>
<tr>
<td>Schools</td>
<td>7,335</td>
<td>7,335</td>
</tr>
<tr>
<td>Observations</td>
<td>36,951</td>
<td>36,188</td>
</tr>
</tbody>
</table>

*Notes:* These regressions restrict attention to the period in which the government transferred monetary resources to be spent under the *Subvención Escolar Preferencial* program (2008–2013). All regressions are weighted by the inverse of the size of the confidence interval of distortions to account for estimation of the dependent variable. Audits in 76 randomly chosen municipalities were implemented by the Comptroller General of Chile to disclose “irregular” expenditures of government transfers. The time of disclosure of irregular payments was May of 2012. The post period are years 2012 and 2013. The “Corrupt” indicator takes the value of one if a municipality has more than 10 percent of the government transfers under “irregular payments.” More about irregular payments can be found in CIPER (2012). Standard errors clustered at the municipality level in parentheses. Significance level: *** p<0.01, ** p<0.05, * p<0.1.