



School markets: The impact of information approximating schools' effectiveness[☆]



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ABSTRACT

The impact of competition on school performance is likely to depend on whether parents are informed about schools' effectiveness or value added (which may or may not be correlated with absolute measures of their quality), and on whether this information influences their school choices, thereby affecting schools' market outcomes. This paper explores this by considering Chile's SNED program, which seeks to identify effective schools, selecting them from "homogeneous groups" of comparable institutions. Its results are widely disseminated, and the information it generates is different from that conveyed by a simple test-based ranking of schools (which turns out to approximate a ranking based on socioeconomic status). We use a sharp regression discontinuity to estimate the effect that being identified as a SNED winner has on schools' enrollment, tuition levels, and socioeconomic composition. Through five applications of the program, we find no consistent evidence that winning a SNED award affects these outcomes.

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1. Introduction

The school choice debate has long recognized that the impact of school choice on academic outcomes may depend on whether parents care about and respond to information on school quality. Research on the U.S. has produced evidence that parents are indeed willing to pay more for houses tied to schools with higher test scores (Black, 1999), and that they react to information on school performance by seeking higher achieving schools for their children (Hastings and Weinstein, 2008). Figlio and Lucas (2004) further suggest that housing valuations react to the placement of schools in discrete categories (e.g., "A" vs. "B"), even when the information used to assign schools to these categories is public.¹

In developing countries, Andrabi et al. (2009) use a randomized design to study the effects of distributing school/child test score

information in Pakistani villages. They find that this leads to overall learning gains that originate in initially low performing private schools and the public sector. In addition, higher achievement private schools lower their fees—the overall result is one of greater price-adjusted quality.

All of these analyses concern the impact of information on schools' absolute outcomes. Although accountability initiatives often begin using such data, they often eventually emphasize information on schools' value added (e.g. Chile and the U.S.). The rationale for this progression is that absolute outcomes conflate schools' value added and their student composition, since all else equal, higher ability children obtain higher test scores. Focusing on value added thus avoids penalizing or rewarding schools for who they enroll, and places the focus on educational productivity.²

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¹ These findings are generally consistent with logit and structural estimates of parental preferences and demand responses; see for instance Bayer et al. (2007), Gallego and Hernando (2007), and Hastings et al. (2006).

² In theoretical work, MacLeod and Urquiola (2009) show that results like those in Black (1999) and Hastings and Weinstein (2008) are consistent with parents not necessarily favoring schools with the highest value added. In their model, signaling concerns lead parents to prefer schools with good reputations, as measured by their testing achievement. Since this depends both on school value added and student composition, parents may be willing to trade off value added and peer quality. An analogous tradeoff emerges if there are peer effects (e.g. Rothstein, 2006), or network-type concerns.

To summarize, while solid research shows that information on schools' absolute testing performance affects school markets, there is less evidence on whether parents' choices and schools' market outcomes would respond if parents were given signals of *value added*, even if these were not necessarily correlated with peer quality.

To address this issue, this paper considers how Chilean schools' (enrollment) market shares, their tuition, and their student composition react when they are identified as performing *well relative to schools that serve similar children*. Specifically, we analyze the SNED,³ a program which since 1996 has relied on schools' test score levels and inter-cohort gains to select good performers from within more than one hundred "homogeneous groups" that contain institutions used by arguably comparable children.

While isolating school effectiveness is difficult, the SNED thus aims to approximate it in a simple manner that shares elements with approaches used elsewhere. Its results are disseminated via newspapers, the internet, parent association meetings, and in some municipalities, by placing banners close to school entry-ways. Finally, aside from identifying winning schools as good performers, the program pays their teachers an annual bonus equal to about 70% of a monthly salary.

This setting is interesting for several reasons. First, the information the SNED generates is different from that conveyed by a school ranking based on absolute test scores. This matters because as Mizala et al. (2007) point out, in Chile such a ranking closely parallels one based on schools' average socioeconomic status. Second, within each homogeneous group, the SNED selects "winners" after ranking schools based on an index. The top-ranked schools are chosen such that winners account for about 25% of enrollments, resulting in clear group-specific cutoff scores. Parents are informed of schools' status (winner or non-winner), but *not* of their index values.⁴

These facts allow us to use a regression discontinuity design to evaluate the effects of winning an award. In this sense, we follow Figlio and Lucas (2004) in asking if parents react to the placement of schools in discrete categories—SNED winner vs. non-winner—even when information on testing performance is public. The difference is that in this case the information delivered concerns value added, as opposed to absolute performance. Additionally, in this case the intervention is not purely informational if the winning schools are rendered more desirable by the distribution of bonuses.

Third, Chile's school market is quite liberalized, and might therefore actually give parents the ability to react to data on effectiveness. Specifically, in urban areas more than half of all schools are private, and about 90% of all schools, religious and secular, are effectively voucher-funded.

We study the effect that being identified as a SNED winner has on a number of schools' outcomes. First, we consider their 1st and 9th grade enrollments, as well as the number of classes they operate at these levels; these grades are the entry points into most schools, and thus perhaps more sensitive to changing demand. Second, we study the probability that schools charge tuition, and the amount they charge. This reflects that schools might respond to increased demand not by expanding but by raising their prices. Third, we consider schools' socioeconomic composition, since schools could leverage the awards to become more selective.

The key finding is that through five rounds of the program, there is no consistent evidence that SNED awards affect any of these outcomes. Our point estimates are often close to zero, although for some outcomes they are sometimes consistent with non-trivial effects. Nevertheless, the lack of evidence of a positive impact holds despite data from multiple allocations and through three robustness checks.

³ Sistema Nacional de Evaluación del Desempeño de los Establecimientos Educativos Subvencionados.

⁴ In the most recent SNED rounds, some information on schools' indices has been made available; this was not the case in all the rounds we analyze, as discussed further below.

First, we address the possibility that the SNED's homogeneous groups might not identify schools that are effectively in competition with each other. To do so, we consider sets of schools that in addition to belonging to the same group operate in the same municipality. For the metropolitan area of Santiago, we also use geo-coded data to ask if these outcomes vary at a given school *relative to its nearby competitors*. Second, to explore the possibility that more educated parents are more sensitive to data on school effectiveness, we restrict attention to homogeneous groups of higher socioeconomic status. Finally, we focus on two subsets of SNED rounds. We first consider the earliest round since "fresh" information system might have the greatest impact; then we focus on the most recent two, since the dissemination of SNED results has increased over time.

In short, we find that information on school effectiveness as delivered by the SNED does not significantly affect schools' market outcomes. We note that this finding is consistent with several possibilities. First, it may reflect that the RD design produces a local estimate, and that parents considering schools close to cutoffs realize they are similar. One must keep in mind, however, that SNED index values are not publicized, and additionally the existence of a teacher bonus means that schools that just win and just miss an award are different ex-post. A second possibility is that while parents might value effectiveness, information on it might ultimately not trump school choices based on characteristics like peer quality. Third, parents might not have noticed the SNED despite its decade-long existence. Whichever is the case, our findings suggest caution regarding the impact of information related to school effectiveness.

Finally, we note that our findings are not necessarily inconsistent with the SNED having improved aggregate test scores in Chile (as Andrabi et al. (2009) find in Pakistan).⁵ It is hard to say anything definitive on this issue, given the fact that the SNED's implementation did not provide for a natural control group (and our research design is not ideally suited to address this question).

2. Chile's school system

In 1981, Chile introduced school finance reforms creating a liberalized school market. Three types of schools operate in Chile:

- 1) *Public* or *municipal* schools are run by 341 municipalities or communes,⁶ which receive a per-student subsidy from the central government. These schools cannot turn away students unless oversubscribed; they are the suppliers of last resort.
- 2) *Private subsidized* or *voucher* schools are independent religious or secular institutions that receive the same per student subsidy as public schools. Unlike the latter, they have wide latitude regarding student selection.⁷
- 3) *Private unsubsidized* schools are also independent, but receive no public funding.

In 2006, private institutions accounted for about 48% of all schools, and voucher schools alone for about 42%. In urban areas, these shares were 65 and 55%, respectively. All private schools can be explicitly for-profit. Some are run by privately or publicly-held corporations that control chains of schools, but the modal one seems to be owned and managed by a principal/entrepreneur. There are few barriers to entry,⁸ and while we have no estimate on the frequency of transactions, it is not rare to see classified ads offering private schools for sale.

⁵ The time series evidence on the evolution of equated performance on Chile's national test, however, would seem to suggest the SNED induced no discrete change in performance, but of course this is simply descriptive.

⁶ Municipalities in Chile are generally called communes, and we henceforth use this terminology.

⁷ In addition, private voucher schools have greater flexibility in setting the parameters of teacher compensation.

⁸ Hsieh and Urquiola (2006), for instance, indicate that roughly one thousand private schools entered the market in the decade following the introduction of voucher financing.

While initially subsidized schools were not allowed to charge tuition to supplement the voucher subsidy, this restriction was eased in 1993. At present about 50% of private voucher schools charge tuition, raising resources equal to about 20% of their state funding. Public schools are allowed to charge fees only at the secondary level, although in practice few of them do.

3. The SNED

As the government liberalized the educational sector, it began exploring mechanisms to provide information that might aid parental school choice. In 1988 the SIMCE⁹ testing system came into existence, and as of the mid-1990s its results had been disseminated, partially through listings of schools' performance in newspapers.¹⁰ The government also began using SIMCE results to allocate resources, e.g., in 1990 it started using 4th grade scores to assign aid to under-performing schools (Chay et al., 2005).

3.1. The SNED: basic details

In the 1990s, the government also began to consider using test scores to promote accountability and transmit incentives. This resulted in the introduction of the SNED, a system which seeks to identify outstanding public or subsidized private schools, and which has distributed awards every two years since 1996.

In an initial step, the SNED assigns schools to "homogeneous groups" constructed in two stages. First, schools are placed into groups based on three characteristics: i) which of Chile's thirteen administrative regions they operate in, ii) whether they are in the rural or urban area, and iii) the type of schooling they offer (primary only, secondary and primary, or special needs). In a second stage, each group is further subdivided using a cluster analysis methodology that groups schools according to the socioeconomic status of their students. This relies on data on parental schooling, household income, and a government-calculated vulnerability index.¹¹

Table 1 (Column 1, Panel A) shows that in 1998, for example, this resulted in schools being assigned to 114 homogeneous groups containing an average of 80 institutions. Among primary schools, these groups accounted for 57 and 72% of the variation in income and mothers' schooling, respectively. The total number of groups has remained roughly stable since then; the first SNED round, 1996, used a different methodology that resulted in only 8 groups, and this is one reason (other than not having access to the full data for that round) we ignore it below.¹²

The second step in the implementation of the SNED is the calculation of the six sub-indices detailed in Table 2. These are aggregated to a single SNED index with a weighting scheme that gives the greatest importance to testing performance. Specifically, schools' test score levels and their inter-cohort changes receive weights of 37 and 28%, respectively. Retention and promotion rates also enter into the calculation, as do data from surveys that seek to measure parental involvement and working conditions. Fig. 1 (Panel A) plots smoothed values of the relationship between the SNED index—and each of its six sub-indices—and schools' average mothers' schooling. As visually clear, it is largely the test score level component (the top segment) which induces a

positive relationship between the overall index and socioeconomic status (SES).

In a third step, the schools within each homogeneous group are ordered according to their aggregate index, and those above a given cutoff are given awards. Each group's cutoff is set such that winning schools account for about 25% of total enrollments.¹³

3.2. SNED: the treatment

For the two years their award is in force, SNED-winning schools are subject to two treatments. First, teachers in selected schools are paid an annual bonus that presently averages about 1000 dollars.¹⁴ Second, winning schools are publicly identified as performing well relatively to an arguably meaningful comparison group. Importantly, for the SNED rounds we considered (all but the two most recent), only information on schools' award status, and not on their index values, was made public.

There are several ways in which the list of winners is publicized. First, since the SNED's inception, it has been announced (by the Minister of Education) in a press conference and published in national newspapers the following day. Administrators at winning schools, particularly at profit or prestige-maximizing institutions, might be expected to inform parents, with subsequent word-of-mouth dissemination.

Table 3 summarizes the results from a Ministry of Education survey in which schools were asked whether they apprise parents regarding their performance in the SNED and the SIMCE tests more generally. Although one must keep in mind that these data are self-reported, they suggest that by 2003 between 80 and 90% of schools engaged in dissemination by way of informing their parents' association, sending notes home, or raising the issue during PTA-type meetings.

Second, since 2002 (i.e., for the last rounds) a Ministry of Education website has allowed parents to determine multiple schools' SNED status.¹⁵ Third, since 2004, some communes have intervened in the process by helping winning schools to advertise their status in a uniform way. For instance, the commune of Santiago¹⁶ posted standardized banners at winning schools during the 2006–2007 round.¹⁷ Further, information regarding the SNED is not released into a vacuum, since the 1990s afforded the public with exposure to school performance data.¹⁸

We underline that to the extent that the SNED seeks to control for SES, the information it generates is quite different from that which simple test scores transmit. For instance, Mizala et al. (2007) show that

¹³ Starting with the 2006 round, this was expanded to 35% of schools, an issue we return to below.

¹⁴ The payment is presently equal to about 70% of the mean monthly salary of a teacher working full time. Its real value has increased over time. In 1996 it was about 470 dollars, but climbed to 565 and 1070 dollars by 2000 and 2006, respectively. These figures are averages. In practice, a global allocation is made to each school, and 90% of it is distributed according to hours worked. The administration has discretion over how the remainder is split; in practice most schools seem to distribute it evenly among instructors. Additionally, as noted, in 2006 the SNED was expanded such that schools accounting for 35 rather than 25% of enrollments receive the award—schools accounting for the first 25% receive a full bonus, while the rest receive only 60% of the full amount. Below, we ignore this issue, treating all schools as if they received the full bonus. We do so because parents only find out whether schools receive an award or not. Additionally, the distinction does not affect our key results.

¹⁵ Fig. 2 in an earlier version of the paper, available at <http://www.nber.org/papers/w13676>, presents a sample download.

¹⁶ By population, Santiago is the largest commune in Chile. It is part of the Santiago metropolitan region, which contains about 50 communes.

¹⁷ Fig. 3 in an earlier version of the paper, available at <http://www.nber.org/papers/w13676>, presents examples of such banners.

¹⁸ Figs. 4 and 5 in an earlier version of the paper, available at <http://www.nber.org/papers/w13676>, illustrate this. Specifically, Fig. 4 shows the front page of a newspaper announcing the edition contains the scores of all schools in a given region; Fig. 5 contains the headline and initial text of a story on the SNED itself, including a synopsis of its methodology.

⁹ Sistema Nacional de Medición de la Calidad de la Educación.

¹⁰ A previous testing system, the Programa de Evaluación del Rendimiento Escolar, was discontinued.

¹¹ This measure (the JUNAEB index) is calculated at the school level to target school lunch-type subsidies.

¹² The main reason for the smaller number of groups in 1996 is that these were constructed on a national rather than regional basis. Additionally, in other parts of the methodology (discussed below) the 1996 round relied on different data. For instance, it did not use 4th grade test scores, repetition, or dropout rates.

Table 1
Homogenous groups, commune/homogenous groups, and sample sizes.

Sample and type of group	SNED wave					Stacked data (6)
	1998 (1)	2000 (2)	2002 (3)	2004 (4)	2006 (5)	
Panel A: full sample						
Homogeneous groups	114	104	109	110	111	–
Total number of schools	9060	9247	9465	9681	9769	–
Avg. no. of schools per group	79.5	88.9	86.8	88.0	88.0	–
Commune/homogeneous groups	1784	1584	1669	1721	1764	–
Total number of schools	8355	8380	8496	8536	8749	–
Avg. no. of schools per group	4.7	5.3	5.1	5.0	5.0	–
Panel B: 2 year sample-urban primary schools						
Homogeneous groups	34	32	32	31	32	161
Total number of schools	2422	2460	2527	2484	2583	12,476
Avg. no. of schools per group	71.2	76.9	79.0	80.1	80.7	77.5
Commune/homogeneous groups	255	206	233	235	266	1195
Total number of schools	1892	1893	2015	1964	2093	9857
Avg. no. of schools per group	7.4	9.2	8.6	8.4	7.9	8.2
Panel C: 4 year sample-urban primary schools						
Homogeneous groups	34	32	32	31	–	129
Total number of schools	2378	2378	2461	2422	–	9639
Avg. no. of schools per group	69.9	74.3	76.9	78.1	–	74.7
Commune/homogeneous groups	252	202	232	234	–	920
Total number of schools	1847	1819	1959	1899	–	7524
Avg. no. of schools per group	7.3	9.0	8.4	8.1	–	8.2
Panel D: 2 year sample-urban secondary schools						
Homogeneous groups	29	28	28	28	30	143
Total number of schools	1026	1081	1170	1314	1677	6268
Avg. no. of schools per group	35.4	38.6	41.8	46.9	55.9	43.8
Panel E: 4 year sample-urban secondary schools						
Homogeneous groups	29	28	28	28	–	113
Total number of schools	1008	1041	1153	1295	–	4497
Avg. no. of schools per group	34.8	37.2	41.2	46.3	–	39.8

Note: Panel A describes all (primary and secondary) homogenous groups and commune/homogeneous groups in the country. Panel B describes *urban* and *primary* level groups, covering only schools with valid indices for each SNED allocation, and for which there are at least enrollment outcomes one year prior and two years thereafter—we label this the 2-year sample. Panel C describes urban and primary level groups, covering schools with valid indices for each allocation, and for which there are at least enrollment outcomes one year prior and four years thereafter—we label this the 4-year sample. Panels D and E describe analogous 2 and 4-year samples for secondary level schools. See [Section 4.4](#).

about 80% of the variation in school-level test scores in Chile can be explained by parental schooling and household income, which implies that rankings based on testing performance largely reflect socioeconomic status. For instance, two programs that chose the top fifth of schools based on their mean language score and their average mothers' schooling would agree on the selection or non-selection of about 85% of all schools. By design, the SNED produces qualitatively different information, since it selects schools from both "rich" and "poor" homogeneous groups. For a simple exercise, compare the 2002 SNED allocation with an award given for having language scores in the top quartile in the same year. 63% of schools would receive neither, and 8% both; 29% would receive one but not the other.

Finally, it is important to be clear that the SNED only seeks to approximate school effectiveness. One way to isolate schools' value added would be to run a large number of randomized experiments, something which is all but impossible for most educational systems to do once, let alone on a sustained basis. The bottom line is that while one could envision better ways of generating data on effectiveness, the SNED represents a reasonable and feasible approach, one which shares relevant characteristics with those used in, for example, New Jersey and New York City.¹⁹

¹⁹ In explicitly placing schools in comparison groups that are then described to parents, the SNED shares elements with New Jersey's school report card system, which uses *district factor groups* made up of schools in districts of similar socioeconomic status. Similarly, New York City places schools in "peer horizons" containing about 40 similar institutions.

Table 2
Inputs and data sources for each of the SNED's six sub-indices.

Sub-index	Weight (%)	Measurement/data source
Effectiveness	37	SIMCE test score levels in Language and Math, measured at the 4th, 8th, and 10th grade levels, for the latest testing round available
Improvement	28	SIMCE inter-cohort gains for the 4th, 8th, and 10th grade levels, measured using the two most recently available testing rounds
Equality of opportunity	22	Repetition and dropout rates (account for 50% of the total Equality of Opportunity component); measured using administrative data Absence of discriminatory practices, such as removing children who fail a grade or become pregnant, or rejecting students when vacancies exist (this accounts for 40% of the total Equality of Opportunity component); measured via a questionnaire administered to Ministry of Education inspectors Integration of physically challenged students, measured via a school-level survey; absence of improper punishments, measured via a survey administered to Ministry of Education school inspectors; (these account for 10% of the total Equality component)
Initiative	6	Schools' educational activities and initiatives, measured using a survey given to administrators
Parent/teacher participation	5	Parental participation and information availability; measured via a school-level survey Parents' perceptions about the quality of the school; measured via a SIMCE survey
Working conditions	2	Schools' placement in a Ministry of Education inspection system

Source: Authors' preparation and Mizala and Romaguera (2004).

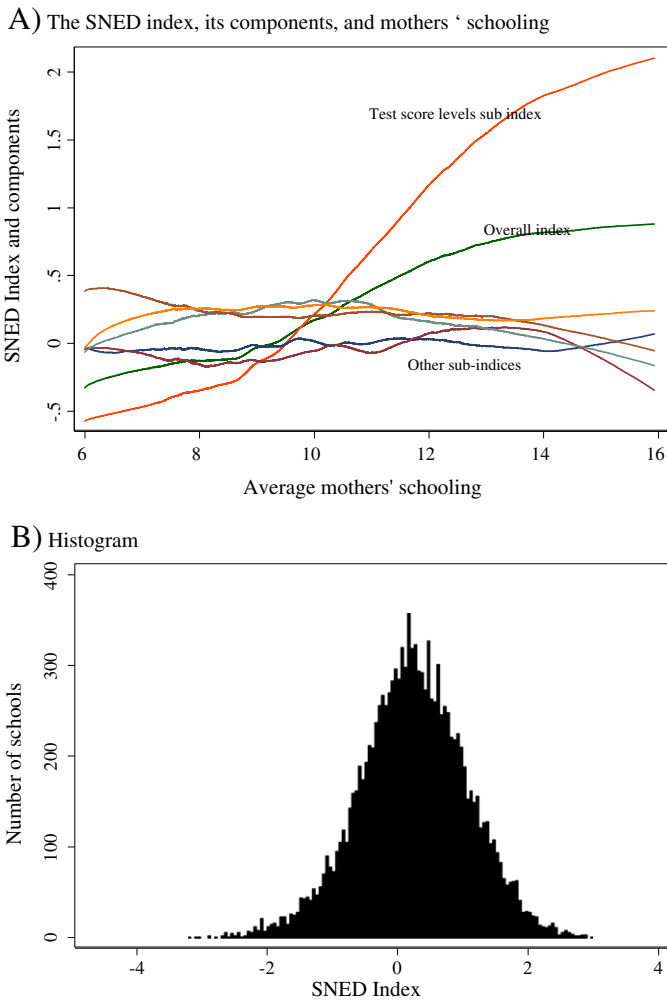


Fig. 1. The SNED index among primary schools.
 Note: Panel A plots fitted values of locally weighted regressions of schools' SNED index (and subindex) values on their average mothers' schooling. Panel B plots a histogram of observed SNED index values, using a bin size of 0.05. Both panels cover 12,476 school/year observations from the 1998, 2000, 2002, 2004, and 2006 SNED rounds (this is the 2-year primary sample, see Section 4.4 and Table 1, Panel B).

4. Empirical strategy

The manner in which the SNED is assigned makes it feasible to evaluate its effect using a regression discontinuity (henceforth, RD) design. Fig. 2 illustrates this among urban and primary-level homogeneous groups. Panel A describes the allocation for 12,496 school/year observations that combine data from the 1998, 2000, 2002, 2004, and

2006 SNED waves. It plots SNED index–cell means of a dummy indicating whether schools obtained an award, where the index is measured relative to the cutoff in each school's homogeneous group. We normalize each group's cutoff to zero to combine data across groups and years. Additionally, for visual clarity index values are in 0.1 point bins (Imbens and Lemieux, 2007). Panel A makes clear that the probability of selection jumps discretely at the cutoff scores—the small amount of (barely visible) noncompliance is due to the first round, 1998 (we present regression evidence below).

This discontinuous relationship can be used to investigate the effect of the SNED on schools' market outcomes. Following van der Klaauw (2002) one could implement:

$$y_{igt} = \beta E(SNED_{igt} | index_{igt}) + f(index_{igt}) + \varepsilon_{igt} \tag{1}$$

$$E(SNED_{igt} | index_{igt}) = \gamma 1\{index_{igt} \geq cutoff_{gt}\} + g(index_{igt}) + \xi_{igt} \tag{2}$$

where y is an outcome of interest, i indexes schools, g orders homogeneous groups, and t stands for different SNED rounds. $E(SNED_{igt} | index_{igt})$ is the probability of receiving an award conditional on having a given index value, f and g are flexible functions of the index, and $1\{index_{igt} \geq cutoff_{gt}\}$ is a dummy that takes on a value of one if a school's index is greater than or equal to the cutoff in its respective homogeneous group. If f and g are continuous at $cutoff_{gt}$ and $E(SNED_{igt} | index_{igt})$ is discontinuous (Fig. 2), then β is non-parametrically identified at that point. Intuitively, under the mentioned continuity assumptions, other factors affecting y will be similar for schools just above and below the cutoff, and their comparison will mimic a randomized design. Discrete differences in outcomes at the cutoff can then be attributed to the award itself.

Below we assume that we have a sharp rather than a fuzzy design, and instead of implementing Eqs. (1)–(2), use a reduced form approach:

$$y_{igt} = b 1\{index_{igt} \geq cutoff_{gt}\} + h(index_{igt}) + e_{igt} \tag{3}$$

We do this in view of the fact that in our data, satisfying $1\{index_{igt} \geq cutoff_{gt}\}$ and actually receiving an award are almost equivalent—in all but the first SNED round we use, the correspondence is indeed perfect. Additionally, we specify h as a quadratic in the index (having tried variants, as discussed below), and also estimate (Eq. (3)) within arbitrarily narrow bands close to the cutoff scores.

4.1. What can an RD design identify in this context?

While Fig. 2 (Panel A) shows that the SNED produces a “clean” RD, it is relevant to ask what this design can identify in our context. As discussed, the SNED treatment has two components: teachers in winning

Table 3
 Information on schools' dissemination efforts, 2003.

How does the school inform parents regarding its:	Type of school	The school does not inform parents (1)	The school informs only if parents request data (2)	The school informs the board of the parents' association (3)	The school informs via notes or other written media sent home (4)	The schools holds information meetings with parents (5)
SNED status	Public	8.3	9.4	36.8	16.7	28.7
	Private voucher	3.9	6.8	35.9	27.0	26.4
SIMCE results	Public	1.0	3.7	27.5	45.6	22.2
	Private voucher	0.2	3.1	26.3	49.3	21.1

Note: The information is based on a survey of schools collected by the Ministry of Education in 2003.

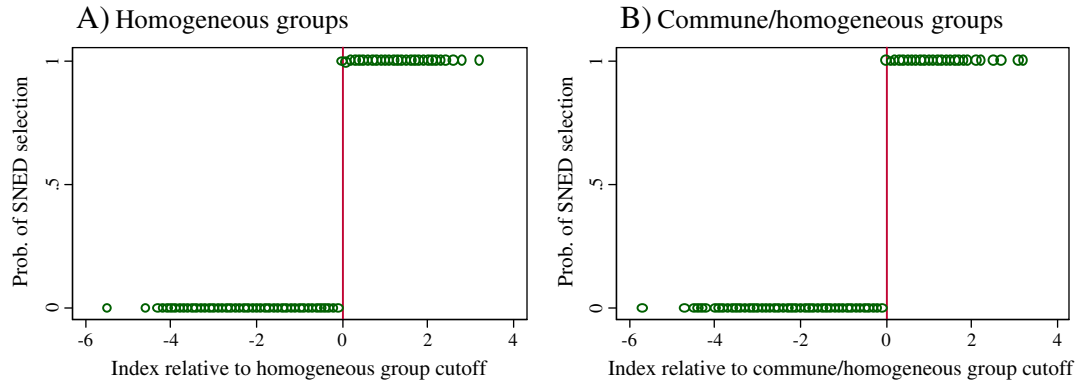


Fig. 2. 1st stage.

Note: Panel A plots index-cell means of a dummy indicating whether schools obtained a SNED award. Each school's index is expressed relative to the normalized cutoff (the vertical line) in its respective homogeneous group. For visual clarity index values are in 0.1 wide bins (Imbens and Lemieux, 2007). Panel B repeats the exercise where the index is measured relative to commune/homogeneous group cutoffs. Both panels describe the 1998, 2000, 2002, 2004, and 2006 rounds. Panel A is based on 12,476 school/year observations, and Panel B on 9857 observations—the 2-year homogeneous groups and commune/homogenous group samples, respectively (see Section 4.4 and Table 1, Panel B).

schools receive bonuses, and these schools are identified as performing well relative to a specific reference group.

Previous work on the SNED has analyzed the effects of the bonuses on testing achievement (e.g., Contreras and Rau, 2008; Contreras et al., 2003, and Gallego, 2008). We do not attempt this because our sense is that the RD approach is not suited to it, since the SNED's existence might affect the behavior of teachers who know their school is within range of winning, regardless of whether this happens or not. In this case, the intensity of the incentives would not vary discretely at the cutoff, even though the bonus payment of course would.

We focus instead on the effect of the awards on schools' market outcomes. A school's award status of course does vary at the cutoff, and a positive effect would be consistent with Figlio and Lucas' (2004) finding that schools' placement in discrete categories has an effect even when the information used in the assignment is public.

That said, should one expect parents to infer much regarding schools' value added from the SNED? We cannot definitively answer this, not least because we do not directly observe value added. One aspect we can address, however, is whether the SNED is so noisy as to resemble a lottery; if it were, then eventually schools above and below a threshold would be equally likely to win, and parents might discount the scheme.

First, it does appear that whether a school wins an award is predictive of its future SNED status: about 60% obtained an award more than once. More specifically, among about 8300 schools in the five rounds we analyze, 48% never received an award; 22% were selected once, and 15, 9, 5 and 2% won awards 2, 3, 4, and 5 times, respectively.

Additionally, Fig. 3 plots the total number of awards won by schools as a function of their index in 1998 (the first year; other years produce similar conclusions). Panel A considers the number won over all five rounds; Panel B considers the first two rounds (1998 and 2000). In each case the total number of awards climbs with the index value. Beyond this, there is a discreet jump (at the cutoff) in the number of awards won. Panel B suggests, for example, that in 1998 parents with primary-aged children could expect winning schools to on average have about one and half more awards than losing schools four years (two rounds) later—for many parents this would be the remainder of their child's time at that school.

As a final descriptive fact, we note that winning an award is positively and significantly correlated with subsequent test score performance even controlling for observables. For instance, Column 1 in Table 4 shows that primary schools that received an award in 2002 had higher math scores in 2004, even after controlling for mothers' schooling and household income (Column 2), and for

homogenous group dummies and testing performance in the previous two years for which scores are available (Columns 3 and 4). Columns 5–8 suggest a similar conclusion for secondary schools.

In short, these results suggest that parents desiring information on school value added may not dismiss the SNED. This should further be the case if the bonus payments render winners more desirable; e.g., if they allow schools to recruit better teachers.

4.2. Outcomes

We explore how SNED selection affects the following school level outcomes:

- 1) *1st and 9th grade enrollments.* To the extent that selected schools experience higher demand, they might end up with greater enrollments. We focus on these grades because they are the entry points into most schools, and school choice might therefore be least constrained at these junctures.²⁰
- 2) *Number of 1st and 9th grade classes operated.* Urquiola and Verhoogen (2009) suggest that profit-maximizing Chilean schools view the choice of the number of classrooms to operate as a serious one, if only because it influences their fixed costs. Additionally, they present evidence suggesting that higher quality schools experience higher demand and operate more classrooms. One might expect, therefore, that SNED winners would be more likely to open an additional classroom. For this and the previous outcome, we rely on annual 1997–2008 administrative data collected at the beginning of each academic year (around February).
- 3) *Tuition.* Since 1994, private voucher schools have been allowed to charge tuition add-ons, and at present about 50% of them do. Using 1997–2006 administrative data, we study whether schools charge tuition at all, as well as the average amount they charge. We consider the former for two reasons. First, simply charging tuition is a signal of perceived quality in Chile, as parents can always use municipal schools free of charge. Second our tuition data are not grade-specific, and may therefore only slowly reflect increases

²⁰ We explored results for the 7th grade, as some secondary schools begin operations at this grade. We omit them because they produce conclusions similar to those for the 9th grade, while providing smaller samples.

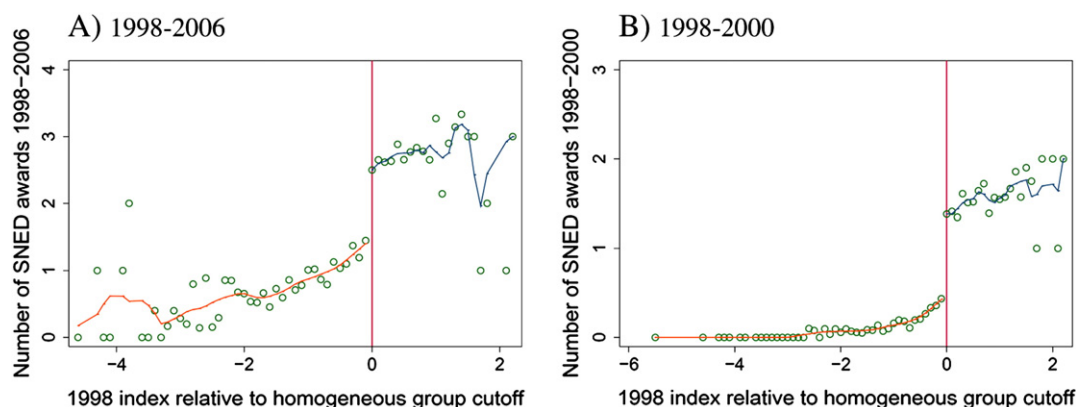


Fig. 3. Total number of times schools have won awards.

Note: This figure plots 1998 SNED index-cell means of the total number of times schools obtained awards during that and subsequent rounds. Panel A refers to all five rounds we analyze (1998–2006), and Panel B only to the first two (1998 and 2000). For visual clarity index values are in 0.1 wide bins (Imbens and Lemieux, 2007).

that first affect lower grades. In contrast, it is easier to observe if an award induces a school that did not charge tuition to begin doing so.

- 4) *Socioeconomic composition*. Finally, we explore whether winning an award affects schools' socioeconomic composition, something one might expect if a perception of higher quality allowed schools to attract a "better" clientele. For this we rely on two types of data. The first is a government-calculated school-level vulnerability index that considers variables like parental schooling and employment, for which we have annual 1st and 9th grade (1997–2008) observations.²¹ For more direct measures, we also use data from the SIMCE system, which at times of testing sends a questionnaire to students' homes. This provides information on household income and parental schooling. As part of the testing system, however, these data are available only for the 4th, 8th, and 10th grades, and each grade is tested only (about) every third year. We discuss further adjustments this necessitates in the Results section below.

4.3. Comparison groups

The most natural way to implement an RD design in our setting is to compare—within homogeneous groups—the outcomes of schools that just qualify for an award with those of schools that just miss receiving one. Such are the main results we review below.

To further the probability that we analyze sets of schools effectively in competition with each other, we carry out two additional exercises. First, we present complementary results in which we apply the strategy not within homogeneous groups, but rather among schools that in addition to being in the same group, are located in the same commune. This is feasible because communes are subsets of regions (the geographical unit used in the construction of the homogeneous groups), and is useful because they come closer to identifying areas where school choice might take place. For instance, the metropolitan region of Santiago has about 50 communes, and although children can in principle attend school in any of these, in practice many stay in their commune of residence.²²

²¹ The index is calculated by JUNAEB, an agency which uses it to target school-lunch type subsidies. The variables it uses are: a) the percentage of mothers with less than eight years of schooling, b) the percentage of household heads with less than eight years of schooling, c) the employment category of the household head, d) the percentage of children who receive welfare payments, e) the percentage of children without access to adequate sewerage systems, and f) the percentage of students in households classified as poor.

²² The 2002 SIMCE data suggest that about 89% of 4th graders using subsidized institutions go to school in the commune in which they live. In Metropolitan Santiago, this figure goes down to 79%. As elsewhere, it is also more common for children in secondary school to travel further to school.

Fig. 4 illustrates the spirit of this analysis by showing a hypothetical homogeneous group that includes schools in three communes, labeled A–C. The cutoff score—a single one for the whole group—separates schools that get an award from those that do not. We use the schools in each of these communes as a *commune/homogeneous group*—i.e. Fig. 4 would yield three rather than one quasi-experiment. Table 1 shows the impact this has on the number of groups considered. Column 1 (Panel A) shows that while for 1998 there were a total of 114 homogeneous groups, the combination with roughly 314 communes results in 1784 commune/homogeneous groups.²³ As Fig. 4 suggests, the latter continue to produce a "clean" first stage (Fig. 2, Panel B).

From the point of view of an RD design, this exercise involves a tradeoff. On the one hand, communes facilitate sorting, and so produce even more homogeneous sets of comparison schools. For instance, in 2002 a full set of homogeneous group dummies accounted for 57% of the variation in income at the 4th grade level, and 72% of the variation in average mothers' schooling. Using commune/homogeneous group dummies, these numbers increase to 68 and 81%, respectively. The intuition is also illustrated in Fig. 4, where we have arbitrarily drawn commune A to be the one with the highest average performance within the homogeneous group, followed by B and C.

On the other hand, moving to commune/homogeneous groups comes at the cost of smaller comparison groups. For example, while in the urban area primary homogeneous groups contain an average of about 71 schools, commune/homogeneous groups have an average of 7 (Table 1, Panel B), such that the density of observations close to the cutoffs can go down significantly (also illustrated in the hypothetical case of Fig. 4).

Finally, for the metropolitan region of Santiago, we use geo-coded data to ask if an award affects a schools' enrollment and tuition outcomes *relative to its nearby competitors*. We still implement comparisons using homogeneous groups, but compare each school's outcomes to schools *within 1 km of its own location*.²⁴

4.4. Samples and analysis windows

Two final issues define the samples analyzed below. First, we restrict attention to urban comparison groups, since urban parents enjoy greater school choice and therefore more opportunities to leverage information

²³ To include a commune/homogeneous group, we require that it contain both winning and losing schools.

²⁴ We thank an anonymous referee for the suggestion of incorporating physical proximity.

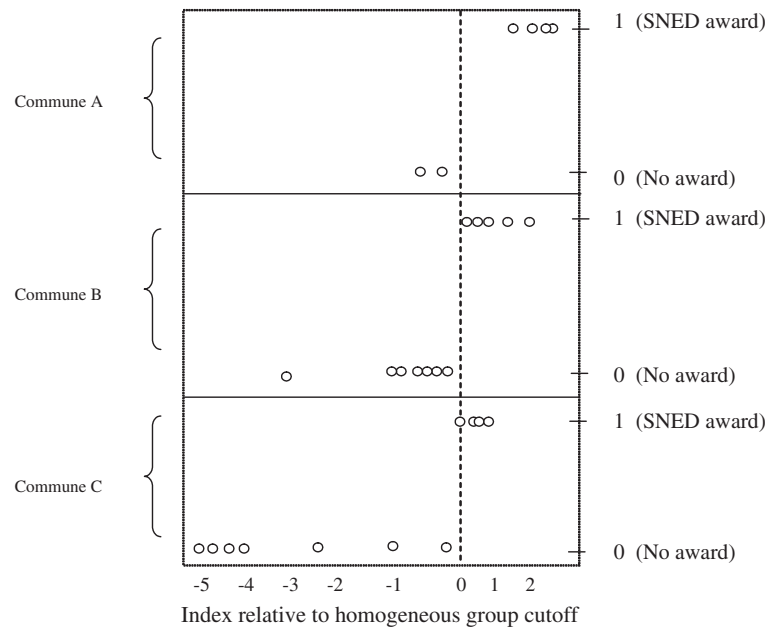


Fig. 4. Division of a homogenous group into three commune/homogeneous groups.

Note: The figure illustrates the construction of commune/homogeneous groups. In this hypothetical case, a homogeneous group contains three communes (communes are fully contained in regions, the basic geographical unit used in the construction of homogeneous groups). The homogenous group-wide cutoff score is common to all three resulting commune/homogeneous groups. For details, see Section 4.3.

on school quality. Second, there is the issue of what chronological windows to analyze.

We consider two alternatives. First, we use what we label a “2-year sample” (Table 1, Panel B) designed to study effects in two year windows, the most natural timeframe to the extent that the SNED waves are two years apart. In this sample, we include schools with valid indices in each round we consider (1998, 2000, 2002, 2004, and 2006), and for which there are at least enrollment outcomes one year prior to each allocation (to check for continuity), as well as two years after. The last round is relevant only for enrollment and vulnerability, for which we have 2008 observations (collected early in the academic year). For all other outcomes, we use only data up to 2006. For the sake of larger samples, we also combine observations from different rounds (clustering standard errors at the school level), and the resulting “stacked” data are described in Column 6 (Table 1, Panel B). Using such data implies that in looking at outcomes two years after each round, for instance, we are asking if schools that received an award in 1998 had higher enrollments in 2000; whether those that received one in 2000 had higher enrollments in 2002, and so on.²⁵

Additionally, Panel C (Table 1) describes a “4-year” sample which allows us to explore effects four years after the 1998, 2000, 2002, and 2004 rounds. This is useful in looking at outcomes that might be slower to change, but raises a caveat because as stated, in general the fact that a school wins an award one year increases the probability that it will do so again.²⁶ This means that while schools that just make and just miss an award may be comparable at baseline (as shown below), four years later some of the former will have received two awards, while the latter will have at most one. This is likely to bias results toward finding positive effects in the 4-year samples.

5. Results

We first present results for primary-level homogeneous groups, then consider primary commune/homogeneous groups, and finally turn to the secondary level.

5.1. First stage

Table 5 presents the first stage results when we analyze the program’s allocation among homogeneous groups. Panel A focuses on the 2-year sample (defined in Section 4.4), and Column 1 shows that among the 12,476 school/year observations it contains, having a baseline index greater than or equal to zero—the normalized cutoff score in schools’ respective groups—raises the probability of receiving an award by about one. Not surprisingly (in light of Fig. 2), the R^2 in this regression is close to one.

While the first stage results are stable in the remaining columns, we discuss them because these describe the specifications and samples used in the tables below. First, Column 2 includes a quadratic in schools’ baseline index as a control, and Column 3 restricts the sample to schools with indices within 0.50 points of their cutoffs, reducing the sample by about half.²⁷ To study the possibility that more educated households are more responsive to data on school quality, Column 4 considers only schools in higher socioeconomic status homogeneous groups.²⁸

Finally, Columns 5 and 6 focus on subsamples of SNED rounds. First, Column 5 considers only the first (1998) round,²⁹ addressing the fact that a “fresh” information program might have the greatest impact—after several implementations, parents might consider schools’ SNED

²⁵ We also carried out our analyses considering outcomes measured one rather than two years after each allocation. We omit these to save space, as they produce similar results.

²⁶ This is despite the fact that some of the SNED sub-indices are measured with some noise, e.g., the inter-cohort changes—see Kane and Staiger (2002), Chay et al. (2005), and Mizala et al. (2007).

²⁷ We experimented with narrower bands, and with requiring that schools be one of, for example, four of the institutions closest to the cutoff. Additionally, we also used simple linear and further polynomial controls. These exercises produced qualitatively similar conclusions.

²⁸ That is, we use only homogeneous groups which the SNED itself catalogued as higher SES. We carried out similar exercises for low SES groups, reaching similar conclusions; we omit these for the sake of space.

²⁹ As stated, there is an earlier 1996 round that used a different methodology.

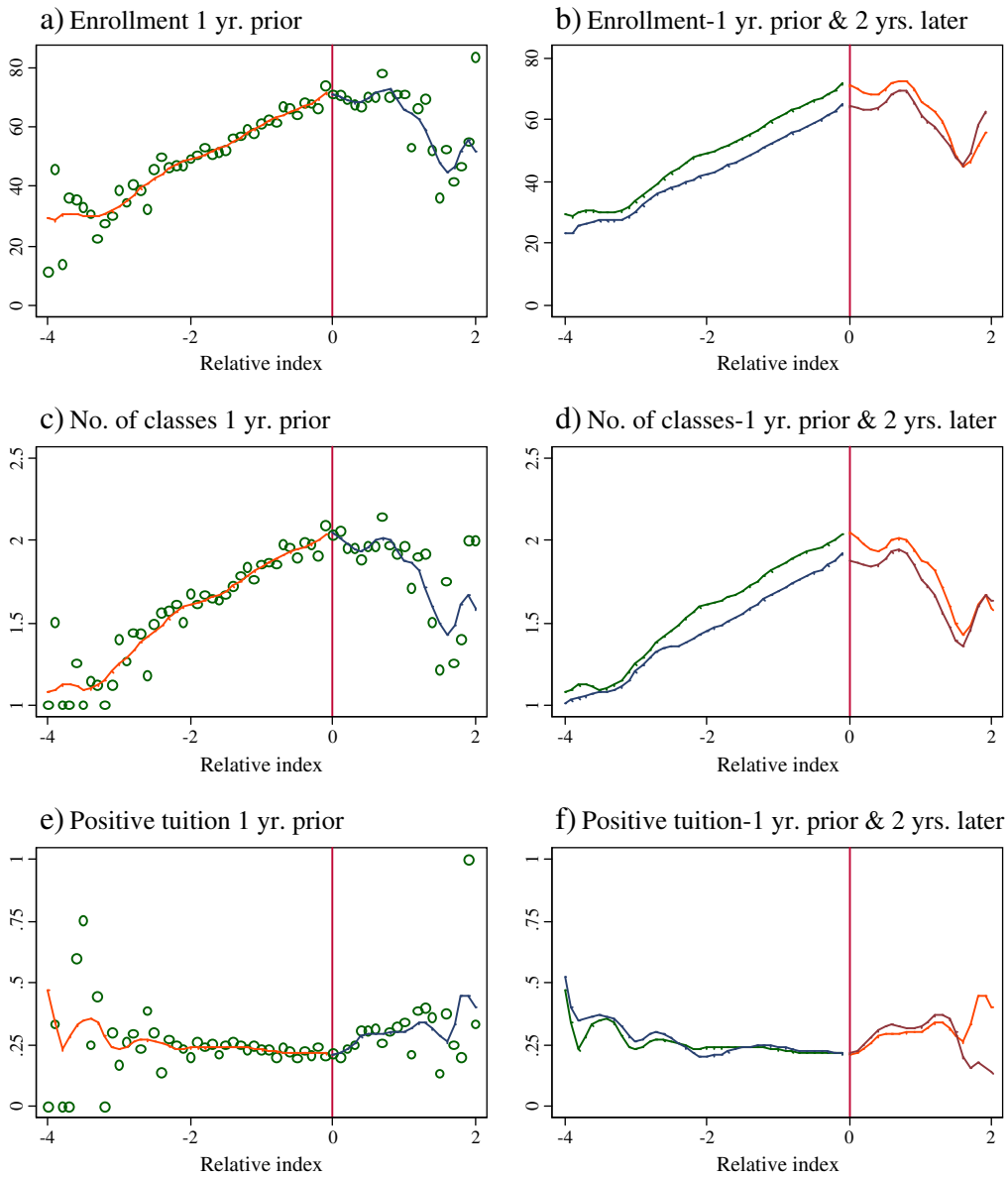


Fig. 5. a. Outcomes at baseline and two years after (2-year primary samples).

Note: The left hand side panels plot index-cell means of schools' outcomes one year prior to each round. Each school's index is expressed relative to the normalized cutoff (the vertical line) in its respective homogeneous group. For clarity, index values are in 0.1 wide bins (see [Imbens and Lemieux, 2007](#)). The curves in these panels plot fitted values of locally weighted regressions of schools' outcomes on their relative index values (estimated separately for schools with negative and non-negative values, both with a bandwidth of 0.2). The right hand side panels replicate the fitted values from the left hand side, and add similar plots for outcomes two years after each round. All estimates are for the primary school 2-year homogeneous group sample (see [Section 4.4](#) and [Table 1, Panel B](#)).

history rather than their performance on the last available round. Second, Column 6 considers the last two (2004 and 2006) waves, investigating the possibility that parents might need an adjustment period to understand the SNED, and to address the fact that the intensity of dissemination has increased over time. The fit is strong throughout, and the R^2 reaches one in the final column. This reflects that the small amount of “slippage” in the allocation took place in 1998. Panel B describes analogous results for the 4-year sample ([Section 4.4](#)), which requires excluding the 2006 wave, thereby reducing the initial (Column 1) sample from 12,476 to 9639 schools.

Finally, still considering the 4-year sample, Panel C explores whether the probability of winning *additional* awards varies discretely at the cutoffs. Column 1 shows that schools with a baseline index greater than or equal to zero are 35% more likely to win

another award two years later.³⁰ Adding a quadratic in the index (Column 2) reduces the estimate by about half, but it is still significant there and in Columns 3 and 6 (not in the higher socioeconomic status or 1998 samples, Columns 4 and 5, respectively). Significance is consistent with some persistence in SNED awards, which as stated, might lead one to expect significant effects four years after each allocation.

³⁰ This is not surprising if only given that schools' test score levels (an input into the aggregate index) are correlated with their socioeconomic composition, which is likely to remain fairly stable over time.

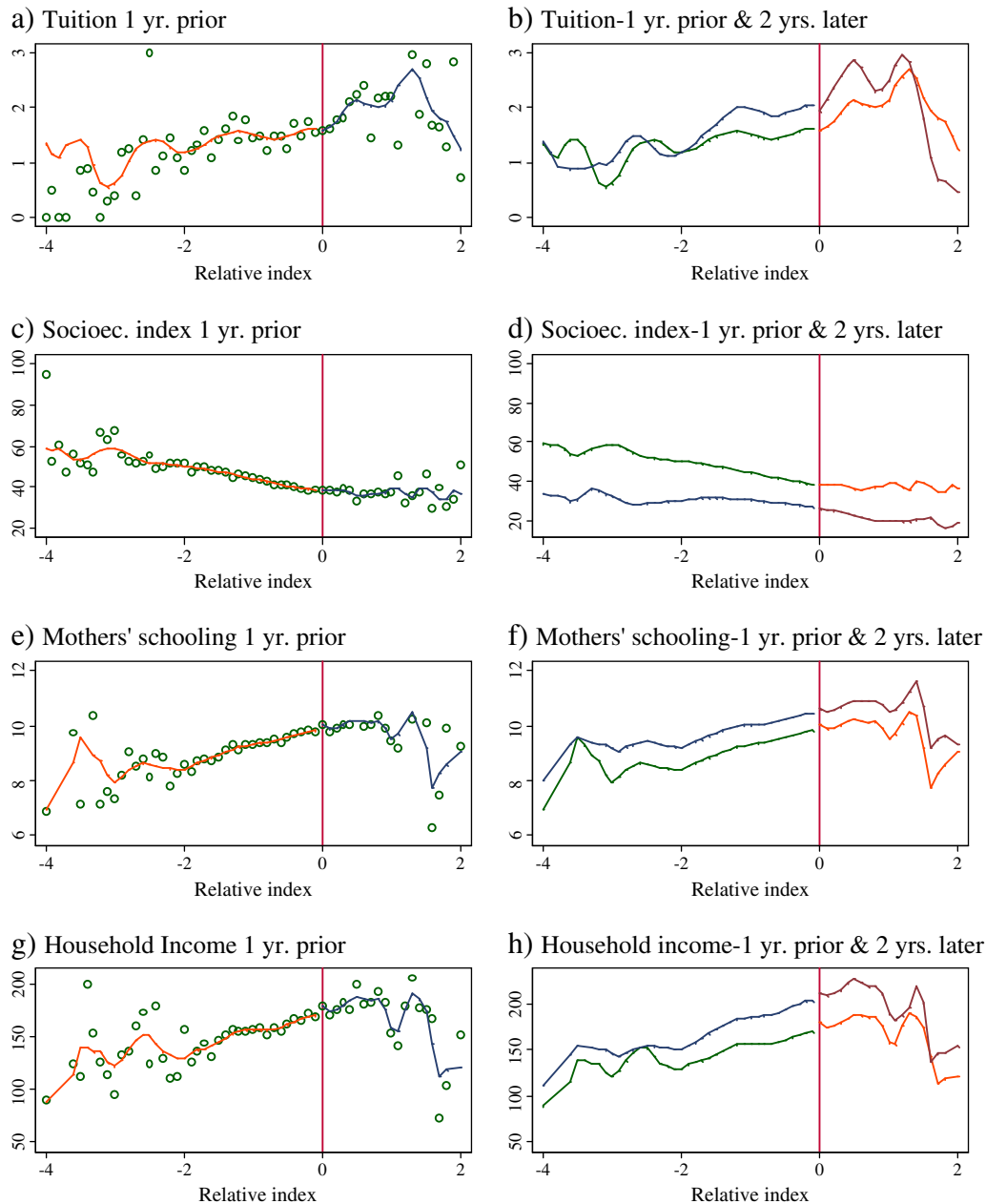


Fig. 5. b. Outcomes at baseline and two years after (2-year primary samples).

Note: The left hand side panels plot index-cell means of schools' outcomes one year prior to each round. Each school's index is expressed relative to the normalized cutoff (the vertical line) in its respective homogeneous group. For clarity, index values are in 0.1 wide bins (see [Imbens and Lemieux, 2007](#)). The curves in these panels plot fitted values of locally weighted regressions of schools' outcomes on their relative index values (estimated separately for schools with negative and non-negative values, both with a bandwidth of 0.2). The right hand side panels replicate the fitted values from the left hand side, and add similar plots for outcomes two years after each round. All estimates are for the primary school 2-year homogeneous group sample (see [Section 4.4](#) and [Table 1, Panel B](#)).

5.2. Enrollment

Turning to the results for enrollment, we first implement a “continuity check” by studying whether enrollment prior to the allocation was greater among schools that obtained an award. Such a difference, if it existed close to the cutoffs, would question an RD approach. [Fig. 5a](#) (Panel A) first presents the graphical evidence for the 2-year sample. It plots index cell means of schools' 1st grade enrollment one year before the baseline allocations, along with the fitted values of locally weighted regressions estimated separately for institutions with negative and non-negative index values. The figure shows an overall positive association between enrollment and schools' relative index, illustrating that even within homogeneous groups, schools

that received an award tended to be larger prior to doing so. [Table 6](#) (Panel A) illustrates this in regression form, showing that this difference was equal to about eight 1st grade students, equivalent to about 20% of a standard deviation.³¹ Columns 2–7 show, however, that this difference falls to less than one student and ceases to be significant when we use a quadratic of the SNED index as a control, or when we focus on any of the more restricted samples. Graphically, this is reflected in that there is no visible break at the cutoff in [Fig. 5a](#), Panel A. In short, as required

³¹ As stated, we selected the samples such that they would include schools with valid enrollment information in all relevant years. This is why the 12,496 observations in [Table 5](#) match those for the 1st stage. For other outcomes, the sample sizes are generally smaller due to missing data.

Table 4
Dependent variable—school average (Math and Language) test score.

	Dep. var.: 8th grade math score in 2004				Dep. var.: 10th grade math score in 2003			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SNED winner in 2002	12.39*** (0.63)	10.32*** (0.62)	7.22*** (0.61)	1.50** (0.62)	29.96*** (1.74)	21.69*** (1.70)	4.35*** (1.21)	2.65** (1.18)
Mothers' schooling in 2002/2001		3.80*** (0.32)	2.01*** (0.31)	1.40*** (0.29)		9.06*** (0.87)	1.65*** (0.61)	1.39*** (0.59)
Household income in 2002/2001		0.03** (0.01)	0.03*** (0.01)	0.02*** (0.01)		0.03** (0.01)	−0.01 (0.01)	−0.01 (0.01)
Math test score in 2002/2001			0.29*** (0.01)	0.21*** (0.01)			1.05*** (0.03)	0.89*** (0.04)
Math test score in 2000/1998				0.38*** (0.02)				0.27*** (0.04)
Homogeneous group dummies	No	No	Yes	Yes	No	No	Yes	Yes
R2	0.435	0.475	0.528	0.584	0.611	0.673	0.860	0.867
N	4108	4108	4108	4108	1137	1137	1137	1137

Note: Columns 1–4 refer to primary level information and Columns 5–8 to secondary data. The mothers' schooling, income, and previous score controls are for 2002 and 2000 for the primary schools, and 2001 and 1998 for the secondary ones. The controls and test scores for 2002 and 2000 are at the 4th and the 8th grade, respectively, and those for 2001 and 1998 are at the 10th grade level.

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

by the RD design, enrollment prior to each allocation appears to be smoothly related to the index in the vicinity of the cutoffs.³²

Panel B (Table 6) considers enrollment two years after awards are made. While there is again a substantial difference in the simplest specification (Column 1), this becomes smaller and insignificant in Columns 2 and 3; in the latter (which controls for the index and restricts the sample to schools closer to the cutoff scores), the point estimate suggests that on average, schools that just received an award had about one less 1st grader two years after doing so (relative to institutions that just missed an award). Column 4 replicates the sample from Column 3, but controls for enrollment one year prior to the awards.³³ The point estimate is now -0.2 , and a 95% confidence interval would rule out differences greater than about 1.5 students. Columns 5–7 (higher SES groups and first and recent rounds) suggest similar conclusions, although with less precision.

Thus, the regressions provide no clear evidence that SNED awards led schools to experience greater enrollment growth. This is also illustrated in Fig. 5a (Panel B), which plots the fitted values from Panel A (one year prior to the awards) and adds a similar plot using data from two years after. The plots shifted down over three year spans, which could reflect declining cohort sizes or changes in data collection procedures. There is no evidence, however, of a change in the relationship close the cutoffs. Finally, Panels C and D in Table 6 present analogous regression evidence for the 4-year sample with similar results, despite the fact that we expect these estimates to be biased upward.

5.3. Number of classes in operation

We now ask whether award winners are more likely to open additional classrooms. To some extent this is a robustness check, since given the absence of evidence of changes in enrollment, increases in the number of classes would correspond to costly class size reductions. Panels C and D in Fig. 5a present the graphical evidence.

The regression results are in Table 7. Beginning with a continuity check using data one year prior to each allocation, Column 1 (Panels A and C for the 2- and 4-year samples, respectively) shows that even

within homogeneous groups, schools that eventually received an award operated 0.1 more 1st grades, equivalent to a tenth of a standard deviation. In both samples, the differences become smaller and insignificant as we add a quadratic in the index or restrict the sample. The lack of a size advantage for winning schools, further, holds whether we look at outcomes two or four (Panels B and D, respectively) years after each allocation. In short, we find no clear evidence that SNED-winning schools were more likely to grow.

5.4. Tuition

These results suggest that schools that obtain a SNED award do not experience higher demand, or else choose to turn away students rather than expand. While it would be surprising for schools to entirely resist the opportunity to increase their revenues, doing so might reflect that raising class sizes or opening new classrooms involves risk. Additionally, schools might have made commitments regarding class or school size.

We unfortunately do not have measures of excess demand, but in this and the next section we study two other ways in which schools could leverage increased demand: by raising their prices or by becoming more selective in terms of socioeconomic status. The first is relevant because the period we analyze was characterized by an increasing trend in the prevalence and magnitude of tuition charges. For instance, in 1997, 38% of voucher schools charged add-on tuition, and the average was about 3 dollars a month. By 2006, these figures had increased to 50% and 14 dollars, respectively.

We first consider whether schools charge tuition at all,³⁴ which is useful partially because we do not observe grade-specific tuition, and average charges may move slowly if schools raise prices only for entering cohorts. In contrast, going from zero to positive tuition is an outcome that is more easily observed. Fig. 5a presents the graphical evidence, which is suggestive of no effect (Panel E), and the fitted values one year prior and two years later essentially overlap (Panel F), particularly close to the cutoff. For more detail, Table 8 (Column 1) suggests that even within homogeneous groups, schools that receive an award are more likely to charge tuition, although the difference is not great. Controlling for the SNED index or restricting samples close to the cutoffs, however, eliminates significant differences—the point estimates

³² We also note that the density of observed SNED index levels (Fig. 1, Panel B) does not suggest that schools or administrators in any way manipulate the running variable (see McCrary, 2008).

³³ We could also compare changes, implicitly restricting the coefficient on prior enrollment to be one; we opt instead for the more flexible specification in Column 4.

³⁴ As stated, primary municipal schools cannot charge tuition. We verified that close to the discontinuities, there are no differences in the proportion of schools that are municipal (as opposed to private voucher).

Table 5
First stage regressions.

	All schools subject to sample selections:			Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.50 points of the cutoff (3)	Within 0.50 points of the cutoff (4)	Within 0.50 points of the cutoff (5)	Within 0.50 points of the cutoff (6)
<i>Panel A: 2-year sample; dep. var.: dummy for receiving an award at baseline</i>						
1{Index ≥ 0}	0.998 ^{***} (0.001)	0.997 ^{***} (0.001)	0.995 ^{***} (0.002)	0.985 ^{***} (0.007)	0.970 ^{***} (0.013)	1.000 ^{***} (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.997	0.997	0.996	0.983	0.972	1.000
N	12,476	12,476	5750	1255	844	2566
<i>Panel B: 4-year sample; dep. var.: dummy for receiving an award at baseline</i>						
1{Index ≥ 0}	0.997 ^{***} (0.001)	0.996 ^{***} (0.002)	0.994 ^{***} (0.003)	0.981 ^{***} (0.009)	0.969 ^{***} (0.013)	1.000 ^{***} (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.997	0.997	0.994	0.979	0.971	1.000
N	9639	9639	4494	994	836	1375
<i>Panel C: 4-year sample; dep. var.: dummy for receiving an award 2 years after the baseline</i>						
1{Index ≥ 0}	0.347 ^{***} (0.012)	0.165 ^{***} (0.018)	0.071 ^{**} (0.027)	0.003 (0.057)	0.053 (0.066)	0.131 ^{**} (0.049)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.112	0.138	0.062	0.086	0.039	0.169
N	9639	9639	4494	994	836	1375*

Note: The table presents 1st stage regressions based on cutoff scores at the homogeneous group level. The dependent variable indicates whether schools received an award, and the key independent variable indicates if their index was greater than or equal to their respective group's cutoff. Panels A and B–C refer to the 2- and 4-year samples respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table 6
1st grade enrollment.

	All schools subject to sample selections:			Higher SES groups only	1998 round	2004 and 2006 rounds ^a	
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: enrollment 1 year prior to allocation</i>							
1{Index ≥ 0}	7.5 ^{***} (1.2)	−0.8 (1.8)	−0.4 (2.1)		0.9 (4.5)	−0.8 (6.2)	0.3 (2.7)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.006	0.027	0.003		0.002	0.003	0.003
N	12,476	12,476	5750		1255	844	2566
<i>Panel B: 2-year sample; dep. var.: enrollment 2 years after allocation</i>							
1{Index ≥ 0}	9.3 ^{***} (1.1)	−0.3 (1.7)	−0.9 (1.9)	−0.2 (0.8)	−0.7 (1.8)	−0.4 (2.4)	−1.5 (1.1)
Enrollment 1 year prior to allocation				0.83 ^{***} (0.01)	0.85 ^{***} (0.03)	0.80 ^{***} (0.02)	0.82 ^{***} (0.01)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.011	0.034	0.004	0.812	0.800	0.819	0.820
N	12,476	12,476	5750	5750	1255	844	2566
<i>Panel C: 4-year sample; dep. var.: enrollment 1 year prior to allocation</i>							
1{Index ≥ 0}	7.2 ^{***} (1.3)	−0.1 (1.9)	0.2 (2.4)		1.0 (4.9)	−0.9 (6.2)	2.1 (3.8)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.005	0.022	0.004		0.006	0.003	0.004
N	9639	9639	4494		994	836	1375
<i>Panel D: 4-year sample; dep. var.: enrollment 4 years after allocation</i>							
1{Index ≥ 0}	9.3 ^{***} (1.2)	0.4 (1.7)	−0.2 (2.1)	−0.3 (1.1)	−0.2 (2.6)	1.6 (2.8)	−0.2 (1.9)
Enrollment 1 year prior to allocation				0.75 ^{***} (0.01)	0.79 ^{***} (0.02)	0.76 ^{***} (0.02)	0.75 ^{***} (0.02)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.012	0.034	0.004	0.732	0.740	0.773	0.723
N	9639	9639	4494	4494	994	836*	1375*

Note: The table presents regressions of schools' 1st grade enrollment on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table 7
Number of 1st grade classes.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: no. of classes 1 year prior to allocation</i>							
1{Index ≥ 0}	0.11*** (0.03)	−0.02 (0.04)	0.01 (0.05)		0.05 (0.10)	0.01 (0.20)	0.03 (0.06)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.002	0.017	0.002		0.001	0.002	0.002
N	12,057	12,057	5661		1244	833	2531
<i>Panel B: 2-year sample; dep. var.: no. of classes 2 years after allocation</i>							
1{Index ≥ 0}	0.13*** (0.03)	−0.03 (0.04)	−0.03 (0.05)	−0.04* (0.02)	−0.04 (0.05)	−0.05 (0.07)	−0.06** (0.03)
1st grades 1 year prior to allocation				0.79*** (0.01)	0.84*** (0.03)	0.77*** (0.02)	0.77*** (0.02)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.004	0.021	0.002	0.718	0.733	0.749	0.686
N	12,057	12,057	5661	5661	1246	833	2531
<i>Panel C: 4-year sample; dep. var.: no. of classes 1 year prior to allocation</i>							
1{Index ≥ 0}	0.11*** (0.03)	−0.02 (0.04)	0.02 (0.06)		0.06 (0.11)	0.03 (0.18)	0.05 (0.09)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.001	0.014	0.003		0.004	0.001	0.002
N	9244	9244	4399		977	809	1357
<i>Panel D: 4-year sample; dep. var.: no. of classes 4 years after allocation</i>							
1{Index ≥ 0}	0.14*** (0.03)	−0.02 (0.04)	−0.03 (0.05)	−0.04 (0.03)	−0.05 (0.07)	−0.02 (0.07)	−0.05 (0.05)
1st grades 1 year prior to allocation				0.70*** (0.01)	0.77*** (0.03)	0.72*** (0.02)	0.70*** (0.02)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.005	0.020	0.002	0.660	0.665	0.704	0.614
N	9244	9244	4399	4399	977	809	1357

Note: The table presents regressions of the number of 1st grades schools operate on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

in Column 4, our preferred specification, are essentially zero. In the two year sample (Panels B) they rule out that awards increase the probability of charging tuition by more than about 2%.

The evidence on absolute tuition (Panels A and B of Fig. 5b, and Table 9) points to similar conclusions. Whether we consider outcomes prior to the allocation or two or four years thereafter, award winners on average collect higher tuition (roughly a tenth of a standard deviation). This is expected, if only given the positive correlation between the SNED index and socioeconomic status (Fig. 1). As before, however, these differences become small and insignificant when we control for the SNED index or restrict the sample to wealthier homogeneous groups, or to the first or last two SNED rounds.

5.5. Socioeconomic composition

Turning to socioeconomic composition, we first analyze whether winning an award lowers schools' vulnerability indices, which range from 0 (highest socioeconomic status) to 100 (lowest). Table 10 (Column 1) and Fig. 5b (Panels C and D) illustrate that even within homogeneous groups, winning schools have lower average vulnerability indices—about five percentage points so (equivalent to one fifth of a standard deviation). These estimates become smaller and insignificant as one adds controls or restricts samples close to the cutoffs. In short, the evidence does not suggest that SNED awards improved schools' SES, although these estimates are somewhat less precise than those for enrollment, for instance.

We complement this evidence with direct measures of SES, although this raises several data-related issues. Specifically, students' socioeconomic characteristics are measured at the 4th, 8th, and 10th grades (through the SIMCE testing system) and each of these grades is surveyed only every third year.³⁵ Adapting these data to regressions like those above therefore requires significant adjustments. First, we assume 4th grade information is representative of primary-aged children, ignoring the 8th grade (if awards mainly impact the composition of entering cohorts, capturing changes using 8th grade data would require longer time series than we have). Second, we use only the 2000, 2002, and 2004 SNED waves. For the first two, we are able to look at outcomes two and four years later; for the last, only two years after each allocation.³⁶

With these caveats, Fig. 5b (Panels E–H) and Tables 11 and 12 present results on average mothers' schooling and household income, respectively. In each table, Column 1 indicates that even within homogeneous groups, SNED-winning schools have “better” peer groups—they contain children whose mothers have about 0.6 more years of schooling, and whose household income is about 20 thousand pesos higher (about 0.3 and 0.4 of a standard deviation,

³⁵ More specifically, we have 4th grade observations for the 1999, 2000, 2002, 2005, and 2006 rounds.

³⁶ Specifically, we use the 1999 data as the “pre” observation for both the 2000 and 2002 waves; and the 2002 tests as the pre-observation for the 2004 wave. For the 2000 wave, the outcome two years later is naturally provided by the 2002 round; we use the 2005 test for the 4-years later outcome (as if it had been collected in 2004). For the 2002 and 2004 waves, outcomes two years later are provided by the 2005 (again, as if it had been collected in 2004) and 2006 waves.

Table 8
Positive tuition.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: positive tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.03** (0.01)	0.01 (0.02)	−0.02 (0.02)		−0.03 (0.06)	−0.03 (0.07)	0.03 (0.04)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.001	0.002	0.003		0.007	0.008	0.002
N	9893	9893	4585		1019	844	1401
<i>Panel B: 2-year sample; dep. var.: positive tuition 2 years after allocation</i>							
1{Index ≥ 0}	0.04*** (0.01)	0.01 (0.02)	−0.02 (0.02)	0.00 (0.01)	0.01 (0.01)	−0.02 (0.02)	−0.01 (0.01)
Positive tuition 1 year prior to allocation				0.97*** (0.00)	0.94*** (0.01)	0.93*** (0.02)	0.99*** (0.00)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.001	0.003	0.003	0.907	0.884	0.842	0.943
N	9893	9893	4585	4585	1019	844	1401
<i>Panel C: 4-year sample; dep. var.: positive tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.04*** (0.01)	0.01 (0.02)	−0.04 (0.03)		−0.06 (0.07)	−0.03 (0.06)	−
Quadratic in index	No	Yes	Yes		Yes	Yes	−
R ²	0.002	0.004	0.004		0.010	0.008	−
N	7217	7217	3119		710	836	−
<i>Panel D: 4-year sample; dep. var.: positive tuition 4 years after allocation</i>							
1{Index ≥ 0}	0.05*** (0.01)	0.02 (0.02)	−0.03 (0.03)	0.01 (0.01)	0.01 (0.02)	−0.01 (0.02)	−
Positive tuition 1 year prior to allocation				0.95*** (0.01)	0.92*** (0.02)	0.94*** (0.02)	−
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	−
R ²	0.003	0.005	0.004	0.868	0.847	0.837	−
N	7217	7217	3119	3119	710	836*	−

Note: The table presents regressions of an indicator for whether schools charge tuition on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a Because we only have tuition data up to 2006 (inclusive), for the 2-year sample the analysis in Column 6 refers only to the 2004 wave. For the 4-year sample (Panels C and D), this specification is not feasible.

respectively). These differences mostly cease to be significant, however, when we restrict the sample to schools close to the cutoff; they mostly also become smaller, and in some cases even suggest negative award effects.

We underline that these results should be viewed with greater caution as the sample sizes are smaller than those above; further, the data are self-reported, and about 20% of urban parents do not return the questionnaires that are their source.

5.6. Robustness checks

The estimates presented thus far rely on the SNED-constructed homogeneous groups. As a first robustness check (described in Section 4), we carry out similar exercises comparing schools that are in the same commune in addition to being in the same homogeneous group. Appendix Tables A.1–A.3 (Panel A) implement this exercise, although for the sake of space we consider only two outcomes: enrollment and whether schools charge tuition.³⁷ The conclusions are similar to those above save for two differences. First, the estimates are somewhat less precise in part because the exercise results in smaller samples close to the cutoff scores. This may be behind a few estimates suggesting

³⁷ More specifically, Table A.1 (Panel A) presents the first stage. Tables A.2 and A.3 present the enrollment and tuition results, respectively. Other outcomes produce similar conclusions.

significantly positive and negative effects, although there is not consistent evidence in either direction. Second, even more than among homogeneous groups, there is persistence in SNED awards.

As a second exercise, we explore the effects of the awards on the same outcomes (enrollment and tuition), but comparing each school's outcomes to that of schools *within 1 km of its own location*.³⁸ This exercise is relevant in view of Gallego and Hernando's (2007) finding that distance to school (along with test scores) is one of the two central determinants of school choice in Chile. We feature it as a robustness check because we can only implement this analysis among schools in the metropolitan region of Santiago, for which we were able to obtain the geo-coded locations.

Specifically, for this region we ask if schools that just won an award (relative to those that just missed getting one) have different outcomes along two dimensions: i) their *share* of the total 1st or 9th grade enrollment among all schools serving these grades within 1 km of their location, and ii) their tuition level as a proportion of the average tuition among all schools that have positive fees within 1 km of their location.

³⁸ The 1 km cutoff is arbitrary; other cutoffs like 0.5 and 2 km produce similar conclusions.

Table 9
Tuition.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.35*** (0.12)	0.15 (0.14)	−0.17 (0.20)		−0.65 (0.69)	−0.69** (0.34)	0.09 (0.41)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.004	0.002	0.002		0.006	0.012	0.003
N	9893	9893	4585		1019	844	1401
<i>Panel B: 2-year sample; dep. var.: tuition 2 years after allocation</i>							
1{Index ≥ 0}	0.51*** (0.15)	0.20 (0.18)	−0.25 (0.26)	−0.05 (0.10)	0.09 (0.32)	−0.15 (0.18)	−0.19 (0.16)
Tuition 1 year prior to allocation				1.19*** (0.04)	1.17*** (0.05)	1.19*** (0.09)	1.35*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.002	0.003	0.002	0.846	0.840	0.824	0.927
N	9893	9893	4585	4585	1019	844	1401
<i>Panel C: 4-year sample; dep. var.: tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.39** (0.13)	0.18 (0.16)	−0.25 (0.23)		−0.68 (0.79)	−0.69** (0.34)	–
Quadratic in index	No	Yes	Yes		Yes	Yes	–
R ²	0.002	0.003	0.003		0.006	0.012	–
N	7217	7217	3119		710	836	–
<i>Panel D: 4-year sample; dep. var.: tuition 4 years after allocation</i>							
1{Index ≥ 0}	0.78*** (0.19)	0.30 (0.23)	−0.39 (0.34)	−0.06 (0.15)	−0.02 (0.46)	0.00 (0.23)	–
Tuition 1 year prior to allocation				1.32*** (0.05)	1.23*** (0.07)	1.36*** (0.11)	–
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	–
R ²	0.004	0.006	0.004	0.807	0.798	0.790	–
N	7217	7217	3119	3119	710	836*	–

Note: The table presents regressions of schools' tuition on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a Because we only have tuition data up to 2006 (inclusive), for the 2-year sample the analysis in Column 6 refers only to the 2004 wave. For the 4-year sample (Panels C and D), this specification is not feasible.

Tables A.4 and A.5 present the results, which in this case are uniformly suggestive of insignificant effects, albeit with smaller sample sizes.

As a third exercise, Tables A.1 (Panels D, E, and F) and A.6 consider the effects of the SNED at the secondary level, looking only at enrollment (at the 9th grade level). The same two caveats raised with respect to commune/homogeneous groups apply: relatively less precision and greater persistence in SNED awards. That said, the conclusions are again qualitatively similar (we omit the results on other outcomes for the sake of space).

Finally, we also explored whether results vary according to school age, a possibility raised by Gallego (2008) given that information might be more relevant for newer schools with less established reputations. Specifically, we calculated the number of years that have passed since each school's creation relative to each SNED round, finding no consistent evidence of significant effects for schools with recent or distant creation dates. Similarly, our conclusions are robust to exploring effects for specific sub-sectors, such as voucher-funded private schools.

6. Conclusion

A longstanding debate in the economics of education concerns to what extent improving educational quality can be achieved simply by relying on competition, as opposed to intervention using tools ranging from accountability (e.g., *No child left behind*) to direct public provision.

Researchers have noted that for competition alone to really deliver, it must be that parents' choices, and therefore schools' outcomes, depend on schools' effectiveness in producing academic attainment. If in contrast households view school choice as a means to obtain better peer groups or reduce travel time, then competition might encourage schools to improve on dimensions other than academic value added.³⁹

Over the past decade the literature has produced evidence that school choice is indeed influenced by information on school quality, at least as measured by test scores. Yet this does not fully answer the question at hand because, going back to the Coleman et al. (1966) report, it is clear that test scores conflate school effectiveness and peer quality.

A key gap in our understanding of school markets, therefore, concerns whether parental choices and schools' outcomes would respond to data on effectiveness per se, even if these were not necessarily correlated with peer quality. This paper has attempted to begin filling that gap by analyzing Chile's SNED, a scheme that seeks to approximate

³⁹ This is relevant because some research suggests that parents care about peer quality per se, e.g., Bayer and McMillan (2005), Card et al. (2007), Gallego and Hernando (2007), Henig (1990), Rothstein (2006), Schneider and Buckley (2002), and Urquiola (2005). Further, some research raises the possibility that parents are either uninformed about school quality, or else select schools using other criteria. See for instance Armour and Peiser (1998), Ascher et al. (1996), Elacqua and Fabrega (2004), Henig (1994), Kleitz et al. (2000), and Lubienski (2003).

Table 10
Vulnerability index measured at the 1st grade.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: vulnerability index 1 year prior to allocation</i>							
1{Index ≥ 0}	—	−0.4	0.2		0.9	2.6	−0.6
	5.4*** (0.5)	(0.7)	(1.0)		(1.6)	(2.3)	(1.1)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.014	0.040	0.004		0.020	0.022	0.003
N	10,619	10,619	4965		759	725	2196
<i>Panel B: 2-year sample; dep. var.: vulnerability index 2 years after allocation</i>							
1{Index ≥ 0}	−0.4	0.7	0.1	0.0	−0.8	−0.6	0.9
	(0.6)	(0.9)	(1.4)	(1.3)	(3.0)	(1.3)	(2.0)
Vulnerability 1 year prior to allocation				0.32*** (0.02)	0.13*** (0.03)	0.74*** (0.02)	0.51*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.001	0.010	0.002	0.056	0.006	0.598	0.075
N	10,619	10,619	4965	4965	759	725	2196
<i>Panel C: 4-year sample; dep. var.: vulnerability index 1 year prior to allocation</i>							
1{Index ≥ 0}	—	0.8	0.1		2.2	2.3	−0.9
	5.0*** (0.6)	(0.8)	(1.1)		(2.0)	(2.3)	(1.5)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.011	0.044	0.002		0.022	0.023	0.001
N	7933	7933	3733		529	703	1138
<i>Panel D: 4-year sample; dep. var.: vulnerability index 4 years after allocation</i>							
1{Index ≥ 0}	—	—	−2.1	−2.1	−1.8	−1.8	−1.2
	3.1*** (0.7)	3.0** (1.0)	(1.7)	(1.7)	(3.8)	(1.5)	(0.7)
Vulnerability 1 year prior to allocation				0.17*** (0.02)	0.17*** (0.06)	0.72*** (0.02)	0.42*** (0.02)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.005	0.005	0.004	0.018	0.027	0.541	0.449
N	7933	7933	3733	3733	529	703	1138*

Note: The table presents regressions of schools' vulnerability index values on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

effectiveness in a manner the public might understand. Perhaps surprisingly, we fail to find systematic evidence that SNED awards impact schools' market outcomes.

It is important to note that this result is consistent with several possible mechanisms. For example, it may reflect that the RD design produces a local estimate, and that parents considering schools close to cutoffs realize they are similar. One must keep in mind, however, that SNED index values are not publicized, so that parents may not know when comparisons concern schools close together in index scores. Further, the existence of the teacher bonus means that schools that just win and those that just miss awards are different ex-post.

Another set of potential explanations relates to how households might view the SNED. First, the scheme might not have registered with parents, despite the fact that it has been in place 15 years. Second, the information it produces might not be “news”—parents might be able to deduce it on their own. Such sophisticated consumers might also discount some of the noisier information contained in the SNED. Further, the awards might confuse parents if they see schools they believe are comparable ending up in different award categories simply because they belong to different homogeneous groups. All

these possibilities reaffirm that interpreting information approximating school effectiveness is likely to be difficult, even if households pay close attention.

Further, even if parents value school effectiveness and are able to discern it, information on it might in the end not sway school choices also based on characteristics like peer composition. This would rationalize stronger reactions to data on average performance (found in the literature) than to information approximating effectiveness. It is also consistent with theoretical work by MacLeod and Urquiola (2009), suggesting that even in the absence of causal peer effects, parents may care about peer composition per se (as well as value added).

The bottom line is that Chile's experience with the SNED raises questions as to the impact of accountability-type schemes that are based on value added. It may be, for example, that distributing information on absolute achievement creates more pressure on schools than information on value added—taken at face value, the combination of our results and those of Andrabi et al. (2009) would seem to suggest that. This might reflect that ultimately absolute achievement may be closer to what parents desire to find out about, or that households might view school quality measures based on absolute achievement as more persistent.

Table 11
Average mothers' schooling.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: avg. mothers' schooling 1 year prior to allocation</i>							
1{Index ≥ 0}	0.6*** (0.1)	0.1** (0.1)	0.1 (0.1)		−0.2 (0.2)		0.3 (0.2)
Quadratic in index	No	Yes	Yes		Yes		Yes
R ²	0.020	0.046	0.006		0.037		0.008
N	7102	7102	3612		655		1381
<i>Panel B: 2-year sample; dep. var.: avg. mothers' schooling 2 years after allocation</i>							
1{Index ≥ 0}	0.5*** (0.1)	0.1 (0.1)	0.0 (0.1)	−0.1 (0.1)	−0.2 (0.1)		−0.1 (0.1)
Mothers' sch. 1 year prior to allocation				0.86*** (0.01)	0.74*** (0.03)		0.89*** (0.01)
Quadratic in index	No	Yes	Yes	Yes	Yes		Yes
R ²	0.018	0.039	0.006	0.754	0.524		0.766
N	7102	7102	3612	3612	655		1381
<i>Panel C: 4-year sample; dep. var.: avg. mothers' schooling 1 year prior to allocation</i>							
1{Index ≥ 0}	0.6*** (0.1)	0.1 (0.1)	0.0 (0.1)		−0.4*** (0.2)		–
Quadratic in index	No	Yes	Yes		Yes		–
R ²	0.022	0.047	0.008		0.050		–
N	4565	4565	2190		357		–
<i>Panel D: 4-year sample; dep. var.: avg. mothers' schooling 4 years after allocation</i>							
1{Index ≥ 0}	0.6*** (0.1)	0.1 (0.1)	0.0 (0.1)	0.0 (0.1)	0.0 (0.1)		–
Mothers' sch. 1 year prior to allocation				0.86*** (0.01)	0.91*** (0.05)		–
Quadratic in index	No	Yes	Yes	Yes	Yes		–
R ²	0.022	0.044	0.008	0.742	0.641		–
N	4565	4565	2190	2190	3578*		–

Note: The table presents regressions of schools' average mothers' schooling on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a Because we only have mothers' schooling data up to 2006 (inclusive), for the 2-year sample the analysis in Column 6 refers only to the 2004 wave. For the 4-year sample (Panels C and D), this specification is not feasible.

Table 12
Household income.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: avg. hhld. income 1 year prior to allocation</i>							
1{Index ≥ 0}	19.9*** (2.8)	10.2*** (3.7)	8.1 (5.6)		−0.9 (17.8)		14.3* (8.5)
Quadratic in index	No	Yes	Yes		Yes		Yes
R ²	0.011	0.018	0.004		0.014		0.004
N	7104	7104	3612		655		1381
<i>Panel B: 2-year sample; dep. var.: avg. hhld. income 2 years after allocation</i>							
1{Index ≥ 0}	25.4*** (3.4)	10.1** (4.4)	5.3 (6.3)	−3.0 (3.1)	−7.6 (9.7)		−8.9* (4.8)
Hhld. income 1 year prior to allocation				1.02*** (0.02)	0.94*** (0.03)		1.09*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes		Yes
R ²	0.012	0.024	0.004	0.748	0.679		0.765
N	7104	7104	3612	3612	655		1381
<i>Panel C: 4-year sample; dep. var.: avg. hhld. income 1 year prior to allocation</i>							
1{Index ≥ 0}	21.9*** (3.2)	8.6* (4.8)	5.6 (7.4)		−8.1 (26.5)		−
Quadratic in index	No	Yes	Yes		Yes		−
R ²	0.014	0.021	0.007		0.026		−
N	4566	4566	2190		357		−
<i>Panel D: 4-year sample; dep. var.: avg. hhld. income 4 years after allocation</i>							
1{Index ≥ 0}	28.6*** (4.2)	10.3* (6.0)	4.9 (9.1)	−1.3 (4.2)	3.5 (14.6)		−
Hhld. income 1 year prior to allocation				1.10*** (0.07)	1.06*** (0.05)		−
Quadratic in index	No	Yes	Yes	Yes	Yes		−
R ²	0.014	0.024	0.006	0.748	0.718		−
N	4566	4566	2190	2190	357		−

Note: The table presents regressions of schools' average household income on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and the notes to Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a Because we only have household income data up to 2006 (inclusive), for the 2-year sample the analysis in Column 6 refers only to the 2004 wave. For the 4-year sample (Panels C and D), this specification is not feasible.

Appendix A

Table A.1

1st stage regressions for primary level commune/homogeneous groups and secondary level homogeneous groups.

	All schools subject to sample selections:			Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)
<i>Primary level commune/homogeneous groups</i>						
Panel A: 2-year sample; dep. var.: dummy for receiving an award at baseline						
1{Index ≥ 0}	1.000*** (0.000)	1.000*** (0.000)	1.000*** (0.000)	0.999*** (0.001)	1.000*** (0.001)	1.000*** (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.999	1.000	0.999	0.996	0.993	1.000
N	9857	9857	4113	924	632	1855
Panel B: 4-year sample; dep. var.: dummy for receiving an award at baseline						
1{Index ≥ 0}	1.000*** (0.000)	0.999*** (0.001)	1.000*** (0.000)	0.999*** (0.001)	0.999*** (0.001)	1.000*** (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.999	0.999	0.999	0.995	0.993	1.000
N	7524	7524	3186	738	623	974
Panel C: 4-year sample; dep. var.: dummy for receiving an award 2 years after the baseline						
1{Index ≥ 0}	0.351*** (0.013)	0.235*** (0.016)	0.127*** (0.031)	0.112* (0.062)	0.109** (0.071)	0.112** (0.054)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.124	0.140	0.062	0.068	0.016	0.169
N	7524	7524	3186	738	623	974
<i>Secondary level homogeneous groups</i>						
Panel D: 2-year sample; dep. var.: dummy for receiving an award at baseline						
1{Index ≥ 0}	0.999*** (0.001)	0.999*** (0.001)	0.997*** (0.003)	0.992*** (0.008)	1.000*** (0.000)	1.000*** (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.999	0.999	0.998	0.994	1.000	1.000
N	6268	6268	2518	664	282	1249
Panel E: 4-year sample; dep. var.: dummy for receiving an award at baseline						
1{Index ≥ 0}	0.999*** (0.001)	0.998*** (0.002)	0.997*** (0.003)	0.989*** (0.011)	1.000*** (0.000)	1.000*** (0.000)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.999	0.999	0.998	0.992	1.000	1.000
N	4493	4493	1898	498	276	653
Panel F: 4-year sample; dep. var.: dummy for receiving an award 2 years after the baseline						
1{Index ≥ 0}	0.460*** (0.017)	0.303*** (0.026)	0.146*** (0.042)	0.149* (0.078)	0.169* (0.010)	0.131* (0.071)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.200	0.219	0.140	0.108	0.034	0.205
N	4493	4493	1898	498	276	653

Note: The table presents 1st stage regressions for primary level commune/homogeneous groups (Panels A–C) and secondary level homogeneous groups (Panels D–F). The dependent variable indicates whether schools received a SNED award, and the key independent variable indicates if their index was greater than or equal to their reference groups.

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table A.2
1st grade enrollment among commune/homogeneous groups.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: enrollment 1 year prior to allocation</i>							
1{Index ≥ 0}	8.5 ^{***} (1.3)	−0.9 (2.0)	−2.9 (2.6)		2.2 (5.1)	−2.3 (7.1)	−5.6 [*] (7.1)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.008	0.025	0.002		0.001	0.005	0.005
N	9857	9857	4113		924	632	1855
<i>Panel B: 2-year sample; dep. var.: enrollment 2 years after allocation</i>							
1{Index ≥ 0}	10.0 ^{***} (1.2)	−0.4 (1.9)	−2.4 (2.4)	0.0 (0.9)	−2.1 (2.2)	−0.1 (3.0)	−0.8 (1.4)
Enrollment 1 year prior to allocation				0.86 ^{***} (0.01)	0.89 ^{***} (0.03)	0.81 ^{***} (0.02)	0.84 ^{***} (0.01)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.013	0.033	0.003	0.817	0.798	0.817	0.814
N	9857	9857	4113	4113	924	632	1855
<i>Panel C: 4-year sample; dep. var.: enrollment 1 year prior to allocation</i>							
1{Index ≥ 0}	8.0 ^{***} (1.4)	−0.4 (2.1)	−3.2 (2.9)		−0.5 (5.6)	−4.2 (7.1)	−8.6 [*] (4.5)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.007	0.021	0.003		0.002	0.004	0.011
N	7324	7324	3186		738	623	974
<i>Panel D: 4-year sample; dep. var.: enrollment 4 years after allocation</i>							
1{Index ≥ 0}	10.3 ^{***} (1.3)	0.3 (2.0)	−2.2 (2.6)	0.3 (1.2)	−3.3 (2.8)	−1.6 (3.3)	1.6 (2.2)
Enrollment 1 year prior to allocation				0.78 ^{***} (0.01)	0.83 ^{***} (0.03)	0.77 ^{***} (0.03)	0.77 ^{***} (0.02)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.015	0.036	0.005	0.757	0.744	0.772	0.743
N	7524	7524	3186	3186	738	623	974 ^{**}

Note: The table presents regressions of schools' 1st grade enrollment on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective commune/homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table A.3
Positive tuition among commune/homogeneous groups.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: positive tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.03 ^{**} (0.01)	0.02 (0.02)	−0.00 (0.03)		0.05 (0.07)	−0.05 (0.07)	0.00 (0.05)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.001	0.002	0.004		0.008	0.009	0.003
N	7764	7764	3252		750	632	994
<i>Panel B: 2-year sample; dep. var.: positive tuition 2 years after allocation</i>							
1{Index ≥ 0}	0.03 ^{***} (0.01)	0.03 (0.02)	0.00 (0.03)	0.00 (0.01)	0.03 [*] (0.01)	0.01 (0.02)	−0.03 (0.02)
Positive tuition 1 year prior to allocation				0.97 ^{***} (0.00)	0.95 ^{***} (0.01)	0.93 ^{***} (0.01)	0.98 ^{***} (0.01)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.001	0.002	0.005	0.913	0.913	0.848	0.940
N	7764	7764	3252	3252	750	632	994
<i>Panel C: 4-year sample; dep. var.: positive tuition 1 year prior to allocation</i>							
1{Index ≥ 0}	0.04 ^{***} (0.02)	0.03 (0.02)	0.00 (0.03)		0.03 (0.08)	−0.05 (0.07)	− (0.07)
Quadratic in index	No	Yes	Yes		Yes	Yes	−
R ²	0.002	0.002	0.005		0.125	0.017	−

Table A.3 (continued)

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
N	5625	5625	2212		524	623	–
<i>Panel D: 4-year sample; dep. var.: positive tuition 4 years after allocation</i>							
1{Index ≥ 0}	0.05*** (0.02)	0.05*** (0.02)	0.02 (0.03)	0.02 (0.01) 0.96*** (0.01)	0.06** (0.02) 0.94*** (0.02)	0.01 (0.03) 0.94*** (0.02)	– – – –
Positive tuition 1 year prior to allocation				Yes	Yes	Yes	–
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	–
R ²	0.003	0.003	0.006	0.882	0.893	0.833	–
N	5625	5625	2212	2212	524	623	–

Note: The table presents regressions of an indicator for whether schools charge tuition on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective commune/homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a Because we only have tuition data up to 2006 (inclusive), for the 2-year sample the analysis in Column 6 refers only to the 2004 wave. For the 4-year sample (Panels C and D), this specification is not feasible.

Table A.4

Enrollment share among nearby schools—Santiago.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: enrollment share among nearby schools; 1 year prior to allocation</i>							
1{Index ≥ 0}	0.017** (0.008)	– 0.007 (0.010)	0.003 (0.017)		–0.028 (0.038)	0.042 (0.044)	0.000 (0.025)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.002	0.006	0.001		0.006	0.015	0.009
N	3894	3894	1739		322	233	778
<i>Panel B: 2-year sample; dep. var.: enrollment share among nearby schools; 2 years after allocation</i>							
1{Index ≥ 0}	0.021*** (0.007)	– 0.009 (0.010)	0.003 (0.016)	0.000 (0.004)	–0.014 (0.010)	–0.017 (0.013)	0.001 (0.006)
Enrollment 1 year prior to allocation				0.91*** (0.02)	0.85*** (0.04)	0.95*** (0.02)	0.92*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.003	0.011	0.000	0.909	0.849	0.935	0.908
N	3894	3894	1739	1739	322	233	778
<i>Panel C: 4-year sample; dep. var.: enrollment share among nearby schools; 1 year prior to allocation</i>							
1{Index ≥ 0}	0.011 (0.008)	– 0.011 (0.012)	0.006 (0.019)		–0.025 (0.046)	0.042 (0.044)	0.010 (0.034)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.001	0.004	0.001		0.011	0.015	0.021
N	3027	3027	1381		259	233	429
<i>Panel D: 4-year sample; dep. var.: enrollment share among nearby schools; 4 years after allocation</i>							
1{Index ≥ 0}	0.019*** (0.007)	– 0.011 (0.010)	0.005 (0.016)	0.000 (0.008)	–0.003 (0.016)	–0.048* (0.026)	0.005 (0.012)
Enrollment 1 year prior to allocation				0.83*** (0.03)	0.72*** (0.07)	0.87*** (0.06)	0.88*** (0.05)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.003	0.010	0.001	0.840	0.744	0.854	0.859
N	3027	3027	1381	1381	259	233	429

Note: The table presents regressions of the share of each school's 1st grade enrollment (among all schools that operate a 1st grade and are located within 1 km) on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective commune/homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table A.5

Tuition relative to average among nearby schools—Santiago.

	All schools subject to sample selections:				Higher SES groups only	1998 round	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)	Within 0.5 points of the cutoff (7)
<i>Panel A: 2-year sample; dep. var.: tuition relative to average among nearby schools; 1 year prior to allocation</i>							
1{Index ≥ 0}	0.023 (0.027)	— (0.035)	−0.012 (0.055)		0.122 (0.167)	0.017 (0.146)	0.007 (0.089)
Quadratic in index	No	Yes	Yes		Yes	Yes	Yes
R ²	0.000	0.002	0.006		0.042	0.009	0.014
N	3066	3066	1392		266	233	431
<i>Panel B: 2-year sample; dep. var.: tuition relative to average among nearby schools; 2 years after allocation</i>							
1{Index ≥ 0}	0.023 (0.026)	— (0.036)	−0.051 (0.053)	−0.041* (0.023)	−0.064 (0.064)	−0.071 (0.075)	−0.048* (0.028)
Tuition 1 year prior to allocation				0.86*** (0.02)	0.81*** (0.04)	0.84*** (0.06)	0.86*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.000	0.002	0.005	0.806	0.756	0.782	0.900
N	3066	3066	1392	1392	266	233	431
<i>Panel C: 4-year sample; dep. var.: tuition relative to average among nearby schools; 1 year prior to allocation</i>							
1{Index ≥ 0}	0.032 (0.031)	0.011 (0.042)	−0.006 (0.065)		0.253 (0.193)	0.017 (0.146)	— —
Quadratic in index	No	Yes	Yes		Yes	Yes	—
R ²	0.001	0.002	0.004		0.051	0.009	—
N	2250	2250	952		171	233	—
<i>Panel D: 4-year sample; dep. var.: tuition relative to average among nearby schools; 4 years after allocation</i>							
1{Index ≥ 0}	0.044*** (0.029)	— (0.041)	−0.046 (0.059)	−0.041 (0.032)	−0.084 (0.093)	−0.040 (0.081)	— —
Tuition 1 year prior to allocation				0.81*** (0.02)	0.75*** (0.04)	0.79*** (0.06)	— —
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes	—
R ²	0.002	0.003	0.003	0.752	0.725	0.716	—
N	2250	2250	952	952	171	233**	—

Note: The table presents regressions of schools' tuition (relative to the average among schools charging tuition within a 1 km radius) on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective commune/homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table A.6

9th grade enrollment among secondary schools.

	All schools subject to sample selections:				Higher SES groups only	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)
<i>Panel A: 2-year sample; dep. var.: enrollment 1 year prior to allocation</i>						
1{Index ≥ 0}	−31.5*** (5.9)	−29.2*** (6.4)	−13.1 (10.1)		6.0 (11.7)	−20.5 (13.0)
Quadratic in index	No	Yes	Yes		Yes	Yes
R ²	0.009	0.009	0.010		0.002	0.016
N	6268	6268	2518		664	1249
<i>Panel B: 2-year sample; dep. var.: enrollment 2 years after allocation</i>						
1{Index ≥ 0}	−24.7*** (5.7)	−29.1*** (6.6)	−11.7 (10.0)	0.6 (3.2)	−2.7 (3.9)	2.9 (4.2)
Enrollment 1 year prior to allocation				0.94*** (0.02)	0.95*** (0.03)	0.89*** (0.02)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.006	0.007	0.010	0.867	0.913	0.888
N	6268	6268	2518	2518	664	1249

Table A.6 (continued)

	All schools subject to sample selections:				Higher SES groups only	2004 and 2006 rounds ^a
	All (1)	All (2)	Within 0.5 points of the cutoff (3)	Within 0.5 points of the cutoff (4)	Within 0.5 points of the cutoff (5)	Within 0.5 points of the cutoff (6)
<i>Panel C: 4-year sample; dep. var.: enrollment 1 year prior to allocation</i>						
1{Index ≥ 0}	−37.0*** (6.8)	−24.5*** (7.7)	−9.7 (12.1)		−1.8 (13.4)	−16.7 (19.8)
Quadratic in index	No	Yes	Yes		Yes	Yes
R ²	0.011	0.013	0.013		0.001	0.036
N	4493	4493	1898		498	653
<i>Panel D: 4-year sample; dep. var.: enrollment 4 years after allocation</i>						
1{Index ≥ 0}	−28.6*** (6.6)	−16.3** (7.9)	−6.1 (11.8)	2.7 (4.1)	−3.2 (5.0)	4.5 (6.1)
Enrollment 1 year prior to allocation				0.90*** (0.02)	0.97*** (0.03)	0.84*** (0.03)
Quadratic in index	No	Yes	Yes	Yes	Yes	Yes
R ²	0.008	0.010	0.009	0.841	0.888	0.856
N	4493	4493	1898	1898	498	653*

Note: The table presents regressions of schools' 9th grade enrollment on a dummy for whether their SNED index was greater than or equal to the cutoff score in their respective homogeneous group. Panels A–B and C–D refer to the 2- and 4-year samples, respectively (See Section 4.4 and Table 1).

*** Denotes significance at the 1% level.

** Denotes significance at the 5% level.

* Denotes significance at the 10% level.

^a For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

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