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Attendance manipulation and efficiency in Chile's school voucher system

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ABSTRACT

To improve the quality of education, one can either directly reward performance or introduce school choice, private provision, and demand subsidies. The Chilean voucher scheme combines both approaches: an attendance-related subsidy favors school choice and creates incentives for schools to promote attendance throughout the year. With imperfect monitoring, however, institutions may respond by manipulating performance indicators. By analyzing audit data, we find evidence that a large fraction of Chilean schools – including public schools – over-report attendance, with a higher prevalence among for-profit and under-achieving institutions. Expenditure data suggest that manipulation among for-profit schools seems to follow rent extraction purposes rather than educational goals.

1. Introduction

In the education sector, as for most publicly provided social services, an important challenge is to create incentives to provide high-quality. Two types of policies have been typically used to provide such incentives. One is to design an accountability system that provides monetary incentives to improve educational outputs. A second alternative is to introduce school choice; competition from neighboring public schools or private – often subsidized – providers would reduce the local monopoly power of public schools, therefore, providing incentives to improve quality. Under such a system, the choices made by students/parents should drive funding from low to high-performance schools.

Evidence of school performance under accountability schemes suggests some positive results, but also unintended consequences (Deming & Figlio, 2016). One unintended consequence is that schools may manipulate variables that are targeted by the incentive system. In a seminal paper, Jacob and Levitt (2003) study the interaction between accountability policies and teacher cheating by analyzing answers to standardized tests in the Chicago Public Schools, finding evidence of teacher cheating that emerges under high accountability pressure. Cullen and Reback (2006), González et al. (2017) and Dee et al. (2019), among others, have confirmed this relationship between gaming and high-stakes scenarios. In a review of accountability in the US, Deming and Figlio (2016) highlight that setting up an accountability system in a diverse higher education system might be challenging.

Introducing competition can be an alternative tool to increase the provision and quality of education. Market-induced pressure on public schools might lead them to improvements and school choice might improve student educational outcomes by moving students/resources to higher quality/efficient institutions. Indeed, there is experimental evidence of positive effects on the achievement of students that attend private schools and that competition increases the performance of public schools.[2] Nevertheless, competition has also resulted in increases in the socioeconomic stratification of the educational system (Epple et al., 2017; Urquiola, 2016), casting doubt on the overall effect.

In this paper, we contribute to the two sets of literature. We do so by studying the attendance-based subsidy scheme in Chile, the basis for this country's universal K-12 school voucher system. Instead of associating payments to the number of enrolled students – which might induce schools to focus on enrolling students while potentially disregarding attendance – the Chilean voucher system provides funding based on the average monthly attendance reported by schools. That incentive structure is similar to subsidies applied in California,

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[1] The opinions expressed in this paper are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank (IDB), its Board of Directors, or the countries they represent. Fajnzylber completed the work on this paper before joining the IDB.

[2] See Urquiola (2016) for a review of the literature.

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Texas, Idaho, Kentucky, and Missouri (Ely & Fermanich, 2013).[3] Just as accountability systems may cause manipulation of targeting variables, attendance-based payments may result in manipulation of attendance registries to capture more resources. Using unique administrative data from audit visits to schools from a government oversight agency and daily attendance data, we can test whether schools game the funding system. We can even estimate manipulation levels at the school level.[4] Because of the institutional diversity of the Chilean educational system, we can identify patterns of manipulation across different types of institutions (public, nonprofit, and for-profit). In addition, we link our estimates of manipulation to achievement data at the school level, allowing us to test whether manipulation reinforces or counters the logic of market competition according to which high-performance schools should get more funding than low-performance schools. Using expenditure data, we can also test if manipulation relates to higher expenditure in educational inputs or to rent extraction.

Our estimates, based on event study methodologies, show substantial manipulation. In particular, the average school over-reports 2.5% of their attendance. Such practice is more salient among schools with profit motives, which on average over-report 3.4% of attendance. However, the average includes schools that do not manipulate attendance reports. Using estimates at the school level and each school's distribution of attendance shocks, we classify 28% of the schools (33.4% of the for-profit schools but also 27.9% of public schools) as manipulative schools. Among manipulative schools, over-reporting rates increase to 11.5% (12.1% among for-profit schools). Additionally, we find that for-profit status, controlling for other determinants, is associated with an over-reporting rate that is 1.8 percentage points higher than public and nonprofit schools. In consequence, our findings suggest that manipulation exists among all school types but is more prevalent and intense among for-profit institutions. We also find that schools with high over-reporting – especially for-profit schools – have low educational achievement and high real estate expenditures. Therefore, our results suggest that manipulation counters the logic of competition-induced quality provision of educational services.

Our results provide a warning about combining incentives and profit motives in educational markets with imperfect monitoring. Indeed, they confirm the concerns about for-profit motives that have emerged at the higher education level, a sector where the empirical evidence suggests that for-profit higher education institutions have high default rates in student loans (Deming et al., 2012), have increased tuition to capture student federal aid (Cellini & Goldin, 2014), and do not provide an education that pays off in the labor market (Cellini & Turner, 2019). Our findings are also consistent with the evidence about private prison contracts that pay per diem for each occupied bed, where private prisons' inmates serve additional days as a result of manipulation of infractions (Mukherjee, 2021). In addition, our results also complement the literature on for-profit charter school operators, known as Education Management Organizations (EMOs). Although there is evidence that a particular large EMO can generate higher achievement than public schools (Dynarski et al., 2018), there also is consistent evidence that EMOs spend fewer resources on instructional inputs than nonprofit charter operators (Singleton, 2017).

In the next section, we describe the institutional setting. Section 3 describes the data and methodology, while Section 4 presents the results and discussion. Section 5 shows some robustness tests and additional results. Finally, Section 6 concludes with suggestions for further research.

[3] Similar attendance-based funding formulas apply to early childhood subsidies, although the 2020 Child Care and Development Block Grant Act encourages funding based on enrollment.

[4] This project started with a request of the Superintendency of Education of Chile (SE, *Superintendencia de Educación de Chile*) and the Interamerican Development Bank to identify and measure the manipulation of attendance at the system level.

2. Institutional setting

2.1. Chile's educational system

Chile is one of the most market-oriented educational systems in the world. In particular, Chile implemented in 1981 a universal voucher system in which: (i) public, private nonprofit, and private for-profit schools can receive funding from the government;[5] (ii) funding is proportional to students' average daily reported attendance. The latter provides a financial incentive to attract students to classes and, in theory, represents the main accountability element of the system: students would attend high-performance schools and walk away from low-performing institutions.[6]

The reform opened the market to heterogeneous educational providers, resulting in a growth of private subsidized education. In 2016, K12 education in Chile had four types of institutions: public (35.8% of 2016 enrollment), private subsidized for-profit (46.2%), private subsidized nonprofit (9.8%), and private non-subsidized (8.1%). In the first three types, schools receive a monthly public subsidy that is proportional to the average (over the last three months) daily attendance as reported to the Ministry of Education (MINEDUC) and where the per-student payment varies across education levels (approximately 102.3 dollars at the elementary level, 102.6 dollars at the middle level, 122.1 dollars at the secondary level). This funding scheme represents almost 90% of total public funding for pre-K and K12 education. One special part of the voucher system is the Preferential School Subsidy (PSS, *Subvención Escolar Preferencial*), which pays schools an additional subsidy for the attendance of each low-SES student. Similar to the main subsidy, the PSS payment is proportional to reported attendance. In 2015, the main voucher represented an expenditure of US$4.48 billion while the total funding from the PSS voucher was US$873 million. Reported attendance was the key parameter in the allocation of over US$5.3 billion, or 2.19% of Chile's 2015 GDP (these numbers include preschool funding). Other funding sources include municipal transfers for publicly run schools (11% of public schools funding) and additional tuition fees in the case of private subsidized schools (14% of private subsidized schools funding) (MINEDUC, 2016).

In principle, school voucher systems hold institutions accountable through competition.[7] Indeed, the Chilean system has openings and closures of schools that results in a turnover rate similar to middle and small-sized firms (Grau et al., 2018). However, the Chilean system, despite its leading position in Latin America, underperforms most OECD countries and lacks equal access to quality education.[8] This unequal access was one of the main triggers of two massive student protests in 2006 and 2011.[9] Both movements focused on the use and distribution of resources and the role of for-profit educational institutions. In response to the protests, the government created two new oversight institutions: the Education Quality Agency and the Superintendency of Education (SE). While the first focuses on measuring and improving educational achievement, the SE mission is to hold institutions accountable for the use of public funds and safeguard individual rights and equal access to quality education. In short, the role of the SE is to regulate educational institutions and make sure that regulatory compliance takes place.

[5] Public schools are administered at the municipal level.

[6] Although the system has changed in recent years (tuition fees policies, admission policies, voucher funding amount, among others), it is still the case that students' attendance is the key parameter of funding formulas.

[7] For a survey on school vouchers, see Epple et al. (2017).

[8] On the 2012 PISA test, Chile scored 423/441 points in math/language. Latin America scored 397/413, and OECD countries scored 494/496. Chile is among the countries with the strongest link between educational achievement and socioeconomic status (OECD, 2016).

[9] See WashingtonPost-Chile'sStudentActivists:ACourseinDemocracy and NewYorkTimes-WithKiss-InsandDances,YoungChileansPushforReform. Gabriel Boric, one of the main leaders of these movements, became President of Chile in March 2022.

Table 1
Funding adjustment according to the estimated percentage x of over-reporting.

Urban schools	Rural schools	Reduction
$x \leq 2\%$	$x \leq 4\%$	0%
$2\% < x \leq 6\%$	$4\% < x \leq 10\%$	$0.5x\%$
$6\% < x \leq 10\%$	$10\% < x \leq 14\%$	$x\%$
$10\% < x \leq 14\%$	$14\% < x \leq 16\%$	$2x\%$
$14\% < x$	$16\% < x$	$3x\%$

2.2. Oversight of attendance reporting

Because daily attendance is the key funding parameter, schools have a powerful incentive to manipulate attendance to increase revenue.[10] In response to this problem, the funding formula includes a variable designed to discourage over-reporting. The MINEDUC constructs for every external observation of real attendance (including SE audits) a "divergence" variable, which measures the difference between the observed attendance and the preceding month's reported average while correcting for potential "weather shocks" in the geographical area (see Appendix B). Then, the government uses the average "divergence" of the last three observations of real attendance to adjust funding.[11] Table 1 shows funding reductions as a function of "divergence" (x) and the urban/rural status of the institution.

From an economic point of view, this adjustment structure is probably insufficient to prevent fraud. Even with perfect monitoring, any school (with no administrative or moral cost of manipulation) would only be partially punished when over-reporting less than 6% in urban settings or 10% in rural areas.

To counter fraud, the SE introduced regulations on the daily procedure to register and report attendance[12] and created the School Attendance Inspection Program (*Programa de Fiscalización de la Asistencia Escolar*). In the program, SE inspectors make unannounced school visits to verify that: (i) the visit day's reported attendance matches the actual attendance; (ii) registries are consistent with information from other databases (e.g., healthcare databases); and (iii) registries follow existing regulations. In addition, other on-site SE audit programs (such as financial audits) also include an inspection of attendance registries.

If an SE inspector detects irregularities in the registry, the SE can demand the return of excess funding and impose a fine between 51 UTM (3,621 USD)[13] and 500 UTM (35,500 USD). In cases of repeated offenses and/or detection of other irregularities, additional penalties include suspension of funding and cancellation of official government accreditation as an educational institution.

However, imposing penalties for over-reporting is a challenging task for the SE as schools may alter attendance sheets when visited and simply argue that the relatively low attendance of that day is due to special circumstances.[14] In such cases, the SE must provide further proof of over-reporting to impose penalties. Despite these challenges, 14.6% of attendance audits in the 2014–2016 period resulted in the SE imposing penalties.[15]

3. Data and methodology

3.1. Data

All subsidized schools in Chile report daily attendance to the MINEDUC. We use reported daily attendance from each school for the period 2013–2016, in addition to observed (audited) attendance and each inspection outcome. To complement the attendance data, we use public data on other school characteristics, such as enrollment, composition, and urban status, among others.[16]

The main data set includes over 1.4 million daily observations per year at approximately 8,400 schools. The average reported daily attendance is slightly above 300 students. Fig. 1 shows histograms for the level and logarithm of daily attendance, showing the wide variation in attendance levels (because of heterogeneity in capacity). We use the logarithm of attendance in the empirical analysis.

Table 2 shows descriptive statistics of the main variables over time. The fifth row shows that over 20% of the audited schools had a mismatch between reported and observed attendance. While the small difference between reported and observed attendance suggests that attendance fraud is an insubstantial problem, the magnitude of the problem might be concealed by on-the-spot adjustment of daily attendance. It is also worth noting that the percentage of institutions audited (ninth row) decreased from 75.6% in 2013 to 10.1% in 2016, due to the SE's decision to lessen its focus on attendance audits in favor of other oversight programs unrelated to attendance.[17]

While the data include all subsidized K12 schools, there is important institutional heterogeneity. In particular, the majority of schools are public (approximately 57%), a large percentage of schools are private subsidized for-profit (about 32%), and a smaller percentage of subsidized schools are nonprofits (about 11%).[18] This rather unique institutional heterogeneity in the Chilean system allows us to examine the differences in regulatory compliance across different types of schools.

3.2. Methodology

Given the daily reporting of attendance and the unexpected nature of SE inspection visits, we use an event study methodology, popular in the finance literature (MacKinlay, 1997).[19] In particular, we focus on the evolution of attendance during the 5 weekdays preceding the visit, the day of the visit, and the 5 days succeeding the visit.[20] This allows us to both measure potential anticipation and fade-out effects (Kothari & Warner, 2007). The event study methodology helps us to estimate an average audit effect on the visited institutions for this 11-day window.

Initially, we assume the following data generation process:

$$y_{irt} = \delta_0 + \sum_{d=5}^{1} \delta_{-d} W_{it+d} + \delta W_{it} + \sum_{d=1}^{5} \delta_d W_{it-d} + \eta_{irt} \tag{1}$$

where variable y_{irt} is the logarithm of attendance at institution i, located in region r, on day t.[21] W_{it} is a dummy variable that indicates that an audit occurred at school i on day t. The coefficients associated

[10] Appendix C discusses a model of manipulation.

[11] If there have not yet been observations of real attendance, the nonexistent observations are assumed to have zero "divergence".

[12] The main SE regulations are: (i) every hour, teachers must record individual and total classroom attendance (Appendix Tables C.1 and C.2 show SE attendance sheets); and (ii) schools must upload the attendance of the second class (8:45 a.m. to 9:30 a.m.), which determines funding.

[13] *Unidad Tributaria Mensual* (Monthly Tax Unit).

[14] According to SE auditors, sometimes institutions obstruct audits by delaying physical access to school, registries or classrooms while altering the attendance sheets.

[15] Institutions can appeal SE penalties, including appeals in the justice system.

[16] Public data are available at http://datosabiertos.mineduc.cl/.

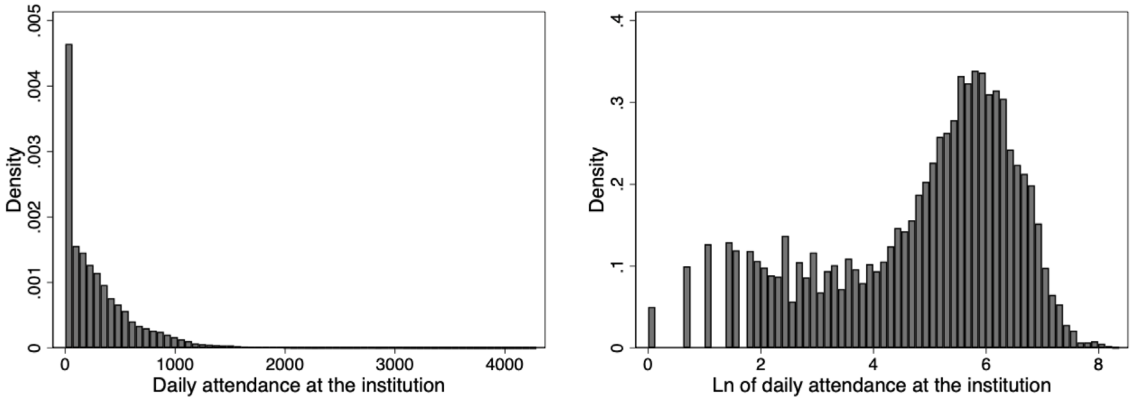
[17] The SE's audit selection criteria depend on an institution's history of attendance and inspections as well as a large random component. To protect the secrecy of the SE's selection criteria, we cannot provide further details.

[18] The for-profit status corresponds to the legal designation provided to us by the SE, although it is possible that some nonprofit institutions may use this designation to avoid the administrative burdens associated with the nonprofit status.

[19] A related method is staggered adoption difference-in-differences (Athey & Imbens, 2018).

[20] If there are multiple visits to an institution in a year, we only consider the first visit to avoid confounding effects.

[21] The country is divided into 15 administrative regions. Each region has many schools.



(a) Attendance levels (b) Logarithm of attendance

Fig. 1. Histogram of daily attendance.

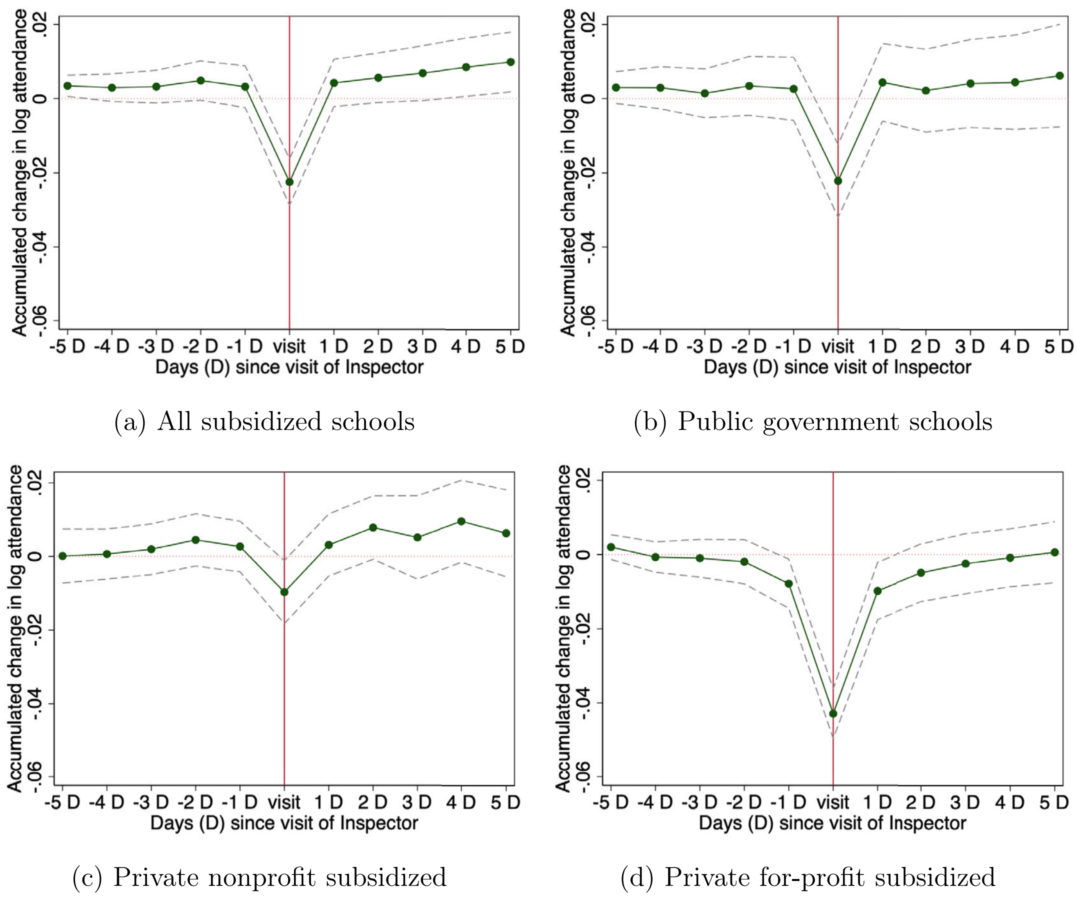
Table 2
Descriptive statistics of selected variables at the school level (means and standard deviations in parentheses).

Variable	Year			
	2013 mean/sd	2014 mean/sd	2015 mean/sd	2016 mean/sd
Attendance in the day	304.830	307.305	308.121	313.803
	(355.0113)	(356.8727)	(359.1579)	(361.8204)
Log of reported attendance	4.809	4.806	4.792	4.819
	(1.6793)	(1.7030)	(1.7237)	(1.7274)
Attendance observed by inspector	376.821	424.748	414.520	382.864
	(371.6719)	(383.6772)	(349.7434)	(267.9267)
Log of observed attendance	5.444	5.621	5.716	5.688
	(1.0945)	(1.0388)	(0.8337)	(0.8026)
Attendance incorrectly reported	0.234	0.235	0.233	0.267
	(0.4231)	(0.4242)	(0.4230)	(0.4426)
Log(reported/observed)	0.003	0.002	0.003	0.010
	(0.0504)	(0.0360)	(0.0306)	(0.1560)
Total enrollment	348.560	349.935	350.339	351.969
	(402.2948)	(403.3286)	(404.3227)	(404.4885)
Log(enrollment/attendance)	0.122	0.115	0.115	0.098
	(0.2436)	(0.2497)	(0.2720)	(0.2033)
Inspected in the year	0.756	0.460	0.198	0.101
	(0.4297)	(0.4984)	(0.3986)	(0.3019)
Public government school	0.579	0.571	0.563	0.573
	(0.4936)	(0.4949)	(0.4961)	(0.4946)
Private nonprofit motive school	0.111	0.112	0.115	0.113
	(0.3139)	(0.3158)	(0.3185)	(0.3166)
Private for-profit motive school	0.310	0.316	0.323	0.314
	(0.4624)	(0.4650)	(0.4676)	(0.4641)
Institution in rural area	0.422	0.413	0.411	0.409
	(0.4939)	(0.4924)	(0.4920)	(0.4917)
Percentage high-priority students	0.695	0.671	0.624	0.630
	(0.2195)	(0.2135)	(0.2093)	(0.2087)
Age of the institution (in years)	18.525	19.405	20.311	21.314
	(5.2256)	(5.4047)	(5.5380)	(5.5691)
Number of institutions	8,443	8,461	8,454	8,388
Daily Observations	1,502,396	1,485,698	1,445,676	1,455,821

with dummies W_{it+d} and W_{it-d} capture potential changes in the 5 days preceding and succeeding the visit, respectively. The error term can be decomposed as $\eta_{irt} = \psi_i + \phi_{rt} + \mu_{it}$, where ψ_i represents a school-specific effect, ϕ_{rt} captures any region-time specific effect (such as a weather shock) in region r on the day t, and $\mu_{it} = \mu_{it-1} + \varepsilon_{it}$ is an idiosyncratic random walk error term.

Given that attendance is a highly autoregressive process, we estimate the following first-difference model (Wooldridge, 2003):

$$\Delta y_{irt} = \alpha_0 + \sum_{d=5}^{1} \beta_{-d} W_{it+d} + \beta W_{it} + \sum_{d=1}^{5} \beta_d W_{it-d} + \tilde{\phi}_{rt} + \varepsilon_{irt} \quad (2)$$



(a) All subsidized schools (b) Public government schools

(c) Private nonprofit subsidized (d) Private for-profit subsidized

Fig. 2. Change in log of attendance around audit day, different samples.

where parameter β_j (j between −4 and 5) captures the change in the logarithm of attendance between a particular day and the preceding day.[22] Because Chile has various climates and weather shocks might cause attendance shocks, we include a region-level time effect $\tilde{\phi}_{rt} = \phi_{rt} - \phi_{rt-1}$. In sum, β_j estimates the additional change in attendance j days around an audit day in comparison to the change in attendance at unaudited schools in the region that day.

We estimate this model on several samples. The main identifying assumption is that the visit day dummies are orthogonal to the school-level daily idiosyncratic shock, controlling for region-level time effects. In other words, the attendance trend should be parallel across schools in the same region. Moreover, if schools are able to anticipate the visit more than 5 days in advance, we should not be able to detect variations in attendance within the window of analysis.[23]

In addition to measuring the average level of over-reporting in audited schools, we are interested in identifying possible differential responses to the incentive structure, i.e., heterogeneity in manipulation. To this end, we need individual school-level measures of over-reporting. To obtain these, we repeat the previous estimation separately for each region and year and estimate institution-specific coefficients for the day before the visit ($\beta_{i,-1}$), the day of the visit (β_i), and the day after the visit ($\beta_{i,+1}$). The time effect $\tilde{\phi}_t$ captures the daily variation common to the region. Thus, the model that estimates over-reporting at each school is:

$$\Delta y_{it} = \alpha + \sum_{d=5}^{2} \beta_{-d} W_{it+d} + \sum_{i \in A} (\beta_{i,-1} W_{it-d} + \beta_i W_{it} + \beta_{i,+1} W_{it+1}) + \sum_{d=2}^{5} \beta_d W_{it-d} + \tilde{\phi}_t + \varepsilon_{it} \quad (3)$$

where A is the set of schools that were audited in the region that year.

4. Results

4.1. Average results

First, we present system-wide estimates of over-reporting based on Eq. (2). Since the magnitude of over-reporting might differ across institutions, we separate the analysis between public, private nonprofit, and private for-profit schools. In the analysis, we use inspectors' observed attendance on the visit day and reported attendance for the other days.[24] That should give us a proper measure of manipulation, even if institutions adjust the reported attendance of the visit day in response to the inspector's visit. They cannot, however, adjust reported attendance for previous days.

[22] Coefficient β_{-5} is interpreted as the difference between the logarithm of attendance at the audited institution and the average logarithm of attendance in the region on the fifth day preceding the visit.

[23] In one of the robustness analyses in Section 5, we expand the observation window to 5 weeks before and after the audit day.

[24] Results using reported attendance for all days are in Appendix Table C.4.

Table 3
Aggregate over-reporting, change in log-attendance.

Variables	(1) Δ attend. all schools	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 days	0.0035**	0.0030	0.0001	0.0020
	(0.00145)	(0.00219)	(0.00375)	(0.00170)
Visit in 4 days	−0.0005	−0.0001	0.0005	−0.0027*
	(0.00130)	(0.00204)	(0.00308)	(0.00140)
Visit in 3 days	0.0003	−0.0015	0.0013	−0.0003
	(0.00146)	(0.00245)	(0.00134)	(0.00149)
Visit in 2 days	0.0017	0.0020	0.0025	−0.0009
	(0.00190)	(0.00304)	(0.00308)	(0.00194)
Visit tomorrow	−0.0017	−0.0008	−0.0018	−0.0059***
	(0.00191)	(0.00325)	(0.00212)	(0.00174)
Visit today	−0.0257***	−0.0248***	−0.0124***	−0.0350***
	(0.00184)	(0.00261)	(0.00243)	(0.00293)
Visit yesterday	0.0267***	0.0265***	0.0128***	0.0331***
	(0.00131)	(0.00157)	(0.00189)	(0.00274)
Visit 2 days ago	0.0014	−0.0022	0.0047***	0.0049**
	(0.00127)	(0.00187)	(0.00148)	(0.00210)
Visit 3 days ago	0.0012	0.0019	−0.0027	0.0024
	(0.00140)	(0.00182)	(0.00354)	(0.00157)
Visit 4 days ago	0.0016	0.0003	0.0044	0.0016
	(0.00181)	(0.00275)	(0.00358)	(0.00184)
Visit 5 days ago	0.0014	0.0018	−0.0033	0.0015
	(0.00126)	(0.00220)	(0.00224)	(0.00142)
Constant	−0.0012***	−0.0008***	−0.0022***	−0.0016***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Observations	5,834,434	3,335,051	657,215	1,841,835
R-squared	0.0583	0.0635	0.1447	0.1073
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the daily change in log attendance at a certain school. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

Table 3 presents the estimation results for all schools and for different subsamples. Despite the large sample size, the coefficients for two or more days before the audit day are not statistically or practically significant, except for the dummy of a visit day in 5 days in the whole sample regression. However, an inspector "visit today" event induces an average negative attendance shock of −0.0257 (2.53% of over-reporting, equivalent to 6.7 students). However, there is variation in the results. Our estimation for public schools is −0.0248 (2.45% of over-reporting, equivalent to 5.6 students), and for private subsidized nonprofit schools is −0.0124 (1.23% of over-reporting, equivalent to 5.7 students). Interestingly, the for-profit subsidized schools seem to over-report more; the coefficient of the visit day is −0.035 (3.4% of over-reporting, equivalent to 9.4 students). In addition, there seems to be a small anticipation effect on the previous date, although the size of the coefficient makes it insignificant in practice (0.6%). Two interpretations have been advanced to explain this effect. First, some schools may be illegally tipped off about the upcoming visit and adjust their attendance beforehand. Another explanation is that attendance audits are sometimes part of a larger audit program (including, for example, a review of financial records), which can take more than one day. Our data, however, does not allow us to test these hypotheses.

The estimates also show that the decrease in reported attendance is short-lived as the following one or two days have positive effects. In other words, attendance rapidly returns to pre-audit levels. To show this, Fig. 2(a), Fig. 2(b), Fig. 2(a) and Fig. 2(d) present predicted attendance during the two-week window surrounding the visit day, where each daily estimate accumulates all the shocks (coefficients) up to date. In all cases, the audit induces a significant decrease in attendance that disappears after one or two days, although the shock is more important among for-profit schools. In sum, our results suggest three initial findings: a non-negligible level of attendance over-reporting, significant heterogeneity in manipulation, and a rapid return to usual reporting practices, implying that audits do not deter fraud.

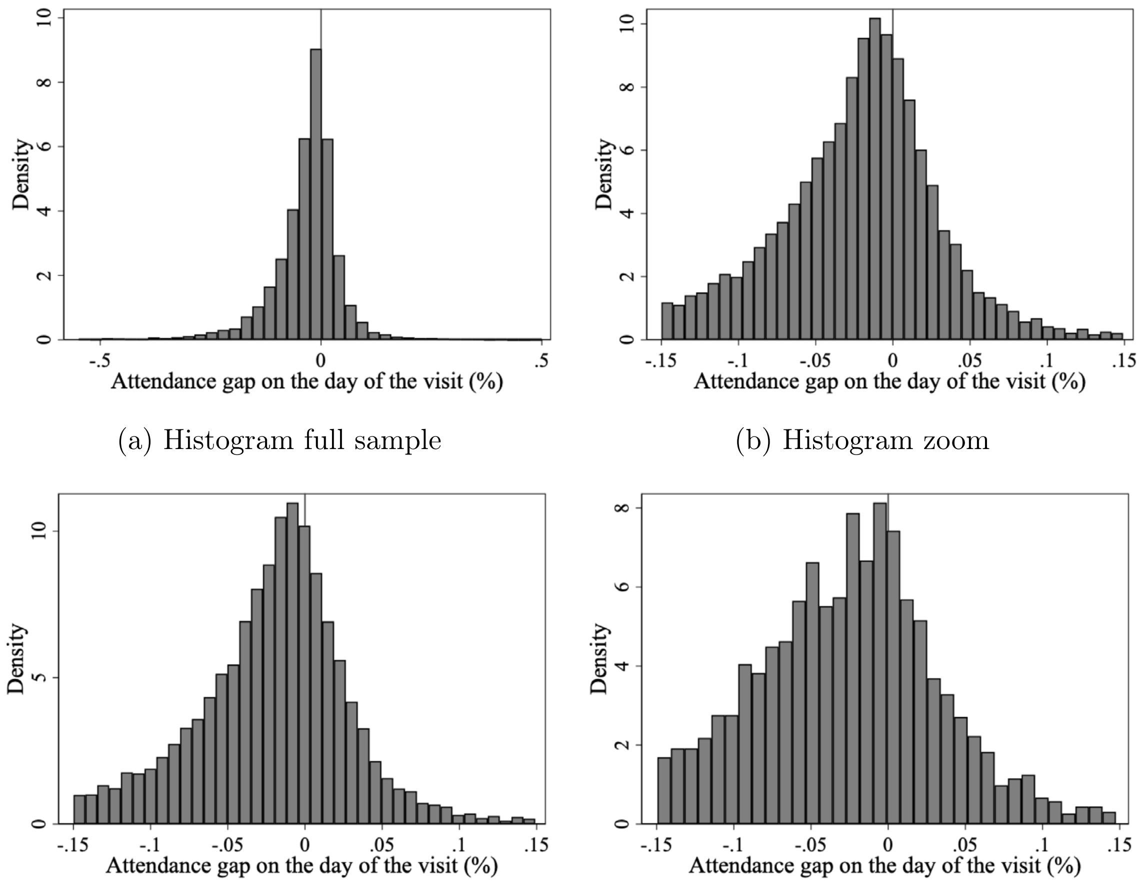
4.2. Characterizing over-reporting

Thus far, we have presented evidence of over-reporting at the aggregate level. However, manipulation has extensive and intensive margins. In other words, the average includes institutions that do not over-report and institutions that significantly over-report. In addition, the institutional classification might hide other factors behind manipulation (such as differences in performance). Understanding manipulation heterogeneity is central to efficiency considerations. Under homogeneous manipulation, the effective subsidy per student is slightly higher than the actual one but the same for all types of schools. In contrast, heterogeneous manipulation results in a greater allocation of resources to cheating schools, which may cause inefficient resource allocation. To address this, we now analyze the institutional characteristics that predict over-reporting rates.

We first estimate Eq. (3) for each region and year to get the visit effect ($\beta_{i,-1}, \beta_i$) and rebound effect ($\beta_{i,+1}$) for each visited school i. To understand the extent of over-reporting, Sub Fig. 3(a) presents the histogram of our estimates of ($\beta_{i,-1} + \beta_i$) for each school visit.[25] Two notable aspects of these figures are: (i) as expected, most of the probability mass lies below the zero line, suggesting extensive over-reporting; and (ii) there is a long tail of negative values, suggesting that a subset of schools incur substantial manipulation. For a better understanding, Sub Fig. 3(b) shows the histogram once we zoom on the schools with an estimated coefficient between −0.15 and 0.15. Again, it is evident that the left tail of the distribution is thicker and longer than the right tail.

In terms of gaming, the funding adjustment formulas (Table 1) provide incentives for bunching of over-reporting just below 6% among

[25] The histogram does not include estimates further than 3 SD from the mean.



(a) Histogram full sample

(b) Histogram zoom

(c) Histogram zoom - Urban

(d) Histogram zoom - Rural

Fig. 3. Histograms of the estimated decrease in attendance.

urban schools and just below 10% among rural schools. To examine if that is the case, Sub Figs. 3(c) and 3(d) show the histograms (zooming in the area around 0) of the estimated manipulation for urban and rural schools, respectively. We do not observe significant bunching, although it is possible that the variance of the estimates may hide this behavior.

Table 4 presents in its columns the fraction of schools we classify as over-reporters, the mean estimated shock of the day before the visit ($\beta_{i,-1}$), the mean estimated shock on the visit day (β_i), the mean estimated shock the day after the visit ($\beta_{i,+1}$), and the mean estimated shock on the visit day (β_i) conditional on being classified as an over-reporter. Since each school's estimate of over-reporting is the negative attendance shock in the visit-day, we classify as significant over-reporters those schools whose visit-day shock is lower than the 10th quantile of each school's distribution of estimated errors.[26] The bottom row shows the estimates for all schools. We identify 28% of all schools as over-reporters, with this figure increasing to 33.4% among for-profit schools. A significant fraction of public schools (27.9%) is also identified as over-reporting. The average shock on the visit day mirrors the estimated effect of the day after the visit, consistent with results in Table 3. As we explained before, the mean includes schools that do not over-report; hence, the last column helps us to understand the over-reporting rate of schools that (we can statistically claim that) engage in manipulation. Our estimates suggest that "cheating" schools over-report 12.2% of their attendance.

So far, the aggregate statistics show that the audit visit has a negative shock that is immediately canceled by a positive shock. However, that might result from the aggregation of the data and not from the actions of each school. To understand if that is the case, Fig. 4 presents the scatterplot of β_i against $\beta_{i,+1}$ and confirms the drop-bounce pattern of manipulation at the school level. In particular, the light color circles show the schools classified as over-reporters, which are indeed further from zero. Meanwhile, the dark color squares show the schools not classified as over-reporters, which are closer to zero. Nevertheless, most schools lie in the upper-left quadrant and near the 45° line. This finding suggests that the lower reported attendance on the visit day reverts to previous levels the next day. In sum, the drop-bounce pattern occurs at the school level.

To identify which factors predict manipulation, Table 5 presents OLS regression results with the school-level visit-day manipulation measure as the dependent variable. The raw differences in over-reporting rates between different types of institutions, conditioning only on year-fixed effects, are presented in column (1). Compared to public schools (the omitted group), nonprofit institutions would over-report a little less (positive coefficient), but for-profit schools would over-report about 1.8% more. In addition, we see that the estimated over-reporting is significantly larger in the year 2016, when the attendance inspection rate was the lowest (Table 2), suggesting a negative relationship between audit intensity and over-reporting.

As the differences might arise from differences between schools in terms of socioeconomic challenges and incentives, column (2) shows the result after controlling for enrollment, the composition of students, rurality, and a measure of the age of the establishment (in years since first registered after 1992). These results confirm that for-profit institutions over-report more than public institutions (2.2%), but also more than nonprofits (the p-value of the Wald test is in the bottom

[26] This is similar to Fisher's exact test.

Table 4
Fraction over-reporting and average estimated coefficients by school type.

School type	Mean				
	Significant over-reporter	Day before coefficient	Visit day coefficient	Day after coefficient	Over-reporters' visit day coefficient
Public schools	0.279	−0.001	−0.023	0.025	−0.119
Private nonprofit	0.151	0.000	−0.012	0.012	−0.114
Private for-profit	0.334	−0.005	−0.035	0.034	−0.129
All schools	0.280	−0.002	−0.026	0.026	−0.122

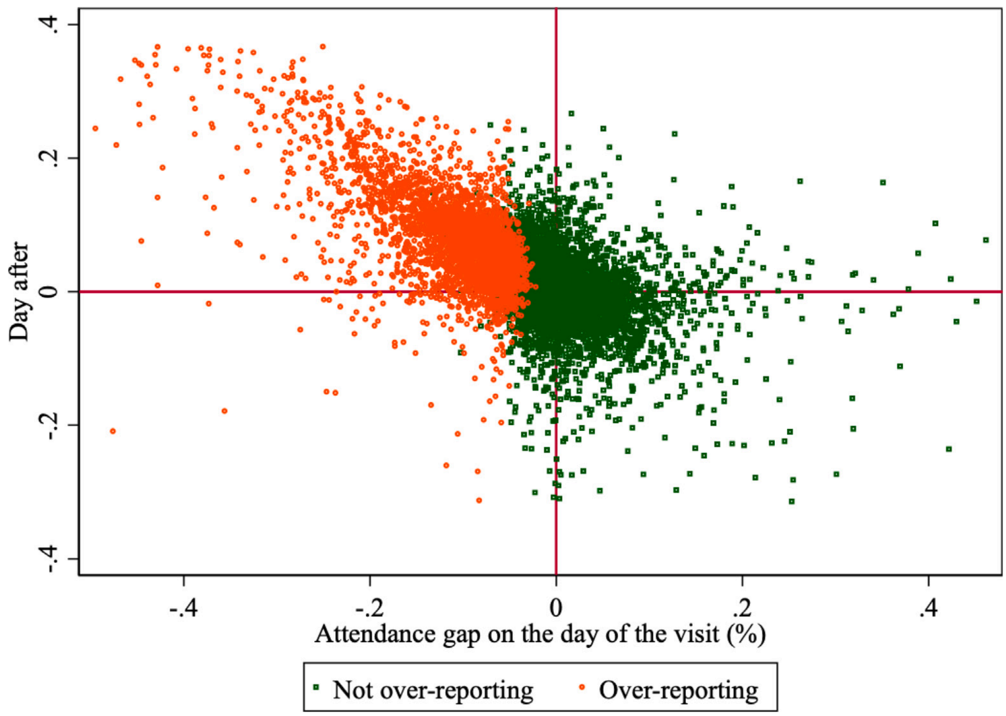


Fig. 4. Scatter plot of β and $\beta_{i,+1}$ coefficients.

row). In addition, we observe that a larger enrollment is associated with lower over-reporting. This is consistent with the fact that larger schools are more likely to be audited (see Table 2) and that larger schools might face higher reputational costs of being caught over-reporting attendance to the authorities.

Similarly, schools with a higher share of low-SES students also exhibit higher over-reporting, which might be explained by the increased financial incentive from the additional voucher subsidy for priority students. An alternative explanation is that low-SES students typically present more erratic attendance behavior, which implies less revenue per enrolled student and may induce over-reporting among schools that serve these students. Meanwhile, rural schools engage in less over-reporting, despite that rural schools have a looser funding adjustment formula (Table 1) and are less likely to be audited (Appendix Table C.3). Older institutions over-report less, which is consistent with the reputational motive.[27]

Finally, as geography might bias our results because of the different allocation of school types across regions, Column (3) shows that the estimates remain qualitatively similar after including regional fixed effects.

In sum, the previous results are generally consistent with the predictions of the simple deterrence model presented in Appendix C. We find a higher prevalence of over-reporting when the probability of being audited is lower (such as during 2016 and in smaller schools) and among schools with lower reputational/moral costs, such as private for-profit, smaller or younger schools. Also, schools facing a higher per-student subsidy (with a higher proportion of low-SES students) are more likely to over-report.

4.3. School achievement and use of resources

Although manipulation is subject to legal and moral objections, it is possible that high-achievement schools (with a high drive for education quality) are the ones engaging in manipulation, which would result in a more productive allocation of resources. Also, over-reporting schools might be using the additional resources on education-enhancing inputs. In both of these cases, manipulation might not necessarily generate an inefficient outcome. To determine whether that is the case, we next analyze the relationship between manipulation, achievement, and school inputs.

[27] The institution age might also signify greater financial stability, with a lower need to over-report.

Table 5
OLS regression on school-level over-reporting effects, all audited schools.

Variables	(1) Estimated over-reporting	(2) Estimated over-reporting	(3) Estimated over-reporting
Private nonprofit motive school	0.0114***	−0.0045	−0.0013
	(0.00394)	(0.00468)	(0.00420)
Private for-profit motive school	−0.0180***	−0.0218***	−0.0183***
	(0.00361)	(0.00397)	(0.00340)
Log enrollment		0.0159***	0.0164***
		(0.00278)	(0.00280)
Percentage high-priority students		−0.0511***	−0.0403***
		(0.00981)	(0.01016)
Institution in rural area		0.0197***	0.0185***
		(0.00714)	(0.00715)
Age of the institution (in years)		0.0006*	0.0007**
		(0.00031)	(0.00031)
Year = 2014	0.0016	−0.0018	−0.0028
	(0.00374)	(0.00360)	(0.00358)
Year = 2015	0.0055	0.0024	0.0016
	(0.00661)	(0.00667)	(0.00682)
Year = 2016	−0.0277***	−0.0310***	−0.0319***
	(0.00380)	(0.00395)	(0.00420)
Constant	−0.0227***	−0.0911***	−0.1027***
	(0.00235)	(0.01892)	(0.02103)
Observations	12,653	12,653	12,653
R-squared	0.0046	0.0152	0.0183
Region FE	No	No	Yes
Year FE	Yes	Yes	Yes
P-value for test: $H_0 : \beta_{For-profit} = \beta_{nonprofit}$	0.00000	0.00001	0.00003

Note: Clustered standard errors at school level in parentheses, *** p<.01; ** p<.05; * p<.1. The dependent variable is the estimated visit-day coefficient for a particular school. The sample corresponds to all schools that were audited. All specifications include year-fixed effects but only column 3 includes regional fixed effects.

Over-reporting and achievement

To measure achievement, we use the normalized "effectiveness" score from the SNED[28] national school evaluation system, which is generated in three steps. First, the SNED classifies schools according to geographic region, urban/rural location, and education level. Second, it clusters schools within each classification based on a socioeconomic vulnerability index, average household income, and average parental education. Third, the SNED defines the "effectiveness score" as the achievement score on the standardized national achievement tests (SIMCE) for each education level tested in the school in comparison to the average score within the cluster of similar schools. Therefore, our measure of achievement already controls to some extent for student socioeconomic backgrounds and differences across education levels.

Using our achievement measure, we estimate a simple linear model of achievement as a function of a "significant overreport" dummy (as classified in Table 4), along with a set of control variables. Although we cannot give a causal interpretation to the estimated coefficients (as the decision to over-report might be related to unobserved determinants of achievement), they help us understand whether over-reporting allocates resources to high, medium, or low-achievement schools. Table 6 shows the results. The first 3 columns include pooled results for all schools while the last 3 columns present the most general specification, separately for public, nonprofit, and for-profit schools.

Column (1) shows the gross differences in achievement across over-reporting groups. We observe a negative relationship between manipulation and achievement: over-reporting schools perform 41.2% of an SD worse than schools not classified as over-reporters. The results after controlling for student composition, location, enrollment, age of the institution, institution type, and municipality and year fixed effects (columns (2) and (3)) still show a significant (although smaller) negative correlation between manipulation intensity and achievement. In particular, columns (2) and (3) show that schools over-reporting perform worse by approximately 8.7% of an SD. In sum, the evidence suggests that manipulation drives resources to low-achievement schools.

Focusing on the institutional heterogeneity, columns (4) to (6) of Table 6 present the achievement analysis by institution type. Among public schools, there is not a significant correlation between over-reporting and achievement (column 4). Among private nonprofit and for-profit schools, we find a negative relationship between over-reporting and achievement. Over-reporting schools have lower achievement by approximately 15% of an SD (columns 5 and 6). This finding suggests that, even after controlling for student composition, manipulation is driving resources to worse-performing schools, countering the traditional notion of an education market where more resources go to better schools.

Over-reporting and use of resources

Our previous results raise a question regarding the use of the "extra" resources that do not result in higher achievement. One hypothesis is that achievement differentials would be even larger if these "extra" resources were not available. On the other hand, schools might use the additional funding for inputs unrelated to achievement. One concern regarding for-profit institutions in Chile is that they might extract rents by investing public funding in real estate assets or paying inflated rent prices to school owners. Furthermore, new government regulations, introduced in 2015, prohibited subsidized schools from having profit motives and restricted profit withdrawals. This could encourage such rent-extracting behavior.

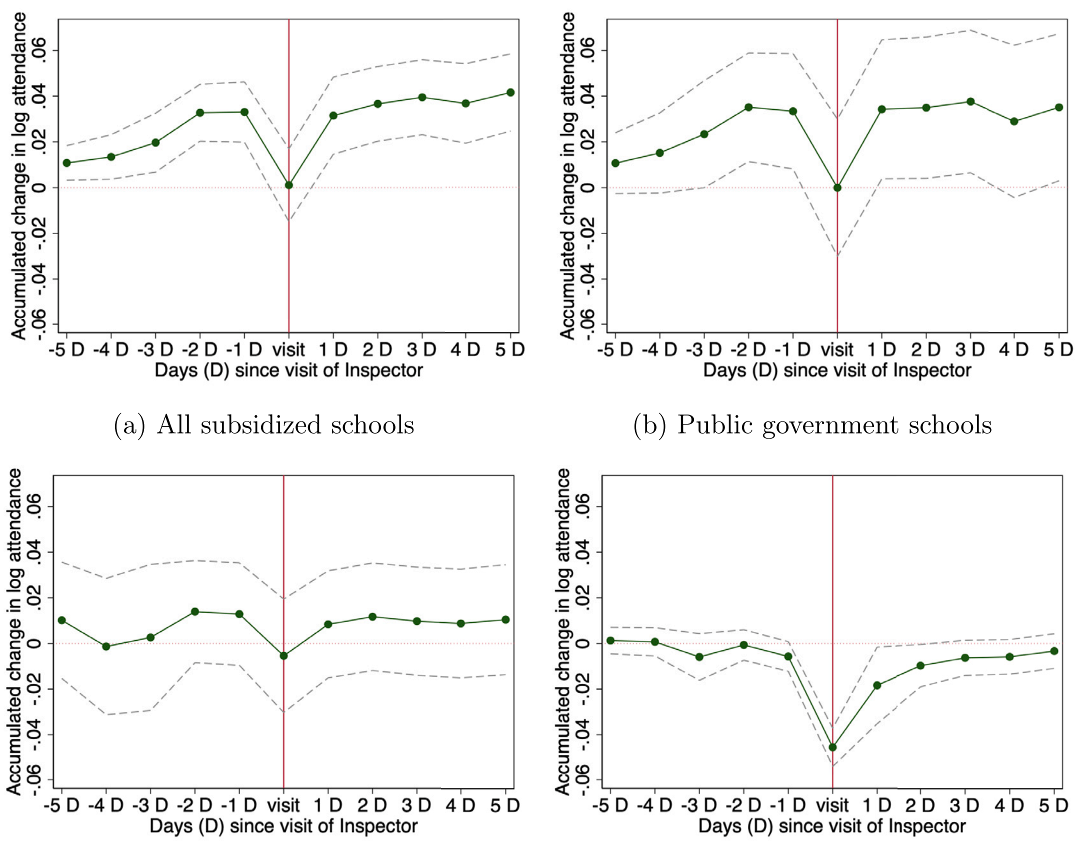
To test whether over-reporting schools spend more resources on real estate (purchase, construction, or rent), we use detailed SE data on school expenditures. We classify these expenditures as human resources

[28] *Sistema Nacional de Evaluación del Desempeño de los Establecimientos Educativos Subvencionados*. See Mizala and Urquiola (2013) for further details.

Table 6
OLS regression on school achievement and over-reporting.

Variables	(1) achiev. all	(2) achiev. all	(3) achiev. all	(4) achiev. public	(5) achiev. nonprofit	(6) achiev. for-profit
Significant over-report	−0.412***	−0.088***	−0.086***	−0.004	−0.158**	−0.143***
	(0.0275)	(0.0225)	(0.0223)	(0.0274)	(0.0671)	(0.0423)
Percent low SES students		−4.034***	−3.613***	−2.853***	−3.961***	−3.515***
		(0.0867)	(0.0990)	(0.2033)	(0.2206)	(0.1449)
Institution in rural area		0.392***	0.388***	0.408***	−0.005	0.301***
		(0.0416)	(0.0417)	(0.0531)	(0.1524)	(0.0945)
Log enrollment		0.211***	0.214***	0.178***	0.189***	0.305***
		(0.0189)	(0.0190)	(0.0281)	(0.0597)	(0.0311)
Nonprofit school			0.368***			
			(0.0375)			
For-profit school			0.224***			
			(0.0309)			
Constant	0.128***	1.284***	0.883***	0.605**	1.568***	0.454**
	(0.0183)	(0.1465)	(0.1537)	(0.2573)	(0.4411)	(0.2271)
Observations	12,246	12,246	12,246	6,832	1,540	3,874
R-squared	0.036	0.510	0.522	0.373	0.730	0.601
Municipality FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes

Note: Clustered standard errors at school level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the SNED's normalized "effectiveness" score for a particular school. The first independent variable is a dummy variable for whether the school was identified as significantly over-reporting. The sample in columns 1 to 3 corresponds to all schools that were audited. Columns 4, 5, and 6 include results for the subsamples of audited public, nonprofit, and for-profit schools.



(a) All subsidized schools (b) Public government schools

(c) Private nonprofit subsidized (d) Private for-profit subsidized

Fig. 5. Change in log of attendance around audit day, sample without "high risk of over-reporting" schools, years 2015 and 2016.

Table 7
Truncated regression for expenditure classes, by school type.

Variables	(1) ln human exp pc	(2) ln educ exp pc	(3) ln oper exp pc	(4) ln build exp pc	(5) ln purchase exp pc	(6) ln rent exp pc
Panel A: Public Schools						
Significant over-report	0.081***	0.045	0.041	0.112***	0.108	0.365**
	(0.0244)	(0.0324)	(0.0276)	(0.0342)	(0.0899)	(0.1478)
Percent low SES students	0.646***	1.296***	0.753***	0.407***	1.224***	0.131
	(0.1013)	(0.1350)	(0.1308)	(0.1435)	(0.3822)	(0.6759)
Institution in rural area	0.074**	−0.105**	0.202***	0.128***	0.457***	0.481*
	(0.0359)	(0.0426)	(0.0384)	(0.0421)	(0.1207)	(0.2671)
Constant	7.196***	3.036***	4.682***	2.621***	1.351***	−2.555***
	(0.0724)	(0.1558)	(0.1609)	(0.1166)	(0.4976)	(0.8067)
Observations	3,444	3,459	3,466	3,264	1,073	1,011
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Nonprofit Schools						
Significant over-report	0.022	0.015	0.058	0.016	−0.280	0.404
	(0.0377)	(0.0785)	(0.0654)	(0.1088)	(0.2331)	(0.3855)
Percent low SES students	0.195***	2.095***	0.285*	−0.651**	0.547	−3.694***
	(0.0748)	(0.1871)	(0.1479)	(0.2574)	(0.4360)	(0.9515)
Institution in rural area	0.188***	−0.206	0.419***	−0.106	−0.015	−1.285
	(0.0473)	(0.1372)	(0.0924)	(0.1505)	(0.4629)	(0.8092)
Constant	7.196***	3.820***	5.158***	4.929***	2.007***	0.640
	(0.0763)	(0.1859)	(0.1671)	(0.4752)	(0.5373)	(3.7668)
Observations	790	786	791	778	524	503
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: For-profit Schools						
Significant over-report	0.014	−0.151***	−0.047	−0.020	0.354**	0.247**
	(0.0208)	(0.0521)	(0.0445)	(0.0493)	(0.1414)	(0.1044)
Percent low SES students	0.034	2.786***	0.062	−0.216	0.385	−0.864**
	(0.0481)	(0.1503)	(0.1402)	(0.1462)	(0.4022)	(0.3696)
Institution in rural area	0.254***	0.004	0.680***	0.516***	0.878***	0.783***
	(0.0320)	(0.0951)	(0.0794)	(0.0920)	(0.2724)	(0.2285)
Constant	7.173***	2.258***	5.376***	3.517***	1.959***	4.282***
	(0.0489)	(0.2125)	(0.1575)	(0.2342)	(0.6117)	(0.5032)
Observations	2,074	2,034	2,074	2,034	1,222	1,496
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Clustered standard errors at school level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variables are the logarithm of the per-student expenditure in six different expenditure classes. The three panels present separate results for the three school types, for the periods where the data was available. The first independent variable is a dummy variable for whether the school was identified as significantly over-reporting. All specifications include regional fixed effects. Sample sizes vary as a function of the non-zero observations on expenditure on a particular class and school type.

(salaries, bonuses, etc.), education (learning materials, student welfare, consulting, training, etc.), operations (utilities, financial reallocations, etc.), building and maintenance of infrastructure, purchases of physical assets (real and movable property), and rent of physical assets (real and movable property). Based on this classification, Table 7 presents truncated regression results for the logarithm of the per-student enrolled expenditure across expenditure classes as a function of the over-reporting classification and socioeconomic/geographic factors, as well as our measure of the age of the institution.[29] Panel A shows that over-reporting public schools spend more on human resources (column 1), building infrastructure (column 4), and rent (column 6). Moving to Panel B, we present findings for over-reporting nonprofit schools. However, we cannot discern any statistical difference given the high variability of the estimates, a result of the limited number of nonprofit schools in our sample. Lastly, Panel C shows the results for for-profit schools. The results from column (2) suggest that for-profit schools classified as over-reporters spend less on education inputs than other for-profit schools, while columns (5) and (6) show that the same schools spend more on acquiring and renting physical assets.

In sum, our results show that manipulation is also correlated with a different allocation of resources. In particular, it drives public subsidies toward low-achievement for-profit schools, which use part of the extra resources on real estate expenditures, potentially as a vehicle for rent extraction.

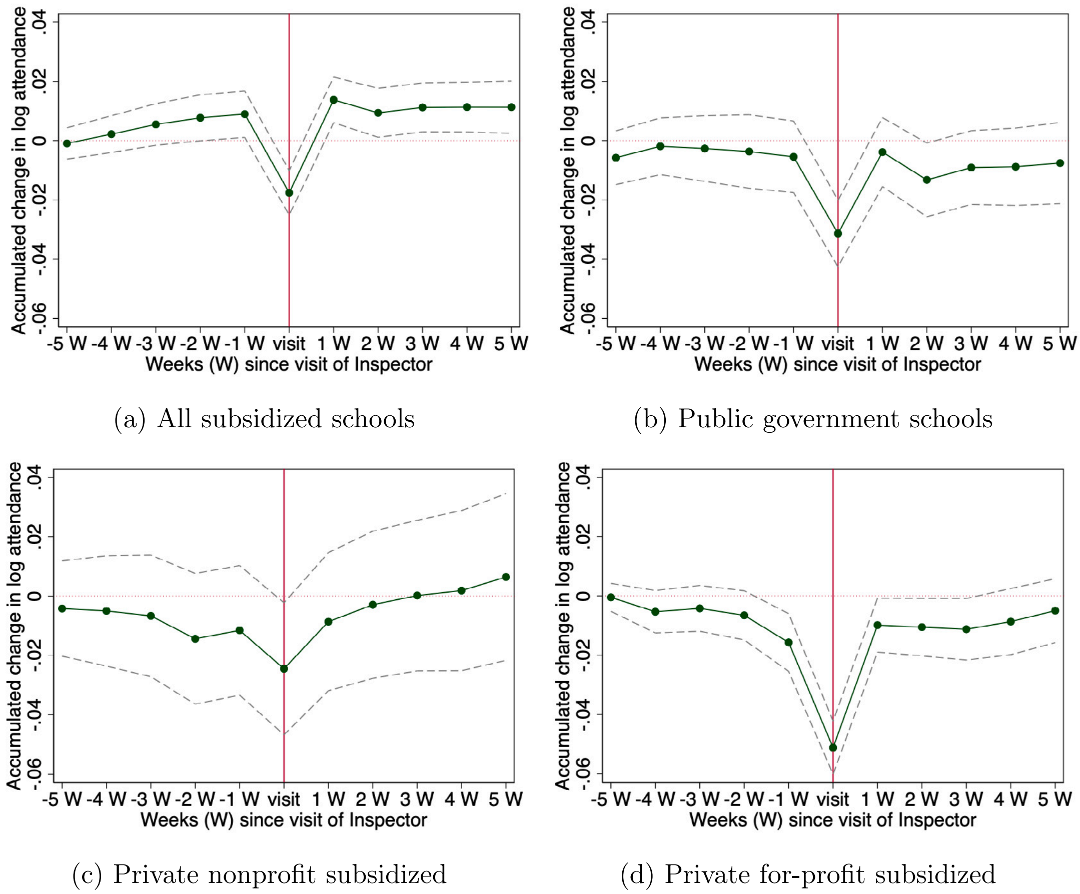
One possible explanation of the achievement results (higher over-reporting among low-performing schools) could be related to a mismatch between costs (related to enrollment) and attendance-based revenue (related to attendance). If fixed costs are related to enrollment (number of rooms, teachers, tables, etc.), then institutions with high attendance volatility students (such as students in poor or rural areas) would have higher difficulties covering their fixed costs from a revenue stream of attendance-related funding. In response, these schools may over-report. The results from this section, however, make this explanation highly unlikely as it shows that the additional resources obtained by high over-reporting schools are not spent on direct quality-enhancing inputs. The alternative rent-seeking explanation seems to be more plausible.

5. Robustness and accountability

5.1. Robustness

Next, we will test how robust are our results to alternative samples or specifications. First, we know our sample is not random. Therefore, our results might be exaggerated because of SE auditing schools that

[29] We use a truncated regression because some expenditure classes (mainly purchases/rent of assets) have zero expenditure.



(a) All subsidized schools

(b) Public government schools

(c) Private nonprofit subsidized

(d) Private for-profit subsidized

Fig. 6. Change in log of attendance around audit day, window of analysis of 10 weeks.

are suspects of manipulation. Therefore, we will drop schools that were audited because of a "high risk of manipulation" in the years 2015 and 2016.[30] Results are presented as event study graphs in Fig. 5.[31]

The results have larger confidence intervals, due to a smaller sample of audited schools. Nevertheless, we find the same pattern as with the full-sample results: there is a drop in the visit day attendance. In particular, attendance drops by 3.1% for all schools, 3.3% for public schools, 1.8% for private nonprofit schools, and 3.9% for private for-profit schools. Therefore, "high-risk schools" do not drive our main results.

A second observation is that although audit visits do not create sustainable corrections in manipulation during the following week, it is possible that manipulation decreases later (after a school meeting, for example). Thus, we extend the window of analysis to 5 weeks before and after the audit day and estimate equation (2), but replace daily dummies with weekly dummies. Fig. 6 shows the medium-term evolution of attendance.[32] Although the evolution of attendance looks smoother, schools have returned to their previous attendance levels after one week. Therefore, the small window of analysis does not drive our results.

5.2. Accountability

Thus far, we have established that institutions over-report attendance and that top-down standard monitoring does not correct this behavior. In this section, we test whether accountability pressure, in the form of additional visits and fines, results in better attendance reporting practices.

In particular, another approach to detect long-term audit effects is to measure the over-reporting rates using a second audit visit. The SE revisits institutions with prior attendance reporting problems. Although institutions might suspect that a new visit is coming, they do not know the exact date of the second visit. An expectation of a second audit is not a problem if the belief of a second audit deters misbehavior. Sub Fig. 7(a) show the results for second visits in an academic year. We see that the change in behavior is only marginal since the over-reporting rate is similar to the first visit's rate and the reported attendance rapidly returns to pre-visit levels.[33] Consequently, we do not find evidence of long-term reductions in over-reporting from second audits.

As mentioned previously, the SE can impose fines on institutions that engage in misbehavior. In the year 2014, the average fine in cases that included attendance misbehavior was 70 UTM (4,970 USD), while the median fine was 54 UTM (3,934 USD).[34] To test whether

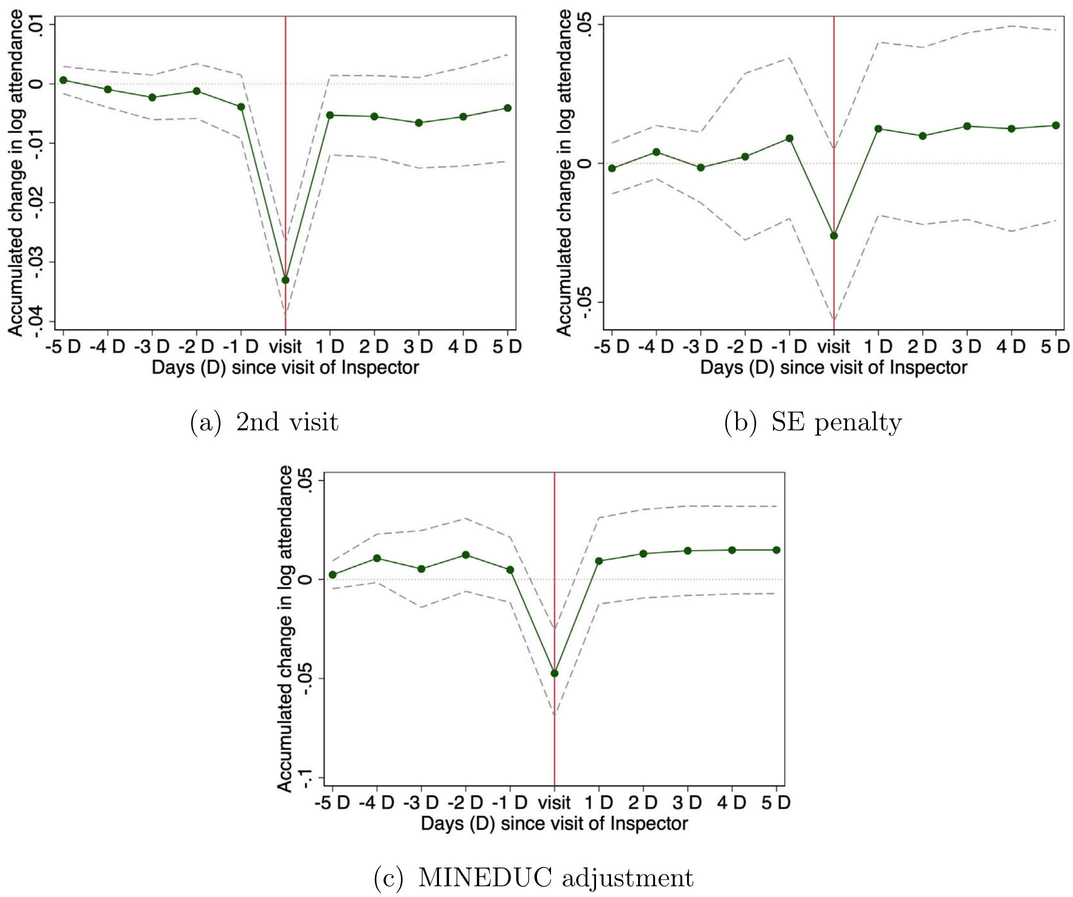
[30] We lack the information for other years.

[31] We cannot report the full regression results because of confidentiality agreements.

[32] Regression results are available in Appendix Table C.5.

[33] Regression results are available in Appendix Table C.6.

[34] The fine is set for all the misbehaviors detected during a visit.



(a) 2nd visit

(b) SE penalty

(c) MINEDUC adjustment

Fig. 7. Analysis of over-reporting under different accountability tools,2013–2016.

financial penalties can correct misbehavior, we examine the subsample of institutions that were audited and fined for incorrect attendance reporting. Note that this is the textbook monitoring situation: there is an audit, it detects a problem, and a penalty ensues. Once again, Sub Fig. 7(b) does not show a decline in (relatively large) over-reporting behavior. Institutions substantially decrease their attendance on the audit day, only to return to their previous behavior on subsequent days.[35]

Finally, the MINEDUC can adjust funding based on consistent evidence that an institution over-reports attendance. Therefore, schools might stop fraud after observing a funding adjustment by the MINEDUC, which signals that over-reporting will cause discounts. The average funding adjustment in the data, conditional on its existence, is equivalent to 5.4% of the school revenue due to public subsidies. To study the effects of the discounts, we analyze over-reporting in the subsample of institutions that had funding adjustments in the preceding year. Sub Fig. 7(c) shows that institutions still over-report the year following the funding penalty.[36]

In conclusion, we find that attendance fraud is quite robust to the existing accountability tools. This casts doubt on the ability of top-down monitoring to address manipulation in a heterogeneous and decentralized social service provision system.

[35] Regression results are available in Appendix Table C.7.

[36] Regression results are available in Appendix Table C.8.

6. Conclusions

In this paper, we present evidence that schools manipulate attendance reports to increase revenue. Although we identify over-reporting schools of all types (including public schools), this behavior is more prevalent among private for-profit schools, and schools with a higher share of high-priority students; and less likely among schools with higher enrollment, in rural areas or that have been operating for a longer time. In terms of achievement, over-reporting nonprofit and for-profit schools tend to underperform relative to equivalent schools in the same group. This implies several challenges to social services provision. First, the manipulation of monetary incentives introduces the need for robust monitoring systems, which are often expensive and do not necessarily guarantee regulatory compliance. Second, the greater manipulation among private schools (although manipulation among public schools is also important) suggests important regulatory challenges for a social service market. These results confirm concerns about the role of for-profit institutions in education, which (Cellini et al., 2020; Deming et al., 2012) have confirmed for higher education. Third, the results also suggest that manipulation introduces a public funds distribution where inferior quality schools can capture more resources, even in a system based on competition. Hence, the consequences of manipulation are not only moral; the quality of education decreases. Fourth, an analysis of expenditure patterns suggests that over-reporting public schools may use part of the additional resources on human resources, building infrastructure, and rent; whereas for-profit schools identified as over-reporters spend less on education inputs than other for-profit schools and more on acquiring and renting physical assets.

Our results also have implications for the use of market tools in the provision of social services. Although markets reward positive innovations, our findings show that negative forms of innovation, such as novel forms of "cheating the government", can also generate rewards and, in fact, may allow bad social service providers to survive. How the government can keep up with "negative innovations" and how malpractices propagate across systems are open questions for future research. Also, whether over-reporting is more intensive in more competitive markets (as seems to be the case in urban settings) or how the public–private competitive mix affects this relationship are additional areas of prospective research.

Over-reporting results among public schools, although smaller than for-profit schools, is also a cause of concern, especially considering that they represent close to 60% of our sample. The evidence on achievement and expenditure suggests that some public institutions may resort to this practice as a way to finance their operational budgets while maintaining similar performance to other public schools.

Finally, the magnitude of the school subsidies that went to over-reporting schools is not negligible. Expanding the over-reporting findings to all the schools in the country, we estimate that approximately 95,000 students would have been over-reported in 2016, implying an overpayment of US$ 125 million, equivalent to 2.8% of the total voucher budget of the previous year.[37] This is the amount that could be saved if perfect monitoring was feasible or the deterrence scheme was effective. One possibility is to use those prospective savings to invest in more efficient monitoring technology. Although feasible, the technology would have to be able to identify each student separately and, more importantly, parents would have to agree to such type of digital recording. An alternative would be to give up attendance monitoring altogether and pay schools based on enrollment; registration is a much easier variable to monitor or verify but alternative schemes to promote daily attendance would have to be put in place to guarantee learning for all types of students.

Data availability

The data that has been used is confidential.

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[37] To estimate the number of over-reported students, we used the school-specific estimates that were considered significant for the day of the visit (see Section 5.2). We then calculated the average over-reporting rate within groups defined by school type, rural location, size, and low-SES percentage. We applied the estimated over-reporting to the average annual attendance to recover the absolute number of students per school. Finally, we multiply the number of over-reported students by the average monthly payment and by 12 months. We did not include the additional payment associated with the Preferential School Subsidy, which can be significant.

Table A.1
Over-reporting of attendance estimation by MINEDUC.

Step	Variable	Result
(1)	Observed difference in the school attendance (η_{school})	10%
(2)	Average difference in province on the same day (ε_{day})	4%
(3)	Average difference in province in the same month (ε_{month})	1%
(4)=(2)–(3)	Estimated attendance shock in province on the day (η_{day})	3%
(5)=(1)–(4)	Estimated over-reporting at school	7%

Appendix A. MINEDUC estimations

Here, we specify how the MINEDUC calculates "divergence" measures (" *divergencias*") from daily reports. The divergence (for institution i, day d, and month m) is calculated in the following steps:

(1) The MINEDUC estimates the attendance shock in the school as the difference between the average attendance reported by the institution in the previous month and the attendance observed by the inspector ($\eta_{school} = \bar{y}_{month} - y_{day}$).
(2) The MINEDUC calculates the average shock in step 1 in all the schools inspected in the province that day ($\bar{\varepsilon}_{day}$).
(3) The MINEDUC calculates the average shock in step 1 in all the institutions inspected in the province that month ($\bar{\varepsilon}_{month}$).
(4) The MINEDUC separates the daily component of the shocks as the difference between the averages calculated in steps 2 and 3 ($\eta_{day} = \bar{\varepsilon}_{day} - \bar{\varepsilon}_{month}$).
(5) Finally, the MINEDUC calculates the divergence at the institution level as the difference between the estimated shock at the institution and the estimated daily component of the shock (over-reporting $= \eta_{school} - \eta_{day}$).

Given the complexity of this over-reporting formula, we present a practical example in Table A.1. A particular school shows 10% higher attendance during the month in comparison to the attendance observed in the audit. However, other schools visited that day also show a difference in attendance (4%), as do other schools visited that month (1%). The MINEDUC estimates the attendance shock of that particular day as the 3% difference. Finally, by subtracting 3% from the observed difference in attendance (10%), the MINEDUC estimates the school's over-reporting to be 7%.

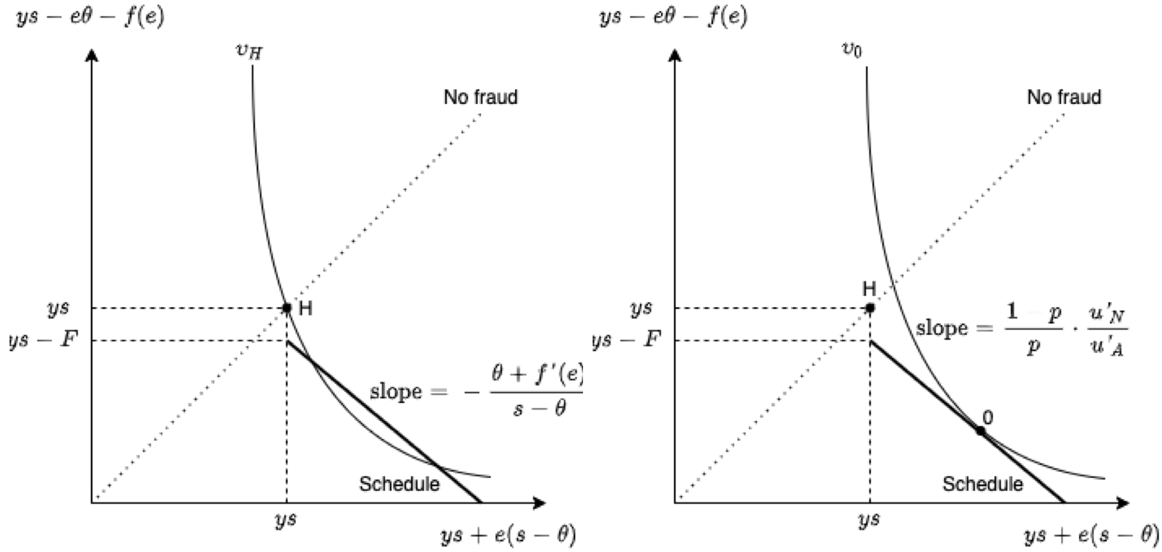
Appendix B. A simple model of deterrence

In our case, the problem facing schools regarding how much attendance to report is similar to that of taxpayers deciding how much income to report. A classic model in the latter literature is based on the deterrence framework introduced by Allingham and Sandmo (1972), who adapt the (Becker, 1968) model of criminal behavior to tax evasion. We modify the (Slemrod, 2019) deterrence model to include a proportional subsidy rather than a proportional tax. As we are particularly interested in heterogeneity across institutions, we include a moral/administrative cost parameter θ associated with manipulation. In making their reporting decision, we assume that schools solve the following expected utility[38] maximization problem:

$$\max_{e\geq 0} \quad v(e) = (1-p)\cdot u(ys + e(s-\theta)) + p \cdot u(ys - e\theta - f(e)) \tag{4}$$

which depends on the "revenue" without an audit ($ys + e(s-\theta)$) and with an audit ($ys - e\theta - f(e)$). The variable $e = \bar{y} - y$ is the

[38] The expected utility can also be interpreted as an expected (generic) profit.



(a) Honest behavior (b) Optimal decision

Fig. B.1. Fraud decision model.

level of manipulation given by the difference between the reported ($\overline{y}$) and actual (y) attendance. The probability of being audited is p.[39] Meanwhile, $u(\cdot)$ is a standard convex utility function ($u' > 0, u'' \leq 0$) that captures the benefit of income or, alternatively, the reputational benefit from the higher expenditure. The penalty $f(e)$ is an increasing function of the detected amount of over-reporting and includes fines, reputational harm, and legal costs (we assume that $f(0) = 0$). Regarding the moral/administrative cost θ, we may think that for a strictly short-term profit-oriented school, $\theta = 0$; while for schools with long-term objectives and brand awareness, $\theta > 0$. Such heterogeneity is important given the high turnover rates among schools in Chile (Grau et al., 2018).

On the intensive margin of manipulation, the first-order condition corresponds to the ratio between the marginal utilities under no audit (N) and under audit (A):

$$\frac{1-p}{p} \cdot \frac{u'_N}{u'_A} = \frac{1-p}{p} \cdot \frac{u'(ys + e^*(s-\theta))}{u'(ys - e^*\theta - f(e^*))} = \frac{\theta + f'(e^*)}{s-\theta} \tag{5}$$

Fig. B.1 shows the graphical analysis of the decision model, assuming a penalty function $f(e > 0) = F - ke$ that includes a lump-sum fine F plus a penalty rate k.[40] The subfigures show on the horizontal axis the input level under no audit and on the vertical axis the input level under audit. The 45° dotted line represents the allocations with no over-reporting, where point H is the honest allocation of a particular school under analysis. Point H has an isoutility curve v_H. The subfigures also show the schedule of available allocations for over-reporting, which start at point $(ys, ys - F)$. The slope of the schedule is the right-hand term of Eq. (5): the ratio between the unit cost of manipulation in the case of detection ($\theta + f'(e)$) and the additional revenue generated by each unit of manipulation ($s - \theta$). Regarding the extensive margin, Sub Fig. B.1(a) assumes an isoutility curve v_H of point H that crosses the schedule; thus, the school will over-report attendance because there is at least one $\tilde{e} > 0$ such that $v(0) < v(\tilde{e})$.[41] Moreover, honest behavior is more likely with a higher initial penalty fee F (moving the schedule down), a higher probability of detection p (flatter isoutility curve), and a higher moral/administrative cost θ. The latter follows since θ decreases the expected utility of dishonest behavior.

Next, Sub Fig. B.1(b) shows the interior solution, where the isoutility curve is tangent to the available schedule (Eq. (5)). Increases in the marginal penalty ($f'(e)$) or higher probabilities of detection (p) result in lower over-reporting (e^*). With regard to the moral/administrative cost θ, we can see that the right side of Eq. (5) increases with θ, since the schedule that the school faces becomes steeper. On the left, we have that the ratio of marginal utilities: (i) decreases with θ if the coefficient of absolute risk aversion ($\gamma = -u''/u'$) is decreasing; and (ii) does not change if γ is constant. Therefore, under decreasing or constant absolute risk aversion, we have that a higher moral/administrative cost decreases manipulation.[42]

In sum, heterogeneity in the moral/administrative cost, under standard assumptions, implies heterogeneity in manipulation: schools with higher reputation or moral costs (schools perceived as high-quality or associated with nonprofit motives) might be less likely to over-report attendance than those with purely short-term profit motives and little regard for reputation. A negative correlation between θ and service quality would imply an inefficient allocation of resources, as low-quality schools would receive higher subsidies due to the manipulation of attendance registries. In the next sections, we provide empirical evidence on the presence and heterogeneity of manipulation.

Appendix C. Additional tables

See Tables C.1–C.8.

[39] We assume p to be exogenous. This analysis is similar to an endogenous audit probability $p(e)$.

[40] Assuming a nonlinear penalty does not fundamentally change the analysis.

[41] If the penalty function is continuous at 0 (no lump-sum F), the institution will over-report if $\frac{\partial v(e)}{\partial e}|_{e=0} > 0$, which will occur whenever $\frac{1}{p} > 2 + \frac{f'(0)-1}{s-\theta}$.

[42] This predicted behavior does not hold in the case of a highly increasing γ, as the lower "revenue" (due to the higher moral/administrative costs) drives schools to take more or greater risks.

Table C.1
Registry of individual attendance for school voucher funds purposes.
Source: Source: *Circular* N°1, Superintendency of School Education (2014).

MES ______________________ DE ______________

DÍA	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	TOTAL MENSUAL	
N°																																ASIST	INASIST
1																																	
2																																	
3																																	
4																																	
5																																	
6																																	
7																																	
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14																																	
...																																	
...																																	
45																																	
ASIST																																	
INASIST																																	
MATRIC																																	

Table C.2
Registry of attendance in each class.
Source: Source: *Circular* N°1, Superintendency of School Education (2014).

DÍA ______________ MES ______________

HORA	SECTOR	INASISTENCIAS	ASISTENCIA TOTAL	ATRASOS	OBSERVACIONES	FIRMAS
1		2	43			
2						
3						
4						
5						
6						
7						
8						
9						
10						
11						
12						

Table C.3
Descriptive statistics of selected variables at the school level (means and standard deviations in parentheses) by inspection status.

Variable	During the year	
	Not inspected mean/sd	Inspected mean/sd
Attendance in the day	256.452	392.480
	(347.3142)	(359.5712)
Log of reported attendance	4.350	5.544
	(1.8766)	(1.0304)
Total enrollment	289.860	447.584
	(390.6795)	(405.0881)
Log(enrollment/attendance)	0.099	0.135
	(0.2326)	(0.2585)
Public government school	0.587	0.546
	(0.4923)	(0.4979)
Private nonprofit motive school	0.103	0.129
	(0.3038)	(0.3346)
Private for-profit motive school	0.310	0.325
	(0.4624)	(0.4685)
Institution in rural area	0.529	0.228
	(0.4992)	(0.4197)
Percentage high-priority students	0.665	0.639
	(0.2219)	(0.2020)
Number of institutions	8,455	8,461

Table C.4
Aggregate over-reporting, change in log-*reported*-attendance.

Variables	(1) Δ attend. all schools	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 days	0.0035**	0.0030	0.0001	0.0020
	(0.00136)	(0.00219)	(0.00375)	(0.00170)
Visit in 4 days	−0.0005	−0.0001	0.0005	−0.0027*
	(0.00127)	(0.00204)	(0.00307)	(0.00140)
Visit in 3 days	0.0003	−0.0015	0.0013	−0.0003
	(0.00150)	(0.00245)	(0.00135)	(0.00149)
Visit in 2 days	0.0016	0.0020	0.0025	−0.0010
	(0.00162)	(0.00304)	(0.00308)	(0.00194)
Visit tomorrow	−0.0017	−0.0008	−0.0018	−0.0059***
	(0.00168)	(0.00325)	(0.00211)	(0.00174)
Visit today	−0.0229***	−0.0221***	−0.0109***	−0.0316***
	(0.00150)	(0.00251)	(0.00220)	(0.00280)
Visit yesterday	0.0243***	0.0245***	0.0119***	0.0295***
	(0.00098)	(0.00148)	(0.00176)	(0.00253)
Visit 2 days ago	0.0013	−0.0023	0.0041***	0.0049**
	(0.00121)	(0.00187)	(0.00136)	(0.00210)
Visit 3 days ago	0.0013	0.0019	−0.0027	0.0025
	(0.00115)	(0.00182)	(0.00354)	(0.00156)
Visit 4 days ago	0.0016	0.0003	0.0043	0.0016
	(0.00165)	(0.00275)	(0.00357)	(0.00184)
Visit 5 days ago	0.0014	0.0018	−0.0032	0.0015
	(0.00117)	(0.00220)	(0.00224)	(0.00142)
Constant	−0.0012***	−0.0008***	−0.0022***	−0.0016***
	(0.00003)	(0.00001)	(0.00001)	(0.00001)
Observations	5,834,434	3,335,051	657,215	1,841,835
R-squared	0.0583	0.0635	0.1447	0.1074
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the daily change in the log attendance at a certain school, where attendance on the day is the visit corresponds to the reported attendance. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

Table C.5
Aggregate over-reporting, weekly change in log-attendance.

Variables	(1) Δ attend. all	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 weeks	−0.0002	−0.0012	−0.0008	−0.0001
	(0.00054)	(0.00092)	(0.00164)	(0.00048)
Visit in 4 weeks	0.0006	0.0008	−0.0002	−0.0010
	(0.00056)	(0.00091)	(0.00123)	(0.00060)
Visit in 3 weeks	0.0007	−0.0002	−0.0003	0.0002
	(0.00048)	(0.00085)	(0.00073)	(0.00052)
Visit in 2 weeks	0.0004	−0.0002	−0.0015	−0.0005
	(0.00050)	(0.00093)	(0.00110)	(0.00036)
Visit in week	0.0003	−0.0004	0.0006	−0.0018***
	(0.00048)	(0.00084)	(0.00097)	(0.00055)
Visit today	−0.0265***	−0.0259***	−0.0129***	−0.0355***
	(0.00170)	(0.00270)	(0.00307)	(0.00293)
Visited within week	0.0063***	0.0055***	0.0032***	0.0083***
	(0.00025)	(0.00039)	(0.00073)	(0.00042)
Visited 2 weeks ago	−0.0009***	−0.0019***	0.0011	−0.0001
	(0.00033)	(0.00053)	(0.00086)	(0.00034)
Visited 3 weeks ago	0.0004	0.0008	0.0006	−0.0001
	(0.00038)	(0.00061)	(0.00081)	(0.00051)
Visited 4 weeks ago	0.0000	0.0001	0.0003	0.0005
	(0.00036)	(0.00055)	(0.00078)	(0.00055)
Visited 5 weeks ago	−0.0000	0.0002	0.0009	0.0007
	(0.00033)	(0.00048)	(0.00086)	(0.00049)
Constant	−0.0012***	−0.0008***	−0.0020***	−0.0014***
	(0.00002)	(0.00004)	(0.00010)	(0.00006)
Observations	5,437,301	3,103,085	613,920	1,719,958
R-squared	0.0601	0.0663	0.1461	0.1072
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the weekly change in log attendance at a certain school. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

Table C.6
Aggregate over-reporting, change in log of attendance, schools audited for a second time.

Variables	(1) Δ attend. all schools	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 days	0.0006	0.0013	−0.0052	−0.0022
	(0.00117)	(0.00142)	(0.00403)	(0.00136)
Visit in 4 days	−0.0016	−0.0013	0.0002	−0.0007
	(0.00115)	(0.00176)	(0.00242)	(0.00097)
Visit in 3 days	−0.0013	−0.0024	−0.0014	−0.0024*
	(0.00128)	(0.00192)	(0.00195)	(0.00124)
Visit in 2 days	0.0011	0.0021	0.0004	−0.0034**
	(0.00187)	(0.00276)	(0.00246)	(0.00132)
Visit in 1 day	−0.0027*	−0.0035	−0.0037**	−0.0126***
	(0.00161)	(0.00235)	(0.00182)	(0.00130)
Visit today	−0.0292***	−0.0320***	−0.0263***	−0.0900***
	(0.00166)	(0.00224)	(0.00310)	(0.00309)
Visit 1 day ago	0.0278***	0.0314***	0.0246***	0.0856***
	(0.00150)	(0.00185)	(0.00279)	(0.00276)
Visit 2 days ago	−0.0002	−0.0021	0.0048***	0.0096***
	(0.00119)	(0.00170)	(0.00180)	(0.00126)
Visit 3 days ago	−0.0011	−0.0021	−0.0029	0.0043***
	(0.00128)	(0.00172)	(0.00266)	(0.00117)
Visit 4 days ago	0.0010	0.0006	0.0017	0.0026**
	(0.00153)	(0.00214)	(0.00255)	(0.00113)
Visit 5 days ago	0.0015	0.0035*	−0.0010	0.0029**
	(0.00133)	(0.00198)	(0.00210)	(0.00114)
Constant	−0.0006***	0.0001***	−0.0020***	−0.0007***
	(0.00001)	(0.00002)	(0.00002)	(0.00002)
Observations	5,689,076	3,365,310	725,118	3,383,963
R-squared	0.0703	0.0806	0.1549	0.0955
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the daily change in log attendance at a certain school. Only the second visits within the academic year were considered in the definition of the visit's dummies. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

Table C.7
Aggregate over-reporting, change in log of attendance, schools penalized by SE.

Variables	(1) Δ attend. all schools	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 days	−0.0018	−0.0039	0.0011	−0.0057
	(0.00467)	(0.00682)	(0.00437)	(0.00623)
Visit in 4 days	0.0059	0.0112*	−0.0000	0.0076
	(0.00370)	(0.00619)	(0.00793)	(0.00572)
Visit in 3 days	−0.0056	−0.0075	0.0055	−0.0154*
	(0.00435)	(0.00600)	(0.00773)	(0.00925)
Visit in 2 days	0.0039	−0.0057	0.0289	0.0083
	(0.01272)	(0.01956)	(0.03598)	(0.00919)
Visit tomorrow	0.0066	0.0186	0.0088	−0.0145**
	(0.00883)	(0.01531)	(0.00717)	(0.00711)
Visit today	−0.0351***	−0.0430***	−0.0246***	−0.0382***
	(0.00548)	(0.00697)	(0.00715)	(0.01209)
Visit yesterday	0.0385***	0.0457***	0.0133**	0.0502***
	(0.00413)	(0.00816)	(0.00527)	(0.00943)
Visit 2 days ago	−0.0026	−0.0066	0.0118*	−0.0065
	(0.00342)	(0.00491)	(0.00676)	(0.00889)
Visit 3 days ago	0.0035	0.0053	−0.0031	0.0014
	(0.00498)	(0.00824)	(0.00598)	(0.00580)
Visit 4 days ago	−0.0009	−0.0071	−0.0061	0.0077
	(0.00952)	(0.01609)	(0.00769)	(0.00707)
Visit 5 days ago	0.0012	0.0017	−0.0029	0.0060
	(0.00872)	(0.01599)	(0.00836)	(0.00582)
Constant	−0.0024***	−0.0023***	−0.0023***	−0.0024***
	(0.00009)	(0.00014)	(0.00032)	(0.00007)
Observations	150,451	84,170	13,877	48,420
R-squared	0.1409	0.1880	0.2855	0.2130
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The dependent variable is the daily change in log attendance at a certain school. The sample only includes institutions that were previously audited and fined for incorrect attendance reporting. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

Table C.8
Aggregate over-reporting, change in log of attendance, schools penalized by MINEDUC.

Variables	(1) Δ attend. all schools	(2) Δ attend. public	(3) Δ attend. nonprofit	(4) Δ attend. for-profit
Visit in 5 days	0.0024	−0.0017	0.0047	0.0035
	(0.00359)	(0.00504)	(0.00449)	(0.00468)
Visit in 4 days	0.0083	0.0133	−0.0018	0.0019
	(0.00594)	(0.00974)	(0.00348)	(0.00553)
Visit in 3 days	−0.0054	−0.0105	0.0076	−0.0051
	(0.00697)	(0.01132)	(0.00561)	(0.00359)
Visit in 2 days	0.0071	0.0093	−0.0051	−0.0008
	(0.00526)	(0.00831)	(0.00515)	(0.00318)
Visit tomorrow	−0.0076*	−0.0109*	−0.0097*	−0.0068
	(0.00441)	(0.00650)	(0.00531)	(0.00530)
Visit today	−0.0523***	−0.0467***	−0.0239	−0.0686***
	(0.00716)	(0.00994)	(0.02375)	(0.00723)
Visit yesterday	0.0567***	0.0547***	0.0266	0.0635***
	(0.00330)	(0.00415)	(0.02245)	(0.00546)
Visit 2 days ago	0.0037*	0.0033	0.0025	0.0021
	(0.00221)	(0.00241)	(0.00419)	(0.00415)
Visit 3 days ago	0.0015	0.0027	−0.0047	0.0041
	(0.00215)	(0.00330)	(0.00610)	(0.00301)
Visit 4 days ago	0.0003	0.0002	−0.0034	−0.0026
	(0.00354)	(0.00545)	(0.00612)	(0.00435)
Visit 5 days ago	0.0001	−0.0011	−0.0002	0.0032
	(0.00219)	(0.00303)	(0.00539)	(0.00372)
Constant	−0.0010***	−0.0010***	−0.0010***	−0.0009***
	(0.00003)	(0.00005)	(0.00002)	(0.00002)
Observations	499,675	264,160	29,409	203,609
R-squared	0.0781	0.1164	0.2614	0.1063
Region-date FE	Yes	Yes	Yes	Yes

Note: Clustered standard errors at a school-year level in parentheses, *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Dependent variable is the daily change in log attendance at a certain school. The sample only includes institutions that had funding adjustments in the preceding year. Column 1 includes daily observations and all schools. Columns 2, 3, and 4 include results for the subsamples of public, nonprofit, and for-profit schools.

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