

Managers' Productivity and Labor Market: Evidence from School Principals

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Abstract

We investigate whether differences in management explain variation in productivity and whether different labor market policies can impact the allocation of managerial effectiveness. Using data on the universe of students and school personnel in Chile, we establish three main findings. First, there is substantial variation in principals' ability to improve students' learning. Second, effective principals are recognized by the school community, decrease teachers' turnover, and obtain higher wages, especially in private schools. Third, despite relatively rigid wages, public schools can attract better principals by improving personnel selection.

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1 Introduction

While it is beyond dispute that the quality of the workforce is a key resource of both private and public enterprise (e.g., Lazear and Shaw, 2007; Finan et al., 2015), only recently have scholars begun to gather observational estimates of worker-specific productivity. Moreover, despite growing evidence that management practices matter (e.g., Bloom et al., 2013), little is known about the productivity of workers in managerial positions and its relation with their labor market outcomes. In particular, there is scant evidence on whether managers' productivity can be recognized by their workers and labor market; and whether differences in personnel selection and wage schemes can help explain the allocation of managerial effectiveness.

In this article, we study managers in the educational context to contribute with new evidence on these important questions. Despite much work on the effects of teachers and schools on students' outcomes, research on the effects of school managers is still meager. This is probably due to the challenge of disentangling the effect of the school principal from other school-related factors, and the dearth in data to do it. Similarly, the study of principals' labor market has been hindered by the prevalence of fixed-wage schemes in education and the hurdles to find variation in personnel selection policies. We overcome these limitations by leveraging the institutional setting and the rich administrative data from Chile that allows us to construct objective measures of managers' performance and to study its relation with wages and personnel selection in the labor market.

We develop a novel extension of the canonical teacher value-added model (VAM) that can be used to measure principals' effectiveness. We find substantial variation across principals in their ability to improve students' learning. A one standard deviation increase in principal effectiveness raises students' course grades by 0.22 standard deviations.¹ Using this measure of effectiveness we investigate various dimensions of principals' management and labor market outcomes, with special attention to the differences between private and public schools. We obtain two main results. First, effective principals are recognized by the school community, increase the retention of teachers, and their effectiveness is compensated in the labor market, especially in private schools. Second, despite having a wage scheme more rigid than private schools, public schools can attract better principals by increasing the competitiveness and transparency of their personnel selection process.

¹A non-trivial effect size in the Chilean context, where a one standard deviation increase in course grades is associated with an 11 percent increase in wages (conditional on test scores, school characteristics, and students demographics).

The first part of our study measures principal effectiveness. We estimate a value-added model using more than 9.8 million student-subject-year observations. This model relates academic achievement to student and school characteristics, and to 9,944 principal fixed effects. An important challenge in the estimation of principals' effects, that does not apply in teacher value-added modeling, is the need to disentangle the effect of the principal from other school-level factors. To address this issue, we follow the seminal work by [Mundlak \(1978\)](#), and the more recent work of [Altonji and Mansfield \(2018\)](#), and include several school-level controls in a correlated random effects fashion. This approach not only allows us to obtain an estimate of effectiveness for all school principals, but also overcomes problems of weak identification that could arise with a two-way school-principal fixed effects model (e.g., [Jochmans and Weidner, 2019](#)). A related challenge is distinguishing principals' effectiveness from the average effectiveness of the school's teachers. Since teachers' effectiveness is itself a potential mechanism by which principals influence student achievement, we do not take teacher effects into account directly. However, we use the average residual at the classroom level as a *within principal* proxy of teachers' value-added.²

In Chile, students do not take nationwide standardized tests every year; thus we measure students' performance in Mathematics and Spanish using their course grades.³ While we show that our results are robust to an alternative measure of principal effectiveness based on test scores, we argue that course grades are better suited to our purposes for the following reasons. First, Chile has a standardized national curriculum and consequently test scores and course grades are strongly positively correlated, supporting the view of using them interchangeably (e.g., [Borghans et al., 2016](#)). Second, as is common in value-added models, we examine performance longitudinally, which should mitigate concerns about grade inflation. Third, grades also capture non-cognitive traits, which are valued by the labor market (e.g., [Bowles and Gintis, 1976](#); [Bowles et al., 2001](#); [Heckman et al., 2006](#)). Indeed, course grades—and not test scores—determine grade retention and high school graduation. Fourth, estimates of principal effectiveness are more precisely estimated using course grades than test scores, as we observe the former every year for all students.

We use our measure of principal effectiveness to investigate several questions. First, do better principals increase teacher retention? Teacher shortages and high turnover rates have recently received considerable attention (e.g., [Ronfeldt et al., 2013](#); [Hanushek et al., 2016](#)), and educational research have found that support from principals is one of the best predictors of teacher attrition (e.g., [Grissom, 2011](#); [Hughes et al., 2015](#)). To investigate this, we leverage a rich employer-employee

²We do not need to construct empirical Bayes posterior estimates of teacher effectiveness, as in [Kane and Staiger \(2008\)](#), because we do not use teacher's value-added as an explanatory variable.

³As in [Peteck and Pope \(2019\)](#), we address the concern that teachers can directly impact grades by using the lead of course grades as the outcome variable, and we estimate the model on a sample of students who changed their teacher from one year to another.

panel from 2008 to 2016 to estimate a model with teacher and year fixed effects. We find that a one standard deviation increase in principal effectiveness decreases the average exit rate of teachers by 9%. This result is mostly driven by private schools, where principals have more leeway to make firing and hiring decisions. In these schools, the principal’s effectiveness is particularly important to decrease the turnover of the younger and higher value-added teachers; and our observational estimates of turnover elasticity suggest that teachers would be willing to pay 8% of their wages for a one standard deviation better principal.

To assess whether our measure of principals’ effectiveness is sensible, we study whether teachers, the school community, and the labor market can recognize effective principals. Using teachers’ surveys, we find that more effective principals can be recognized, i.e., teachers working for them are more likely to agree with statements such as the principal *does a good job*, *promotes a good work climate*, and *makes good decisions*. Effective principals also receive fewer complaints from the school communities. A one standard deviation increase in principals’ effectiveness is associated with a decrease in total complaints of 17.5%. These results are robust to permutation tests and to the adjustments for multiple hypothesis testing proposed by [Romano and Wolf \(2005\)](#). Regarding the principals’ labor market, we show that principals’ productivity is rewarded, especially by private schools. A one standard deviation increase in principals’ effectiveness is associated with 3% higher wages in private schools, but with only 0.7% higher wages in public schools. Our estimates further suggest that the association between principals’ effectiveness and wages is mostly driven by the bonus component of wages, which is consistent with the fact that public schools rely more on statutory payments and less on pay-for-performance.⁴

The equilibrium relationship between principals’ effectiveness and wages motivates our third and final question: can public schools attract high-quality principals despite having more rigid wages? As we show with a two-sided selection model, if higher quality candidates demand higher compensation, then higher wages are a necessary condition for attracting those candidates. However, higher wages might not suffice nor be the only relevant variable as workers’ choice also depends on their idiosyncratic preferences and the labor demand that they face, i.e., school principals can only choose among the job offers that are available to them. As highlighted by economists and sociologists in the past (e.g., [Abowd and Farber, 1982](#); [Logan, 1996](#)), when choice is constrained by opportunity, personnel selection can accentuate or counteract the sorting of workers based on wages. Moreover, poor selection policies may lead public schools to a loss of talented and motivated workers who would take a position despite a low payment because of strong idiosyncratic preferences for the public sector (e.g., pro-social behavior). Although the selection dimension of

⁴We also find that the gender gap against females is eight times larger in private schools than it is in public schools, and that tenure is more relevant to explain wage variation in public than in private schools.

the labor market is of high practical relevance, empirical progress has met with at least two important hurdles. First, it is difficult to measure an individual's quality. Second, it is hard to find quasi-experimental variation in personnel selection policies.

In this study, we overcome these limitations by exploiting our measure of principals' effectiveness and the features of a reform that changed the selection process of public school principals. In 2011, Chile enacted the Law 20.501 of *Quality and Equity in Education*. This Law comprised a set of policies intended to level conditions between public and private schools, with a special focus devoted to the improvement of leadership in public schools. Before the reform, the appointment of a new principal in a public school was the exclusive responsibility of the municipalities; this process was unsupervised by the central government and was therefore more prone to patronage (e.g., Akhtari et al., 2018; Colonnelli et al., 2019). Under the new system, principals must be elected through public, competitive, and transparent competitions in a process that is lead by a third-party human resources agency and that is overseen by the Civil Service agency responsible for selecting the highest-level bureaucrats of the central government. Leveraging the timing of the new selection system's adoption, we implement a staggered difference-in-differences approach to assess the effects of this policy on the effectiveness of public schools' principals. Our estimates suggest that this new selection process increased principal effectiveness in public schools by 0.04 standard deviations, enough to close one-half of the public-private achievement gap in about 7 years.

Related Literature

Related research on school principals has focused on the effect of principal attributes on students' performance (e.g., Eberts and Stone, 1988; Clark et al., 2009; Béteille et al., 2012) and on the measurement of principal effectiveness by itself (e.g., Coelli and Green, 2012; Dhuey and Smith, 2014; Grissom et al., 2015). A few studies have also looked at the relationship between principals and teachers (e.g., Harris and Sass, 2006; Jacob and Lefgren, 2008) and at the labor market of school principals (e.g., Cullen et al., 2016). In this context, our research offers a broader, unified study of school principals, and, in doing so, it contributes to a number of different branches of the economics literature.

First, our work relates to a large body of research estimating and discussing the validity of value-added models (e.g., Kane and Staiger, 2008; Rothstein, 2010; Kane et al., 2013; Chetty et al., 2014; Bacher-Hicks et al., 2014; Bacher-Hicks et al., 2014; Rothstein, 2015; Chetty et al., 2016; Angrist et al., 2017). In contrast to most papers that focus on the effects of teachers and schools on student

outcomes, we contribute new evidence to an incipient literature that looks at the effectiveness of school principals (Branch et al., 2012; Coelli and Green, 2012; Dhuey and Smith, 2014; Grissom et al., 2015). Specifically, we extend the canonical value-added model with teacher random effects by including school correlated random effects and principal fixed effects.

Second, our paper also relates to recent research on the importance of management and management practices in the private sector (e.g., Bloom and Van Reenen, 2007; Bloom et al., 2013; Bender et al., 2018) and public sector institutions (e.g., McCormack et al., 2014; Bloom et al., 2015; Lavy and Boiko, 2017; Rasul and Rogger, 2018; Fenizia, 2019). By attempting to understand what effective principals do, our work contributes to research on teacher turnover (e.g., Hanushek et al., 2004; Ransom and Sims, 2010; Ronfeldt et al., 2013; Hendricks, 2014; Hanushek et al., 2016) and student tracking (e.g., Duflo et al., 2011; Card and Giuliano, 2016).

Finally, our analysis of the labor market of school principals relates to classical studies on the relationship between workers' wage and productivity (e.g., Marshall, 1890; Hicks, 1963; Robinson, 1969; Lazear and Rosen, 1981), and more specifically to research on personnel economics (see Lazear and Shaw, 2007; and Finan et al., 2015 for reviews). On one hand, our study relates to literature on school personnel (e.g., Rothstein, 2015; Biasi, 2018; Loyalka et al., 2019), as we study the relationship between principals' effectiveness and compensation. On the other hand, our analysis using quasi-experimental variation is close to that of Ferraz and Finan (2011) and Dal Bó et al. (2013), who in different settings show that competition and compensation can help to improve state capabilities.

2 Institutional Setting and Data

This section discusses the main educational reforms implemented in Chile since the eighties and the current functioning of the educational system. It also describes the data used in our analysis and presents some descriptive statistics.

Historical Background

In 1981, under a dictatorship, Chile implemented an educational reform that privatized and decentralized primary and secondary education. Publicly funded school vouchers were created with flat voucher funds following any children either to public schools or to the private schools that

agreed to accept the voucher as payment of tuition.⁵ These vouchers provided full coverage of tuition fees in public schools, but not necessarily in private subsidized schools, which were allowed to charge fees on top of the part covered by the voucher. Indeed, while public schools could not profit, private schools that agreed to accept the vouchers could do it (Elacqua, 2009). The reform was predicated upon the idea that, since parents were free to choose between schools, market forces should lead to an increase in the quality of education through school competition.⁶ The *laissez faire* architecture of this system is still in place today, making the Chilean case unique for having long-term experience with nationwide school vouchers where both governmental and private schooling sectors coexist and compete.⁷ As of 2018, enrollment at private, subsidized private, and public schools represented a 7, 53, and 40 percent respectively.

Alongside the privatization of the educational system, the 1981 reform also decentralized it by transferring the control of public schools from the central government to municipal authorities. Administrative departments of municipal education and municipal education corporations were created to administrate the public schools. Not surprisingly, the efficacy and probity of these departments/corporations were strongly related to that of the local governments (Guerra and Arcos, 2012). As a consequence of this change, many school teachers from public schools lost their jobs and had to either reapply for them now in the municipalities, or to find jobs in the private sector. Moreover, in order to *free* the labor market of teachers, union contracts were revoked, giving public schools greater flexibility in hiring and firing teachers.

After the restoration of democracy in 1990, the teacher's association was reinstated and teacher pay was increased. Public school teachers once again belong to a national teachers union, while those in private schools are generally members of school-level teachers' associations. During the period we study, teachers' wages in the public sector were determined by a rigid formula that was negotiated between the government and the national teachers union. Wages are subject to seniority increments, and other adjustments such as allowances for leadership responsibilities, professional training, and for working in difficult conditions. Teachers in private schools are also eligible for some of these allowances, but they are mainly subject to the Private Labor Code, implying that their wages are often individually negotiated with the schools.

In 2011, and after massive students' protests, the country enacted a reform aimed at improving

⁵A reform in 2008 established a new voucher targeted to low-income student. This represented the first major change to the voucher policy program. For an evaluation of this policy, see Neilson (2019).

⁶Evidence on whether public or private schools are relatively more effective in improving students' learning is mixed (e.g., Hsieh and Urquiola, 2006; Contreras et al., 2018), probably due to difficulties in accounting for unobserved factors determining the selection of children attending each type of school.

⁷The map presented in Figure A.2, in the appendix, exemplifies the coexistence of public and private schools in a low-income municipality at the north of Santiago.

quality and equity in education (Law 20.501). This reform comprised a set of policies intended to level conditions between public and private schools, with a special focus devoted to improve the leadership in public schools. This law increased the attributions of school principals and created a new system to appoint them. After the reform, all new principals at public schools i) must be elected in a public and competitive contest run by an external human resources firm and overseen by the same agency responsible for selecting the highest-level bureaucrats of the central government; ii) are allowed to form their own management teams without having to call a contest for those positions, i.e., they can choose the Deputy Director, the Inspector General and the Chief Technician of the school; iii) can fire up to a 5% of teachers with a bad or regular teacher evaluation; and finally iv), consistent with their higher responsibilities and the leadership they must exercise in their position, principals at public schools get an special bonus in accordance to a rule that depends on the total number of students enrolled and the concentration of poor students in the establishment where they work.⁸

The Current Educational System

Compulsory education in Chile consists of 12 years divided into two cycles: primary school from 1st to 8th grade and secondary school from 9th to 12th grade. Most of the students enter the educational system when they are six years old and finish when they are eighteen. On average, each of the 9,000 schools in the country enrolls about 300 students, excluding preschools, adults' schools, and special education schools. According to the Chilean Ministry of Education, 46% of the schools are in areas considered rural, and 49% of the students are eligible for a special subsidy targeted to the poor.

Like many countries, Chile has a nationwide standardized curricula.⁹ The Chilean curriculum is determined by the Ministry of Education for each grade and subject, and it affects the school curricular offerings and the instructional resources directly. It also works as a system of accountability (Valverde, 2004). Indeed, as exemplified by Figure A.3 in the appendix, the government not only provides teachers with the curriculum guides and official textbooks, but also with lesson plans and exams. Finally, the curriculum guidelines also establish minimum content goals and fundamental

⁸The reform included other measures as well, such as a new retirement plan for teachers, bonuses for teachers with good evaluations, adjustments to the severance payments, introduction of public contests for the position of educational superintendent, more rights for teachers and teacher assistants, and more funding for both public and subsidized private schools. For details, see "Ley 20.501 Calidad Y Equidad de la Educación".

⁹Countries that have a national curriculum include: France, Hungary, Ireland, Italy, Japan, Korea, the Netherlands, New Zealand, Norway, Portugal, Singapore, Spain, and the United Kingdom. While most States in the U.S. follow common guidelines for a core curriculum, there is no national curriculum as such.

objectives for education, which ultimately determine course grades and grade retention.

Teachers in a particular subject determine the course grade in that subject. Students are evaluated continuously throughout the year, and, in general, each subject's annual grade is based on more than four evaluations. Grades are awarded on a scale from 1 to 7 in intervals of 0.1, with a minimum passing grade of 4. The Ministry of Education also administers a national standardized test (SIMCE by its acronym in Spanish). This test is limited to a subset of subjects (Math, Spanish, History, Science and English), and it is taken annually but only by students in the 4th, 8th, and 10th grades.

The regulations for the operation and organization of schools differ between the public and the private-subsidized sectors, thus giving school providers and schools in both sectors different levels of autonomy to hire, pay, and administrate the schools (Paulo et al., 2017).¹⁰ However, school principals in both systems need to have relevant training and teaching experience to be eligible for the position. As managers of the schools, principals are responsible for multiple tasks such as setting goals, reviewing students' and teachers' progress, keeping a safe school climate, and being a hub for interaction among teachers, students, and parents. According to the Chilean Law, all school principals should at least: i) develop, monitor, and evaluate the goals and objectives of the school; ii) study strategies for the implementation of the national curricula; iii) organize and guide the technical-pedagogical work and professional development of teachers; and iv) ensure that parents or guardians receive regular information on the operation of the school and the progress of their children.

Data and Descriptive Statistics

We use administrative data from the Ministry of Education, the Superintendency of Education, and the Chilean Civil Service.

To estimate principals' effectiveness, we use a panel at the student-year-subject level. This panel spans the period from 2011 to 2016 and has information on the academic performance of all students, by subject and classroom, from the first through the twelfth grade. Specifically, the students' records contain their gender, age, subject-specific course grades, attendance rate, and promotion status. For cohorts of students that take standardized exams, it is also possible to link

¹⁰Unlike subsidized private schools, public schools are subject to a more restrictive labor regulation that specifies procedures for recruitment, dismissal, and remuneration. These schools also have to comply with special regulations regarding the purchase of goods and services.

our data to their test scores in Math and Spanish.¹¹ We match this data set with a nationwide census of teachers containing rich information on the specific subjects and classrooms taught by them every year, as well as their characteristics (e.g., gender, age, type of degree, hours of contract). For a subset of these teachers, we recover their perceptions about the school principal from survey responses collected by the government. We also leverage data from a yearly school panel that includes several school characteristics such as the type of administration (e.g., public, subsidized-private, or private), an indicator if the school is located in a rural area, its total enrollment, fraction of disadvantaged students, fraction of parents with a college degree, parents' income level, the identity of the school principal, and whether she or he was elected through the new selection system. Finally, we complement this data with i) characteristics of the municipalities where the schools are located, and ii) administrative records of complaints filed against the school.

We consider all schools to estimate principals' and teachers' effectiveness, but we circumscribe most of our analysis to public schools and private schools receiving vouchers because only those schools report detailed information to the government (e.g., we do not observe the wages paid in private schools that do not receive vouchers). Preschools, adults' schools, and special education schools are also excluded from our analysis. Panels A and B of Table 1 present descriptive statistics by type of school. We see that students at subsidized private schools do better in terms of course grades, test scores, and grade retention. In terms of school characteristics, these private schools tend to serve more students, have larger classrooms, have fewer teachers per student, and have slightly better attendance. All these differences are likely related to the fact that only a 24% of private schools are in rural areas versus a 62% of public schools. In terms of school finance, public schools receive a larger subsidy but they also serve more disadvantaged students. The share of students considered poor, and who are therefore eligible for special subsidies, is 57% in public schools and 36% in private schools.¹²

Our analysis of the principals' labor market uses detailed administrative data from the Superintendency of Education. In Chile, every school that receives a voucher from the government must provide a detailed report of their sources of income and their expenditures. These records allow us to observe all compensations paid to every school worker, by month, between 2015 and 2017. We classify compensation items into three categories: minimum wage, statutory payments, and bonuses. *Minimum wage* corresponds to a per-hour legal-minimum payment for teachers, defined by the Ministry of Education. *Statutory payments* include compensation components regulated by law but unrelated to performance, such as payments for experience and for teacher certification;

¹¹As mentioned before, the SIMCE examination is only taken by students in specific grades, usually 4th, 8th, and 10th grade, and it has not been systematically run every year in the country.

¹²Table A.1 in the appendix presents more detailed descriptive statistics of students, schools, and principals.

it also includes other payments assigned to those who work extra hours, in rural schools, or in schools where it is “difficult” to teach according to the Ministry of Education. Finally, *bonuses* encompasses compensation components related to workers’ performance, such as individual and collective performance bonuses, payments from the national system of performance assessment, bonuses paid directly by the school owner in the case of subsidized private schools, and other discretionary payments and gratifications related to transportation, food, and holidays.

On average, principals earn around 2,700 USD per month. According to the representative survey CASEN (in 2015), the wage of school principals is placed at the 65th percentile of the wage distribution (or the 51st percentile when we only consider workers from similar cohorts and who attained higher education).¹³ Panel C of Table 1 presents the descriptive statistics of principals’ demographics and wages. Compared to private schools, public schools pay lower wages to the principals and their compensation relies more on statutory payments and less on bonuses. In public schools, a 35.3% of the wage corresponds to the minimum wage, a 54.4% to statutory payments, and a 10.3% to bonuses; while in private schools these figures correspond to 50.1, 27.5, and 22 percent. Finally, in terms of demographic characteristics, public school principals have more tenure and most of them are male; in contrast to private schools, where 61% are female.

3 Estimation of Principals’ Effectiveness

In this section, we present the main model used to measure principals’ effectiveness. We consider a specification that relates academic achievement to student characteristics, school characteristics, and to the school principal, as follows:

$$\begin{aligned}
 Y_{it+1} = & \gamma_t + \rho_g(i,t) + \beta_0 A_{it} + \beta_1 f(Y_{it-1}, \bar{Y}_{it-1}, \rho_g(i,t)) \\
 & + \theta_{p(i,t)} + \phi_0 X_{s(i,t)t} + \phi_1 \bar{X}_{s(i,t)} + \kappa_0 A_{p(i,t)} + e_{it+1},
 \end{aligned} \tag{1}$$

where Y_{it+1} is a measure of student achievement, such as the course grade obtained by student i in year $t + 1$, and γ_t and ρ_g stand for year and grade fixed effects. As is standard in value-added models (e.g., Kane and Staiger, 2008; Chetty et al., 2014), we include a third degree polynomial at the student and classroom level in the lagged dependent variable interacted with students’ grade level. We also control for student’s age A_{it} and principal tenure $A_{p(i,t)}$. Since we only observe a few principals switching between schools, we do not include school fixed effects. Instead, we use correlated random effects (e.g., Mundlak, 1978; Chamberlain, 1980) to account for school hetero-

¹³The average monthly wage of 2,739 USD corresponds to roughly 11 times the legal minimum wage.

geneity. Specifically, we include a combination of fixed and time-varying characteristics of each school. Time-variant school characteristics include total enrollment, the fraction of disadvantaged students, the share of low-income and high-income parents, and the share of parents with a college degree. Fixed school characteristics include the across-time average of the previous list of time-variant characteristics as well as indicators for whether the school is public, subsidized private, or private, and for whether it is considered a rural school. This set of fixed school characteristics is augmented with characteristics of the municipality where the school is located: average years of education, income per capita, and the 2011 rates of crime, unemployment, and poverty.¹⁴ Building on [Altonji and Mansfield \(2018\)](#), our approach attempts to absorb the across-schools variation in unobservable school characteristics by controlling for the school averages of its observed characteristics. Moreover, this approach helps us to overcome problems of weak identification that could arise in our case if we added school fixed effects given that only some principals switch schools within our time frame (e.g., [Jochmans and Weidner, 2019](#)).

This parsimonious specification allows us to estimate a precise single measure of effectiveness for each principal, which may be interpreted as a weighted average effect across years if principal effects *drifted* differentially over time. As we discuss later, a standard selection-on-observables assumption is necessary for the fixed effect $\theta_{p(i,t)}$ to identify the causal impact of principal p on student i 's achievement. Importantly, our empirical model does not take teacher effects into account directly, i.e., the specification does not include teacher fixed effects. The reason is that principals' effectiveness is at least in part mediated by teachers because one way in which principals affect student achievement is via their effects on teachers. Principals can affect teacher effectiveness through feedback and opportunities for their development, the resources they supply to teachers, and the protection they give teachers from distractions, among other mechanisms. As a result, taking teacher effects directly into account in this model would remove an important component of the variation in principal effectiveness. Nevertheless, we can use the residual from this specification to compute a relative measure of teacher effectiveness (relative to other teachers working under the same school principal).

To estimate our model, we leverage a panel at the student-subject-year level from 2011 to 2016, and we focus on the performance of the student in the two subjects for which we observe course grades every year: Math and Spanish. For estimation, we exclude preschools, adults' schools, and special education schools. We also exclude classes that had more than one teacher per year and eliminate the bottom and top one percent of classroom size outliers. Finally, to avoid the classification of easy-graders as high value added teachers, we follow [Petek and Pope \(2019\)](#) and

¹⁴Following [Wooldridge \(2010\)](#), we also add the across-time average of year fixed effects dummies to account for our unbalanced panel.

restrict the sample to students for whom the teacher, in a given subject, changed between t and $t + 1$. We end-up with an estimation sample of 9,824,556 student-subject-year observations, which comprises 2,149,388 students, 72,325 teachers, and 9,944 principals. The standard deviation of our estimated fixed effects is 0.22; in other words, one standard deviation in principals' effectiveness is associated with a 0.22 standard deviations increase in students' course grades. This is a non-trivial effect size as reduced form evidence suggests that one standard deviation increase in GPA is associated with an 11% increase in wages, even after accounting for test score achievement and several student and school level characteristics.¹⁵

Although for each principal $p = (1, \dots, P)$, her estimated effectiveness $\hat{\theta}_{p(i,t)}$ is an unbiased estimate of her true effect on students' achievement, the standard deviation of $\hat{\theta}_{p(i,t)}$ - which summarizes the overall variability in principals' effectiveness - is an upwardly biased estimate of the standard deviation of $\theta_{p(i,t)}$. This occurs because $\hat{\theta}_{p(i,t)}$ equals $\theta_{p(i,t)} + \hat{\epsilon}_p$, where $\hat{\epsilon}_p$ is a least squares sampling error. As pointed out by Aaronson et al. (2007), this can be particularly serious in the case of teachers because of the small populations of students used to identify their value-added. This is less of a concern in our case as there are many more observations of students per principal than per teacher. Nevertheless, we compute the adjusted standard deviation of $\hat{\theta}_{p(i,t)}$ using the formula proposed in Krueger and Summers (1988):

$$SD(\theta_p) \approx \sqrt{\text{var}(\hat{\theta}_p) - \sum_{p=1}^P \hat{\sigma}_p^2 / P}$$

where $\hat{\sigma}_p^2$ is the standard error of $\hat{\theta}_{p(i,t)}$. Reassuringly, the adjusted standard deviation of principal effectiveness is 0.20, not much different from 0.22. This is particularly encouraging given the fact that this adjustment neglects the covariances among the ϵ_p , slightly underestimating the standard deviation of $\theta_{p(i,t)}$. Building on this adjusted standard deviation, we could adjust our measure of principal effectiveness by its "reliability". However, in light of the small adjustment factor (i.e., $\tilde{\theta}_{p(i,t)} = \hat{\theta}_{p(i,t)} \times (SD(\theta_{p(i,t)})/SD(\hat{\theta}_{p(i,t)})) = 0.20/0.22$), and the fact that it is also slightly biased, we decided to present our results using our unadjusted measure of principal effectiveness $\hat{\theta}_{p(i,t)}$.

Discussion: On the use of course grades

Our preferred measure of principal effectiveness comes from a value-added model that uses students' course grades instead of test scores as the dependent variable. The main reason for this is

¹⁵Table A.2 in the appendix presents this result. For this exercise, we consider the universe of Chilean students who graduate from high-school in 2008 and 2009 and we look at their wages 8 to 9 years after high-school graduation.

that students in Chile do not take nationwide standardized exams in every grade or necessarily more than once in their lives, which creates two drawbacks associated with using test scores. First, it is unclear how to compute teacher value-added without contiguous measurements of achievement, and the gap between different exams for a given student (if she takes more than one exam) is at least 4 years. Second, principals' fixed effects would be noisier as they would be estimated using a significantly smaller sample. While our model for course grades is estimated using 9,824,556 student-subject-year observations, the same model for test scores (based on students who are observed to take at least 2 tests) only uses 235,434 observations. As a robustness check, and despite these issues, we estimate an alternative measure of principal effectiveness using test scores as the dependent variable in our main specification (1). As shown by Table A.3 in the appendix, most of the results are robust to the use of this alternative measure of principal effectiveness.

While it could be argued that test scores are more objective and fair than course grades, we claim that the latter is closer to what we are interested in for measuring principals' and teachers' effectiveness. This is because course grades -and not test scores- is the metric that determines high school graduation and advancement into higher level courses. Moreover, it also captures the non-cognitive dimensions of students' development such as perseverance, dependability, and consistency (e.g., Bowles and Gintis, 1976) and research has shown that these non-cognitive traits are strongly related to both school and labor market outcomes (e.g., Bowles et al., 2001; Heckman et al., 2006).¹⁶ Furthermore, Chile has a national standardized curriculum and consequently our data gives purchase to the view that test scores and course grades can be used interchangeably as they are strongly positively correlated (e.g., Borghans et al., 2016). Figure A.1 in the appendix shows a strong relationship between test scores and course grades. A one standard deviation increase in course grades is associated to a 0.6 standard deviation increase in test scores. Moreover, regarding our empirical specification, we examine students' performance longitudinally, which should help to mitigate concerns about teachers or school variability in grades. Finally, we estimate specification (1) focusing on future, instead of contemporaneous, course grades. This is particularly important to measure teacher value-added since teachers can directly impact their students' grades, potentially confounding easy-graders with high value-added teachers. In the spirit of Petek and Pope (2019), specification (1) addresses this concern by looking at future course grades Y_{it+1} and controlling by the lagged course grades Y_{it-1} in a restricted sample that includes the 86% of students for whom the teacher, in a given subject, changed between t and $t + 1$. Intuitively, our specification will give credit to a math teacher if her students improved their math course grade

¹⁶Indeed, as shown by Table A.2 in the appendix, we estimate that a one standard deviation increase in GPA is associated to an 11 percent increase in wages, even after controlling for test scores, school characteristics, and students demographics. Non reported regressions also suggest that a one standard deviation decrease in GPA increases the likelihood of grade retention by 200%, from a base of 0.045 percentual points.

after having her as a teacher, controlling by the students' past achievement. Table A.4 in the appendix presents a comparison between the full sample and the restricted sample that we use for estimation. There is no evidence suggesting selective attrition.¹⁷

Discussion: On the identification assumption

Despite the fact that quasi-experimental measurements of value-added seem to support the assumptions underlying observational value-added models (e.g., Kane and Staiger, 2008; Angrist et al., 2017), the use of these models as a personnel tool is still controversial (e.g., Rothstein, 2010; Chetty et al., 2014; Bacher-Hicks et al., 2014; Rothstein, 2015; Chetty et al., 2016). The main reason of concern in the teachers' setting is that classroom assignments may not be exogenous conditional on the typical controls, and consequently estimates of teachers' effects based on these models should not be interpreted as causal. An analogous threat in our setting is that conditional on observables, the correlation between the assignment of students to principals depends on unobserved determinants of students' achievement. Although this threat to identification is ultimately untestable, in what follows we present some exercises that seem reassuring.

First, we deal with the potentially endogenous sorting of students into schools using correlated random effects. Arguably, concerns about the assignment of students to principals should be ameliorated once school heterogeneity is taken into account. To assess the performance of our approach, we focus on a subset of principals who switched schools and that we observe for at least two periods in each school. For them, we estimate our main specification (1) but including school fixed effects directly. Encouragingly, this alternative measure and our main measure of principal effectiveness are strongly related. As shown by Panel A of Figure 1, we find that a 1 standard deviation increase in our main measure of principals' effectiveness implies a 0.86 standard deviation increase in our alternative measure. Moreover, we do not find indications of systematic bias in our main measure of principal effectiveness, relative to this alternative measure from a model with school fixed effects.¹⁸

As a secondary check, we estimate our main specification (1) again but now including principal-school fixed effects. Then, in the spirit of a balance test, we focus on principals who moved across

¹⁷We do not find evidence of selective sample attrition in terms of grades, subject, attendance, student performance, or teachers characteristics. But, as expected, the restricted sample excludes smaller, rural, and public schools where teachers change less often from one year to another.

¹⁸Figure A.4, in the appendix, compares the agreement between the main and alternative measure of principal effectiveness. Following Bland and Altman (1986), we plot the difference between the two measures of principal effectiveness against the average between them.

schools to assess how changes in a principal’ measured effectiveness depend on the differences in the (predetermined) school characteristics. Specifically, we estimate:

$$\hat{\theta}_{ps} - \hat{\theta}_{ps'} = \alpha + \beta(\bar{X}_s - \bar{X}_{s'}), \quad (2)$$

where $\hat{\theta}_{ps} - \hat{\theta}_{ps'}$ represents the difference in the estimated fixed effect of principal p when she worked at schools s and s' respectively; and $\bar{X}_s - \bar{X}_{s'}$ represents the difference in the predetermined characteristics (as of 2010) of schools s and s' , with the vector X including: enrollment, share of disadvantage students, subsidy per student, teachers per 100 students, and the school average test scores (Math and Spanish) of 4th graders. Figure 1, Panel B, plots the β coefficients and 95% confidence intervals obtained from separate OLS regressions of the principal effectiveness differential on the differential in predetermined characteristics of the schools. Reassuringly, we do not find much evidence of an association between changes in school characteristics and changes in principal fixed effects.

In our setting, principal fixed effects would identify the causal effect of principals on students under a *strict exogeneity* or *selection on observables* assumption, i.e., conditional on observable characteristics, the correlation between the assignment of students to principals and other determinants of students achievement is innocuous. As mentioned before, this identification assumption is ultimately untestable —what Holland (1986) called “the fundamental problem of causal inference”—however, we can leverage the panel structure of our data to implement a specification diagnostic in the spirit of Rothstein (2010). Specifically, focusing on a subset of students who switched between schools (and who were consequently exposed to more than one principal), we can implement a falsification test. The intuition of the test is simple: if the effectiveness of the principal in the school of destination impacts students’ learning in the school of origin, then the identification assumption of the model is probably wrong. Specifically, we estimate the following variation of our main specification:

$$Y_{it+1} - Y_{it-1} = \alpha + \gamma_0 \hat{\theta}_p^{s_0} + \gamma_1 \hat{\theta}_{p'}^{s_1} + \phi_0 X_{s(i,t)t} + \phi_1 \bar{X}_{s(i,t)} + e_{it+1}, \quad (3)$$

where s_0 and s_1 stand for the schools of origin and destination, respectively; $\hat{\theta}_p^{s_0}$ is the estimated effectiveness of principal p who is working in the school of origin; and $\hat{\theta}_{p'}^{s_1}$ is the estimated effectiveness of principal p' who is working in the school of destination. It is worth noticing that we use “jackknife” estimates of principal effectiveness for this exercise, i.e., we estimate principal fixed effects again but now excluding students who switched schools (to avoid a mechanical correlation). We also include schools’ correlated random effects to isolate the effect of the principal from the

effects of the school. The parameter of interest is γ_1 , which should equal zero if the model is correctly specified.

We perform this specification diagnostic for both a model of gains and a model with lagged dependent variable. Table A.5 in the appendix presents the results. We do not find much evidence of positive correlation between the standardized course grade gains of the students (the pre-assignment variable) and the effectiveness of their future principal (the treatment variable). On one hand, the model of gains suggests a negative correlation between students' current achievement and the effectiveness of future principals. On the other hand, the model with lagged dependent variable shows a positive but very small effect of future principals on current achievement.¹⁹

4 Principal's Effectiveness, School Management, and Wages

In this section, we use our measure of principal's effectiveness to investigate three important issues. We start by studying whether principals' effectiveness has an impact on teachers' turnover. Then, we explore how teachers and the school communities perceive the management practices of effective principals. Finally, we present our findings on the association between principals' effectiveness and principals' compensation.²⁰

4.1 Principal's effectiveness and teachers' turnover

Teacher shortages have recently received considerable attention from policymakers as they impose financial costs on schools and may affect students' outcomes. Teacher turnover is at the center of this debate, given the well-known difficulties experienced by new teachers, and the fact that teacher turnover seems to have adverse effects on school effectiveness, especially in lower-achievement schools (e.g., Ronfeldt et al., 2013; Hanushek et al., 2016). Building on previous research, we study the relationship between teacher turnover, wages, and other school characteristics, and we present novel evidence on the impact of principals' effectiveness on teachers' retention. We leverage data from 2008 to 2016 of the universe of prime-age teachers (between 22 and 62 years old), to

¹⁹While this is consistent with the idea that models with lagged dependent variable do not really reflect "value-added", our results are in line with Chetty et al. (2017), as the effect size on the future principal's effectiveness is quite small compared with the main effect, which remains stable.

²⁰We also explored whether principals' effectiveness is related to the sorting of students across classroom. This analysis is available in Appendix B. Our findings suggest that tracking policies are not very common in Chilean schools (the sorting index only represents a 7% of an empirical upper bound), and consequently, the impact of principals on this margin is small.

estimate the following linear probability model:

$$\text{Exit}_{jt+1} = \alpha_j + \gamma_t + \beta \hat{\theta}_{p(j,t)} + \Phi X_{s(j,t)t} + \epsilon_{jt+1}, \quad (4)$$

where Exit_{jt+1} equals 1 if teacher j experiences either a job-to-job or a job-to-non-teaching employment transition the next period $t + 1$, while γ_t and α_j stand for year and teacher fixed effects. The vector $X_{s(j,t)t}$ includes school-level characteristics such as the share of poor students and the school size. It also includes the average wage paid by school s in the period 2015-2017 (our wage data only starts in 2015). The parameter of interest is β and it reflects the association between the standardized measure of principal effectiveness obtained in section 3 and the teachers' turnover.

Table 2 presents point estimates and bootstrap standard errors (100 replications) separately for all schools, private schools, and public schools. Column 1 shows the results obtained from a specification without teacher fixed effects but that controls by the average wage paid by the school. We find a significant association between principals' effectiveness and teacher's turnover. A one standard deviation increase in principal effectiveness decreases the average exit rate of teachers by 6% in private schools and by 4% in public schools. Column 1 also suggests that a 10% increase in wages decreases the likelihood that a teacher leaves a private school by 7%, implying an observational turnover elasticity of -0.7. However, we do not find an statistically significant association between wages and teachers' turnover in public schools.²¹ Taken together, these results suggest that the average *willingness to pay* of teachers (in private schools) for a one standard deviation more effective principal is equivalent to a 8% increase in their wages. Thus, replacing the bottom 5% of principals by the average principal could save around 69,120 USD per school-year, the equivalent of 28 yearly vouchers for students.²²

Column 2 presents estimates from the less conservative specification that includes teacher and year fixed effects. Results suggest that, once we leverage *within* teacher variation, there is no significant impact of principal effectiveness on teachers' turnover in public schools. Nevertheless, a one standard deviation increase in principal's effectiveness decreases teacher turnover by 8% in private schools, a finding that is consistent with the fact that principals have more leeway to make firing and hiring decisions in the private sector. To explore heterogeneous effects by teachers' experience and effectiveness, we estimate model (4) separately for teachers of different ages and of different estimated value-added. Columns 3 to 5 present the analysis by teachers' age. We start by

²¹While the negative correlation between wages and labor turnover is well known (e.g., Manning, 2011), the research in the context of teachers' labor market has conveyed different results. For instance, Hanushek et al. (2004) finds no effects of wages on the probability that teachers leave the profession, while Ransom and Sims (2010) find a highly significant effect. Closer to the latter, Hendricks (2014) estimates an average turnover elasticity of -1.4.

²²The average yearly wage paid to teachers in Chile is 18,000 USD, and schools have 24 teachers on average. The average annual subsidy per student corresponds to 2,424 USD.

noticing that the average exit rate decreases monotonically with the age group, from 15% among the youngest teachers to 5% among the oldest. We do not find evidence of heterogeneous effects in public schools, but estimates suggest that principal effectiveness is more negatively associated with the turnover of younger teachers in private schools. Regarding teacher quality, columns 6 to 8 present the results for different groups of teachers classified based on their location within the empirical distribution of our estimated measure of teacher value-added.²³ Again, we do not find evidence of heterogeneous effects in public schools, but the estimates suggest that an increase in principal effectiveness decreases teacher turnover of medium and high value-added teachers in private schools.²⁴ Thus, if principals can recognize good teachers -as previous research has shown (e.g., [Jacob and Lefgren, 2008](#))- and can decrease teachers’ turnover, then giving principals more discretion in the management of teachers could be an effective way to use that local knowledge, especially in public schools where bureaucratization may hinder the use of it.

4.2 Principal’s effectiveness and perceptions of the school community

In the spirit of recent research looking at managerial practices within schools (e.g., [Di Liberto et al., 2015](#); [Bloom et al., 2015](#)), in this subsection we explore how teachers and the school community perceive the practices of their principals. We start by analyzing a set of surveys that ask teachers about their level of agreement with different statements, such as the *principal does a good job* and the *principal promotes a good work climate*. Every teacher must provide an answer within a range from 1 to 4 (or from 1 to 5 in some years), where 1 represents high disagreement with the statement and 4 (or 5) represents a high level of agreement. We use their responses -which are publicly available for the years 2010, 2011, 2014, and 2015- to create a dummy variable at the survey-respondent level that equals one if the teacher “highly agrees” with a given statement about the school principal, i.e., her response is at the top of the specific scale for that question. Then, we take the average across respondents at the school-year level and assign this to the corresponding school principal. Using this principal-level data set, we estimate the following model:

$$y_p = \phi_{m(p)} + \beta \hat{\theta}_p + \epsilon_p, \tag{5}$$

where y_p represents the fraction of respondents working with principal p that “highly agree” with statement y , $\phi_{m(p)}$ is a municipality fixed effect, and $\hat{\theta}_p$ is the standardized measure of principal

²³Notice that teacher value-added is estimated from the residual of equation (1). Consequently, our measure of teacher value-added is relative to the school principal, i.e., it is computed from deviation in the achievement of different classrooms exposed to the same school principal.

²⁴All these results are robust to non-linear transformations of model (4), i.e., a logistic regression model.

effectiveness discussed in section 3. We present our results in Panel A of Table 3. Column 1 presents the estimated β coefficients and column 2 presents bootstrap standard errors (100 replications) to account for the fact that $\hat{\theta}_p$ is an estimate itself. Column 3 presents the p-values adjusted for multiple hypothesis testing using the step-down procedure proposed in Romano and Wolf (2005), which controls by the family-wise error rate to prevent us from declaring a true null hypothesis to be false.²⁵ Moreover, column 4 reports a one-sided p-value obtained from a permutation test. To compute this number, we randomly reshuffled our principal fixed effects 200 times, and then we calculate the proportion of sampled permutations where the absolute value of the coefficients obtained using the reshuffled fixed effect was greater than or equal to our $\hat{\beta}$ estimate. Intuitively, this exercise gauges how likely would it be to obtain our results just by chance. Finally, columns 5 and 6 present the mean of the dependent variable and the number of observations used for estimation. The number of observations changes because not all questions are asked every year.

We find that more effective principals do a better job according to teachers. Results in Table 3 show that more effective principals are associated with a larger fraction of their teachers highly agreeing with positive statements about their management. Ordered by effect size, we find that one standard deviation increase in principal effectiveness increases agreement with the statements *principal does a good job* by a 5%; *principal promotes a good work climate* by 4%; *principal is a good manager*, *principal knows students needs*, *principal is good at communicating*, *principal engages parents*, *principal engages teachers*, and *principal knows teacher needs* by 3%; and *principal makes good decisions*, *principal can be trusted*, *principal includes teachers*, and *principal is effective* by 2%. Reassuringly, all these results are robust to our permutation test and to adjustments in our inference strategy to control for multiple hypothesis testing.

We also leverage administrative data of complaints filed by the school community against the school. This data is recorded to monitor that holders of educational establishments comply with the laws, regulations, and instructions issued by the Superintendency of Education. As before, we build a data set at the principal level. Specifically, we consider the number of complaints that were filed against the school (per 100 students) and we average them across years to then assign the average to the corresponding school principal. Using this average number of complaints as the dependent variable y_p , we estimate specification (5) again. Table 3, Panel B, presents our results. We find that a one standard deviation increase in principal effectiveness is associated with a decrease in the total number of complaints filed against a school by 17.5%. Once we desegregate the complaints in different categories, the most important and robust effect corresponds to a decrease in complaints for bullying by 16.6%. We interpret this result as indicative of an association between

²⁵A similar approach is taken by Bassi et al. (2016) in the context of heterogeneous treatment effects of a policy evaluation in Chile.

principal effectiveness and more discipline within the schools. There is also suggestive evidence that better principals decrease complaints related to the absence of teachers and denied enrollment by 20% and 12%, although these results do not pass our additional robustness checks.²⁶ The evidence presented here suggests that better principals are recognized by the school community. But, is the principals’ effectiveness also recognized by the labor market?

4.3 Principal’s effectiveness and wages in public and private schools

Public sector compensation usually does not include pay for performance (Finan et al., 2015), and although there is a good rationale for this,²⁷ it has been argued that fixed compensation schemes make it difficult to attract and keep the best personnel in public schools. This discussion, which has motivated several studies on the effects of pay for performance (e.g., Rothstein, 2015; Cullen et al., 2016; Biasi, 2018) and teacher firing policies (e.g., Staiger and Rockoff, 2010; Boyd et al., 2011; Cowen and Winters, 2013), is also relevant to the Chilean case. Appendix Figure A.6, Panel A plots the distribution of hourly wages, residualized with respect to year and county fixed effects, in both private and public schools. Perhaps not surprisingly, we find that public school wages are significantly less spread and are 0.09 log points lower than in the private schooling sector. Moreover, Panel B of Figure A.6 shows that, like in the US, wages in public schools rely less on pay-for-performance. On average, the bonus component of wages represents 22% of the principal’s salary in private schools but only 9% in public schools.

To study the relationship between principals’ effectiveness and principals’ compensation, we use administrative data on public and subsidized private schools (from 2015 and 2017) to estimate the following regression model:

$$\ln(\text{wage}_{pt}) = \alpha + \rho_{m(p,t)} + \gamma_t + \beta\hat{\theta}_p + \Phi X_{pt} + \epsilon_{pt}, \quad (6)$$

where $\ln(\text{wage}_{pt})$ represents the logarithm of the average hourly wage paid to principal p at time t , γ_t are year fixed effects, and $\rho_{m(p,t)}$ is a fixed effect at the level of the municipality in which principal p works at time t . The parameter of interest is β , and it represents the association

²⁶We also checked the robustness of our result to adding randomly-selected subsets of school-control variables, as in Card et al. (2018). Figure A.5 in the appendix shows the results from this exercise. We observe that, for any number of control variables, the average and the median point estimate across randomizations is very close to our baseline estimate for both: the average agreement of teachers across statements and the total number of complaints filed against the school.

²⁷Performance pay for bureaucrats can create severe multi-tasking problems, where bureaucrats focus on the incentivized dimension of their job at the expense of the non-incentivized dimension (Holmstrom and Milgrom, 1987).

between a principal’s wage and her standardized measure effectiveness $\hat{\theta}_p$ described in section 3. The vector X_{pt} includes principal level controls such as tenure, tenure squared, an indicator for whether the principal is female, and for whether she has a permanent contract. This specification also allows us to study how the different components of wages relate to principal effectiveness. For this, we decompose the dependent variable $\ln(\text{wage}_{pt})$ into two components: $\ln(\text{base}_{pt})$ and $\ln(\text{wage}_{pt}/\text{base}_{pt})$, where “base” corresponds to the sum of the minimum legal wage and the statutory payments described in section 2, and base_{pt} corresponds to the total wage minus the bonuses.

The point estimates and bootstrap standard errors (100 replications) obtained from these regressions are presented in Table 4 for private and public schools. Panel A shows the results for $\ln(\text{wage}_{pt})$ with and without controls, and Panel B shows the results (with controls) for the two dependent variables $\ln(\text{base}_{pt})$ and $\ln(\text{wage}_{pt}/\text{base}_{pt})$. We find that a one standard deviation increase in principals’ effectiveness is associated with a 3% increase in wages in private schools, but it is related to only a 1% increase in wages in public schools. We also find that the tenure profile is concave and more relevant to explain wage variation in public schools, and that women earn lower wages, especially in private schools. Regarding the association between principals’ effectiveness and the different wage components, Panel B suggests that about half of the association between principals’ effectiveness and wages is accounted for by the payments over the base in private schools; and that the association between principals’ effectiveness and pay-for-performance is also present in public schools (although the point estimate is three times smaller in public than in private schools). For some, the higher “quality premium” paid by private schools might suggest that public schools are worse equipped to attract high-quality principals.

5 Selection of School Principals

Motivated by the previous results on the compensation of school principals, we start this section with a simple theoretical framework that guides our empirical inquiry on how public schools can attract effective principals. Then, we discuss the reform that created a new system to select principals in Chilean public schools and we evaluate its impact.

5.1 Two-sided selection in the labor market

If higher quality candidates demand higher compensation, then higher wages can attract those candidates. However, higher wages might not suffice nor be the only relevant variable because

workers' matching to employers also depends on: i) their idiosyncratic taste, i.e., workers might have specific preferences for the public or private sector, and ii) the labor demand that they face, i.e., the personnel selection process of the employers constraints workers' choice. Indeed, the intuition derived from models with two-sided selection is that schools could offset the "labor supply effect" by making informed choices. To exemplify this,²⁸ consider principal p who obtains an indirect utility from working in school type $s \in \{\text{public, private}\}$ that is given by $U_{ps} = K_s + \omega_s \theta_p + e_{ps}$, where e_{ps} represents the idiosyncratic preferences of principal p for school type s . Thus, a principal will apply to a school type s with probability:

$$\Pr(U_{ps} \geq U_{ps'}) \equiv \mathbf{q}_{is}.$$

As in [Abowd and Farber \(1982\)](#), schools will choose from the pool of applicants following a random utility rule, such that the $\Pr(\text{being selected by } s \mid \text{applying to } s, \theta_i)$ is given by:

$$\Pr(\beta_s \theta_p > \varepsilon) \equiv \mathbf{p}_{is},$$

Under some assumptions,²⁹ we can compute \mathbf{q}_{is} , \mathbf{p}_{is} , and $\mathbf{q}_{is} \times \mathbf{p}_{is}$ for different levels of θ_p . In Panel A of [Figure 2](#), we plot these objects to exemplify how personnel selection rules could offset the self-selection of lower quality workers into public schools. This simple model also allows us to simulate how the allocation of principals' effectiveness (into schools of different type) depends on the selection and pay-for-performance parameters β_s and ω_s . Formally, the expected quality of principals in a school type s is given by:

$$E[\theta_p \mid \text{school type} : s] = \int_{\theta} \theta_p f_{\theta|c}(\theta_p \mid \text{school type} : s) d\theta,$$

and we know (from Bayes rule) that $f_{\theta|c}$ is given by:

$$f_{\theta|s}(\theta_p \mid \text{school type} : s) = \frac{\Pr(\text{school type} : s \mid \theta_p) \times f_{\theta}(\theta_p)}{\Pr(\text{school type} : s)},$$

where $\Pr(\text{school type} : s \mid \theta_p)$ is given by $\mathbf{p}_{ic} \times \mathbf{q}_{ic}$ (a function of the parameters β_s and ω_s); $\Pr(\text{school type} : s)$ is a scale factor given by N_s/N ; and $f_{\theta}(\theta_p)$ is the pdf of θ_p , which we will assume

²⁸In [Appendix D](#), we present a more thorough exposition of a two-sided matching model for the labor market. We build on [Logan \(1996\)](#)'s model, which is itself a variant of the deterministic two-sided matching models studied in game theory. The advantage of this model is twofold, it is more explicit on the timing of the game and on the schools' determinants of choice, and it provides a closed form expression for the probability that worker p ends-up in school s . Nevertheless, results are qualitatively similar to the ones derived from a simpler sketch à la [Abowd and Farber \(1982\)](#), such as the one presented here.

²⁹In this case, we assumed that e_{ps} follows a type I extreme value distribution, principals cannot anticipate the schools' selection rule, and that private schools have a larger pay-for-performance component than public schools.

normal. In Panel B of Figure 2 we present a simulation obtained using numerical integration to show how the allocation of principal effectiveness in public schools, $E[\theta_p \mid \text{Public School}]$, depends on the selection and payment parameters. As expected, the payment component ω_s generates self-selection of workers' based on their quality θ_p .³⁰ However, this figure shows that the self-selection of workers' is not the only driver of the final match in the labor market, and that the selection parameter β_s can accentuate or counteract this à la Roy sorting of workers. Thus, poor selection policies may lead to a loss of talented and motivated workers who would take a position despite a low payment because of strong idiosyncratic preferences. In this regard, the model delivers good news as it implies that public schools can attract more effective principals without losing the pro-social behavior wanted in a pool of candidates (e.g., Dal Bó et al., 2013; Ashraf et al., 2018; Deserranno, 2019). In what follows, we leverage quasi-experimental variation to estimate the effects that a new personnel selection policy had on public schools.

5.2 Increasing competitiveness and transparency in personnel selection

With the purpose of improving the management and administration of public education and of strengthening managerial performance, in 2011 a new system to select public school principals was established. Before the reform, the appointment of public school principals was the exclusive responsibility of the municipalities; this process was unsupervised by the central government and was therefore more prone to patronage (e.g., Akhtari et al., 2018; Colonnelli et al., 2019). Since the reform, principals are elected through public, competitive, and transparent competitions in a process that is lead by a third-party human resources firm and that is overseen by the Civil Service, an agency responsible for selecting the highest-level bureaucrats of the central government. As established by Law, the Civil Service is mandated to act as “the guarantor of the merit and suitability of the applicants, in public, competitive, and transparent competitions to recruit professionals with pedagogical leadership, management capacity, and strategic vision”.

The implementation of the new selection system was part of a more comprehensive reform intended to level conditions between public and private schools (Law 20.501). Since this reform was enacted, public school principals are allowed to form their own management teams (i.e., the Deputy Director, the Inspector General and the Chief Technician of the school) without having to call a contest for those position and they can fire up to 5% of teachers with a bad or regular teacher

³⁰In this model, higher quality candidates benefit more from a larger share of payments related to performance (on average). However, this intuition does not necessarily hold if workers are risk averse and the performance payment is based on a noisy signal of workers' quality as in Rothstein (2015). While this point is important, the main insight of our model is on workers' selection and not compensation, and consequently we leave this extension for future work.

evaluation. Moreover, in recognition of the relevance of principals, the reform also increased their compensation in accordance with a deterministic rule based on the number of students enrolled and the concentration of disadvantaged students in the establishment in which they work.³¹

We exploit the timing of adoption of this new selection system to study its impact on the allocation of principal effectiveness. We use digitized data from all the competitions for the position of school principal between 2012 and 2016 to identify the time when a school opened a vacancy and the time when a new principal was appointed. As shown by Figure A.7 in the appendix, the adoption of this system was staggered. This is because the replacement of principals was not mandatory, and contests did not always succeed at appointing a principal. As expected, the number of principals elected under the new regime increased over time, with around 370 new principals elected every year since 2012. Table A.6 in the appendix presents some descriptive statistics to compare schools based on their adoption of the new system. Columns 1 and 2 present the average and standard deviation of school characteristics for public schools that did not adopt the new system and those that adopted it within our time frame. Overall, we observe that schools that adopted the new selection system tend to be larger, less rural, have fewer poor students, and have better test scores than non-adopters. Consistently, these schools are located in municipalities with higher income per-capita and more years of schooling. In columns 4 and 5 we present the characteristics of early (2012 and 2013) versus late (post 2014) adopters. We find that early adopters tend to be larger and less rural, but differences are smaller, and we find no differences in tests scores. Moreover, there are no striking differences in terms of municipality characteristics.

Regarding principals' characteristics, in Table A.7 we present the differences between principals who were selected by the new system and those who were not. We find that principals selected under the new system are less likely to have worked as teachers and are more likely to have worked at administrative positions (e.g., Deputy Director, Inspector General, etc.). Interestingly, they are also more likely to have worked in the private schooling sector in the past. Regarding their characteristics, principals elected with the new system are slightly younger and more likely to have a college degree. In the same vein of the previous descriptive statistics, Table A.8 compares the characteristics of the schools of origin with those of the school of destination for principals elected with the new system. We find no differences in terms of school wages, but it seems that these principals are leaving schools with more students, in rural areas, and with fewer resources per student (a byproduct of having fewer disadvantaged students for whom there is a targeted voucher). In line with a preference for amenities hypothesis, these principals are arriving at

³¹We have also leveraged this arbitrary rule to study the effect of higher wages on principal effectiveness with a regression discontinuity design. This analysis is presented in Appendix C. Although suggestive, the lack of precision led us to exclude this exercise from the main results.

municipalities that have higher income and more years of schooling.

To assess the effects of this new selection system, we estimate the following staggered difference-in-differences regression:

$$y_{st} = \alpha_s + \alpha_t + \beta ADP_{st} + \sum_t \Phi'_t X_s I[year = t] + \epsilon_{st}, \quad (7)$$

where s and t stand for school and year, and the dependent variable y_{st} corresponds to the standardized version of our measure of principal effectiveness discussed in section 3. In Chile, the new selection system is known as ADP (the acronym of *Alta Dirección Pública*), thus ADP_{st} is a dummy variable that takes the value one from the first year the school selected a principal using this system, and X_s is a vector of predetermined (as of 2010) school and municipality characteristics.³² Finally, α_s and α_t are school and year fixed effects, and ϵ_{st} is an error term robust to heteroscedasticity.

Our parameter of interest is β , and it captures the change in principal effectiveness after a school selects its principal using the new system. Results are shown in Table 5. In light of [Athey and Imbens \(2018\)](#), we report robust standard errors in parenthesis and robust standard errors clustered at the school-level in brackets. Estimates from column 1 suggest that after a school selects a principal using the ADP system, there is a statistically significant increase in principal effectiveness of 0.037 standard deviations. This effect size is large enough to close one-half of the public-private achievement gap in 7 years. Columns 2 to 4 show that controlling flexibly by school and municipality characteristics during the pre-reform period does not affect the significance nor the effect size of our estimates. In column 5, we follow [Crump et al. \(2009\)](#) and truncate our analysis sample based on a propensity score that estimates the probability that a school selects a principal under the new system. We estimate an effect of 0.040 standard deviations using this truncated sample. Column 6 presents the results when we keep only those schools that adopted the new system, and therefore we identify the effect of ADP selection from the timing of adoption *among* adopters. We find slightly smaller but overall similar results: schools that selected a principal using the ADP system earlier experienced a statistically significant increase in principal effectiveness of 0.035 standard deviations. Finally, as a robustness check, we perform two placebo exercises. First, we consider any principal turnover that happened before the reform (2008-2011) in public schools as a treatment. Second, we consider any principal turnover that happened after the reform (post 2011) in private schools as a treatment. In both cases, the placebo treatment

³²The set of school controls includes income per student, share of disadvantaged students, total enrollment, and 4th grade Math and Spanish test scores. Municipality level controls include poverty rate, average household income, unemployment rate, average years of education, and literacy rate.

takes the value one if there is a change in the principal and stays as a one afterwards. Reassuringly, we find that turnover itself does not impact principal effectiveness in these placebo exercises.

The key identification concern in our setting is that conditional on time-invariant school characteristics, year aggregate shocks, and differential trends parametrized by pre-reform school and municipality characteristics, there might still be unobserved confounding factors that correlated with the timing of adoption of this new system and other determinants of principals’s effectiveness. To partially address this concern, we estimate a variation of model (7) with a dynamic treatment. Specifically, we estimate the following event-study regression:

$$y_{st} = \alpha_s + \alpha_t + \sum_{j=-4}^{-2} \beta_j I[k = j] + \sum_{j=0}^4 \beta_j I[k = j] + \sum_t \gamma'_t X_s I[year = t] + \epsilon_{st}, \quad (8)$$

where k is the year relative to the first year the school selected a principal with the new system. The estimation sample includes all public schools independent of whether they elected a principal via the new system or not. For those schools that used the new system, we include a window of four years around the adoption to facilitate the study of the timing of the effect (results are robust to not imposing this restriction). Figure 3 presents this dynamic version of the staggered difference-in-differences approach. This figure provides visual support on our identification strategy, as the point estimates tend to be negative but small, around zero, and not significant in the pre-period. The slightly negative effect in the pre-period may be an indication that schools appointing an ADP principal had some turnover leading to lower effectiveness of the principal in the pre-period. Nevertheless, the effect size on principal effectiveness after her selection via the ADP system suggests that our results are not reflecting reversion to the mean. Indeed, in the after period we observe an increase in principal effectiveness that remains stable over time. As shown by Panel B, we find similar patterns when we flexibly control for pre-reform school characteristics.

6 Conclusion

We leverage large administrative data sets, and a novel extension of the canonical teacher value-added model, to measure principals’ and teachers’ effectiveness while accounting for schools’ heterogeneity. Results show that there is substantial variation in the ability that school principals have to improve students’ course grades and test scores. Our study also provides suggestive evidence that principal effectiveness is mediated by the retention of teachers, and that the school community and the labor market can recognize effective management. Thus, policies aimed at

hiring more effective principals may be a cost-effective approach to improve school outcomes.

It has been said that the fixed-wage schemes characterizing the public sector may be a hurdle to attract effective leaders. Regardless of whether this is true, insights from a two-sided model of the labor market suggest that the public sector could attract high-quality candidates (with strong preferences for public service) by improving personnel selection. To assess this idea, we use quasi-experimental variation coming from a reform that changed the selection process of principals in Chilean public schools. Results show that improving the competitiveness and transparency of competitions for the position can increase the average effectiveness of the principals appointed to public schools. How these schools fare in the long run as a consequence of this reform is an important question for future work.

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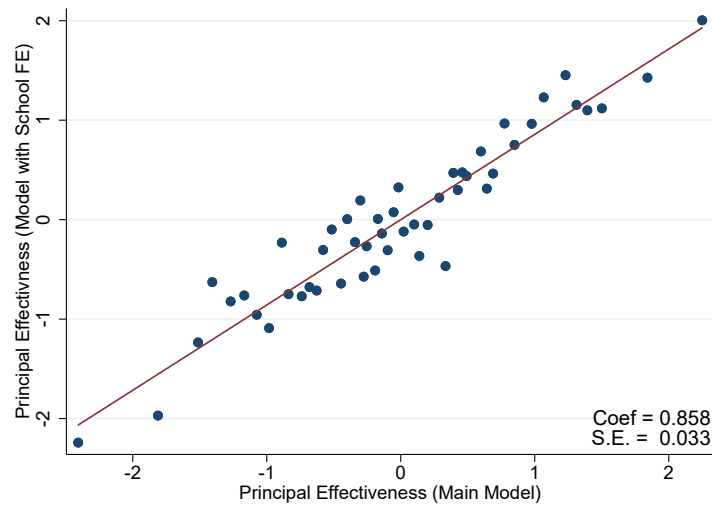
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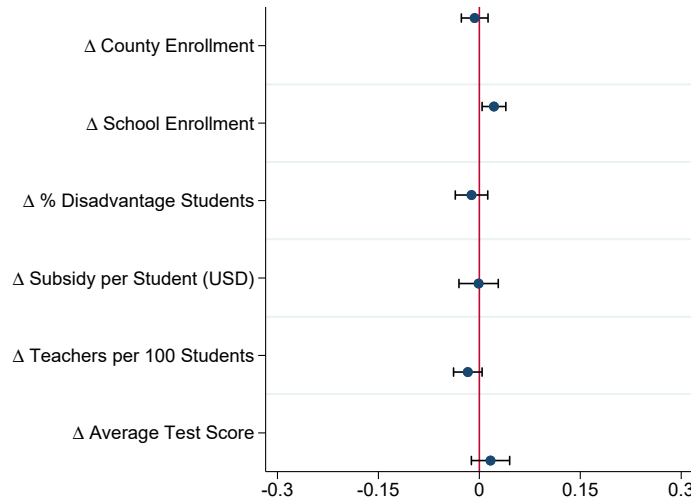
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Figures and Tables

Figure 1: Schools as confounder



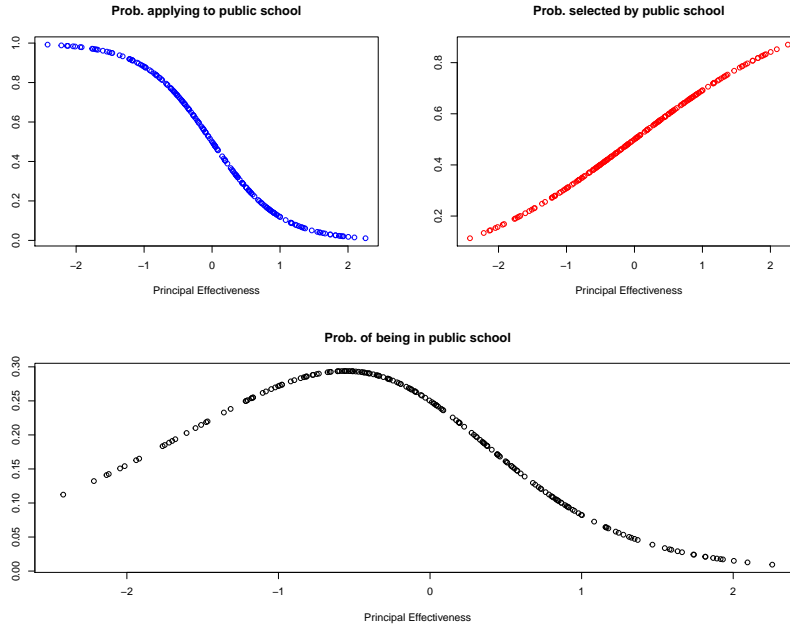
A. Comparison of alternative measures of principal effectiveness



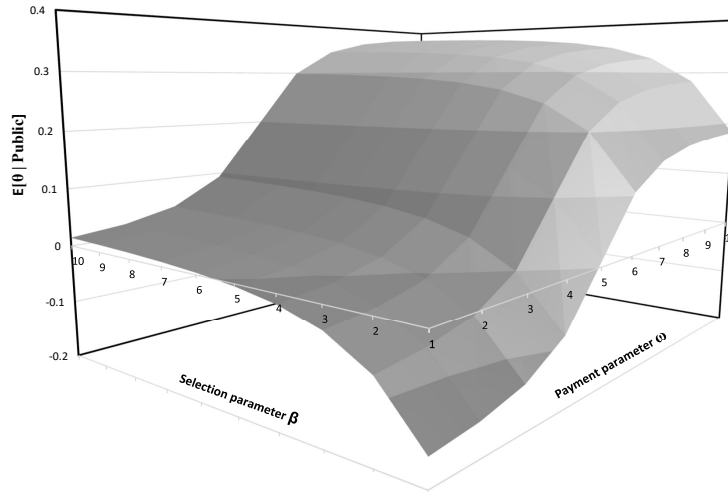
B. Δ Principal effectiveness and Δ School characteristics

Notes: We focus on 300 school principals who switched schools at some point and that we observe for at least two periods in each school. Using this sample, we estimate our main specification but including school fixed effects instead of correlated random effects. Panel A shows the relationship between the measure of principal effectiveness obtained from this model and our main measure of principal effectiveness. Panel B also considers principals switching schools and shows how changes in the measure of principal effectiveness relates to changes in predetermined school characteristics (before 2010), as described by equation (2).

Figure 2: Simulation of a two-sided matching model



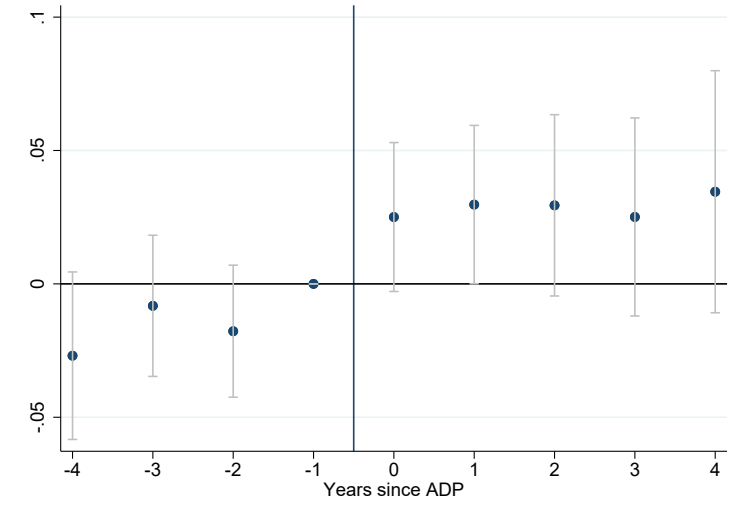
A. Workers' self-selection and personnel selection rules



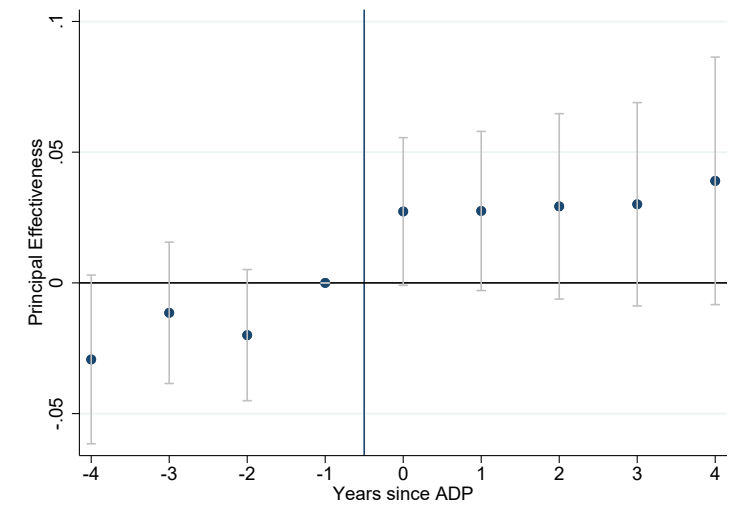
B. Allocation of θ_p , compensation, and personnel selection

Notes: Panel A shows a simple simulation that exemplifies how personnel selection rules can counteract the self-selection component of labor markets. For this, we assume that the idiosyncratic preferences of principals follow a type I extreme value distribution, that principals do not anticipate the schools' selection rule, and that private schools have a larger pay-for-performance component ω than public schools. Panel B shows how the allocation of principal effectiveness depends on the selection and payment parameters of the theoretical model presented in section 5.1. To construct this figure, we created a grid for β_p and w_p from 1 to 10, and compute $E[\theta_p \mid \text{school type: Public}]$ for each cell of this grid. We set all other parameters $(h_p, h_v, \beta_{0p}, \beta_{0v}, w_v, \beta_v)$ at constant baseline levels (1,1,1,1,5,5).

Figure 3: Event study



A. Without Controls



B. With Controls

Notes: This figure shows the impact of the new selection system on the effectiveness of public schools' principals. Specifically, it plots the point estimates and 95% confidence intervals estimated from equation (8). Panel A only considers school and year fixed effects. Panel B considers school and year fixed effects and also controls by school and municipality characteristics during the pre-reform period (measured in 2010), interacted with year dummies.

Table 1: Differences between public and private schools

	(1)	(2)	(3)
	Public	Private	Difference
Panel A: Student characteristics			
Math course grade (standardized)	-0.06 (0.99)	0.04 (1.00)	0.09 (0.00)***
Spanish course grade (standardized)	-0.08 (1.03)	0.05 (0.98)	0.14 (0.00)***
Math test scores (standardized)	-0.27 (1.00)	0.16 (0.96)	0.43 (0.00)***
Spanish test scores (standardized)	-0.21 (1.01)	0.12 (0.98)	0.32 (0.00)***
Grade retention (%)	5.11 (17.30)	3.80 (14.79)	-1.31 (0.00)***
Panel B: School characteristics			
Enrollment	215.90 (303.61)	415.69 (475.76)	-199.79 (0.00)***
Class size	17.17 (11.76)	22.47 (13.39)	-5.30 (0.00)***
Annual subsidy per student (USD)	2977.05 (4302.89)	1628.20 (2398.08)	1348.85 (0.00)***
Share of disadvantaged students	57.30 (32.88)	36.72 (34.72)	20.58 (0.00)***
Teachers per hundred students	9.15 (12.44)	6.38 (22.14)	2.76 (0.00)***
Rural school	62.40 (48.44)	24.45 (42.98)	37.95 (0.00)***
School attendance	85.78 (9.23)	86.51 (9.90)	-0.73 (0.00)***
Panel C: Principal characteristics			
Wage (USD)	2593.73 (2301.79)	2896.42 (1548.07)	-302.69 (0.00)***
% Base salary	35.34 (13.48)	50.25 (20.74)	-14.92 (0.00)***
% Bonus	10.26 (15.11)	22.23 (20.64)	-11.97 (0.00)***
% Statutory	54.40 (18.37)	27.51 (18.24)	26.88 (0.00)***
Tenure	28.70 (11.72)	25.69 (12.47)	3.01 (0.00)***
Age	54.85 (8.84)	53.58 (10.97)	1.27 (0.00)***
Female	44.73 (49.73)	60.85 (48.82)	-16.12 (0.00)***

Notes: This table shows the differences between private and public schools in terms of students', schools', and principals' characteristics. Columns 1 and 2 present the average and standard deviation, respectively; and column 3 presents the difference between both and the p-value of the difference (in parenthesis). These descriptive statistics corresponds to i) students who belong to our main estimation sample, ii) schools that are subsidized private and public (we exclude private schools), and iii) principals for whom we can obtain an estimate of effectiveness.

Table 2: Teacher turnover and principal effectiveness

Dependent variable: 1 if teacher leaves the school next period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All		Teachers by age group			Teachers by Value-Added		
			∈ [22, 37)	∈ [37, 52)	∈ [52, 67)	Low VA	Med. VA	High VA
All Schools								
Principal Effectiveness	-0.007*** (0.001)	-0.009*** (0.002)	-0.011*** (0.003)	-0.008** (0.003)	-0.007*** (0.002)	-0.008*** (0.002)	-0.010*** (0.002)	-0.009*** (0.002)
Log Teachers' Pay	-0.041*** (0.003)							
Mean Exit rate	0.100	0.102	0.145	0.0983	0.0574	0.107	0.0981	0.102
Private Schools								
Principal Effectiveness	-0.006*** (0.001)	-0.008*** (0.002)	-0.012*** (0.004)	-0.004 (0.004)	-0.004 (0.005)	-0.005 (0.004)	-0.012*** (0.004)	-0.006* (0.003)
Log Teachers' Pay	-0.074*** (0.004)							
Mean Exit rate	0.107	0.110	0.148	0.0956	0.0572	0.115	0.105	0.109
Public Schools								
Principal Effectiveness	-0.004*** (0.001)	-0.002 (0.002)	0.004 (0.005)	-0.002 (0.004)	-0.004 (0.003)	-0.002 (0.004)	0.001 (0.004)	-0.005 (0.004)
Log Teachers' Pay	-0.004 (0.004)							
Mean Exit rate	0.0913	0.0932	0.139	0.102	0.0575	0.0966	0.0896	0.0936
Teacher FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the results from the linear probability model described by equation (4). For the estimation we focus on prime age teachers, between 22 and 62 years old, from 2008-2016. The dependent variable equals 1 if a teacher experienced a job-to-job or job-to-non-teaching employment transition. School-level controls include the average wage paid to teachers in their school, the share of disadvantaged students, and school size. All specifications include year and teacher fixed effects. Bootstrap standard errors (100 replications) are in parenthesis.

Table 3: Principal effectiveness and perceptions of the school community

	(1)	(2)	(3)	(4)	(5)	(6)
	β Coef.	Std. Error	Perm. p-val	R-W p-val	\bar{Y}	Obs.
Panel A: Teachers' Responses						
<i>Aggregate Score</i>	0.013***	(0.004)	0.000	0.010	0.508	8337
Does a good job	0.022***	(0.005)	0.000	0.010	0.474	6306
Promotes good work climate	0.021***	(0.006)	0.000	0.010	0.555	6212
Is a good manager	0.015***	(0.004)	0.000	0.030	0.510	7443
Is good at communicating	0.018***	(0.005)	0.000	0.010	0.532	6313
Engages teachers	0.014***	(0.005)	0.005	0.030	0.473	7418
Engages parents	0.016***	(0.004)	0.000	0.010	0.492	7445
Knows teacher needs	0.013***	(0.004)	0.000	0.030	0.468	7438
Knows student needs	0.020***	(0.005)	0.000	0.010	0.533	6311
Makes good decisions	0.012***	(0.004)	0.010	0.030	0.489	7446
Can be trusted	0.012**	(0.005)	0.005	0.059	0.542	6295
Includes teachers	0.011***	(0.004)	0.000	0.030	0.511	8323
Is effective	0.010**	(0.004)	0.010	0.059	0.480	7439
Panel B: School Complaints						
<i>N Complaints</i>	-0.209***	(0.057)	0.015	0.060	1.192	4847
Bullying/Discrimination	-0.105***	(0.027)	0.010	0.079	0.633	4847
Teacher Absenteeism	-0.019**	(0.008)	0.190	0.347	0.094	4847
Denied Enrollment	-0.020*	(0.011)	0.165	0.386	0.159	4847
Accidents	-0.024	(0.020)	0.015	0.347	0.080	4847
Charges Extra Fees	0.006	(0.005)	0.075	0.525	0.036	4847
Poor Infrastructure	-0.040	(0.028)	0.095	0.347	0.119	4847
Resource Accountability	-0.008	(0.014)	0.255	0.762	0.071	4847

Notes: In Panel A, we first create a dummy at the survey respondent level where the variable takes a value of one if the survey response is “highly agree” with the statement. In cases when the survey had 5 or 4 options we always use the highest number to create the dummy. Then, we take the average across respondents at the school-year level and assign this to a principal. In Panel B, we consider all complaints (per 100 students) filed against a school (as recorded by the *Superintendencia de Educación*) and we sum them across years to then assign them to a principal. All models include municipality fixed effects. Columns 1 and 2 report the estimated coefficients and bootstrapped standard errors (100 replications) from specification (5). Column 3 reports the results from a permutation test for which we randomly reshuffled principal fixed effects 200 times. The p-value of the test is calculated as the proportion of sampled permutations where the value of $\hat{\beta}_s$ was greater than or equal to our estimate $\hat{\beta}$. Column 4 presents p-values adjusted for multiple hypothesis testing using the step-down procedure of Romano and Wolf (2005). Finally, columns 5 and 6 present the mean of the dependent variable and the number of observations used for estimation.

Table 4: Principal compensation and principal effectiveness

Panel A: All Wages				
	<i>Private Schools</i>		<i>Public Schools</i>	
	ln(Wage)	ln(Wage)	ln(Wage)	ln(Wage)
Principal Effectiveness	0.026*** (0.008)	0.024*** (0.008)	0.007** (0.003)	0.004 (0.003)
Tenure		0.009*** (0.002)		0.010*** (0.001)
Tenure ²		-0.000*** (0.000)		-0.000*** (0.000)
Female		-0.119*** (0.012)		-0.014*** (0.005)
Observations	5,964	5,964	6,319	6,319
R-squared	0.232	0.265	0.414	0.521

Panel B: Wage Components				
	<i>Private Schools</i>		<i>Public Schools</i>	
	ln(Base)	ln($\frac{Wage}{Base}$)	ln(Base)	ln($\frac{Wage}{Base}$)
Principal Effectiveness	0.012 (0.008)	0.012*** (0.005)	0.000 (0.003)	0.004** (0.002)
Tenure	0.003 (0.002)	0.006*** (0.001)	0.010*** (0.001)	0.000 (0.001)
Tenure ²	0.000 (0.000)	-0.000*** (0.000)	-0.000** (0.000)	-0.000 (0.000)
Female	-0.068*** (0.012)	-0.051*** (0.010)	-0.012** (0.005)	-0.002 (0.004)
Observations	5,964	5,964	6,319	6,319
R-squared	0.178	0.149	0.515	0.518

Notes: This table presents the results from specification (6). We focus on a sample of principals for whom we have an standardized measure of effectiveness and detailed wage data from 2015 to 2017. All specifications include year and municipality fixed effects. Bootstrapped standard errors (100 replications) clustered at the principal level are in parenthesis.

Table 5: Effect of ADP selection on principal effectiveness

Dependent variable: Standardized school principal effectiveness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Truncated sample	Within ADP	Placebo I	Placebo II
ADP turnover	0.037*** (0.011) [0.020]	0.036*** (0.011) [0.020]	0.038*** (0.011) [0.020]	0.039*** (0.011) [0.020]	0.040*** (0.012) [0.022]	0.035*** (0.012) [0.015]		
Principal turnover							0.014 (0.062) [0.079]	-0.007 (0.011) [0.022]
Observations	21,674	21,674	21,674	21,674	19,406	12,767	8,965	23,670
R-squared	0.857	0.857	0.857	0.853	0.852	0.784	0.973	0.882
# of Schools	3167	3167	3167	3167	2818	1663	2550	3349
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School controls	Yes	No	Yes	No	No	No	No	No
Municipality controls	Yes	Yes	No	No	No	No	No	No

Notes: This table presents the effects of the new selection system (ADP) on the standardized measure of principal effectiveness discussed in section 3. “ADP turnover” is a dummy that takes the value one after the first time a school selects a principal under the ADP system. Columns 1 to 4 estimate the regressions described by equation (7). Column 5 follows Crump et al. (2009) and truncates the sample based on a propensity score that estimates the probability that a school selects a principal under the ADP system. The optimal cut-off in our case is 11%. Column 6 estimates the main regression only within schools that selected a principal under the ADP system. Column 7 shows a placebo exercise where “Principal turnover” is a dummy that takes the value one after a principal turnover in a public school in the period 2009-2010 (pre-ADP reform). The number of schools who had a principal turnover in 2009 or 2010 is 872. Column 8 shows a similar placebo exercise where we focus on school turnover after 2012 but in private schools. The number of private schools that had a turnover after 2012 is 2,223. Robust standard errors in parenthesis, and robust standard errors clustered at school level in brackets.

APPENDIX (For Online Publication)

Manager’s Productivity and Labor Market: Evidence from School Principals

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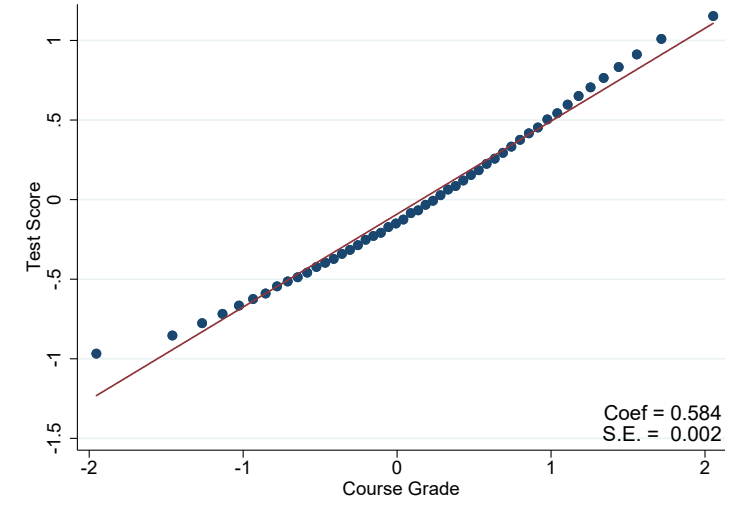
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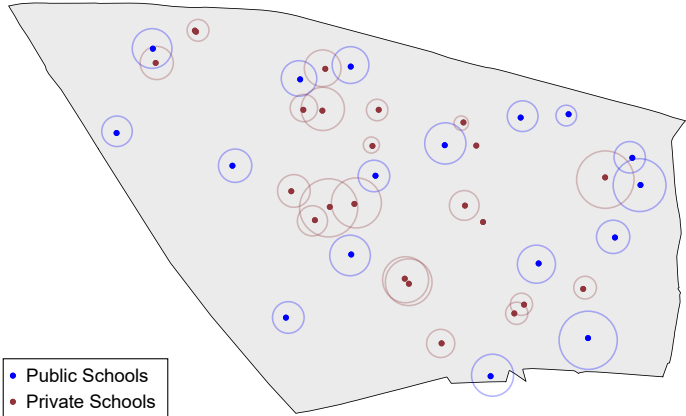
A Additional figures and tables

Figure A.1: Course grades and test scores



Notes: This figure shows the plot of a regression of test scores on course grades, including subject-grade and school fixed effects. The estimation sample includes 1,061,231 students for whom we observe test score and course grades contemporaneously for Math and Spanish. Robust standard errors are clustered at the school level.

Figure A.2: Public and private schools in a municipality



Notes: This figure shows the distribution of public and private schools in a low-income municipality at the north of Santiago, Conchalí. Both types of schools have co-existed and competed in Chile for at almost 40 years.

Figure A.3: Standardized national curriculum

Currículum Nacional
UNIDAD DE CURRÍCULO Y EVALUACIÓN
MINISTERIO DE EDUCACIÓN

Documentos Curriculares
Recursos Digitales
Asignaturas
Niveles escolares

Bases curriculares
Progresión de aprendizaje
Programa
Temas de estudio
Estándares e indicadores de calidad
Marco legal
Rubricaciones y estudios
Evaluación

Orientaciones para evaluar los aprendizajes

La evaluación forma parte constitutiva del proceso de enseñanza. Cumple un rol central en la promoción y en el logro del aprendizaje.

Para que se logre efectivamente esta función (la evaluación) debe tener como objetivos:

- Medir progreso en el logro de los aprendizajes
- Ser una herramienta que permita la autorregulación del alumno
- Proporcionar información que permita conocer fortalezas y debilidades de los estudiantes y, sobre esa base, retroalimentar la enseñanza y potenciar los logros esperados dentro de la asignatura
- Ser una herramienta útil para orientar la planificación

PROGRESIÓN DE OBJETIVOS DE APRENDIZAJE

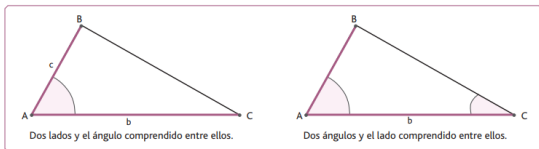
	1° Básico	2° Básico	3° Básico	4° Básico	5° Básico	6° Básico
GEOMETRÍA	OA16 Describir, comparar y construir figuras 3D (cubos, paralelepípedos, esferas y conos) con diversos materiales.		OA16 Describir cubos, paralelepípedos, esferas, conos, cilindros y pirámides de acuerdo a la forma de sus caras, el número de aristas y de vértices.			OA11 Demostrar que comprenden el concepto de línea de una superficie, en cubos y paralelepípedos calculando el área de sus redes (plantillas) asociadas.
			OA17 Reconocer en el entorno figuras 2D que están trasladadas, reflejadas y rotadas.	OA17 Demostrar que comprenden una línea de simetría: • identificando figuras simétricas 2D • creando figuras simétricas 2D • dibujando una o más líneas de simetría en figuras 2D • usando software geométrico	OA18 Demostrar que comprenden el concepto de congruencia, usando la traslación, la reflexión y la rotación en cuadrículas y mediante software geométrico	OA14 Realizar tessellaciones de figuras 2D usando traslaciones, reflexiones y rotaciones.
			OA18 Demostrar que comprenden el concepto de ángulo: • identificando ejemplos de ángulos en el entorno • estimando la medida de ángulos, usando como referente ángulos de 45° y de 90°	OA18 Trasladar, rotar y reflejar figuras 2D. OA19 Construir ángulos con el transportador y compararlos.		OA15 Construir ángulos agudos, obtusos, rectos, extendidos y completos con instrumentos geométricos y software geométrico.
						OA16 Identificar los ángulos que se forman entre dos rectas que se cortan (pares de ángulos opuestos por el vértice y pares de ángulos complementarios).

PLAN DE CLASE N° 3
Tiempo: 90 minutos

- Objetivos de la clase:**
- Construir triángulos geoméricamente conociendo la medida de dos lados y el ángulo comprendido entre ellos o la medida de dos ángulos y la longitud del lado comprendido entre ellos.

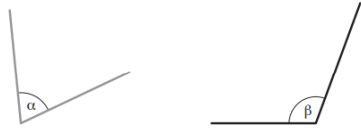
INICIO / 15 minutos

- Revisen la tarea en conjunto.
- Muestre el siguiente triángulo en la pizarra, explicando que en la clase anterior aprendieron a construir triángulos dadas las longitudes de tres lados y en esta clase aprenderán a construir triángulos dadas las longitudes de dos lados y el ángulo comprendido entre ellos y, también, dados dos ángulos y el lado comprendido entre ellos.



Instrucciones: Lee con atención el enunciado de las preguntas y haz un círculo a la letra con la respuesta correcta. Debes marcar solo una alternativa.

1. Observa la siguiente imagen. Se sabe que el ángulo α mide 57° y el ángulo β mide 123° .

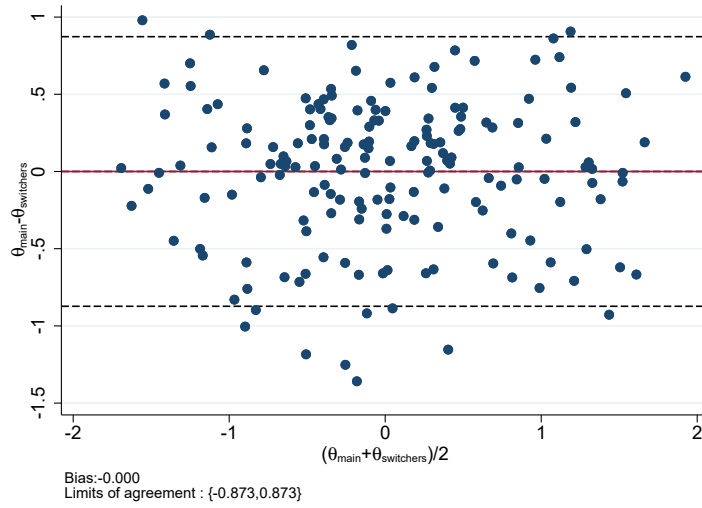


Al unir los ángulos α y β por uno de sus lados, y haciendo coincidir el vértice, se forma:

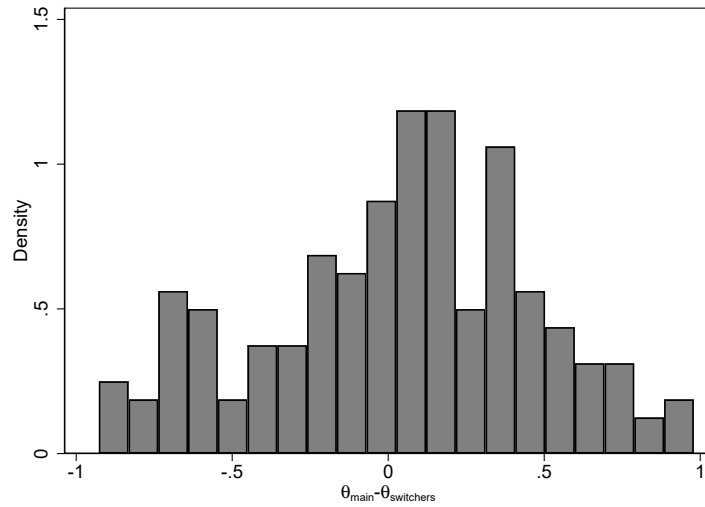
- A. Un ángulo agudo, porque α y β son agudos.
- B. Un ángulo obtuso, porque $57^\circ + 123^\circ$ es mayor que 90° y menor que 180° .
- C. Un ángulo extendido, porque $57^\circ + 123^\circ$ es igual a 180° .
- D. Un ángulo obtuso, porque el ángulo β tiene una gran abertura.

Notes: This figure displays different materials that are available to teachers as part of the national curriculum in Chile. Through the National Curriculum webpage (top left), teachers can access the specific topics that must be covered by grade and year (top right) and specific lesson plans and exams related to a given topic (bottom left and right, respectively).

Figure A.4: Schools as confounder



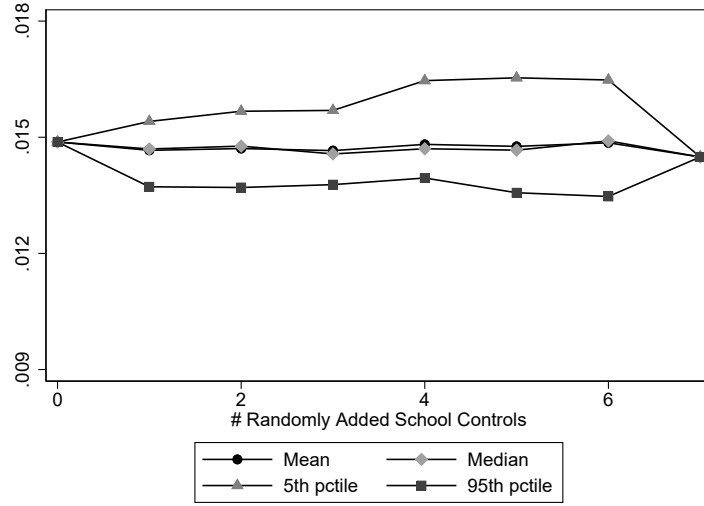
A. Bland-Altman Plot



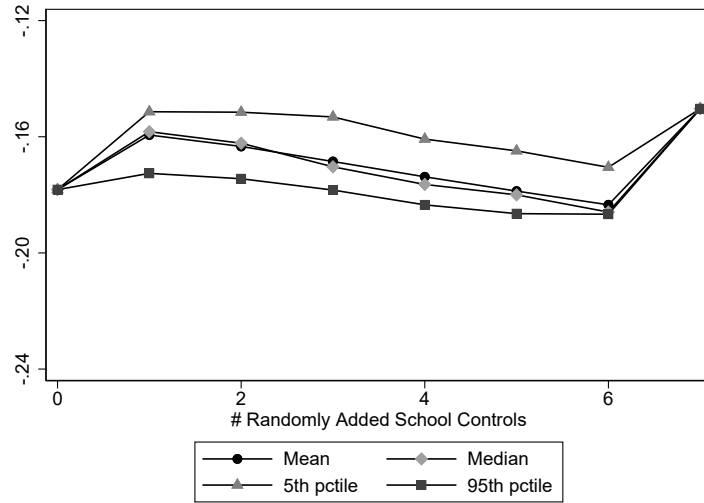
B. Bland-Altman Histogram

Notes: Panel A shows a Bland-Altman plot (e.g., [Bland and Altman, 1986](#)) comparing our main measure of principal effectiveness to an alternative measure that uses school fixed effects instead of correlated random effects, as described in section 3. Panel B shows the histogram of the difference among these two measures.

Figure A.5: Coefficient stability



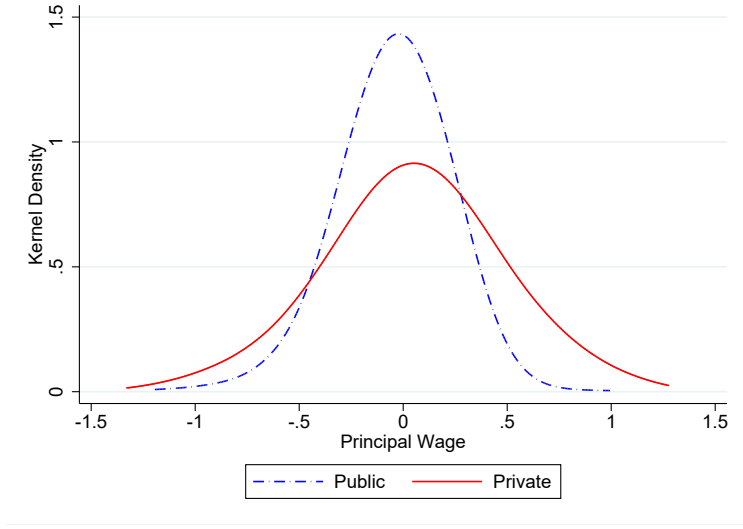
A. Teachers' Surveys



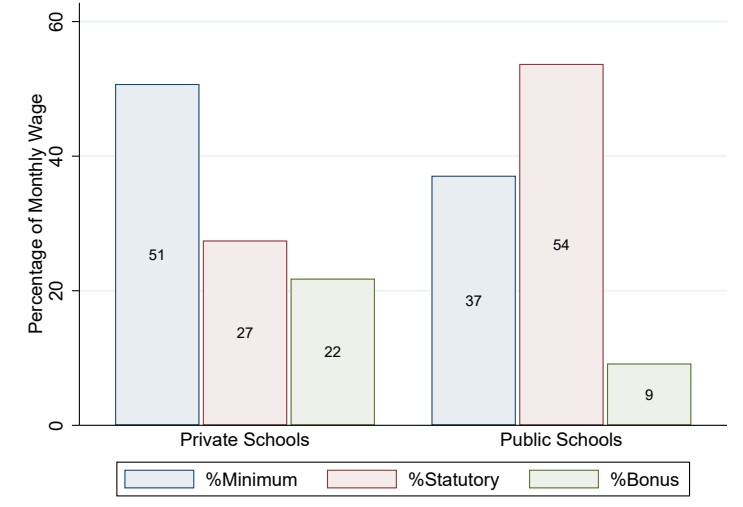
B. School Complaints

Notes: Panel A shows the impact of principal effectiveness on the average agreement of teachers across statements. Panel B shows the impact of principal effectiveness on the total number of complaints filed against the school. In this figure we randomly add subsets of the full set of school control variables. We carry out 150 random draws of controls. We always include the baseline municipality fixed effects and we randomize over the other controls, including: total enrollment, share of disadvantaged students, income per student, teachers per student, and indicators for whether the school is rural, private, or public. The point estimate from the baseline specification corresponds to 0 in the x-axis and the one with all the controls to 7 in the x-axis.

Figure A.6: Principals' wages



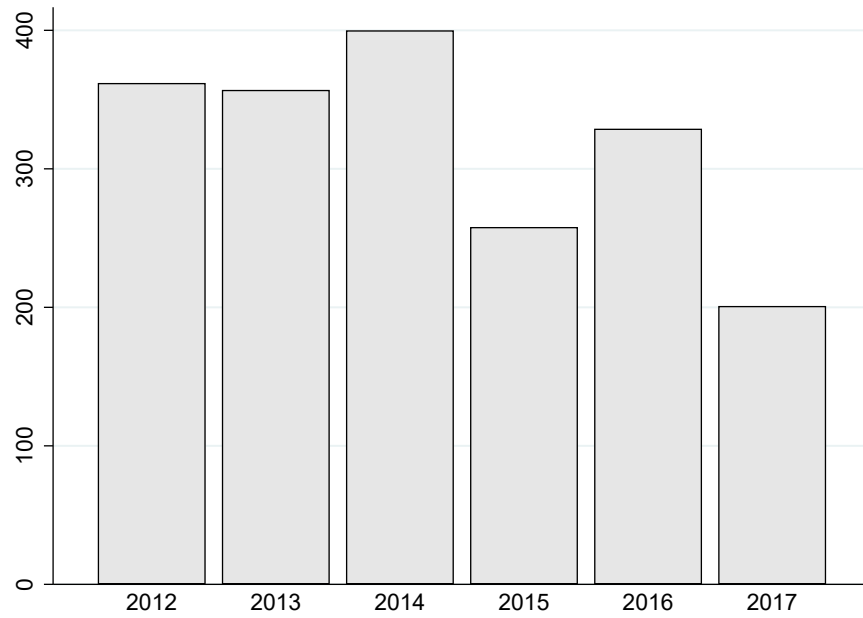
A. Residualized Log Wage



B. Wage Components

Notes: Panel A presents the distributions of log principals' wages in both public and subsidized private schools. Log principals' wages are residualized with respect to year and municipality fixed effects. Panel B decomposes the average monthly wage of school principals into the three components discussed in the data section: minimum legal wage, statutory payments, and bonuses. We present the share that each of these components represents of the principal' monthly wage, separately for subsidized private and public schools.

Figure A.7: Number of new principals elected by ADP, per year



Notes: This figure shows the number of schools that elected a principal through the new ADP selection system for the first time, by year.

Table A.1: Summary statistics

	(1)	(2)	(3)	(4)	(5)
	Mean	Standard deviation	Median	10th pctile	90th pctile
Panel A: Student characteristics					
Math course grade	5.2	0.8	5.2	4.1	6.3
Spanish course grade	5.2	0.7	5.2	4.3	6.2
Math test scores	257.2	49.7	258.8	190.9	320.4
Spanish test scores	254.2	49.7	256.8	185.5	317.7
Grade retention (%)	4.3	15.8	0.0	0.0	0.0
Panel B: School characteristics					
Enrollment	299.3	397.4	153.0	3.0	817.0
Class size	20.0	12.9	21.0	2.0	37.0
Annual subsidy per student (USD)	2424.3	3704.6	1446.5	840.8	4089.8
Share of disadvantaged students	49.0	35.1	58.0	0.0	93.0
Teachers per hundred students	8.0	17.1	5.7	2.6	14.3
Rural school	46.6	49.9	0.0	0.0	100.0
School attendance	86.1	9.5	87.8	76.2	95.1
Panel C: Principal characteristics					
Wage (USD)	2738.8	1982.4	2525.3	1587.0	3920.8
% Base salary	42.5	18.9	35.9	24.0	72.3
% Bonus	16.0	18.9	7.5	0.8	48.5
% Statutory	41.5	22.7	43.3	10.5	69.0
Tenure	27.3	12.2	29.0	10.0	42.0
Age	54.2	9.9	55.0	40.0	65.0
Female	52.5	49.9	100.0	0.0	100.0

Notes: This table presents summary statistics for students, schools, and principals. We only include subsidized private and public schools.

Table A.2: Wages and GPA

	<i>Dependent variable: Log Monthly Wage</i>				
	(1)	(2)	(3)	(4)	(5)
GPA (standarized)	0.171*** (0.002)	0.171*** (0.002)	0.155*** (0.002)	0.154*** (0.003)	0.118*** (0.003)
Observations	195,913	195,911	195,911	106,244	104,276
R-squared	0.057	0.104	0.115	0.078	0.081
Female FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	No	Yes	Yes	Yes	Yes
Type School FE	No	No	Yes	Yes	Yes
Income Quintil FE	No	No	No	Yes	Yes
PSU Score	No	No	No	No	Yes

✕ **Notes:** To construct this table, we use a sample of students who graduated from high school in 2007 or 2008, took the college admission test (PSU) immediately after, and provided their socioeconomic information to access financial aid. For them, we observe wages in the formal sector in 2017. GPA corresponds to the average course grades in high school. We control by several characteristics such as student gender, type of school (public, subsidized private or private), the municipality where the school is located, and their income (using income quintile dummies). In column 5, we also control by the average score (in Math and Spanish) obtained by the student in the PSU.

Table A.3: Comparison table

Dependent Variable: Principal Effectiveness (standardized)

	$\hat{\theta}_p$ Course Grades SD($\hat{\theta}_p$) = 0.221; 9,824,556 Obs.			$\hat{\theta}_p$ Test Scores SD($\hat{\theta}_p$) = 0.316; 235,434 Obs.		
	β Coef	Std. Error	Obs.	β Coef	Std. Error	Obs.
Survey Responses and Complaints						
Aggregate Score	0.013***	(0.004)	8337	0.014***	(0.005)	4877
N Complaints	-0.209***	(0.057)	4847	-0.033***	(0.012)	4342
Teachers' Turnover Rate						
Public Schools	-0.002	(0.002)	165027	0.003	(0.003)	120075
Private Schools	-0.008***	(0.002)	193271	0.001	(0.002)	161897
Log Hourly Wages						
Wages (Public School)	0.007**	(0.003)	6319	0.007*	(0.003)	4667
Wages (Private Schools)	0.026***	(0.007)	5953	0.044***	(0.007)	5060
ADP Personnel Selection						
Without Controls	0.038***	(0.011)	22411	0.027**	(0.014)	13239
With Controls	0.037***	(0.012)	21674	0.042***	(0.014)	12908

Notes: This table compares our main results with those obtained when we use an alternative measure of principal effectiveness based on test scores. †: Notice that we cannot estimate teacher value-added using test scores as students do not take exams every year. Thus, we use our main measure of teacher value-added based on course grades.

Table A.4: Descriptive statistics for three different samples

	$\Delta \text{Teacher}=0$		$\Delta \text{Teacher}=1$		Complete Sample	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Primary (2-8)	0.82	0.38	0.82	0.39	0.82	0.39
Secondary (9-11)	0.18	0.38	0.18	0.39	0.18	0.39
Subject = Math	0.50	0.50	0.50	0.50	0.50	0.50
Course Grade	5.68	0.62	5.66	0.63	5.66	0.63
% Attendance	92.73	6.72	92.12	7.28	92.21	7.21
% Rural School	0.14	0.34	0.08	0.27	0.09	0.28
% Public School	0.45	0.50	0.36	0.48	0.37	0.48
School Size	625	465	822	611	794	596

ix:

Notes: This table presents descriptive statistics of students in three different samples. $\Delta\text{Teacher} = 0$ corresponds to students that are not included in our analysis because they do not experience a change in teacher between t and $t + 1$ (1,791,983 observations). $\Delta\text{Teacher} = 1$ corresponds to students considered for the analysis (10,917,716 observations). Notice that for some of them there are no lagged course grades so they are not considered in the value-added estimation. For comparison, we also present the same descriptive statistics for the whole sample (12,709,699 observations).

Table A.5: Assignment of students to principals

Panel A: Regression of Gain Achievement				
	(1)	(2)	(3)	(4)
	Course grade at school s_0			
	Math	Spanish	Math	Spanish
$\hat{\theta}_{p(i)}$ at school s_0	0.119*** (0.010)	0.117*** (0.009)	0.128*** (0.010)	0.124*** (0.010)
$\hat{\theta}_{p(-i)}$ at school s_1			-0.018** (0.008)	-0.013* (0.007)
Observations	116,349	119,778	116,349	119,778

Panel B: Regression with Lagged Achievement Control				
	(1)	(2)	(3)	(4)
	Course grade at school s_0			
	Math	Spanish	Math	Spanish
$\hat{\theta}_{p(i)}$ at school s_0	0.194*** (0.009)	0.195*** (0.007)	0.188*** (0.009)	0.187*** (0.008)
$\hat{\theta}_{p(-i)}$ at school s_1			0.012* (0.007)	0.017*** (0.007)
Observations	116,349	119,778	116,349	119,778

Notes: This table shows the results from the validation exercise discussed in section 3. All specifications include grade fixed effects. Bootstrapped standard errors (100 replications) clustered by school of origin.

Table A.6: School and municipality characteristics, by ADP adoption

	(1)	(2)	(3)	(4)	(5)	(6)
	Never ADP	Ever ADP	Difference	Early ADP	Late ADP	Difference
Panel A: School characteristics						
Total enrollment	100.330 (186.517)	454.654 (351.519)	354.325 (0.000)***	489.115 (352.286)	433.365 (349.506)	-55.750 (0.001)***
Rural school	0.838 (0.369)	0.256 (0.437)	-0.582 (0.000)***	0.224 (0.418)	0.276 (0.447)	0.051 (0.015)**
Share of disadvantaged students	0.702 (0.220)	0.508 (0.225)	-0.193 (0.000)***	0.493 (0.240)	0.518 (0.214)	0.026 (0.018)**
Income per student	19.631 (25.551)	6.455 (1.859)	-13.175 (0.000)***	6.471 (1.798)	6.446 (1.896)	-0.025 (0.784)
4th grade test score (Spanish)	255.565 (30.287)	255.610 (22.027)	0.045 (0.961)	256.303 (21.944)	255.203 (22.077)	-1.100 (0.344)
4th grade test score (Math)	238.975 (33.034)	245.459 (24.510)	6.484 (0.000)***	245.793 (24.177)	245.262 (24.715)	-0.531 (0.682)
Graduation test score (Spanish)	414.049 (43.775)	436.469 (57.979)	22.420 (0.000)***	439.418 (55.863)	434.231 (59.561)	-5.188 (0.380)
Graduation test score (Math)	418.480 (41.965)	441.136 (55.204)	22.657 (0.000)***	441.347 (54.580)	440.977 (55.794)	-0.370 (0.948)
Panel B: Municipality characteristics						
Share of households in poverty	0.124 (0.075)	0.082 (0.056)	-0.042 (0.000)***	0.082 (0.057)	0.082 (0.055)	-0.000 (0.952)
Income per capita	1.699 (0.489)	2.151 (1.115)	0.453 (0.000)***	2.223 (1.400)	2.107 (0.892)	-0.115 (0.033)**
Unemployment rate	0.080 (0.047)	0.080 (0.047)	0.001 (0.626)	0.083 (0.050)	0.079 (0.045)	-0.004 (0.064)*
Average years of schooling	8.974 (1.124)	9.998 (1.315)	1.024 (0.000)***	9.930 (1.385)	10.041 (1.269)	0.110 (0.083)*
Observations	3,029	1,820	4,849	695	1,125	1,820

Notes: This table presents the differences between public schools that have selected principals under the ADP system and schools that have not. It also shows the differences between early (2012-13) adopters and late (post 2014) adopters of the ADP selection system. All characteristics are measured in 2010 (pre-reform). Columns 1 and 2 present the statistics for ADP and non-ADP, while column 3 presents the difference and the p-value of the difference (in parenthesis). Columns 4 and 5 present the statistics for early and late adopters, while column 6 presents the difference between both and the p-value of the difference.

Table A.7: Characteristics of principals in public schools, by ADP status

	(1) Not ADP	(2) ADP	(3) Difference
Panel A: Ever worked			
As teacher	0.567 (0.405)	0.473 (0.404)	-0.094 (0.000)***
As admin. support worker	0.156 (0.308)	0.262 (0.358)	0.107 (0.000)***
As administrative worker	0.751 (0.386)	0.831 (0.362)	0.080 (0.000)***
In a private school	0.034 (0.182)	0.077 (0.267)	0.043 (0.000)***
Panel B: Principal characteristics			
College degree	0.846 (0.361)	0.899 (0.301)	0.053 (0.000)***
Age	54.759 (8.709)	53.547 (8.347)	-1.211 (0.000)***
Female	0.463 (0.499)	0.477 (0.500)	0.014 (0.325)
Observations	3,046	1,985	5,031

Notes: This table compares the characteristics of public schools' principals who have been appointed under the ADP system and those who have not. Columns 1 and 2 present the average and standard deviation of different characteristics, and column 3 presents the difference among these two groups and its p-value (in parenthesis).

Table A.8: Characteristics of origin and destination schools of ADP principals

	(1)	(2)	(3)
	School of Origin	School of Destination	Mean Difference
Panel A: School characteristics			
Monthly principal wage (1000 USD)	2.594 (0.888)	2.601 (0.609)	0.007 (0.029)
Monthly school wages (1000 USD)	0.993 (0.192)	0.999 (0.186)	0.006 (0.007)
Share of disadvantaged students	34.946 (23.400)	62.725 (16.685)	27.779 (0.716)***
Average test scores	-0.178 (0.637)	-0.245 (0.656)	-0.067 (0.026)***
Total enrollment	459.152 (351.429)	432.645 (321.247)	-26.507 (11.866)**
Income per student	8.048 (3.944)	10.699 (3.179)	2.651 (0.126)***
Rural school	0.247 (0.412)	0.221 (0.411)	-0.025 (0.015)*
Panel B: Municipality characteristics			
Share of households in poverty	0.073 (0.054)	0.033 (0.019)	-0.040 (0.002)***
Income per capita	2.358 (1.244)	3.489 (1.830)	1.131 (0.075)***
Unemployment rate	0.081 (0.044)	0.079 (0.026)	-0.002 (0.002)
Average years of schooling	10.126 (1.354)	10.833 (1.278)	0.707 (0.068)***
Observations	1,610	1,610	3,220

Notes: This table compares the school of origin and destination of principals elected by the new ADP selection system. Columns 1 and 2 present the average and standard deviation of different characteristics of the schools and the municipalities where schools are located. Column 3 presents the mean difference between these two groups and the standard deviation of the difference (in parenthesis).

B Management of classrooms: Students' sorting

Principals could also affect schools through the assignment of students to classrooms (tracking) and the assignment of classrooms to teachers (matching). A rationale for this could be to help teachers to target their lessons (e.g., [Duflo et al., 2011](#); [Card and Giuliano, 2016](#)). As pointed out by [Rothstein \(2010\)](#), sorting may also happen through efforts on the principal's part to reward favored teachers through the allocation of easy-to-teach students, or through parental requests (e.g., [Jacob and Lefgren, 2007](#); [Kalogridis and Loeb, 2013](#)). Thus, the assessment of tracking is also important as it represents a potential source of bias in models of teacher value-added (e.g., [Horváth, 2015](#)). To assess the extent of tracking in Chile, and to explore how principal's effectiveness is related to it, we construct a sorting index at the school-year level à la [Kremer and Maskin \(1996\)](#). Specifically, we measure how students sort across classrooms by estimating the following regression:

$$\bar{y}_{cg} = \alpha_g + \tau_{st}y_{i(c,g)} + \epsilon_{cg}, \quad (9)$$

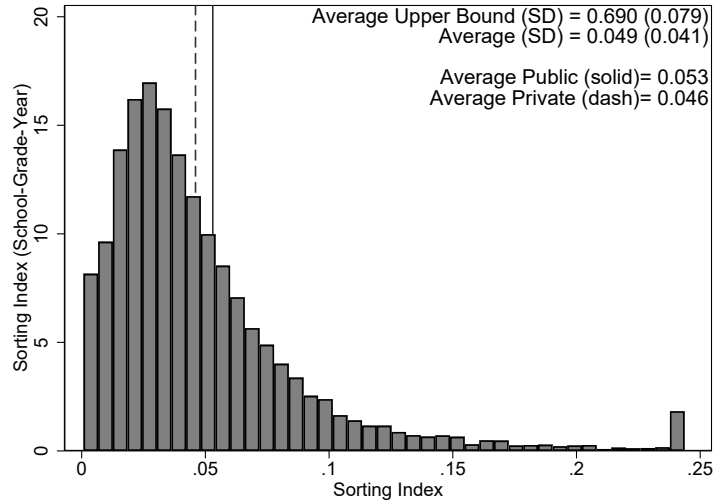
where \bar{y}_{cg} stands for the average course grade of classroom c of grade g , and $y_{i(c)}$ represents the course grade of student i who sits in classroom c . We estimate this specification by school and year, including grade fixed effects. The sorting index is given by our estimate of τ_{st} :

$$\tau_{st} = \frac{Cov(\bar{y}_{cg}, y_{i(c,g)})}{Var(y_{i(c,g)})}.$$

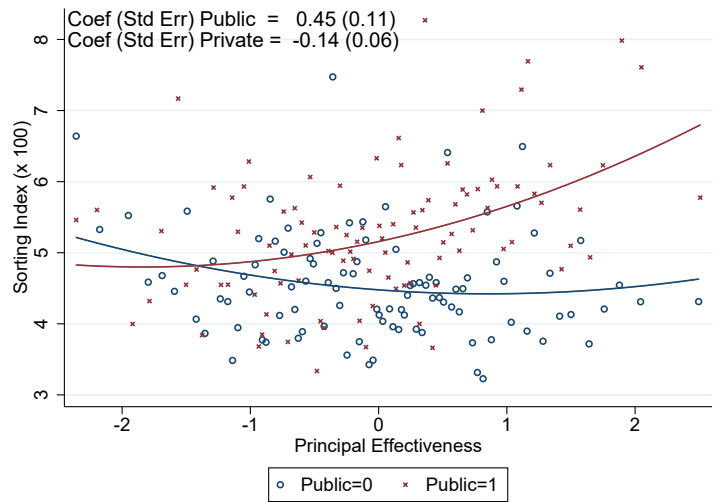
Intuitively, perfect sorting is a case in which all variation in $y_{i(c,g)}$ comes from variation between instead of within classrooms. Indeed, this metric is equivalent to the R^2 from a regression of $y_{i(c,g)}$ on classroom dummies. We first evaluate the prevalence of students' sorting in Chilean schools. [Figure C.1, Panel A](#), plots the histogram of our estimated sorting index τ_{st} . We find that, on average, $\hat{\tau}_{st}$ is 0.049 with a standard deviation of 0.041. In order to benchmark this number, we construct a theoretical upper bound by ranking students within a school-grade and sorting them across all classrooms in a given grade in the order determined by their ranking. The average upper bound that we obtain from this procedure is 0.690 with a standard deviation of 0.079. From this exercise, we conclude that sorting based on course grades is not very common in Chile as it only represents a 7% of this empirical upper bound. In spite of the low prevalence of sorting in Chilean schools, we explore the association between principal effectiveness and our sorting index. [Figure C.1, Panel B](#), depicts a bin scatter with the relationship between the sorting index and principal effectiveness in public and private schools. We find that a one standard deviation increase in principal effectiveness increases sorting by 0.005 pp over a base of 0.053 in public schools and decreases it by 0.002 pp over a base of 0.046 in private schools. It is worth noticing that, despite

the fact that these effects are small in magnitude, the low baseline levels of sorting imply large effect sizes.

Figure C.1: School-level sorting



A. Sorting Index



B. Sorting Index and Principal Quality

Notes: Panel A plots the histogram of a school-year measure of sorting of students across classrooms. Specifically, it plots the τ_{st} coefficients from several [Kremer and Maskin \(1996\)](#) type of regressions described in appendix B. The top right of the figure presents the actual between-classrooms (and within school-grade) sorting and a theoretical upper (UB) of sorting. To construct this UB, we rank students within a school-grade, according to their course grades, and then we sort them across classrooms in the order determined by their ranking. Panel B shows the relationship between our measure of sorting and standardized principal effectiveness, by type of school. Bootstrapped standard errors (100 replications) are clustered at the principal level.

C Principals' compensation in public schools

Do higher wages attract more effective principals into public schools? Although this question is of high practical relevance, empirical progress has met with at least two important hurdles. First, it is difficult to measure an individual's quality, and second, different wages for a given position are not typically assigned exogenously. In this section, we attempt to answer that question by leveraging quasi-experimental variation in principal compensation together with our measure of principal effectiveness.

Related research on this topic has shown that higher wages can attract higher quality candidates into the workforce of the state (see Dal Bó et al., 2013), as well as better politicians into the legislative body (Ferraz and Finan, 2011), pointing to a positive relationship between compensation and the development of state capabilities. In this spirit, the *Quality and Equity in Education* Law passed in Chile in 2011 increased the compensation for school principals using an arbitrary rule that depends on the school size, e.g., total enrollment, and the concentration of disadvantaged students, e.g., share of students eligible for a voucher targeted to the poor. Since 2012, the compensation of the principal increases discontinuously in schools with more than 60% of disadvantaged students, and around the school-size thresholds of 400, 800 or 1,200 enrolled students.

In what follows, we focus our empirical analysis on the cutoff with the largest mass around it: the school-size threshold of 400 enrolled students. Our main analysis uses administrative data on wages between 2015 and 2017. We also digitized data on raw wages for the pre-reform period (2009 to 2011) in order to perform placebo checks. Administrative data on wages was obtained directly from the Superintendency of Education while digitized wage data comes from Chilean municipalities and was obtained via a Freedom-of-Information request (only a subset of municipalities responded).

Empirical strategy and results

We are interested in the following relationship between wages and principal effectiveness:

$$\hat{\theta}_p = \beta \ln(\text{wage}_p) + f(r_p) + e_p, \quad (10)$$

where $\hat{\theta}_p$ is the standardized version of the measure of principal effectiveness discussed in section 3, $\ln(\text{wage}_p)$ is the logarithm of the average monthly wage paid to the school principal, $f(r_p)$ is a function of the school size, and e_p is an error term. A potential threat to identification in this setting is that the wage paid by a given school to the principal is not exogenous. If that is the

case, ordinary least squares (OLS) estimates of equation (10) will not recover the causal effect of wages. We attempt to address this concern by estimating a two-stage least squares model (2SLS) with the following first-stage equation:

$$\ln(\text{wage}_p) = \pi Z_p + g(r_p) + v_p, \quad (11)$$

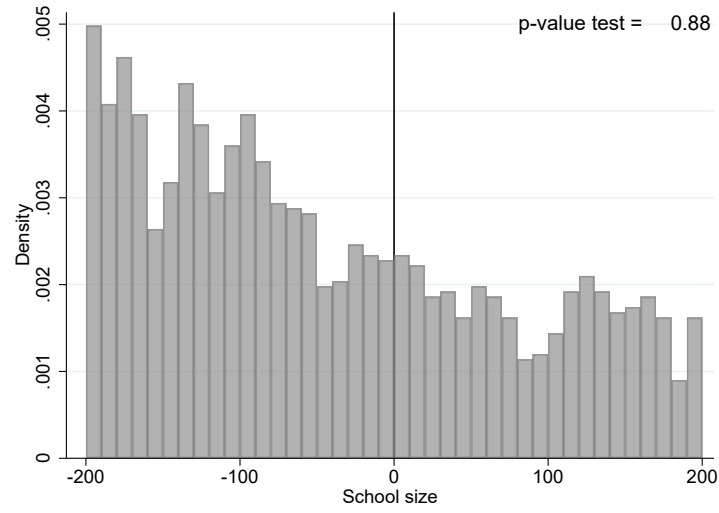
where $Z_p = 1(r_p > 0)$ is a dummy that equals 1 if the school has at least 400 students, and $g(r_p)$ is a linear function of the school size $r_p = (\text{School Size} - 400)$, at both sides of the threshold.

We start by noticing that covariates are balanced between “treated” and “untreated” schools. Schools right above the cutoff look much like schools below. Table D.1 presents a simple comparison of baseline characteristics for schools at both sides of the cutoff. These estimated coefficients come from separate regressions of each baseline characteristic on the initial eligibility indicator Z_p , substituting $\ln(\text{wage}_p)$ by the corresponding characteristic in equation (11). Column 3 of this table presents the p-values from the permutation test for the difference proposed by Canay and Kamat (2017). We do not find any statistically significant difference. Moreover, visual inspection of the density of the running variable in Figure D.1 does not indicate manipulation of total enrollment. Indeed, we passed the test proposed by Cattaneo et al. (2018), which fails to reject the null hypothesis of equal densities around the cutoff.

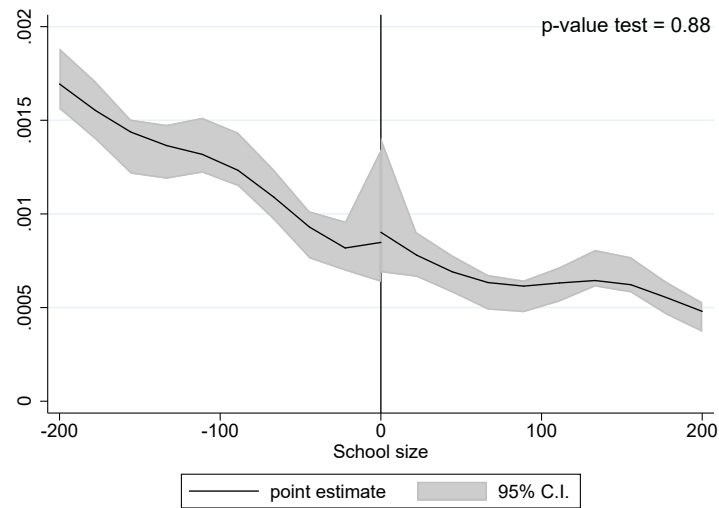
Our main results are presented in Table D.2. For estimation, we consider a sample of schools within an optimal bandwidth of 104 students around the cutoff and a linear school size polynomial.³³ Columns 1 to 3 focus on the post-reform period for which we have wage data available (2015-2017). Column 1 shows the first stage. We find that crossing the school-size threshold increases wages by 10.5%, a statistically significant effect but with an F-test slightly below 10. Column 2 suggests that schools that are marginally above the school-size threshold have principals that are 0.21 standard deviations more effective, compared to schools marginally below the threshold. The results from our 2SLS approach are presented in column 3. We estimate a large but imprecise effect. A 10% increase in wages is associated with an average increase in principal effectiveness of 0.2 standard deviations, but at a 5% level of risk, this effect ranges from -0.03 to 0.44. Finally, we present a placebo exercise in which we estimate the first stage and reduced form models but during the pre-reform period (2009-2011). Columns 4 and 5 show these results. Reassuringly, we find that before the Law of *Quality and Equity in Education* of 2011, crossing the school-size threshold had a zero effect both on wages and principal effectiveness.

³³We select the optimal bandwidth based on the reduced form regression, following Calonico et al. (2014). Table D.3 shows the robustness of our results to different bandwidths.

Figure D.1: Manipulation of school enrollment



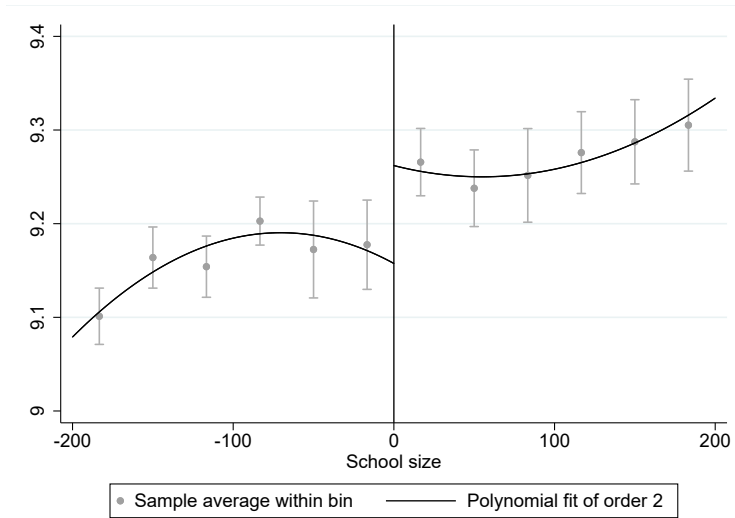
A. Manipulation: Histogram



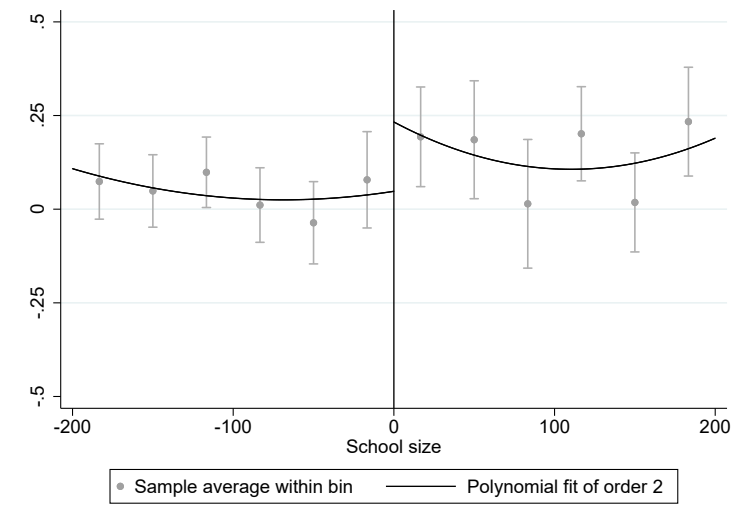
B. Manipulation: Density

Notes: Panels A and B present the histogram and density of the running variable: school enrollment. The vertical red line shows the cut-off at 400 students. At the top right of the figure, we present the p-value for the manipulation test around the cut-off proposed by Cattaneo et al. (2018).

Figure D.2: Wages and principal effectiveness



A. Effect on Wages



B. Effect on Principal Effectiveness

Notes: Panels A and B present the histogram and density of the running variable: school enrollment. The vertical red line shows the cut-off at 400 students. At the top right of the figure, we present the p-value for the manipulation test around the cut-off proposed by Cattaneo et al. (2018). Panels B and C present mean wages and principal effectiveness around the cut-off. Each dot represents the average within the bin, while the vertical lines shows the confidence interval at 95%.

Table D.1: Balance in observables around the cut-off

	(1)	(2)	(3)
	Unrestricted sample	Restricted sample	
	OLS	OLS	Canay & Kamat p-value
Panel A: School characteristics			
Rural School	0.12*** (0.02)	0.03 (0.04)	1.00
Share of poor students	0.44 (1.03)	-2.97 (1.85)	0.62
4rd grade Spanish test score	0.03 (0.05)	0.00 (0.08)	0.55
4rd grade maths test score	-0.08* (0.04)	-0.00 (0.07)	0.55
H-S grad Spanish test score	-0.02 (0.04)	0.08 (0.07)	0.55
H-S grad math test score	-0.01 (0.04)	0.12* (0.07)	0.49
Panel B: Municipality characteristics			
Share of HH in extreme monetary poverty	0.33* (0.17)	-0.17 (0.35)	0.85
Average HH per capita income	0.02 (0.06)	-0.02 (0.08)	0.66
Unemployment rate	-0.07 (0.26)	-0.16 (0.50)	0.07
Average years of schooling	-0.13* (0.07)	0.09 (0.12)	0.61
	-0.63*** (0.17)	0.07 (0.30)	0.21

Notes: This table shows the regression presented in equation (11) for different school and municipality characteristics measured before the reform. Column 1 presents the results for the full sample, while column 2 shows the result using the optimal bandwidth proposed by Calonico et al. (2014). Column 3 presents the p-value suggested by Canay and Kamat (2017) for the test of the difference around the cut-off. Robust standard errors are presented in parenthesis.

Table D.2: Wages and principal effectiveness

Dependent variable: Standardized school principal effectiveness

	(1)	(2)	(3)	(4)	(5)
	Post-Reform			Pre-Reform	
	Wage	Principal effectiveness	Principal effectiveness	Wage	Principal effectiveness
Log wage			2.041* (1.204)		
Above threshold	0.105*** (0.034)	0.214** (0.106)		0.010 (0.041)	0.032 (0.111)
Observations	782	782	782	471	471
F excl instr			9.383		

Notes: This table presents the results from equation (11). Log wage is the logarithm of monthly wages, and principal effectiveness is a standardized measure of our estimated principal fixed effect discussed in section 3. We select the optimal bandwidth from the reduced form regression following Calónico et al. (2014). This optimal bandwidth is 96. All regressions include a linear polynomial around the cut-off. Robust standard errors are presented in parenthesis.

Table D.3: Wages and principal effectiveness: Robustness to bandwidth selection

	(1)	(2)	(3)	(4)	(5)
<i>Bandwidth</i>	Optimal	90	100	110	120
Panel A: First stage					
Above threshold	0.105*** (0.034)	0.088** (0.037)	0.102*** (0.035)	0.097*** (0.033)	0.083*** (0.032)
Panel B: Reduced form					
Above threshold	0.214** (0.106)	0.166 (0.114)	0.204* (0.108)	0.229** (0.102)	0.166 (0.114)
Panel C: 2SLS					
Log wage	2.041* (1.204)	1.888 (1.528)	2.002 (1.262)	2.355* (1.326)	2.698* (1.551)
Observations	782	663	752	834	920
F excl instr	9.383	5.708	8.936	9.200	6.824

Notes: This table presents the robustness of the results in table D.2 to different bandwidths around the cut-off. Robust standard errors are in parenthesis.

D A two-sided matching model

This section builds on Logan (1996) to simultaneously investigate schools' preferences to offer a job and workers' choice given the job offers. The model is based on an underlying random matching model of the labor market, which itself is a stochastic variant of deterministic two-sided matching models studied in game theory (e.g., Roth and Sotomayor, 1990).³⁴ The timing of the model is the following:

- Workers apply to all available schools.
- Schools evaluate applicants and make offers according to a decision rule.
- Workers evaluate the received offers and choose the highest-utility alternative.

The school's decision

Similar to Abowd and Farber (1982), an underlying random utility model is defined to describe the decision of schools regarding whether or not to make jobs available to particular workers. For school j , the utility of hiring worker i of ability θ_i is defined as:

$$U_j(i) = m_j + \beta_j \theta_i + \epsilon_{1ij}, \quad (12)$$

while j 's utility of not hiring worker i is:

$$U_j(-i) = s_j + \epsilon_{0ij}, \quad (13)$$

where m_j represents market effects on the utility of hires in general (e.g., reflecting the need for filling the position), β_j is the increase in utility that the school would experience from hiring a worker of marginally higher quality, and s_j is simple a baseline utility that school j derives from its present state of staffing. Finally, ϵ_{1ij} and ϵ_{0ij} represent factors that are not known to the observer but that influence the utility of school j of hiring or not hiring worker i .

When expression (12) is greater in value than expression (13), employer j makes a job available: $o_{ij} = 1$, zero otherwise. Thus, the exact probability that school j will make an offer depends on

³⁴This game is a random variant of the “college admissions” game of the formal game theory literature, and because the deterministic results are transferable to the random matching game, it is known that at least one stable matching of employers and workers exists such that no worker-employer pair who are not matched to each other can improve their utilities by abandoning any current pair and establishing a new match together.

the distribution of the differences between the two error terms, as well as on the non-stochastic parts of (12) and (13). If ϵ_{1ij} and ϵ_{0ij} are *iid* type I extreme value, then the difference will follow a logistic distribution, and the probability that j will make an offer is given by:

$$Pr(o_{ij}) = \frac{\exp(\beta_{0j} + \beta_j \theta_i)}{1 + \exp(\beta_{0j} + \beta_j \theta_i)}, \quad (14)$$

where $\beta_{0j} = m_j - sj$, and the offer of unemployment is always available to the workers, i.e., $Pr(o_{i0}) = 1$.

The worker's decision

Assuming that employers act independently of one another, conditional on workers' quality θ_i , then each applicant would be presented some set O_k of offers from the employers as a whole. There will be $R = 2^J$ distinct possible offering sets when J employers make separate decisions. Given this, the probability that worker i obtain a given offering set O_k is given by:

$$Pr(S_{ik}) = \prod_{m \in O_k} Pr(O_{im} = 1) \prod_{n \in \bar{O}_k} Pr(O_{in} = 0), \quad (15)$$

where m is an element (offer) of set K and n is an element of the complement set of O_k . A worker will choose her most preferred offer from the offering set that she faces. This is specified as a second random utility model. The indirect utility that worker i obtains from the job offered by employer j is defined as:

$$V_{i(j)} = h_j + w_j \theta_i + v_{ij}, \quad (16)$$

where h_j represents a baseline level of payments and amenities, w_j is a pay-for-performance component offered by the employer, and v_{ij} represents idiosyncratic preferences of the worker for a given job. Workers evaluate simultaneously every job offer that they find available to choose the one that delivers the highest utility. If v_{ij} follows a type I extreme value distribution, then the probability that worker i selects job j given the set of offers O_k is given by this polytomous conditional logit:

$$Pr(A_{ij} | O_k) = \begin{cases} \frac{\exp(h_j + w_j \theta_i)}{\sum_{h \in O_k} \exp(h_h + w_h \theta_i)} & , \quad j \in O_k \\ 0 & , \quad j \notin O_k. \end{cases} \quad (17)$$

Given our assumptions about the distribution of the random components in (12), (13), and (16), and further assuming that these random components are mutually independent, the probability

that worker i ends-up in job j is given by:

$$\begin{aligned}
Pr(A_{ij}) &= \sum_{k=1}^R Pr(A_{ij} | S_{ik}) \times Pr(S_{ik}) \\
&= \sum_{k=1}^R Pr(A_{ij} | S_{ik}) \times \prod_{m \in O_k} Pr(O_{im} = 1) \times \prod_{n \in \bar{O}_k} Pr(O_{in} = 0) \\
&= \sum_{k: j \in O_k} \frac{\exp(h_j + w_j \theta_i)}{\sum_{h \in O_k} \exp(h_h + w_h \theta_i)} \times \prod_{m \in O_k} \frac{\exp(\beta_{0m} + \beta_m \theta_i)}{1 + \exp(\beta_{0m} + \beta_m \theta_i)} \\
&\quad \times \prod_{n \in \bar{O}_k} \frac{1}{1 + \exp(\beta_{0n} + \beta_n \theta_i)}.
\end{aligned}$$

Importantly, from this model we can obtain the expected quality of the workforce in a given school, which depends on the choices of both sides of the labor market. The expected quality of the workforce in school j is given by:

$$E[\theta_i | \text{school} = j] = \int_{\theta} \theta_i f_{\theta | \text{school}=j}(\theta_i | \text{school} = j) d\theta.$$

Simulation

We are interested in the allocation of worker quality in the public and private sectors. More specifically, we seek to understand how the allocation of principal effectiveness in a given sector depends on the *selection* parameter β_j and the *pay-for-performance* parameter w_j of the model. For this purpose, we will consider a particular case of the model with only two schools, one private and one public. In this setting, there are only four possible offering configurations from public and private schools $(p, v) \in \{(0, 0), (0, 1), (1, 0), (1, 1)\}$. Thus, the probability that worker i is at a public school given her quality is given by:

$$\begin{aligned}
Pr(A_{ip} | \theta_i) &= \left(\frac{\exp(h_p + w_p \theta_i)}{\exp(h_p + w_p \theta_i) + \exp(h_v + w_v \theta_i)} \right. \\
&\quad \times \frac{\exp(\beta_{0p} + \beta_p \theta_i)}{1 + \exp(\beta_{0p} + \beta_p \theta_i)} \times \frac{\exp(\beta_{0v} + \beta_v \theta_i)}{1 + \exp(\beta_{0v} + \beta_v \theta_i)} \left. \right) \\
&\quad + \left(1 \times \frac{\exp(\beta_{0p} + \beta_p \theta_i)}{1 + \exp(\beta_{0p} + \beta_p \theta_i)} \times \frac{1}{1 + \exp(\beta_{0v} + \beta_v \theta_i)} \right). \tag{18}
\end{aligned}$$

In this case, the expected principal effectiveness in the public school is given by:

$$E[\theta_i | \text{Public}] = \int_{\theta} \theta_i f_{\theta|\text{Public}}(\theta_i | \text{Public}) d\theta \quad (19)$$

From Bayes' rule, we know that:

$$f_{\theta|p}(\theta_i | \text{Public}) = \frac{Pr(A_{ip} | \theta_i) \times f_{\theta}(\theta_i)}{Pr(\text{Public})},$$

where $Pr(A_{ip} | \theta_i)$ is given by (18) and $Pr(\text{Public})$ is a scale factor equal to the fraction of public schools (0.5 in this case). Assuming that $f_{\theta}(\theta_i)$ is a standard normal, we can compute $E[\theta_i | \text{Public}]$ using numerical integration. More importantly, we can study how this object depends on β_p and w_p , two relevant parameters related selection and payment policies in public schools. The results from this simulation are qualitatively similar to those shown by Figure 2 in the main text.

E Data

This project combines students' performance and employer-employee records, provided by the Ministry of Education, with labor market outcomes coming from the Education Superintendency and the Civil Service. The authors did not have access to personal identifiers because the data files were anonymized by the Ministry of Education using a unique number. This appendix describes each data file used in the analysis.

After publication, we are willing to provide the code for replication by others who can independently obtain rights to these data.

Student performance

The Ministry of Education provided access to the performance records of all students between 2011 and 2016. For each student we observe classroom and subject identifiers, as well as an identifier of the teacher by subject and classroom. For all students, we observe course grades by subject. For cohorts of students that take standardized exams, it is also possible to link our data to their test scores in the SIMCE exam. The SIMCE examination is only taken by students in some specific grades, usually 4th, 8th, and 10th grade, and it has not been systematically run every year in the country. Our main specification considers leads and lags of course grades. Thus, we only use 4 years of data (2012-2015). We exclude students for whom the teacher does not change in a given subject from one year to another; and we also exclude classes that had more than one teacher per year as well as the bottom and top one percent of classroom size outliers. All and all, our estimation sample includes 2,127,713 students.

Panel of school workers

The Ministry of Education provided access to a panel of teachers between 2008 and 2017. These records include 13,693 unique schools and 331,167 unique teachers. For each worker we observe the following characteristics: gender, age, tenure in the system, certification, type of contract, hours of contract, and her occupation within the school. Based on the latter, we identify the principal in each school by year. In cases with more than one principal in a given year, we choose the one with more hours of contract in the school (if there is a tie, then we chose the most senior worker).

School characteristics

The Ministry of Education provided access to a panel of 13,693 schools between 2008 and 2017. These records include the following information for each school: type of administration (e.g., public, subsidized-private or private), an indicator if the school is in a rural area, its total enrollment, concentration of disadvantaged students, and the municipality where the school is located. Using the national representative survey CASEN, we add characteristics of the municipality where the school is located. Specifically, we add the following characteristics: average years of education, income per-capita, and the 2011 rates of crime, unemployment, and poverty. Moreover, from SIMCE surveys we were able to recover the shares of low-income and high income parents, and the share of parents with a college degree.

For the analysis, we remove private schools that do not receive vouchers because we do not observe wages for those. Preschools, adults' schools, and special education schools are also excluded. All and all, we end-up with 11,320 schools.

Wages

The Superintendency of Education provided access to a monthly panel of workers from 2015 to 2017. These records correspond to reports that every school receiving vouchers must provide to the Superintendency in order to report the use of public resource. For each worker we observe the school where she is working and detailed data on wages. Specifically, we observe worker's compensation by item. We classify the raw wage as the sum of these items and we also classify these items into three categories:

- Minimum wage: corresponds to a per-hour legal-minimum payment for teachers, defined by the Ministry of Education.
- Statutory payments: include compensations regulated by law but unrelated to performance, such as payments for experience and for teacher certification. We include all payments defined by the Union Law of 1996 as well as other payments defined by subsequent Laws, such as: *Mejoramiento*, *Condiciones Dificiles*, *Profesor Encargado*, *Excelencia Pedagógica*, *UMP*, *Titulo y Mencion*, *Planilla Complementaria*), and other compensations assigned to those who work extra hours, in rural schools, or in schools where it is "difficult" to teach according to the Ministry of Education.

- Bonuses: encompasses compensations related to workers' performance, such as individual and collective performance bonuses (e.g., AVDI), payments from the national system of performance assessment (e.g., AEP, SNED), bonuses paid directly by the school owner in the case of private schools, and other discretionary payments and gratifications related to transportation, food, and holidays.

Complaints against the schools

The Superintendency of Education provided access to all complaints filed against the school between 2014 and September 2018. These records have the number of complaints by category. The categories include: i) bullying and discrimination (also includes behaviors of sexual connotation against students or teachers), ii) denied enrollment (for instance because of disciplinary measures), iii) poor infrastructure (includes lack of furniture), iv) teacher absenteeism (or lack of teachers), v) school accidents, vi) charge of extra fees (or ask for extra materials), vii) resource accountability (irregularities in the use of vouchers or misreporting of attendance).

Complaints are often filed by parents. While teachers could also file complaints through the Superintendency, most of the time their complaints go directly to the Labor Directorate or justice system directly.

Teacher surveys

The Ministry of Education provided access to the survey responses of teachers. Every time students take the nationwide standardized exam SIMCE, teachers must fill a survey created by the Ministry. For our analysis, we only consider questions about the school principal (e.g., The principal does a good job, the principal promotes a good work climate). According to the availability of the questions in each year, we took the surveys from 2009 to 2015 for teachers from 4th, 8th and 10th grade.

In the SIMCE survey every teacher must provide an answer within a range from 1 to 4 (or from 1 to 5 in some years), where 1 represents high disagreement with the statement and 4 (or 5) represents a high level of agreement with it. We use their responses to create a dummy variable at the survey respondent level that equals one if the teacher "highly agrees" with the statement about the principal, i.e., her response is at the top of the specific scale for that question. Then, we take the average across respondents at the school-year level and assign this to the corresponding

school principal.

Civil service

The Civil Service provided access to records of the contest implemented to elect principals in public schools from 2011 to 2016. While these contests are direct responsibility of the municipalities, the Civil Service oversees them and records data on them. For every school we observe a panel of contests. Specifically, we observe when a contest was called and what was the outcome of the contest (whether the position was filled or not). Based on this, we create an identifier at the school-year level indicating if the school chose a principal through the new system each year.

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