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**Nº 239** **SCHOOL MARKETS: THE IMPACT OF INFORMATION APPROXIMATING SCHOOL EFFECTIVENESS**

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# School markets: The impact of information approximating schools' effectiveness

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**Abstract**

The impact of competition on academic outcomes 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. To explore these issues, this paper considers Chile's SNED program, which seeks to identify effective schools, selecting them from within "homogeneous groups" of arguably comparable institutions. Its results are widely disseminated, and the information it generates is quite different from that conveyed by a simple test-based ranking of schools (which in Chile, turns out to largely resemble a ranking based on socioeconomic status). We rely on 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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## I. 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. Recent research has produced clear evidence that parents are indeed willing to pay more for houses tied to schools with higher test scores (Black, 1999; Fack and Grenet, 2008), 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 values react to the placement of schools in discrete categories (e.g., "A" vs. "B"), even when the information used to assign them to these categories is public.[1] These results suggests that initiatives to distribute data on school performance, by improving parents' information sets, may allow them to act in ways that put pressure on schools to raise their value added, much as the World Bank's (2004) *World Development Report* has argued.

In recent theoretical work, however, MacLeod and Urquiola (2009) show that all the results cited above are consistent with a setting in which parents do <u>*not*</u> choose schools with the highest value added. In their model, signaling concerns lead parents to indeed prefer the schools with the best reputations, as measured by their testing achievement. However, schools' reputations depend both on their own value added and their student composition, since all else equal, higher innate ability children obtain higher test scores. As a result, parents may be willing to select a school that has relatively low value added as long as its student composition is good enough to compensate. Although the model in MacLeod and Urquiola (2009) features no peer effects, such a tradeoff also emerges in settings in which parents care about peer quality. For example, this could be because there are true peer effects (e.g. Rothstein , 2006) or because parents value access to "good" networks.

To summarize, while solid research shows that information on schools' absolute testing performance affects school markets, there is distinctly less evidence on whether parents' choices and schools' market outcomes would respond if parents were given signals measuring 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

[1] These findings are generally consistent with logit and structural estimates of parental preferences and demand responses See for instance Hastings, Kane, and Staiger (2006), Bayer, Ferreira and McMillan (2007), and Gallego and Hernando (2007).

analyze the SNED,[2] a program which since 1996 has relied mainly 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 which shares elements with approaches used elsewhere, including New Jersey and New York City. 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 percent of a monthly salary.

This setting is interesting for several reasons. First, the information the SNED generates is indeed quite 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 turns out to be close to 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 percent 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.[3] These facts allow us to use a regression discontinuity design to evaluate the effects of winning an award. In this sense, this paper broadly follows Figlio and Lucas (2004) by 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 is much more explicitly aimed at approximating value added, as opposed to absolute performance. Additionally, in this case the intervention is not purely informational, as 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 most of these are run for-profit. About 90 percent 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. We focus on these grades because they are the entry points into most schools, and thus perhaps more sensitive to changing

[2] *Sistema Nacional de Evaluación del Desempeño de los Establecimientos Educativos Subvencionados*.
[3] 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.

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. First, we address the possibility that the SNED's (more than one hundred) 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. 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 rounds to address the possibility that a "fresh" information system might have the greatest impact; then we focus on the most recent two, addressing the possibility that parents might need time to understand informational schemes, and the fact that the dissemination of SNED results has increased over time.

In short, an RD design suggest that information on school effectiveness—at least as it is delivered by the SNED—does not significantly affect schools' market outcomes. We note 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, so that parents do not know when comparisons concern schools close together in index scores. Further, 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 affect school selections based on characteristics like proximity or peer quality. Third, the SNED might not have sufficiently registered with parents, despite its decade-long existence. Whichever is the case, our findings suggest caution in reaching conclusions on the impact of information related to school effectiveness.

By way of closing this introduction, we note that much interest surrounding the SNED has focused on whether it affected testing achievement. We do not attempt such an analysis, mainly because our regression discontinuity approach is likely not suited to this purpose, as the program could affect behavior at schools that end up on either side of the cutoffs that determine selection. A related point is that our findings are not necessarily inconsistent with the SNED having had a positive impact on aggregate

achievement, although descriptive evidence suggests that average testing performance did not change discretely with its introduction.

## II. Chile's school system

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

1) *Public* or *municipal* schools are run by 341 municipalities or communes,[4] which receive a per-student subsidy from the central government. These schools cannot turn away students unless oversubscribed, and 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.[5]
3) *Private unsubsidized* schools are also independent, but receive no public funding.

In 2003, private institutions accounted for about 45 percent of all schools, and voucher schools alone for about 36 percent. In urban areas, these shares were 62 and 48 percent, respectively. All private schools can be explicitly for-profit, and Elacqua (2005) calculates that about 70 percent of them are indeed operated as such. 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,[6] and while we have no estimate on the frequency of transactions, it is not rare to see classified ads offering private schools for sale.

While initially subsidized schools were not allowed to charge tuition to supplement the voucher subsidy, this restriction was eased in 1994. At present about 50 percent of private voucher schools charge tuition, and back of the envelope calculations suggest that they raise resources equal to about 20 percent of their State funding. Public schools are allowed to charge fees only at the secondary level, although in practice few of them do.

## III. The SNED

[4] Municipalities in Chile are generally called communes, and we henceforth use this terminology.

[5] In addition, private voucher schools have greater flexibility in setting the parameters of teacher compensation. Specifically, municipal school teachers work under sector-specific legislation that protects their positions and makes their pay relatively rigid and largely dependent upon experience. In contrast, private school teachers work under the legislation covering most of the private sector, and are subject to collective bargaining at the school level.

[6] 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.

As the government liberalized the educational sector, it began exploring mechanisms to provide information that might aid parental school choice. In 1988 the SIMCE[7] testing system came into existence, and as of the mid-1990s its results had been disseminated, partially through listings of schools' performance in newspapers.[8] 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).

### A. 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 (private unsubsidized schools are not part of the scheme), and which distributed awards in 1996, 1998, 2000, 2002, 2004, 2006, and 2008.

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.[9]

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 percent 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.[10]

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 percent, respectively. Retention and promotion rates also enter into the calculation, as do

[7] *Sistema Nacional de Medición de la Calidad de la Educación*.
[8] A previous testing system, the *Programa de Evaluación del Rendimiento Escolar*, was discontinued.
[9] This measure (the JUNAEB index) is calculated at the school level to target school lunch-type subsidies.
[10] 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 drop out rates.

data from surveys that seek to measure parental involvement and working conditions. Figure 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 percent of total enrollments.[11]

### B. 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 1,000 dollars.[12] 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 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 these data are self-reported in an environment in which administrators are encouraged to make information available, they suggest that by 2003 between 80 and 90 percent of schools engaged in dissemination by way of informing their parents' association, sending notes home, or raising the issue during PTA-type meetings.

[11] In the last SNED round, 2006, this was expanded to 35 percent of schools, an issue we return to below.

[12] The payment is presently equal to about 70 percent 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 1,070 dollars by 2000 and 2006, respectively. These figures are averages. In practice, a global allocation is made to each school, and 90 percent 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 percent of enrollments receive the award—schools accounting for the first 25 percent receive a full bonus, while the rest receive only 60 percent 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.

Second, since 2002 (i.e., for the last three rounds) a Ministry of Education website has allowed parents to determine multiple schools' SNED status. Figure 2 shows an image of this site, which after parents select a given commune, returns a listing of all the schools within it, and indicates whether they won an award.

Third, since 2004 (for the three most recent rounds), some communes have intervened in the process by helping winning schools to advertise their status in a uniform way. For instance, Figures 3a and 3b present pictures of schools in the communes of *Santiago* and *Providencia*, respectively.[13] These pictures display standardized banners these municipalities posted at winning schools during the 2006-2007 round. The images show close-ups of the banners and illustrate their placement in visible places.

Further, information regarding the SNED is not released into a vacuum, since the 1990s afforded the public with exposure to school performance data. Figure 4 illustrates this by showing the front page of a newspaper announcing the edition contains the scores of all schools in a given region. Further, Figure 5 contains the headline and initial text of a story on the SNED itself, including a synopsis of its methodology.

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 about 80 percent 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 percent 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 percent of schools would receive neither, and 8 percent both; 29 percent would receive one but not the other.

Further, each SNED round in some sense produces new information. Among about 8,300 schools in the five rounds we focus on, 50 percent of schools either never or always received an award (48 and 2 percent, respectively). 22 percent were selected once, and 15, 10, and 4 percent won awards 2, 3, and 4 times, respectively.[14]

[13] By population, *Santiago* is the largest commune in Chile. Both *Santiago* and *Providencia* are in the *Santiago* metropolitan area, which contains about 50 communes.

[14] The following table shows the distribution of schools according to the number of awards these received:

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.

We nevertheless mention, as a descriptive fact, that winning an award is positively and significantly correlated with subsequent 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.

## IV. Empirical strategy

The manner in which the SNED is assigned makes it feasible to evaluate its effect using a regression discontinuity (henceforth, RD) design. Figure 6 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 so that we can 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:

| Total number of awards | All six rounds | Five rounds considered below |
|---|---|---|
| 0 | 48 | 48 |
| 1 | 22.7 | 21.8 |
| 2 | 13.6 | 14.9 |
| 3 | 8.2 | 8.7 |
| 4 | 4.4 | 4.5 |
| 5 | 2.3 | 2.1 |
| 6 | 0.8 | -- |
| Total | 100 | 100 |

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

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

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 homogenous group. If $f$ and $g$ are continuous at $cutoff_{gt}$ and $E(SNED_{igt}|index_{igt})$ is discontinuous (Figure 6), then $\beta$ 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 we have a sharp rather than a fuzzy design, and instead of implementing (1)-(2), use a reduced form approach:

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

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 (3) within arbitrarily narrow bands close to the cutoff scores.

**A. What can an RD design identify in this context?**

While Figure 6 (Panel A) shows that the SNED produces a "clean" RD, it is relevant to ask what this can identify in our context. As detailed in Section III, the SNED treatment has two components: teachers in winning 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, Flores, and Lobato, 2003, Contreras and Rau, 2008, 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. Schools' award status of course does vary discretely 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. Further, to the extent that bonus payments render winning schools more desirable, they provide a further reason to expect positive estimates from reduced form estimations like (3).

### B. Outcomes, hypotheses, and data

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.[15]
2) *Number of 1st and 9th grade classes operated*. Urquiola and Verhoogen (forthcoming) 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 percent 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 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.[16] For more direct measures, we

[15] 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.
[16] 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

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.

### C. 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 present other 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 basic 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 area of *Santiago*, the largest in Chile, 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.[17]

Figure 7 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. Figure 7 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 homogenous groups, the combination with roughly 314 communes results in 1,784 commune/homogenous groups.[18] As Figure 7 suggests, the latter continue to produce a "clean" first stage (Figure 6, 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 percent of the variation in income at the 4th grade level, and 72 percent

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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.

[17] The 2002 SIMCE data suggest that about 89 percent 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 percent. As elsewhere, it is also more common for children in secondary school to travel further to school.

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

of the variation in average mothers' schooling. Using commune/homogeneous group dummies, these numbers increase to 68 and 81 percent, respectively. The intuition is also illustrated in Figure 7, 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 Figure 7).

**D. Samples and analyses 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 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 award 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 only have 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.[19]

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 in general the fact that a school wins an award one year increases the probability that it will do so again.[20] This means that while schools that just make and just miss an award may

[19] 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.

[20] 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).

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.

## V. Results

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

### A. 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 IV.D), 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 Figure 6), 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.[21] 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.[22]

Finally, columns 5 and 6 focus on subsamples of SNED rounds. First, column 5 considers only the first (1998) round,[23] addressing the fact that a "fresh" information program might have the greatest impact—after several implementations, parents might consider schools' SNED history rather than their performance on the last available round, and each individual iteration might therefore generate less "news". Second, Column 6 considers the last two (2004 and 2006) waves we consider, 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

[21] 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.

[22] That is, we use only homogeneous groups which the SNED methodology itself catalogued as higher SES. We also did similar exercises for low SES groups, reaching conclusions similar to those discussed below; we omit these for the sake of space.

[23] As stated above, there is an earlier 1996 round we do not have access to.

amount of "slippage" in the allocation took place in 1998. Panel B describes analogous results for the 4-year sample (Section IV.D), which requires excluding the 2006 wave, thereby reducing the initial (column 1) sample from 12,476 to 9,639 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 percent more likely to win another award two years later.[24] 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.

**B. 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. Figure 8a (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 this difference was equal to about eight 1st grade students, equivalent to about 20 percent of a standard deviation.[25] 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 Figure 8a, Panel A. In short, as required by the RD design, enrollment prior to each allocation appears to be smoothly related to the index in the vicinity of the cutoffs.[26]

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

[24] This is to be expected in 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.

[25] 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.

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

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.[27] The point estimate is now -0.2, and a 95 percent 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 Figure 8a (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.

**C. Number of classes in operation**

We now ask whether award winners were 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 Figure 8 present the graphical evidence, which not surprisingly is quite similar to that for enrollment in panels A and B.

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 schools that received an award were more likely to grow.

**D. Tuition**

[27] 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.

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 percent of voucher schools charged add-on tuition, and the average was about 3 monthly dollars. By 2006, these figures had increased to 50 percent and 14 dollars, respectively.

We first consider whether schools charge tuition at all,[28] 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 easily observed. Figure 8a presents the graphical evidence, which is suggestive of no effect—there is no break in the relationship one year prior to the allocation (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 on average 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 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 two percent.

The evidence on absolute tuition (panels A and B of Figure 8b, 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 (about 500 more pesos per month, equivalent to roughly a tenth of a standard deviation). This is expected, if only given the positive correlation between the SNED index and socioeconomic status (Figure 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.

### E. Socioeconomic composition

[28] 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).

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 Figure 8b (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 (visually, there is no break at 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.[29] 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.[30]

With these caveats, Figure 8b (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, 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 percent of urban parents do not return the questionnaires that are their source.

### E. Robustness checks

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

[30] 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.

The estimates presented thus far rely on the SNED-constructed homogeneous groups. As a robustness check (described in Section IV), we carry out similar exercises comparing schools that are in the same commune in addition to being in the same homogenous group. Appendix tables A.1-A.4 (Panel A) implement this exercise, although for the sake of space we limit the discussion and focus on only three outcomes: enrollment, whether schools charge tuition, and vulnerability.[31] 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 significantly positive and negative effects, although there is not consistent evidence in either direction. Second, even more than among homogenous groups, there is persistence in SNED awards.

Finally, Tables A.1 (Panel B) and A.5 consider the effects of the SNED at the secondary level, looking only at enrollment (in this case at the 9$^{th}$ 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). 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.

## VI. Conclusion

A longstanding debate in the economics of education concerns to what extent improving educational quality can be achieved simply by relying on competition, and to what extent it necessitates intervention through initiatives ranging from accountability (e.g., *No child left behind*) to raising expenditure.

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

[31] More specifically, Table A.1 (Panel A) presents the first stage. Tables A.2, A.3, and A.4 present the enrollment, tuition, and vulnerability results, respectively. We omitted the remaining outcomes because they produce similar conclusions.

to obtain better peer groups or reduce travel time, [32] then competition might encourage schools to improve on dimensions other than academic value added.[33]

Over the past decade the literature has produced evidence that parental school choice is indeed influenced by information on school quality, at least as measured by test scores.[34] Yet this does not fully answer the question at hand because, going back to the Coleman (1966) report, it is clear that test scores conflate school effectiveness and peer quality. In the specific case of Chile, in fact, a ranking of schools based on their test scores is quite close to one based on their socioeconomic status.

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 it 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 effectiveness in a manner the public might understand.[35] Perhaps surprisingly, we fail to find systematic evidence that SNED awards impact schools' market outcomes.

It is important, however, that these results are 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, 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 SNED awards are different ex-post, a further reason to expect a positive effect.

Second, while parents might value school effectiveness, information on it might in the end not sway school choices based on characteristics like peer composition. This would rationalize stronger reactions to data on average performance (found in the recent literature) than to information approximating effectiveness. It would also be consistent with theoretical work by Rothstein (2006) and MacLeod and Urquiola (2009), suggesting that parents might be less sensitive to data on value added than to information on absolute achievement. These issues might be particularly relevant in Chile, where school choice has given schools ample opportunities to differentiate.

---

[32] This is relevant because some research suggests that parents care about peer quality per se, e.g., Henig (1990), Schneider and Buckley (2002), Bayer and McMillan (2005), Urquiola (2005), Rothstein (2006), Card, Mas, and Rothstein (2007), and Gallego and Hernando (2007).

[33] This is relevant because some research raises the possibility that parents are either uninformed about school quality, or else select schools using other criteria. See for instance Henig (1994), Ascher et al. (1996), Armour and Peiser (1998), Kleitz et al. (2000), Lubienski (2003), and Elacqua and Fabrega (2004).

[34] See also Bogart and Cromwell (1997), Brunner et al. (2001), and Downes and Zabel (2002).

[35] 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.

Another set of potential explanations relates to how households might or might not view the SNED. First, the scheme might not have registered with parents, despite the fact that it is eleven years old and there are many ways for households to find out about it. 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 that goes into the SNED. Further, the SNED results 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. The bottom line is that interpreting information approximating school effectiveness is likely to be difficult, even if households pay close attention.

Finally, a lack of clear evidence of an impact might also arise if schools respond to awards in ways that precisely counteract demand responses (for instance, by becoming more selective as demand increases), although one would expect this to show up in our measures of socioeconomic composition or tuition, and there is no clear evidence of that.

We also note that our results are not necessarily inconsistent with the SNED having improved aggregate test scores in Chile. For example, schools might have all modified their behavior and become more productive in the expectation that the program would render parents better informed, or in the hope of winning the bonus payments. 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. There is evidence on the evolution of equated performance on Chile's national test, however, that would seem to suggest the SNED induced no discrete change in performance,[36] but of course this is just a descriptive result.

[36] The Table below presents equated SIMCE scores, and suggests there was no discrete change in SIMCE performance after 1996/1998, particularly in the 4th grade, the one that might show the earliest effect.

| Test | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 4th grade Math | | | 248 | | | 250 | | | 247 | | | 248 | 248 |
| 4th grade Language | | | 251 | | | 250 | | | 251 | | | 255 | 253 |
| 8th grade Math | | | | 250 | | | 250 | | | | 253 | | |
| 8th grade Language | | | | 253 | | | 250 | | | | 251 | | |
| 10th grade Math | 242 | | | | 250 | | | 248 | | 246 | | | 252 |
| 10th grade Language | 249 | | | | 250 | | | 252 | | 253 | | | 254 |

Source, SIMCE, Ministry of Education

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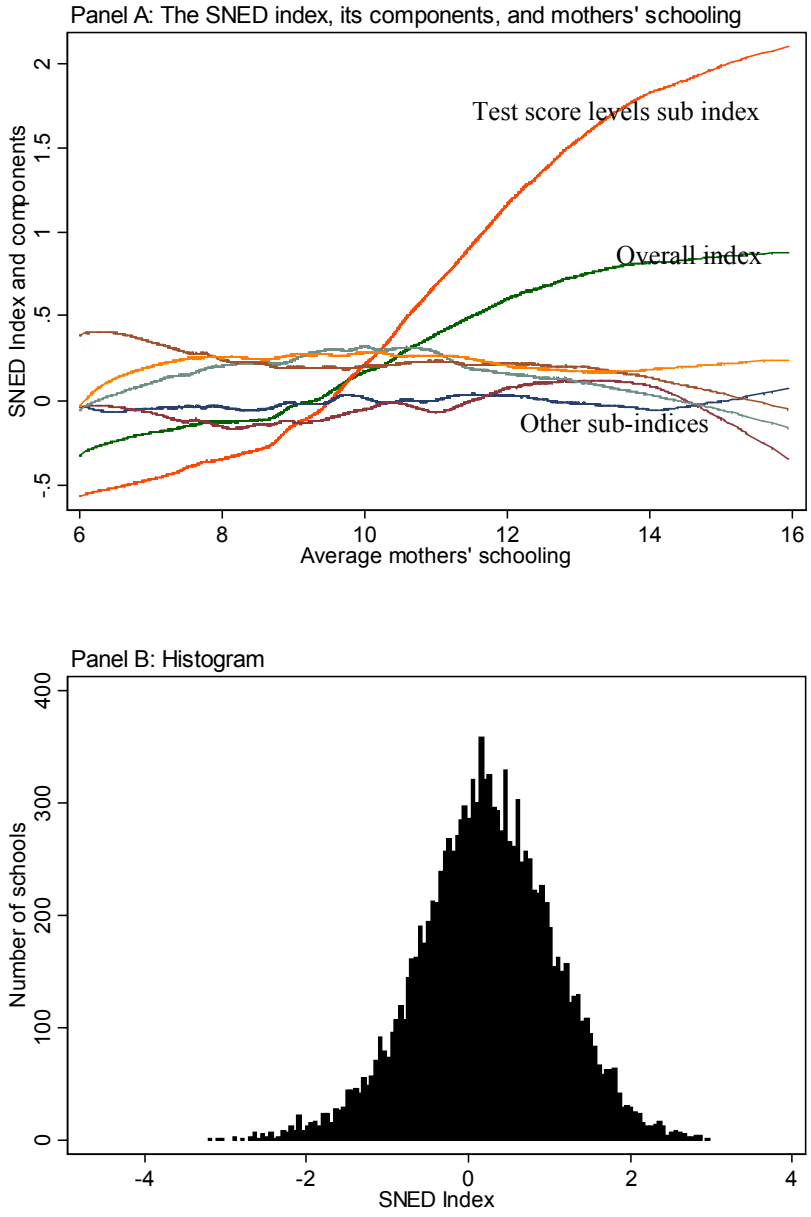
Figure 1: The SNED index among primary schools




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 IV.D and Table 1, panel B).

Figure 2: Ministry of Education website output, 2007

COMUNIDAD ESCOLAR
Información a tu Alcance

HOME

RESULTADOS DEL SISTEMA NACIONAL DE EVALUACION DEL DESEMPEÑO DE LOS ESTABLECIMIENTOS EDUCACIONALES SUBVENCIONADOS (SNED 2006-2007)

Usted está en: Resultados SNED 2006-2007 | Detalle por Comuna

VOLVER AL INICIO DE SUS CONSULTAS

IMPRIMA AQUI

Detalle por Comuna

COLEGIOS DE LA COMUNA

LO PRADO

| RBD | NOMBRE DEL ESTABLECIMIENTO | LETRA | Nro. | SELECCIONADO SNED |
|---|---|---|---|---|
| 10140 | FRINGS COLLEGE | * | 672 | SI |
| 10171 | ESCUELA VILLA LOURDES | * | 1025 | SI |
| 10156 | ESCUELA BASICA JARDIN LO PRADO | * | 1356 | SI |
| 11835 | ROYAL SCHOOL | * | 1261 | SI |
| 10147 | ESCUELA BASICA BABILONIA | * | 1159 | SI |
| 10149 | RAPA NUI | * | 371 | NO |
| 10160 | BIENVENIDO AUBA UGALDE | * | 910 | NO |
| 10142 | DOCTORTA VICTORIA GARCIA | * | 811 | NO |
| 10103 | ESCUELA BASICA SOR TERESA DE LOS ANDES | D | 414 | NO |
| 10091 | ESCUELA BASICA PROFESORA GLADYS VALENZUE | D | 403 | NO |
| 25744 | ESCUELA DE PARVULOS HEYDDIE II | * | 1942 | NO |
| 25838 | ESCUELA DE PARVULOS MY GARDEN SCHOOL | * | 1991 | NO |
| 25800 | ESCUELA DE PARV PARTI ARCA DE NOE DE LO | * | 1969 | NO |
| 10010 | ESCUELA BASICA CIUDAD DE CARACAS | F | 293 | SI |
| 10097 | ESCUELA BASICA MARISCAL DE AYACUCHO | D | 409 | SI |
| 10119 | ESCUELA BASICA POETA VICENTE HUIDOBRO | E | 420 | SI |
| 10106 | ESCUELA BASICA POETA PABLO NERUDA | D | 417 | SI |
| 10095 | ESCUELA BASICA IGNACIO CARRERA PINTO | D | 408 | NO |
| 10118 | ESCUELA BASICA ATATURK | E | 419 | NO |
| 10092 | ESCUELA BASICA GOLDA MEIER | D | 404 | NO |
| 10089 | ESCUELA BASICA JAIME E. GOMEZ GARCIA | D | 400 | NO |
| 10114 | ESCUELA BASICA ESTADOS UNIDOS | E | 389 | NO |
| 10128 | ESCUELA BASICA DE ADULTOS | F | 425 | NO |
| 10176 | ESCUELA BASICA RENACER | * | 934 | NO |
| 24730 | COLEGIO ADVENTISTA SANTIAGO PONIENTE | * | 0 | NO |
| 10162 | TENIENTE DAGOBERTO GODOY N.3 | * | 0 | SI |
| 10076 | COMPLEJO EDUCACIONAL PEDRO PRADO | A | 86 | NO |
| 10127 | ESCUELA DIFERENCIAL QUILLAHUA | F | 423 | SI |
| 25433 | ESCUELA ESPECIAL RUCAMALEN | * | 1799 | NO |
| 25839 | ESC ESP PART CENTRO DE ESTUDIOS DEL LENG | * | 1988 | NO |

VOLVER AL INICIO DE SUS CONSULTAS

Note: The figure shows an image of a Ministry of Education website which allows parents to type in a given commune (*Lo Prado*, in this example). The site returns a listing of all the schools in the commune, including unique administrative identifiers (Column 1), their name (Column 2), and their SNED status (Column 6, which labels selected schools with a "SI" and non-selected schools with a "NO").

Figure 3a: Pictures of SNED winners in the municipality of *Santiago*, 2006

Panel A: *República de Mexico* school


Panel B: *República de Mexico* school

Panel C: *Instituto Nacional* high school

Panel D: *Barros Arana* boarding school


Panel E: *Barros Borgoño* high school

Note: The figure displays pictures of banners announcing the featured schools' selection in the 2006 SNED round. These banners were posted at winning schools by the government of the Commune of *Santiago*.

Figure 3b: Pictures of SNED winners in the municipality of *Providencia*, 2006

Panel A: *Juan Pablo Duarte* school


Panel B: *Juan Pablo Duarte* school

Panel C: *Tajamar* high school

Panel D: *Lastarria* school


Note: The figure displays pictures of banners announcing the featured schools' selection in the 2006 SNED round. These banners were posted at winning schools by the government of the Commune of *Providencia*.

Figure 4: Newspaper headlines and articles on SIMCE performance, 2000

"La Segunda" te invita al cine a precios rebajados
CUPONES EN PAGINA 46

Segunda

EXCLUSIVO En la Región Metropolitana

# RESULTADOS DEL SIMCE COLEGIO POR COLEGIO

*La Segunda entrega a sus lectores el listado oficial del Ministerio de Educación con los resultados para las comunas de la Región Metropolitana del Sistema de Medición de la Calidad de la Educación aplicado a los alumnos de Octavo Básico el 15 y 16 de noviembre del 2000. Junto al nombre del establecimiento aparece su dependencia (municipal, subvencionado, particular) la cantidad de alumnos que rindió la prueba y los puntajes obtenidos en Matemática, Castellano, Historia y Ciencias Naturales.*

| Establecimiento | Dpcia | N° Alum | PUNTAJES PRUEBA 2000 MAT | CAST | HIST | C.NAT |
|---|---|---|---|---|---|---|
| **ALHUÉ** | | | | | | |
| Liceo Municipal Sara Troncoso | MD | 67 | 243 | 237 | 253 | 253 |
| Escuela G 121 | MD | 12 | 210 | 238 | 241 | 233 |
| Escuela G 123 | MD | 8 | 225 | 226 | 250 | 244 |
| **BUIN** | | | | | | |
| Liceo A 131 | MC | 297 | 256 | 265 | 263 | 266 |
| Escuela Rayen Mapu D 807 | MC | 63 | 220 | 226 | 228 | 236 |
| Colegio De Maipo E 803 | MC | 76 | 224 | 238 | 235 | 251 |
| Escuela Alto Jahuel E 811 | MC | 103 | 238 | 247 | 231 | 243 |
| Liceo Los Guindos Polivalente E 814 | MC | 23 | 220 | 218 | 223 | 231 |
| Escuela Villaseca F 802 | MC | 28 | 212 | 207 | 232 | 254 |
| Escuela Fco. Javier Kruger A. F 805 | MC | 56 | 227 | 230 | 230 | 239 |
| Escuela Valdivia De Paine F 806 | MC | 56 | 239 | 229 | 235 | 240 |
| Escuela Lo Salinas F 809 | MC | 21 | 237 | 252 | 240 | 263 |
| Escuela Humberto Moreno Ramirez F 810 | MC | 44 | 206 | 218 | 221 | 215 |
| Escuela Los Rosales Del Bajo F 816 | MC | 31 | 215 | 222 | 228 | 232 |
| Escuela Viluco F 817 | MC | 36 | 225 | 227 | 231 | 237 |
| Escuela Andres Puig Ferrer G 808 | MC | 15 | 261 | 256 | 253 | 251 |
| Escuela Los Aromos De El Recurso G 813 | MC | 19 | 209 | 216 | 229 | 231 |
| Liceo Poliv. Moderno Cardenal Caro | PSC | 179 | 243 | 251 | 250 | 256 |
| Saint Mary College 60 | PS | 17 | 224 | 224 | 222 | 236 |
| Colegio Buin | PP | 10 | 277 | 275 | 290 | 289 |
| Colegio San Isidro | PP | 35 | 310 | 309 | 320 | 321 |
| Colegio Particular Saint James School | PP | 27 | 313 | 311 | 293 | 297 |
| **CALERA DE TANGO** | | | | | | |
| Colegio Poetisa Gabriela Mistral 796 | MC | 62 | 213 | 208 | 228 | 227 |
| Colegio Calera De Tango F 798 | MC | 54 | 218 | 209 | 223 | 213 |
| Escuela Lonquen F 800 | MC | 35 | 240 | 235 | 241 | 241 |
| Colegio San Ignacio 8 | PS | 37 | 260 | 267 | 277 | 279 |
| Esc. Bas. Past. San Felipe Diacono 92 | PP | 26 | 281 | 281 | 309 | 291 |
| **CERRILLOS** | | | | | | |
| Escuela Pedro Aguirre C. D 33 | MD | 84 | 220 | 219 | 224 | 212 |
| Escuela Part. Amor De Dios 281 | PS | 43 | 250 | 247 | 237 | 252 |
| Liceo Dr Luis Vargas Saicedo B 69 | MD | 67 | 226 | 226 | 230 | 234 |
| Escuela Condores De Plata D 259 | MD | 58 | 216 | 216 | 217 | 223 |
| Escuela Lothar Kommer B. D 262 | MD | 83 | 207 | 222 | 220 | 216 |
| Escuela D 271 | MD | 100 | 219 | 229 | 230 | 226 |
| Escuela Jose J. Prieto V. D 279 | MD | 111 | 229 | 238 | 237 | 241 |
| Colegio Estrella Reina de Chile E 267 | MD | 53 | 232 | 223 | 220 | 225 |
| Escuela Santa Adela E 269 | MD | 39 | 241 | 246 | 250 | 269 |
| Chilean Eagles College 2 | PSC | 117 | 241 | 243 | 253 | 249 |
| Colegio Los Conquistadores | PSC | 119 | 269 | 273 | 264 | 260 |
| Colegio Mater Dei | PSC | 70 | 275 | 273 | 268 | 265 |
| Escuela Industrial y Básica Don Orione | PSC | 130 | 267 | 271 | 272 | 269 |
| Saint Rodric College 963 | PS | 64 | 253 | 262 | 258 | 261 |
| Saint Maurine's 824 | PSC | 33 | 266 | 270 | 264 | 278 |

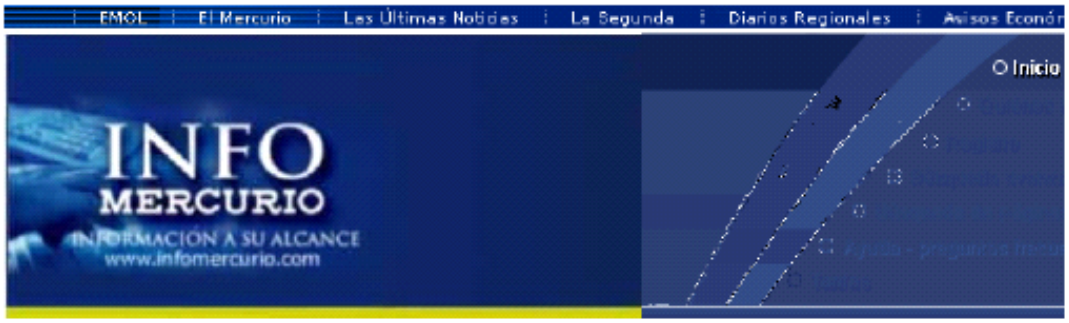


| Establecimiento | Dpcia | N° Alum | PUNTAJES PRUEBA 2000 MAT | CAST | HIST | C.NAT |
|---|---|---|---|---|---|---|
| Escuela Bás. Particular Carlos Pezoa Véliz 387 | PS | 33 | 217 | 224 | 237 | 231 |
| Escuela Básica Particular N 1095 La Frontera | PSC | 42 | 238 | 238 | 223 | 220 |
| Colegio Polivalente Saint Orland 3 | PSC | 90 | 236 | 241 | 241 | 238 |
| Escuela Basica Anexo La Frontera 1095 | PSC | 40 | 239 | 251 | 229 | 250 |
| **COLINA** | | | | | | |
| Escuela Bas.Marcos Goycoolea Cortes D 360 | MC | 95 | 235 | 225 | 227 | 221 |
| Sta. Teresa Del Carmelo E 357 | MC | 69 | 238 | 239 | 249 | 243 |

| Establecimiento | Dpcia | N° Alum | PUNTAJES PRUEBA 2000 MAT | CAST | HIST | C.NAT |
|---|---|---|---|---|---|---|
| Colegio Cristobal Colon | PSC | 42 | 257 | 259 | 260 | 267 |
| Liceo Avenida Principal 918 | PSC | 103 | 232 | 229 | 222 | 224 |
| Colegio Libertador San Martin | PSC | 91 | 236 | 240 | 238 | 253 |
| Escuela El Carmen 270 | PSC | 129 | 257 | 254 | 258 | 249 |
| Colegio Patrocinio El Sembrador #2 1231 | PS | 28 | 244 | 242 | 244 | 265 |
| Escuela San Diego 402 | PSC | 114 | 275 | 280 | 271 | 270 |
| Hellen's College 580 | PSC | 74 | 257 | 260 | 237 | 260 |
| Colegio Ines De Suarez 815 | PS | 37 | 253 | 254 | 253 | 262 |

Note: The figure presents an image from *La Segunda*, a national newspaper. The headline reads: "Exclusive in the Metropolitan Region—SIMCE results school by school." The tables then report, by commune, each school's name, its type (municipal, voucher private, etc.), its enrollment, and its test scores in Math, Language, History, and Natural Sciences.

Figure 5: Newspaper article on the SNED allocation, calculation, and teacher bonuses

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EN COLEGIOS DE SANTIAGO:

## DESTACAN SISTEMA PARA EVALUAR EL DESEMPEÑO

**Premio a la Excelencia consulta un bono trimestral por jornadas de 30 y 44 horas de trabajo con promedios de 3[illegible] mil y de 55 mil a 64 mil pesos, según la extensión de la labor.**

---

Un grupo de maestros de colegios de la Región Metropolitana recibieron ayer los diplomas que acreditan a esos establecimientos con el Premio a la Excelencia, inserto en el Sistema Nacional de Evaluación de Desempeño (SNED), que distingue cada dos meses la labor de colegios para mejorar su calidad y nivel del quehacer académico.

El Secretario Regional Ministerial de Educación, Antonio Pacheco dijo que la iniciativa se suma a los puntos básicos de la reforma educacional que propicia el Gobierno, especialmente en favor de los sectores de más bajos ingresos.

El SNED integra ahora el premio con incentivos remuneracionales a los profesores de colegios que exhiban el mejor desempeño en todo el país, además de recibir cada establecimiento, a futuro, fondos especiales para sus propios programas educacionales.

Para ello, dijo, el Ministerio de Educación y las secretarías regionales respectivas asignarán a cada plantel un puntaje especial, según la efectividad de sus servicios, su propia superación, nuevas iniciativas, así como integración y participación del magisterio en sus proyectos; todo ello, sobre la base del nivel socioeconómico del colegio y su condición de urbano o rural, entre otros aspectos.

En cuanto a la aplicación del sistema en la Región Metropolitana, cada profesor favorecido por el buen rendimiento de los planteles respectivos ha recibido un bono de excelencia, en forma trimestral, con un promedio de $ 35.000 a $ 44.000 por jornada de 30 horas y de $ 55.000 a $ 64.000 por jornada de 44 horas.

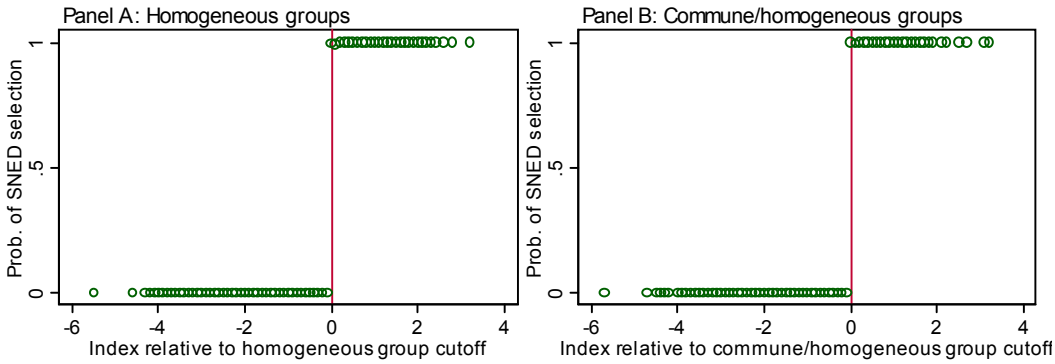
En la oportunidad, el Seremi resaltó que el récord de mayor bono en la región fue el liceo politécnico Sagrado Corazón, de Lo Espejo, en donde un maestro recibió trimestralmente casi 95.000 pesos, logrando 380.000 pesos en total durante el año pasado.

Respecto a las principales condiciones para aplicar el nuevo sistema, el Seremi metropolitano de Educación destacó, entre otras, la capacidad del colegio para incorporar innovaciones educativas y fijar condiciones académicas con mayor participación docente, así como una plena participación de padres y apoderados.

En tal sentido, añadió Pacheco, se ordenará la prelación de los planteles con indicaciones de matrículas de mayor a menor puntaje, para entregarles la

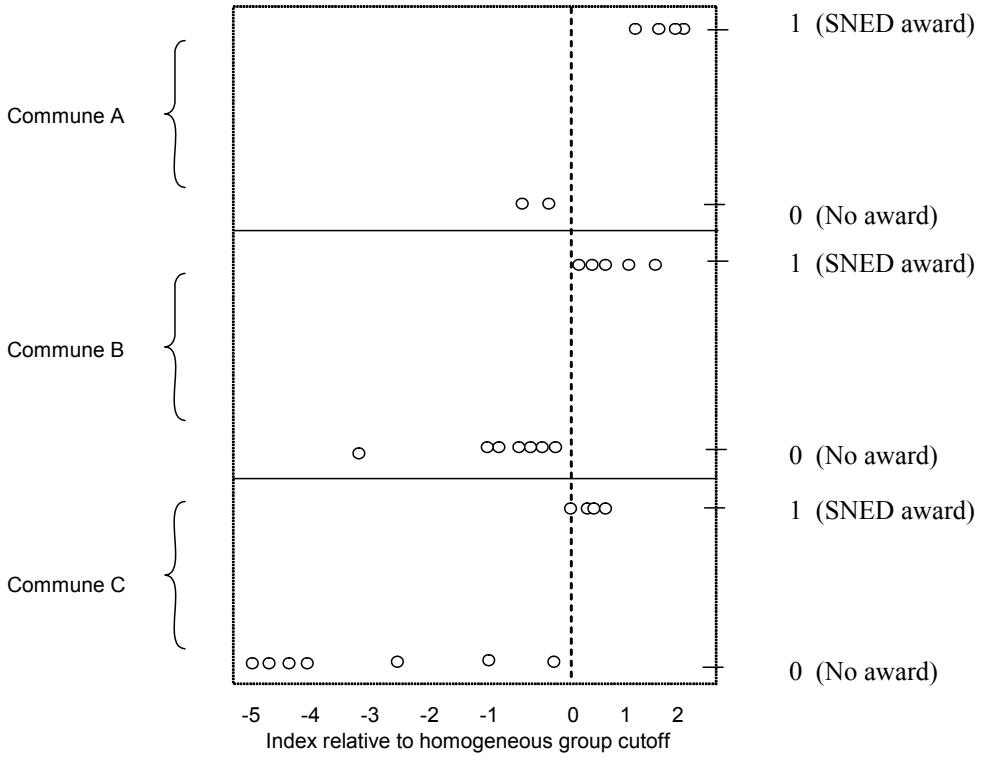
Note: The figure presents an online version of a story in *El Mercurio*, a national newspaper. It describes the implementation of the SNED in the metropolitan region, and notes the fact that teachers can receive non-negligible bonuses as a result of the scheme. It highlights the fact that one specific teacher received an annual bonus of about 600 dollars.

Figure 6: 1st stage



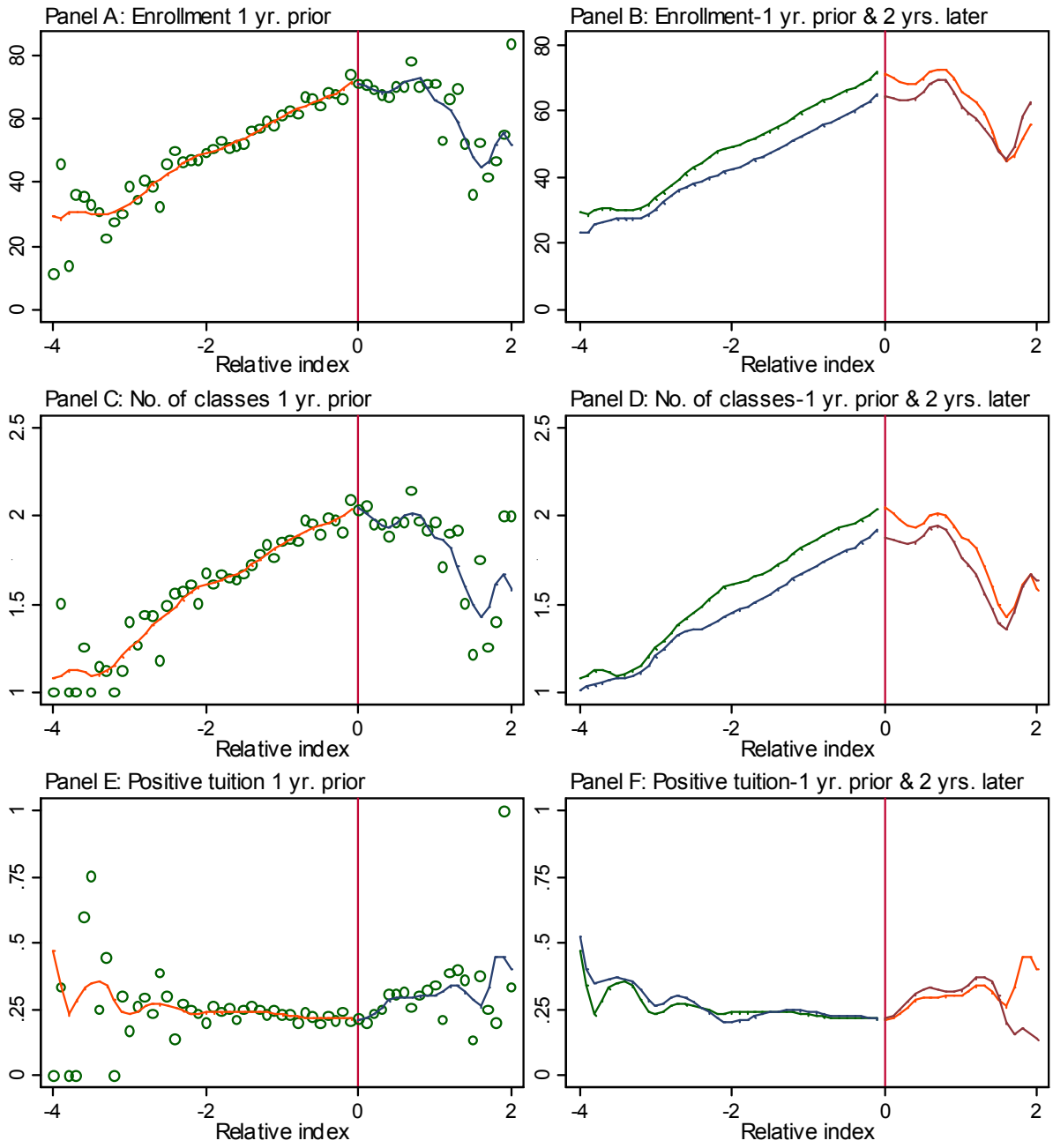

Note: Panel A plots index-cell means of a dummy indicating whether schools obtained a SNED award. Each school's index is expressed relative to the normalized cutoff (the vertical line) in its respective homogeneous group. For visual clarity index values are in 0.1 wide bins (Imbens and Lemieux, 2007). 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 9,857 observations—the 2-year homogeneous groups and commune/homogenous group samples, respectively (see Section IV.D and Table 1, Panel B).

Figure 7: Division of a homogenous group into three commune/homogeneous groups



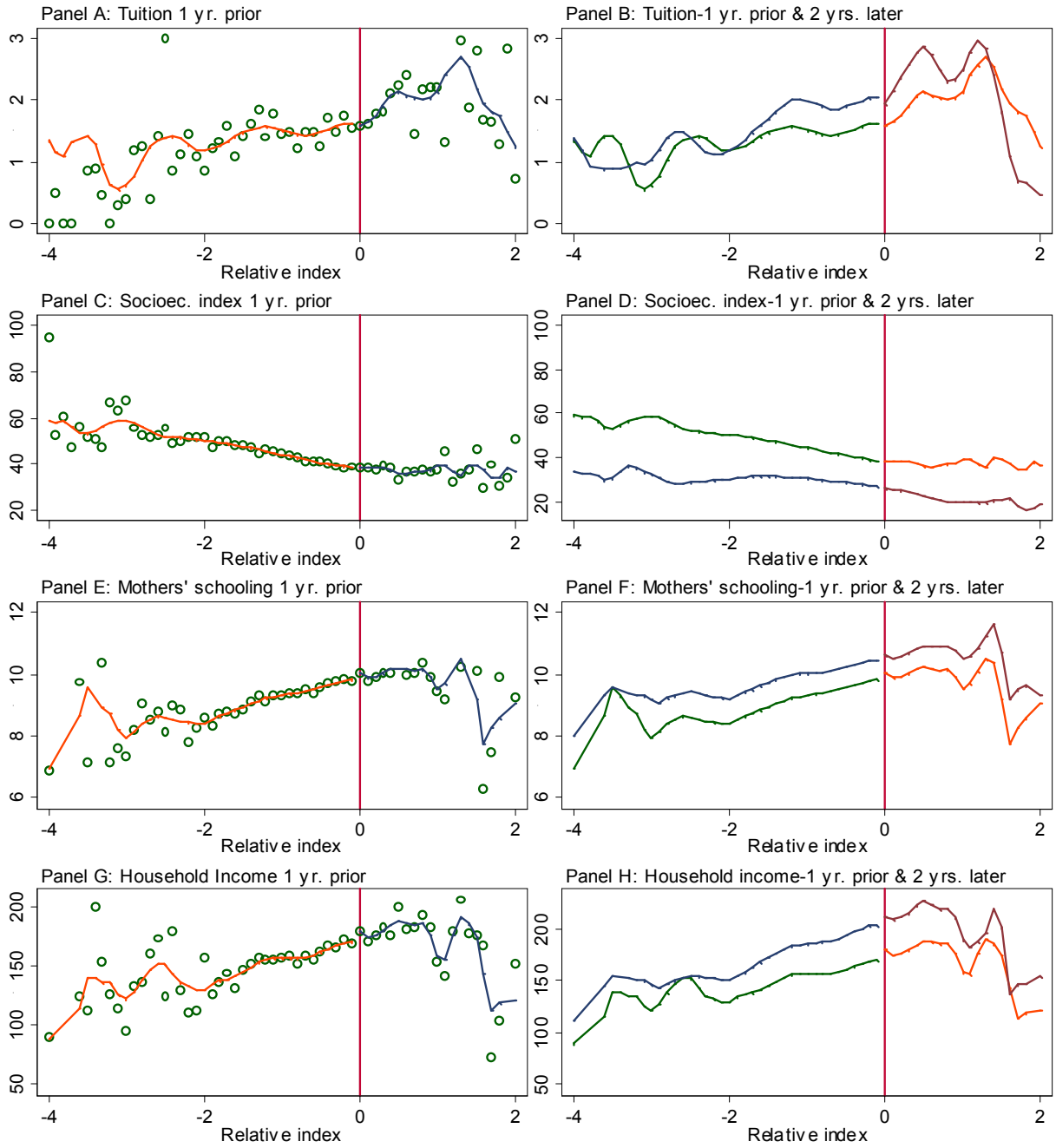

Note: The figure illustrates the construction of commune/homogeneous groups. In this hypothetical case, a homogeneous group contains three communes (communes are fully contained in regions, the basic geographical unit used in the construction of homogeneous groups). The homogenous group-wide cutoff score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

Figure 8a: 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 IV.D and Table 1, Panel B).

Figure 8b: 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 IV.D and Table 1, Panel B).

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

| Sample and type of group | SNED wave | | | | | Stacked data |
|---|---|---|---|---|---|---|
| | 1998 (1) | 2000 (2) | 2002 (3) | 2004 (4) | 2006 (5) | (6) |
| Panel A: Full sample | | | | | | |
| Homogeneous groups | 114 | 104 | 109 | 110 | 111 | -- |
| Total number of schools | 9,060 | 9,247 | 9,465 | 9,681 | 9,769 | -- |
| Avg. no. of schools per group | 79.5 | 88.9 | 86.8 | 88.0 | 88.0 | -- |
| Commune/homogeneous groups | 1,784 | 1,584 | 1,669 | 1,721 | 1,764 | -- |
| Total number of schools | 8,355 | 8,380 | 8,496 | 8,536 | 8,749 | -- |
| 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 | 2,422 | 2,460 | 2,527 | 2,484 | 2,583 | 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 | 1,195 |
| Total number of schools | 1,892 | 1,893 | 2,015 | 1,964 | 2,093 | 9,857 |
| 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 | 2,378 | 2,378 | 2,461 | 2,422 | -- | 9,639 |
| 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 | 1,847 | 1,819 | 1,959 | 1,899 | -- | 7,524 |
| 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 | 1,026 | 1,081 | 1,170 | 1,314 | 1,677 | 6,268 |
| 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 | 1,008 | 1,041 | 1,153 | 1,295 | -- | 4,497 |
| 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 IV.D.

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 percent of the total Equality of Opportunity component); measured using administrative data<br>- 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 percent of the total Equality of Opportunity component); measured via a questionnaire administered to Ministry of Education inspectors.<br>- 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 percent 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<br>- 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).

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.

Table 4: Dependent variable—schools 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 *** | 10.32 *** | 7.22 *** | 1.50 ** | 29.96 *** | 21.69 *** | 4.35 *** | 2.65 ** |
| | (0.63) | (0.62) | (0.61) | (0.62) | (1.74) | (1.70) | (1.21) | (1.18) |
| Mothers' schooling in 2002/2001 | | 3.80 *** | 2.01 *** | 1.40 *** | | 9.06 *** | 1.65 *** | 1.39 *** |
| | | (0.32) | (0.31) | (0.29) | | (0.87) | (0.61) | (0.59) |
| Household income in 2002/2001 | | 0.03 ** | 0.03 *** | 0.02 *** | | 0.03 ** | -0.01 | -0.01 |
| | | (0.01) | (0.01) | (0.01) | | (0.01) | (0.01) | (0.01) |
| Math test score in 2002/2001 | | | 0.29 *** | 0.21 *** | | | 1.05 *** | 0.89 *** |
| | | | (0.01) | (0.01) | | | (0.03) | (0.04) |
| Math test score in 2000/1998 | | | | 0.38 *** | | | | 0.27 *** |
| | | | | (0.02) | | | | (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 | 4,108 | 4,108 | 4,108 | 4,108 | 1,137 | 1,137 | 1,137 | 1,137 |

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 $4^{th}$ and $8^{th}$ grade, respectively, and those for 2001 and 1998 are at the $10^{th}$ grade level.

Table 5: First stage regressions

| | All schools subject to sample selections: | | | Higher SES groups only | 1998 round | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|
| | All | All | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: 2-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 0.998 *** | 0.997 *** | 0.995 *** | 0.985 *** | 0.970 *** | 1.000 *** |
| | (0.001) | (0.001) | (0.002) | (0.007) | (0.013) | (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 | 5,750 | 1,255 | 844 | 2,566 |
| **Panel B: 4-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 0.997 *** | 0.996 *** | 0.994 *** | 0.981 *** | 0.969 *** | 1.000 *** |
| | (0.001) | (0.002) | (0.003) | (0.009) | (0.013) | (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 | 9,639 | 9,639 | 4,494 | 994 | 836 | 1,375 |
| **Panel C: 4-year sample; Dep. var.: Dummy for receiving an award 2 years after the baseline** | | | | | | |
| 1{Index≥0} | 0.347 *** | 0.165 *** | 0.071 ** | 0.003 | 0.053 | 0.131 ** |
| | (0.012) | (0.018) | (0.027) | (0.057) | (0.066) | (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 | 9,639 | 9,639 | 4,494 | 994 | 836 | 1,375 |

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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 7.5 *** | -0.8 | -0.4 | | 0.9 | -0.8 | 0.3 |
| | (1.2) | (1.8) | (2.1) | | (4.5) | (6.2) | (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 | 5,750 | | 1,255 | 844 | 2,566 |
| **Panel B: 2-year sample; Dep. var.: Enrollment 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 9.3 *** | -0.3 | -0.9 | -0.2 | -0.7 | -0.4 | -1.5 |
| | (1.1) | (1.7) | (1.9) | (0.8) | (1.8) | (2.4) | (1.1) |
| Enrollment 1 year prior to allocation | | | | 0.83 *** | 0.85 *** | 0.80 *** | 0.82 *** |
| | | | | (0.01) | (0.03) | (0.02) | (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 | 5,750 | 5,750 | 1,255 | 844 | 2,566 |
| **Panel C: 4-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 7.2 *** | -0.1 | 0.2 | | 1.0 | -0.9 | 2.1 |
| | (1.3) | (1.9) | (2.4) | | (4.9) | (6.2) | (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 | 9,639 | 9,639 | 4,494 | | 994 | 836 | 1,375 |
| **Panel D: 4-year sample; Dep. var.: Enrollment 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 9.3 *** | 0.4 | -0.2 | -0.3 | -0.2 | 1.6 | -0.2 |
| | (1.2) | (1.7) | (2.1) | (1.1) | (2.6) | (2.8) | (1.9) |
| Enrollment 1 year prior to allocation | | | | 0.75 *** | 0.79 *** | 0.76 *** | 0.75 *** |
| | | | | (0.01) | (0.02) | (0.02) | (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 | 9,639 | 9,639 | 4,494 | 4,494 | 994 | 836 | 1,375 |

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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: No. of classes 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.11 *** | -0.02 | 0.01 | | 0.05 | 0.01 | 0.03 |
| | (0.03) | (0.04) | (0.05) | | (0.10) | (0.20) | (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 | 5,661 | | 1,244 | 833 | 2,531 |
| **Panel B: 2-year sample; Dep. var.: No. of classes 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.13 *** | -0.03 | -0.03 | -0.04 * | -0.04 | -0.05 | -0.06 ** |
| | (0.03) | (0.04) | (0.05) | (0.02) | (0.05) | (0.07) | (0.03) |
| 1st grades 1 year prior to allocation | | | | 0.79 *** | 0.84 *** | 0.77 *** | 0.77 *** |
| | | | | (0.01) | (0.03) | (0.02) | (0.02) |
| Quadratic in index | No | Yes | 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 | 5,661 | 5,661 | 1,246 | 833 | 2,531 |
| **Panel C: 4-year sample; Dep. var.: No. of classes 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.11 *** | -0.02 | 0.02 | | 0.06 | 0.03 | 0.05 |
| | (0.03) | (0.04) | (0.06) | | (0.11) | (0.18) | (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 | 9,244 | 9,244 | 4,399 | | 977 | 809 | 1,357 |
| **Panel D: 4-year sample; Dep. var.: No. of classes 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.14 *** | -0.02 | -0.03 | -0.04 | -0.05 | -0.02 | -0.05 |
| | (0.03) | (0.04) | (0.05) | (0.03) | (0.07) | (0.07) | (0.05) |
| 1st grades 1 year prior to allocation | | | | 0.70 *** | 0.77 *** | 0.72 *** | 0.70 *** |
| | | | | (0.01) | (0.03) | (0.02) | (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 | 9,244 | 9,244 | 4,399 | 4,399 | 977 | 809 | 1,357 |

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-Drefer to the 2- and 4-year samples, respectively (See Section IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table 8: Positive tuition

| | All schools subject to sample selections: | | | | Higher SES groups only | 1998 round | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Positive tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.03 ** | 0.01 | -0.02 | | -0.03 | -0.03 | 0.03 |
| | (0.01) | (0.02) | (0.02) | | (0.06) | (0.07) | (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 | 9,893 | 9,893 | 4,585 | | 1,019 | 844 | 1,401 |
| **Panel B: 2-year sample; Dep. var.: Positive tuition 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.04 *** | 0.01 | -0.02 | 0.00 | 0.01 | -0.02 | -0.01 |
| | (0.01) | (0.02) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| Positive tuition 1 year prior to allocation | | | | 0.97 *** | 0.94 *** | 0.93 *** | 0.99 *** |
| | | | | (0.00) | (0.01) | (0.02) | (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 | 9,893 | 9,893 | 4,585 | 4,585 | 1,019 | 844 | 1,401 |
| **Panel C: 4-year sample; Dep. var.: Positive tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.04 *** | 0.01 | -0.04 | | -0.06 | -0.03 | -- |
| | (0.01) | (0.02) | (0.03) | | (0.07) | (0.06) | -- |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | -- |
| $R^2$ | 0.002 | 0.004 | 0.004 | | 0.010 | 0.008 | -- |
| N | 7,217 | 7,217 | 3,119 | | 710 | 836 | -- |
| **Panel D: 4-year sample; Dep. var.: Positive tuition 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.05 *** | 0.02 | -0.03 | 0.01 | 0.01 | -0.01 | -- |
| | (0.01) | (0.02) | (0.03) | (0.01) | (0.02) | (0.02) | -- |
| Positive tuition 1 year prior to allocation | | | | 0.95 *** | 0.92 *** | 0.94 *** | -- |
| | | | | (0.01) | (0.02) | (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 | 7,217 | 7,217 | 3,119 | 3,119 | 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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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 9: Tuition

| | All schools subject to sample selections: | | | | Higher SES groups only | 1998 round | 2004 2006 rounds[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.35 *** | 0.15 | -0.17 | | -0.65 | -0.69 ** | 0.09 |
| | (0.12) | (0.14) | (0.20) | | (0.69) | (0.34) | (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 | 9,893 | 9,893 | 4,585 | | 1,019 | 844 | 1,401 |
| **Panel B: 2-year sample; Dep. var.: Tuition 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.51 *** | 0.20 | -0.25 | -0.05 | 0.09 | -0.15 | -0.19 |
| | (0.15) | (0.18) | (0.26) | (0.10) | (0.32) | (0.18) | (0.16) |
| Tuition 1 year | | | | 1.19 *** | 1.17 *** | 1.19 *** | 1.35 *** |
| prior to allocation | | | | (0.04) | (0.05) | (0.09) | (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 | 9,893 | 9,893 | 4,585 | 4,585 | 1,019 | 844 | 1,401 |
| **Panel C: 4-year sample; Dep. var.: Tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.39 ** | 0.18 | -0.25 | | -0.68 | -0.69 ** | -- |
| | (0.13) | (0.16) | (0.23) | | (0.79) | (0.34) | -- |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | -- |
| $R^2$ | 0.002 | 0.003 | 0.003 | | 0.006 | 0.012 | -- |
| N | 7,217 | 7,217 | 3,119 | | 710 | 836 | -- |
| **Panel D: 4-year sample; Dep. var.: Tuition 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.78 *** | 0.30 | -0.39 | -0.06 | -0.02 | 0.00 | -- |
| | (0.19) | (0.23) | (0.34) | (0.15) | (0.46) | (0.23) | -- |
| Tuition 1 year | | | | 1.32 *** | 1.23 *** | 1.36 *** | -- |
| prior to allocation | | | | (0.05) | (0.07) | (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 | 7,217 | 7,217 | 3,119 | 3,119 | 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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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 10: Vulnerability index measured at the 1st grade

| | All schools subject to sample selections: | | | | Higher SES groups only | 1998 round | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Vulnerability index 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | -5.4 *** | -0.4 | 0.2 | | 0.9 | 2.6 | -0.6 |
| | (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 | 4,965 | | 759 | 725 | 2,196 |
| **Panel B: 2-year sample; Dep. var.: Vulnerability index 2 years after allocation** | | | | | | | |
| 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 *** | 0.13 *** | 0.74 *** | 0.51 *** |
| | | | | (0.02) | (0.03) | (0.02) | (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 | 4,965 | 4,965 | 759 | 725 | 2,196 |
| **Panel C: 4-year sample; Dep. var.: Vulnerability index 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | -5.0 *** | 0.8 | 0.1 | | 2.2 | 2.3 | -0.9 |
| | (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 | 7,933 | 7,933 | 3,733 | | 529 | 703 | 1,138 |
| **Panel D: 4-year sample; Dep. var.: Vulnerability index 4 years after allocation** | | | | | | | |
| 1{Index≥0} | -3.1 *** | -3.0 ** | -2.1 | -2.1 | -1.8 | -1.8 | -1.2 |
| | (0.7) | (1.0) | (1.7) | (1.7) | (3.8) | (1.5) | (0.7) |
| Vulnerability 1 year prior to allocation | | | | 0.17 *** | 0.17 *** | 0.72 *** | 0.42 *** |
| | | | | (0.02) | (0.06) | (0.02) | (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 | 7,933 | 7,933 | 3,733 | 3,733 | 529 | 703 | 1,138 |

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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table 11: Average mothers' schooling

| | All schools subject to sample selections: | | | | Higher SES groups only | 1998 round | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Avg. mothers' schooling 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.6 *** | 0.1 ** | 0.1 | | -0.2 | | 0.3 |
| | (0.1) | (0.1) | (0.1) | | (0.2) | | (0.2) |
| Quadratic in index | No | Yes | Yes | | Yes | | Yes |
| $R^2$ | 0.020 | 0.046 | 0.006 | | 0.037 | | 0.008 |
| N | 7,102 | 7,102 | 3,612 | | 655 | | 1,381 |
| **Panel B: 2-year sample; Dep. var.: Avg. mothers' schooling 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.5 *** | 0.1 | 0.0 | -0.1 | -0.2 | | -0.1 |
| | (0.1) | (0.1) | (0.1) | (0.1) | (0.1) | | (0.1) |
| Mothers' sch. 1 year prior to allocation | | | | 0.86 *** | 0.74 *** | | 0.89 *** |
| | | | | (0.01) | (0.03) | | (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 | 7,102 | 7,102 | 3,612 | 3,612 | 655 | | 1,381 |
| **Panel C: 4-year sample; Dep. var.: Avg. mothers' schooling 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.6 *** | 0.1 | 0.0 | | -0.4 *** | | -- |
| | (0.1) | (0.1) | (0.1) | | (0.2) | | -- |
| Quadratic in index | No | Yes | Yes | | Yes | | -- |
| $R^2$ | 0.022 | 0.047 | 0.008 | | 0.050 | | -- |
| N | 4,565 | 4,565 | 2,190 | | 357 | | -- |
| **Panel D: 4-year sample; Dep. var.: Avg. mothers' schooling 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.6 *** | 0.1 | 0.0 | 0.0 | 0.0 | | -- |
| | (0.1) | (0.1) | (0.1) | (0.1) | (0.1) | | -- |
| Mothers' sch. 1 year prior to allocation | | | | 0.86 *** | 0.91 *** | | -- |
| | | | | (0.01) | (0.05) | | -- |
| Quadratic in index | No | Yes | Yes | Yes | Yes | | -- |
| $R^2$ | 0.022 | 0.044 | 0.008 | 0.742 | 0.641 | | -- |
| N | 4,565 | 4,565 | 2,190 | 2,190 | 357 | | -- |

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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Avg. hhld. income 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 19.9 *** | 10.2 *** | 8.1 | | -0.9 | | 14.3 * |
| | (2.8) | (3.7) | (5.6) | | (17.8) | | (8.5) |
| Quadratic in index | No | Yes | Yes | | Yes | | Yes |
| $R^2$ | 0.011 | 0.018 | 0.004 | | 0.014 | | 0.004 |
| N | 7,104 | 7,104 | 3,612 | | 655 | | 1,381 |
| **Panel B: 2-year sample; Dep. var.: Avg. hhld. income 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 25.4 *** | 10.1 ** | 5.3 | -3.0 | -7.6 | | -8.9 * |
| | (3.4) | (4.4) | (6.3) | (3.1) | (9.7) | | (4.8) |
| Hhld. income 1 year prior to allocation | | | | 1.02 *** | 0.94 *** | | 1.09 *** |
| | | | | (0.02) | (0.03) | | (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 | 7,104 | 7,104 | 3,612 | 3,612 | 655 | | 1,381 |
| **Panel C: 4-year sample; Dep. var.: Avg. hhld. income 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 21.9 *** | 8.6 * | 5.6 | | -8.1 | | -- |
| | (3.2) | (4.8) | (7.4) | | (26.5) | | -- |
| Quadratic in index | No | Yes | Yes | | Yes | | -- |
| $R^2$ | 0.014 | 0.021 | 0.007 | | 0.026 | | -- |
| N | 4,566 | 4,566 | 2,190 | | 357 | | -- |
| **Panel D: 4-year sample; Dep. var.: Avg. hhld. income 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 28.6 *** | 10.3 * | 4.9 | -1.3 | 3.5 | | -- |
| | (4.2) | (6.0) | (9.1) | (4.2) | (14.6) | | -- |
| Hhld. income 1 year prior to allocation | | | | 1.10 *** | 1.06 *** | | -- |
| | | | | (0.07) | (0.05) | | -- |
| Quadratic in index | No | Yes | Yes | Yes | Yes | | -- |
| $R^2$ | 0.014 | 0.024 | 0.006 | 0.748 | 0.718 | | -- |
| N | 4,566 | 4,566 | 2,190 | 2,190 | 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 IV.D and the notes to Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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.

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[1] |
|---|---|---|---|---|---|---|
| | All | All | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff | Within 0.50 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Primary level commune/homogeneous groups** | | | | | | |
| **Panel A: 2-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 1.000 *** | 1.000 *** | 1.000 *** | 0.999 *** | 1.000 *** | 1.000 *** |
| | (0.000) | (0.000) | (0.000) | (0.001) | (0.001) | (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 | 9,857 | 9,857 | 4,113 | 924 | 632 | 1,855 |
| **Panel B: 4-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 1.000 *** | 0.999 *** | 1.000 *** | 0.999 *** | 0.999 *** | 1.000 *** |
| | (0.000) | (0.001) | (0.000) | (0.001) | (0.001) | (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 | 7,524 | 7,524 | 3,186 | 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 *** | 0.235 *** | 0.127 *** | 0.112 * | 0.109 ** | 0.112 ** |
| | (0.013) | (0.016) | (0.031) | (0.062) | (0.071) | (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 | 7,524 | 7,524 | 3,186 | 738 | 623 | 974 |
| **Secondary level homogeneous groups** | | | | | | |
| **Panel D: 2-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 0.999 *** | 0.999 *** | 0.997 *** | 0.992 *** | 1.000 *** | 1.000 *** |
| | (0.001) | (0.001) | (0.003) | (0.008) | (0.000) | (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 | 6,268 | 6,268 | 2,518 | 664 | 282 | 1,249 |
| **Panel E: 4-year sample; Dep. var.: Dummy for receiving an award at baseline** | | | | | | |
| 1{Index≥0} | 0.999 *** | 0.998 *** | 0.997 *** | 0.989 *** | 1.000 *** | 1.000 *** |
| | (0.001) | (0.002) | (0.003) | (0.011) | (0.000) | (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 | 4,493 | 4,493 | 1,898 | 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 *** | 0.303 *** | 0.146 *** | 0.149 * | 0.169 * | 0.131 * |
| | (0.017) | (0.026) | (0.042) | (0.078) | (0.010) | (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 | 4,493 | 4,493 | 1,898 | 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. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 8.5 *** | -0.9 | -2.9 | | 2.2 | -2.3 | -5.6 * |
| | (1.3) | (2.0) | (2.6) | | (5.1) | (7.1) | (3.2) |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | Yes |
| $R^2$ | 0.008 | 0.025 | 0.002 | | 0.001 | 0.005 | 0.005 |
| N | 9,857 | 9,857 | 4,113 | | 924 | 632 | 1,855 |
| **Panel B: 2-year sample; Dep. var.: Enrollment 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 10.0 *** | -0.4 | -2.4 | 0.0 | -2.1 | -0.1 | -0.8 |
| | (1.2) | (1.9) | (2.4) | (0.9) | (2.2) | (3.0) | (1.4) |
| Enrollment 1 year | | | | 0.86 *** | 0.89 *** | 0.81 *** | 0.84 *** |
| prior to allocation | | | | (0.01) | (0.03) | (0.02) | (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 | 9,857 | 9,857 | 4,113 | 4,113 | 924 | 632 | 1,855 |
| **Panel C: 4-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 8.0 *** | -0.4 | -3.2 | | -0.5 | -4.2 | -8.6 * |
| | (1.4) | (2.1) | (2.9) | | (5.6) | (7.1) | (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 | 7,324 | 7,324 | 3,186 | | 738 | 623 | 974 |
| **Panel D: 4-year sample; Dep. var.: Enrollment 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 10.3 *** | 0.3 | -2.2 | 0.3 | -3.3 | -1.6 | 1.6 |
| | (1.3) | (2.0) | (2.6) | (1.2) | (2.8) | (3.3) | (2.2) |
| Enrollment 1 year | | | | 0.78 *** | 0.83 *** | 0.77 *** | 0.77 *** |
| prior to allocation | | | | (0.01) | (0.03) | (0.03) | (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 | 7,524 | 7,524 | 3,186 | 3,186 | 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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Positive tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.03 ** | 0.02 | -0.00 | | 0.05 | -0.05 | 0.00 |
| | (0.01) | (0.02) | (0.03) | | (0.07) | (0.07) | (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 | 7,764 | 7,764 | 3,252 | | 750 | 632 | 994 |
| **Panel B: 2-year sample; Dep. var.: Positive tuition 2 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.03 *** | 0.03 | 0.00 | 0.00 | 0.03 * | 0.01 | -0.03 |
| | (0.01) | (0.02) | (0.03) | (0.01) | (0.01) | (0.02) | (0.02) |
| Positive tuition 1 year prior to allocation | | | | 0.97 *** | 0.95 *** | 0.93 *** | 0.98 *** |
| | | | | (0.00) | (0.01) | (0.01) | (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 | 7,764 | 7,764 | 3,252 | 3,252 | 750 | 632 | 994 |
| **Panel C: 4-year sample; Dep. var.: Positive tuition 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | 0.04 *** | 0.03 | 0.00 | | 0.03 | -0.05 | -- |
| | (0.02) | (0.02) | (0.03) | | (0.08) | (0.07) | -- |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | -- |
| $R^2$ | 0.002 | 0.002 | 0.005 | | 0.125 | 0.017 | -- |
| N | 5,625 | 5,625 | 2,212 | | 524 | 623 | -- |
| **Panel D: 4-year sample; Dep. var.: Positive tuition 4 years after allocation** | | | | | | | |
| 1{Index≥0} | 0.05 *** | 0.05 ** | 0.02 | 0.02 | 0.06 ** | 0.01 | -- |
| | (0.02) | (0.02) | (0.03) | (0.01) | (0.02) | (0.03) | -- |
| Positive tuition 1 year prior to allocation | | | | 0.96 *** | 0.94 *** | 0.94 *** | -- |
| | | | | (0.01) | (0.02) | (0.02) | -- |
| Quadratic in index | No | Yes | Yes | Yes | Yes | Yes | -- |
| $R^2$ | 0.003 | 0.003 | 0.006 | 0.882 | 0.893 | 0.833 | -- |
| N | 5,625 | 5,625 | 2,212 | 2,212 | 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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] 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: Vulnerability (at the 1st grade) among commune/homogeneous groups

| | All schools subject to sample selections: | | | | Higher SES groups only | 1998 round | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Panel A: 2-year sample; Dep. var.: Vulnerability index 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | -5.3 *** | 0.6 | 4.2 | | 1.2 | 7.2 ** | 3.2 ** |
| | (0.6) | (0.9) | (1.1) | | (1.6) | (2.8) | (1.3) |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | Yes |
| $R^2$ | 0.015 | 0.043 | 0.008 | | 0.016 | 0.025 | 0.012 |
| N | 8,312 | 8,312 | 3,537 | | 566 | 531 | 1,578 |
| **Panel B: 2-year sample; Dep. var.: Vulnerability index 2 years after allocation** | | | | | | | |
| 1{Index≥0} | -0.9 *** | 1.3 | 3.0 * | 0.6 | 2.1 | -2.8 | 5.7 ** |
| | (0.6) | (1.0) | (1.5) | (1.5) | (3.2) | (1.9) | (2.5) |
| Vulnerability 1 year prior to allocation | | | | 0.38 *** | 0.16 *** | 0.75 *** | 0.51 *** |
| | | | | (0.02) | (0.05) | (0.02) | (0.04) |
| Quadratic in index | No | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.002 | 0.009 | 0.001 | 0.054 | 0.011 | 0.635 | 0.083 |
| N | 8,312 | 8,312 | 3,537 | 3,537 | 566 | 531 | 1,578 |
| **Panel C: 4-year sample; Dep. var.: Vulnerability index 1 year prior to allocation** | | | | | | | |
| 1{Index≥0} | -4.9 *** | 1.7 * | 4.5 *** | | 1.0 | 8.0 ** | 3.8 ** |
| | (0.6) | (1.0) | (1.3) | | (1.9) | (2.8) | (1.7) |
| Quadratic in index | No | Yes | Yes | | Yes | Yes | Yes |
| $R^2$ | 0.012 | 0.047 | 0.002 | | 0.007 | 0.028 | 0.011 |
| N | 6,115 | 6,115 | 2,627 | | 391 | 510 | 797 |
| **Panel D: 4-year sample; Dep. var.: Vulnerability index 4 years after allocation** | | | | | | | |
| 1{Index≥0} | -3.4 *** | -5.0 *** | -3.7 * | -4.7 ** | -5.6 | -0.9 | -2.1 ** |
| | (0.8) | (1.1) | (2.0) | (2.2) | (5.0) | (1.9) | (0.8) |
| Vulnerability 1 year prior to allocation | | | | 0.22 *** | 0.27 *** | 0.73 *** | 0.49 *** |
| | | | | (0.02) | (0.07) | (0.03) | (0.03) |
| Quadratic in index | No | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.003 | 0.007 | 0.012 | 0.034 | 0.050 | 0.542 | 0.452 |
| N | 6,115 | 6,115 | 2,627 | 2,627 | 391 | 510 | 797 |

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 commune/homogeneous group. Panels A-B and C-D refer to the 2- and 4-year samples, respectively (See Section IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

Table A.5: 9th grade enrollment among secondary schools

| | All schools subject to sample selections: | | | | Higher SES groups only | 2004 and 2006 rounds[1] |
|---|---|---|---|---|---|---|
| | All | All | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff | Within 0.5 points of the cutoff |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: 2-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | |
| 1{Index≥0} | -31.5 *** | -29.2 *** | -13.1 | | 6.0 | -20.5 |
| | (5.9) | (6.4) | (10.1) | | (11.7) | (13.0) |
| Quadratic in index | No | Yes | Yes | | Yes | Yes |
| $R^2$ | 0.009 | 0.009 | 0.010 | | 0.002 | 0.016 |
| N | 6,268 | 6,268 | 2,518 | | 664 | 1,249 |
| **Panel B: 2-year sample; Dep. var.: Enrollment 2 years after allocation** | | | | | | |
| 1{Index≥0} | -24.7 *** | -29.1 *** | -11.7 | 0.6 | -2.7 | 2.9 |
| | (5.7) | (6.6) | (10.0) | (3.2) | (3.9) | (4.2) |
| Enrollment 1 year prior to allocation | | | | 0.94 *** | 0.95 *** | 0.89 *** |
| | | | | (0.02) | (0.03) | (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 | 6,268 | 6,268 | 2,518 | 2,518 | 664 | 1,249 |
| **Panel C: 4-year sample; Dep. var.: Enrollment 1 year prior to allocation** | | | | | | |
| 1{Index≥0} | -37.0 *** | -24.5 *** | -9.7 | | -1.8 | -16.7 |
| | (6.8) | (7.7) | (12.1) | | (13.4) | (19.8) |
| Quadratic in index | No | Yes | Yes | | Yes | Yes |
| $R^2$ | 0.011 | 0.013 | 0.013 | | 0.001 | 0.036 |
| N | 4,493 | 4,493 | 1,898 | | 498 | 653 |
| **Panel D: 4-year sample; Dep. var.: Enrollment 4 years after allocation** | | | | | | |
| 1{Index≥0} | -28.6 *** | -16.3 ** | -6.1 | 2.7 | -3.2 | 4.5 |
| | (6.6) | (7.9) | (11.8) | (4.1) | (5.0) | (6.1) |
| Enrollment 1 year prior to allocation | | | | 0.90 *** | 0.97 *** | 0.84 *** |
| | | | | (0.02) | (0.03) | (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 | 4,493 | 4,493 | 1,898 | 1,898 | 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 IV.D and Table 1). ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

[1] For the 4-year sample, the table excludes the 2006 SNED wave, since outcomes are not available.

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